

SMITHSONIAN

MISCELLANEOUS COLLECTIONS.

VOL. XXX.



"EVERY MAN IS A VALUABLE MEMBER OF SOCIETY WHO BY HIS OBSERVATIONS, RESEARCHES,
AND EXPERIMENTS PROCURES KNOWLEDGE FOR MEN."—SMITHSON.

WASHINGTON:
PUBLISHED BY THE SMITHSONIAN INSTITUTION.

1887.

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S. F. BAIRD,

Secretary S. I.

SCIENTIFIC WRITINGS
OF
JOSEPH HENRY.

VOLUME I.



WASHINGTON:
PUBLISHED BY THE SMITHSONIAN INSTITUTION.
1886.

INTRODUCTORY NOTE.

IN these volumes the principal scientific writings of Joseph Henry are collected for the first time. They include his contributions to various societies and journals, together with notices of a few of his earlier communications which were never published in full. These productions, extending over a long and busy life, are naturally grouped under two periods: the first comprising the record of his researches from 1824 to 1846 (a period of 23 years), during his professorial career at Albany and Princeton; the second that of his scientific work from 1847 to 1878 (a period of 32 years), during his directorship of the Smithsonian Institution at Washington.

It will be observed that Professor Henry's contributions to science were given to the world from time to time throughout more than half a century, and published in widely remote places. Most of them are now scarce, many are practically inaccessible, hardly any individuals and few public libraries can be supposed to possess them all. It is noteworthy, and indeed is characteristic of their author, that he sedulously abstained from publishing any of his researches of the later period or reproducing any of the earlier ones—very important though he knew them to be—through the inviting channel of the "Smithsonian Contributions," or "Miscellaneous Collections," or in any way at the expense of the Smithsonian fund.

It has seemed to the Regents of the Smithsonian Institution that justice to the scientific name and memory of their distinguished Secretary who made the Institution

what it is, no less than a due regard to the history of physical science in this country, and the interests of its present votaries, require that these writings should now be collected and made available: also that their publication and distribution may be fittingly undertaken by the Smithsonian Institution. Accordingly, at a meeting of the Regents, held January 17, 1883, "Dr. Maclean having called the attention of the Board to the fact that the sundry papers of Professor Henry on scientific subjects had not been published in the series issued by the Smithsonian Institution, it was *Resolved*, That the Secretary be requested to have the scientific writings of Prof. Joseph Henry collected and published." At the next stated meeting of the Board, held January 16, 1884, Dr. Asa Gray, Hon. W. L. Wilson, and Prof. S. F. Baird were appointed a committee to supervise the publication of Prof. Henry's writings.

It was decided by them to include in this collection only the published writings of Prof. Henry, and to arrange these chronologically. A departure from this arrangement has been made in transferring the series of papers detailing Henry's extended observations on the phenomena of sound from the position of their successive dates of publication (1873 to 1877), and interpolating them between papers published in 1855. This has been done for the purpose of equalizing the size of the two volumes. The second volume is thus made to commence with the series of Meteorological Essays, which, also published originally in successive years (from 1855 to 1859), are here presented continuously. These Essays, although of a more popular character than the other writings of the author, contain much original observation and generalization, and therefore well deserve a place in this collection.

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SCIENTIFIC WRITINGS OF JOSEPH HENRY.

PART I.

FROM 1824 TO 1846.

SCIENTIFIC PAPERS AND ABSTRACTS.

CHEMICAL AND MECHANICAL EFFECTS OF STEAM.

(Proceedings of the Albany Institute, vol. I, p. 30.)*

October 30, 1824.

Joseph Henry read a communication on the chemical and mechanical effects of steam, with experiments designed to illustrate the great reduction of temperature in steam of high elasticity when suddenly expanded.

REFRIGERATION BY RAREFACTION OF AIR.

(Proceedings of the Albany Institute, vol. I, p. 39.)

March 2, 1825.

Joseph Henry read a communication on the production of cold by the rarefaction of air, accompanied with experiments.

One of these experiments most strikingly illustrated the great reduction of temperature which takes place on the sudden rarefaction of condensed air. Half a pint of water was poured into a strong copper vessel, of a globular form, and having a capacity of five gallons—a tube of one-fourth of an inch in caliber, with a number of holes near the lower end

*[The "Transactions of the Albany Institute," vol. I, part I, is dated on page 3, "June, 1828." The title page of the volume bears date "1830." Part II of same volume is an "Appendix" of 74 (independently numbered) pages, and comprises brief abstracts of proceedings; here cited as "Proceedings" for conciseness.]

and a stop-cock attached to the other extremity, was firmly screwed into the neck of the vessel; the lower end of the tube dipped into the water, but a number of the holes were above the surface of the liquid, so that a jet of air mingled with water might be thrown from the fountain. The apparatus was then charged with condensed air, by means of a powerful condensing pump, until the pressure was estimated at nine atmospheres; during the condensation the vessel became sensibly warm. After suffering the apparatus to cool down to the temperature of the room, the stop-cock was opened; the air rushed out with great violence, carrying with it a quantity of water, which was instantly converted into snow; after a few seconds the tube became filled with ice, which almost entirely stopped the current of air. The neck of the vessel was then partially unscrewed, so as to allow the condensed air to rush out around the sides of the screw; in this state the temperature of the whole atmosphere was so much reduced as to freeze the remaining water in the vessel; the stop-cock and tube at the same time became so cold that the fingers adhered to them, in the same manner that they are sometimes found to stick to the latch of a door on an intensely cold morning. This experiment was exhibited to the Institute within six feet of a large stove, and in a room the temperature of which was not less than eighty degrees of Fahrenheit's thermometer.

LECTURE ON FLAME.

(Proceedings of the Albany Institute, vol. I, p. 59.)

March 21, 1827.

Mr. Joseph Henry delivered a lecture on flame, accompanied with experiments.

ON SOME MODIFICATIONS OF THE ELECTRO-MAGNETIC APPARATUS.

(Transactions of the Albany Institute, vol. 1, pp. 22-24.)

Read October 10, 1827.

The subject of electro-magnetism, although one of the most interesting branches of human knowledge, and presenting at this time the most fruitful field for discovery, is perhaps less generally understood in this country than almost any other department of natural science.

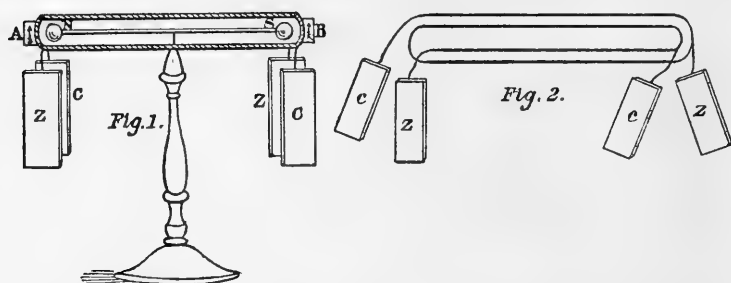
Our popular lecturers have not availed themselves of the many interesting and novel experiments with which it can so liberally supply them ; and, with a few exceptions, it has not as yet been admitted as a part of the course of physical studies pursued in our higher institutions of learning. A principal cause of this inattention to a subject offering so much to instruct and amuse is the difficulty and expense which formerly attended the experiments—a large galvanic battery, with instruments of very delicate workmanship, being thought indispensable. But this bar to the advancement of electro-magnetism no longer exists, several improvements having been made in the principles and arrangement of the apparatus, which tend considerably to simplify its construction and use. Mr. Sturgeon, of Woolwich, who has been perhaps the most successful in these improvements, has shown that a strong galvanic power is not essentially necessary, even to exhibit the experiments on the largest scale. On the contrary, he has proved that it may be almost indefinitely diminished, provided the magnetic force be proportionately increased. On this principle he has constructed a set of instruments, with large magnets and small galvanic elements, which, from their size and the facility of their operations, are well calculated either for the private study or the public lecture room.*

Mr. Sturgeon's suite of apparatus, though superior to any other, as far as it goes, does not however form a complete

* Annals of Philosophy, new series, vol. 12, page 375.

set; as indeed it is plain that his principle of strong magnets cannot be introduced into every article required, and particularly into those intended to exhibit the action of the earth's magnetism on a galvanic current, or the operation of two conjunctive wires on each other. To form therefore a set of instruments, on a large scale, that will illustrate all the facts belonging to this science, with the least expense of galvanism, evidently requires some additional modification of the apparatus, and particularly in those cases in which powerful magnets cannot be applied. And such a modification appears to me to be obviously pointed out in the construction of Prof. Schweigger's Galvanic Multiplier:* the principles of this instrument being directly applicable to all the experiments in which Mr. Sturgeon's improvement fails to be useful; and to those only can it be successfully applied. The following description of the figures in *Plate I*† will render my meaning sufficiently clear.

Fig. 1, is an apparatus on the plan of the Multiplier, to show the deflection of a large magnetic needle. It consists of a coil of wire, A B, of an oblong form, about ten inches in length and one and a half in width, with a small galvanic element attached to each end; the coil is formed of about twenty turns of fine copper or brass wire, wound with silk,



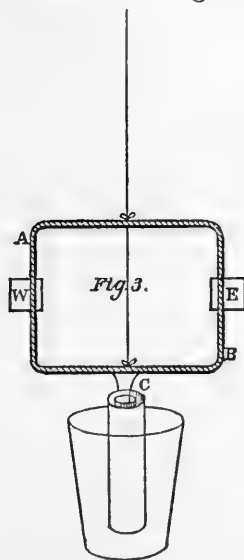
to prevent contact, and the whole bound together so as to have the appearance of a single wire. The attachment of the zinc and copper is more plainly shewn in Fig. 2, which

*See Green's *Electro-Magnetism*, page 30.

†[The figures, copied from the original copper-plate illustration, are here reproduced in the text for facility of reference.]

represents a coil of only two turns of wire: on the left side of the figure the plates are soldered directly to the ends of the wire of the coil; on the right, the plate of zinc Z, is attached to the part of the wire ending with copper on the other side, while the plate of copper on the right corresponds to the zinc on the left. By this arrangement, we can instantly reverse the direction of the currents, and deflect the needle either to the right or left, by merely holding a tumbler of acidulated water so as to immerse one or the other of the double plates into the fluid. The arrows at B, formed of two pieces of card, are intended to show the direction of the currents, and they should point in the course of the wires going from the copper. N S, is the needle, about nine and a half inches long, made by binding together several watch springs, touched separately, so as to form a compound magnet; at the ends are two balls of pith, to show the movement of the needle more plainly. This instrument is complete in itself, and we receive the full effect of the instantaneous immersion of the galvanic element.

Fig. 3, represents a modification of De la Rive's ring on a large scale. A B, is a coil about nine inches by six, with a small cylinder of copper, enclosing another of zinc, without bottoms, soldered to its extremities, which end at c, the whole being suspended by a fibre of raw silk, so as to swing freely in a cup of acidulated water. When this apparatus is made sufficiently light, it invariably places itself, after a few oscillations, at right angles to the magnetic meridian. W and E, are two pieces of card, with letters on them, to show which side of the coil will turn to the east or west: they may be properly placed by recollecting that the current from the copper to the zinc has a tendency to circulate in a direction contrary to that of the sun.



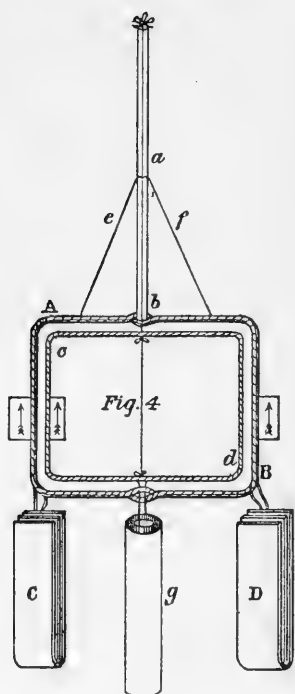
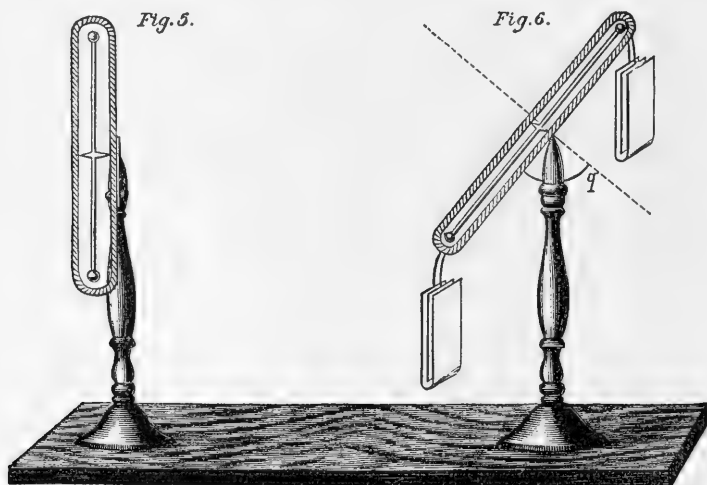
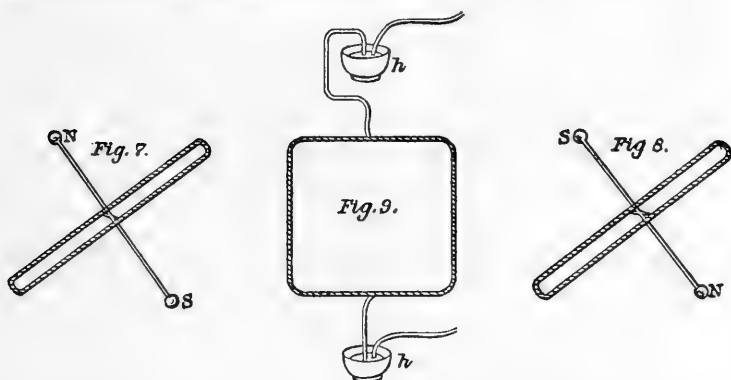


Fig. 4, is designed to show the action of two conjunctive wires on each other; A B, is a thick multiplying coil, with galvanic plates attached, in the same manner as shown in Fig. 2; *c d*, is a lighter coil, with a double cylinder, precisely similar to Fig. 3, and suspended within the other by a fibre of silk, passing through a glass tube, (*a*) the end of which is inserted into an opening (*b*) in the upper side of A B; *e f* are two wires supporting the glass tube. When the cylinder *g* and the plates C are placed in vessels of acidulated water, the inner coil will immediately arrange itself so that the currents in both coils will circulate the same way: if the vessel be removed from C, and D placed in the fluid, the coil *c d* will turn half-way round and again settle, with the currents flowing in



the same direction. Instead of the cylinder, a separate battery of greater power may be used, by suspending the inner coil, as shown in Fig. 9; *h h* are cups with mercury—the upper wire should turn on a fine steel point.

Figs 5 and 6, are front and side views of a modification of an instrument, described by Mr. Sturgeon. It consists of a dipping needle, surrounded by a multiplying coil, turned edgewise, but in all other respects similar to that of Fig. 1. If, when the needle is placed in the magnetic meridian, and the coil in the plane of the dip, a galvanic current be passed through it in a direction opposite to that of the sun, the north end of the needle will turn up, as in Fig. 7; but if in



the contrary direction, it will turn down, as Fig. 8. If the coil be placed at right angles to the dip, as shown in the dotted line, Fig. 6, and the current passed in the first mentioned direction, the needle will not alter its position, but will be more firmly fixed in it: if passed in the contrary direction, it will turn half-way round and dip with its south end. The quadrant *q* permits the coil to be readily placed, either in the plane of the dip or at right angles to it.

TOPOGRAPHICAL SKETCH OF THE STATE OF NEW-YORK, DESIGNED CHIEFLY TO SHOW THE GENERAL ELEVATIONS AND DEPRESSIONS OF ITS SURFACE.

(Transactions of the Albany Institute, vol. I, pp. 87-112.)

Read October 28, 1829.

The Topography of the state of New-York, viewed either in relation to that of the continent of North America in general, or only in reference to the space included within its own political boundaries, presents many interesting and peculiar features.

The two great lakes, and their outlets, forming a natural boundary on the north and west; the continued chain of water communication of the Hudson and Lake Champlain, along the whole eastern section; the connected series of smaller lakes in the interior, together with several large streams which rise in the middle of the state, and pass through its southern boundary; all give to the surface of New-York a diversity of aspect, and a facility of internal navigation, possessed by no other section of our own country, and perhaps not surpassed by any of equal extent on the surface of the globe.

The eastern portion of the United States, designated by geographers as the Atlantic slope, is separated from the central part, or the great valley of the Mississippi, by a marked natural division, consisting of a continuous swell or ridge of land extending from Alabama to the south shore of Lake Ontario. This ridge is the true *water shed* of the country, and determines the course of the rivers falling into the Atlantic on the one side, and those into the Mississippi on the other. It has a mean height of about 3000 feet; and cannot be crossed at any point south of the state of New-York, by an elevation of less than two thousand feet above the ocean. Upon the acclivities of this ridge are based an indeterminate number of spurs, hills, and collateral subordinate ridges, which often rise to a much greater height than the crest of the *water shed*. These subordinate ranges are not continuous, but are often cut through by the Atlantic rivers: They have,

however, nearly the same direction as the main ridge; and in passing through North-Carolina and Virginia, assume the form of four principal ranges, nearly parallel to each other. The three westernmost of these mingle together in the northern part of Pennsylvania, and form a mountain chain, which diverges to the east from the great water shed, and in passing through the state of New-York, occupies the space between Seneca lake and the Hudson river. At first sight, it appears to terminate at the valley of the Mohawk; but it soon rises again on the north side of the river, and forms the mountain district between Ontario and Champlain; is afterwards cut through by the valley of the latter, and then passes on towards the sources of the Connecticut. The remaining ridge of the four parallel ones continues separate from the others, and suddenly turns to the east in Pennsylvania, crosses the state of New-Jersey, and is deeply cut through by the Hudson at West-Point, where it forms the highlands of that river. It afterwards passes to the north in nearly a straight line, and forms the dividing ridge between the waters of the Hudson and those of the Connecticut: at the sources of the latter, it mingles with the other mountain chain, and they then together pass on to the northeast, and may be traced even to the coast of Labrador. The opening between these ridges forms a long, deep, and narrow valley, in which is situated the part of the Hudson river between West-Point and Glen's Falls, and the whole of Lake Champlain. South of this state, the several collateral ridges are cut through by the Susquehanna, the Potomac, and several other streams of less magnitude, which rise near the crest of the water shed, and flow with a rapid descent to the ocean. This fact has been stated as something peculiar in the topography of our country, and has given rise to the fallacious hope of finding practicable canal passes through the river vallies from the waters of the Atlantic to those of the Mississippi; but the water shed, in its uninterrupted continuity, every where rises as an insuperable barrier, and the lowest pass yet found south of New-York is elevated more than 2000 feet above the ocean. As a whole, these mountains are known

by the name of the Appalachian system; but the parallel ridges are perhaps most generally referred to as the Alleghanies; and these again, in their course, have received different local names, such as the Blue Ridge in Virginia, the Catskill in New-York, and the White Mountains in New-Hampshire. From the above sketch of the great mountain system of our country, the peculiar topographical features of the state of New-York will be readily understood.

The Appalachian system may be said to occupy the principal part of the state; and, indeed, through the whole district, the mountains appear to be only partially interrupted by the vallies of rivers, or depressed by the basins of lakes. The entire surface may perhaps be best described as an elevated tract of country, with indentations in various places below its general level. The most important depressions of the surface are the great basins in which are situated the Lakes Erie and Ontario, and the long narrow valley which contains the Hudson river and Lake Champlain. The two last are connected with each other by a valley occupied by the Mohawk river and the Oneida lake; and with it may be considered as separating the whole mountain system of this state into three principal divisions. The first of these, and the largest of the whole, occupies the space situated south of the Mohawk river and the Ontario valley, and between the Hudson river and Lake Erie. The second is the mountain district north of the Mohawk, and between Lake Champlain and the east end of Lake Ontario. The third division comprises that part of the mountain range on the east side of the Hudson river included within this state. The first division is separated into two parts, by the basins of Seneca and Cayuga lakes, and by an elevated valley extending from the head of the former to the valley of the Chemung or Tioga river, at Newtown.

The western subdivision, or the part of the state between Seneca lake and Lake Erie, is occupied by that portion of the mountain system which we have called the *water shed*. This, in its course from the south, in Pennsylvania and New-York, forms a high table land of about two thousand feet in

mean elevation. The highest part of it comprises the surface of the counties of Steuben, Allegany, Cattaraugus and Chautauqua; and a little to the north of these, it begins to decline, and finally descends, by three principal steps, to its terminations on the south shore of Lake Ontario. The great elevation and geographical importance of this table, may be inferred from the fact, that it gives rise to several streams of water, which find the level of the ocean at points almost as distant as the extremities of the continent. The head branches of the Allegany, of the Genesee, and of the Susquehanna, are all found inosculating with each other in the county of Allegany; while their waters separately mingle with the ocean in the gulf of St. Lawrence, the Chesapeake bay, and the gulf of Mexico. But the following heights, from actual survey, will serve to give a more definite idea of its general elevation.

Chautauqua lake, the largest* sheet of water on this table, and the most elevated of its size in the United States, is 1291 feet above the level of the ocean, and 723 feet higher than Lake Erie, although only eight miles distant: its discharged waters descend to the ocean, along the western declivity of the water shed, through the Ohio and the Mississippi rivers. The lowest pass to the east, over a swell of land near Casadaga outlet in Chautauqua county, is 1720 feet high; and another pass in the same swell is 1972 feet. The lowest notch in the height of land between Elm and Little Valley creeks, in Cattaraugus county, is 1725 feet; and between Little Valley and Big Valley, the lowest pass is 2144 feet above the level of the ocean. Franklinville has an elevation of 1580 feet, and Angelica 1428 feet, although both are situated in vallies. This height of land extends close to the shore of Lake Erie, as it may be seen by the map, that one of the head branches of the Allegany, a tributary of the Ohio, rises within four or five miles of the lake. The surface is not broken, but consists of large swells of land, with broad shallow vallies intervening. The principal indenta-

*It is 18 miles long, contains 16,000 square acres, and discharges 2295 cubic feet of water per minute.—*Whippo's Report*.

tion of the surface, is the valley of the Genesee river, which may be considered as an arm of the Ontario valley, extending into the state of Pennsylvania. The extreme southern branches of this river rise at an elevation of more than 2500 feet.

The space between Seneca lake and the Hudson, and south of the Mohawk, is occupied by the mountain chain formed by the union of the three parallel ridges before mentioned, as mingling in Pennsylvania, and passing through New-York. The surface is much more uneven than that of the part just described, and presents the general appearance of a number of ridges in a north and south direction. The highest of these is the Catskill mountains, which bound the valley of the Hudson on the west, and rise in some places nearly 4000 feet higher than the level of the ocean. The Round Top is 3804, and the High Peak is 3718 feet, above the level of the tide waters of the Hudson.* The principal indentations of the surface of this subdivision of the mountain part of the state, are the vallies of the Susquehanna, the Delaware, and their several branches. By a reference to the map, it will be seen that the Chemung river, the main branch of the Susquehanna, and the Delaware river, when viewed in connexion with each other, present an almost entire water course, extending along the Pennsylvania line, from Painted Post, in Steuben county, to the northwest angle of the state of New-Jersey, the only interruption being the space between the Delaware and the Susquehanna. The vallies in which these rivers are situated, cross the mountains in an east and west direction; but their several tributaries, viz., the two branches of the Susquehanna, the Unadilla and the Chenango rivers, the Owego and the Cayuta creeks, besides several smaller streams, descend to the south, and intersect the principal vallies in a remarkable manner, nearly at right angles to their general course. These streams all rise on a narrow table land, which is situated a little south of the line of the Erie canal, and may be traced on the map as forming the water shed, between the heads of streams

*As measured by Capt. Patridge.

flowing to the north and the south, in an uninterrupted course, from the Catskill mountains to the head of Seneca lake. Along the summit of this table land, are a number of small, but highly elevated lakes, which give a peculiar character to this region. The first of these, from the east, and the largest of the whole, is Otsego lake, the outlet of which forms the Susquehanna river. It is a beautiful sheet of water, surrounded by high hills; is nine miles in length, three in breadth, and elevated 1193 feet above the surface of the ocean. The next is Schuyler's lake, which also gives a branch to the Susquehanna: It is situated a few miles to the west of Otsego lake, in the same county; its exact elevation is not known, but it cannot be less than 1200 feet. The other lakes worthy of notice on this table land, are Cazenovia, Skaneateles and Owasco. These are on the northern declivity, and discharge their waters to the north: they are scarcely as much elevated as the two just mentioned; the first being about 900 feet, the second 840, and the last 670 feet above the level of the ocean. It might be supposed, by an inspection of the map, that Cayuga and Seneca lakes were also highly elevated on this table land; but this is not the case, as the former is only 387 and the latter 447 feet above the level of tide. They in reality occupy two long narrow ravines, which deeply indent the surface of the adjacent country, and are separated from each other by a ridge which rises to the height of more than 800 feet above Cayuga lake. The smaller lakes above mentioned are situated several hundred feet above the highest level of the Erie canal, and form inexhaustible reservoirs to supply it with water.

It may be here remarked, that this is an advantage possessed by no other canal route in this country, as it is a curious feature in the physical geography of the United States, that except in the swamps along the southern sea coast, no lake is to be found east of the Mississippi and south of the latitude of the southern boundary of New-York, while almost every river north of this degree issues from a lake or a pond.*

* Gallatin's Report.

The following tables of ascents and descents will serve to give a correct idea of the general configuration of the surface of the whole of the first division of the state, or that part situated between the Hudson and Lake Erie.

No. 1, is a section in an east and west direction from the Hudson to Lake Erie. It commences at the level of tide in the river, and passes over the several ridges to the village of Bath, in Steuben county, and then crosses the high table land to Lake Erie.—No. 2, also begins on the Hudson, at Kingston landing, and follows principally the valleys of streams along the Pennsylvania line to Bath, where it intersects with No. 1.—Nos. 3, 4, 5, 6, 7, 8 and 9, are sections at right angles to Nos. 1 and 2. The five last, pass from points on the south shore of Ontario up the slope of the great depression which contains this lake, to the summit of the table land, and then down the valley of streams to the Susquehanna and the Allegany rivers.—No. 3, is from a point in the valley of the Mohawk, and passes over the ridge to the head waters of the Susquehanna, and then descends this river to the Pennsylvania line.—No. 4, extends entirely across the state, from the St. Lawrence to the Susquehanna river, and exhibits the deep depression of the Mohawk valley below the level of the ridges on each side.

The several distances given in these tables are in most cases straight lines, measured from point to point on a map, but the elevations are all from actual surveys, made at the expense of the state.

The elevations in table No. 1, between the Hudson river and Bath, are from the survey of William Morell, Esq. The remaining elevations of this table, as well as those in No. 2, are from the personal survey of the writer of this article. The elevations in both these tables were taken under the direction of Messrs. Hammond, Morell and Pitcher, as commissioners to explore the route of a state road through the southern tier of counties, in 1825. No. 3, is from the survey of Dr. William Campbell and De Witt Clinton, Jun. The remaining six tables were taken from the reports and maps of Messrs. Geddes, Roberts, Hutchinson, Young and Whippo,

engineers employed by the canal commissioners to explore the routes of 15 proposed canals, in 1825.

It must be premised with regard to these heights, that as they are points on routes explored for roads and canals, they are the elevations of the lowest passes near the line of survey, and are consequently less than the general height of the several ridges.

No. I.—*Table of Ascents and Descents across the Ridges from Catskill, on the Hudson, to the Village of Bath, in Steuben County, and thence to Lake Erie.*

Route.	Miles.		Feet.	
From the Hudson river, at Catskill, to Madison village -----	4	rises	184	
Cairo -----	7	11 rises	226	410
Shinglekill at Cairo -----	0	11 falls	40	370
Catskill mountain summit -----	13	24 rises	1542	1912
Valley of the Schoharie at Gilboa -----	10	34 falls	742	1170
Head waters of the Delaware -----	10	44 rises	716	1886
Delhi on the Delaware -----	18	62 falls	502	1384
Height of land between the Delaware and Susquehanna -----	5	67 rises	759	2143
Susquehanna river at the junction of the Outlet creek -----	17	84 falls	1143	1000
Unadilla river one mile above its junction with the Susquehanna -----	5	89 falls	27	973
Between Unadilla and Chenango -----	6	95 rises	657	1630
Valley of the Chenango at Oxford -----	6	101 falls	669	961
Between Chenango and Tioughnioga or Homer river -----	13	114 rises	133	1094
Valley of the Tioughnioga at the junction of the Otselic -----	6	120 falls	159	935
Between Tioughnioga and Owego creek -----	8	128 rises	445	1380
Valley of the Owego at Richford -----	7	135 falls	285	1095
Between the Owego and the deep valley of Cayuga lake -----	4	139 rises	275	1370
Valley of the Cayuga lake at Ithaca -----	10	149 falls	962	408
Between the Cayuga valley and the Seneca inlet at Catharine landing -----	11	160 rises	849	1257
Catharine landing -----	7	167 falls	801	456
Between the Seneca valley and Mud creek, a branch of the Conhocton -----	9	176 rises	1188	1644
Valley of Mud creek one mile below Mud lake -----	4	180 falls	528	1116
Between Mud creek and Conhocton -----	6	186 rises	463	1579
Conhocton valley at the village of Bath -----	4	190 falls	489	1090
Between Conhocton and Canistota -----	7	197 rises	750	1840
Canistota valley at Arkport -----	9	206 falls	646	1194
Between the Canistota and Genesee -----	8	214 rises	868	2062
Genesee valley at Angelica -----	10	224 falls	634	1428
Between the Genesee valley and Oil creek -----	13	237 rises	59	1487

No. I.—*Table of Ascents and Descents, &c.—Continued.*

Route.	Miles.		Feet.		
Oil creek valley, a tributary of the Allegany--	2	239	falls	39	1448
Between Oil creek and Ellicottville-----	12	251	rises	696	2144
Ellicottville, on a tributary of the Allegany--	8	259	falls	630	1514
Between Ellicottville and the Conewango-----	3	262	rises	621	2135
Conewango valley at the junction of Clear creek,	15	277	falls	885	1250
Between Conewango valley and Chautauque					
lake-----	8	285	rises	716	1966
Chautauque lake-----	18	303	falls	675	1291
Between Chautauque and Lake Erie-----	1	304	rises	61	1352
Lake Erie at Portland harbor-----	7	311	falls	787	565

No. II.—*Table of Ascents and Descents from the Hudson, at Kingston Landing, to Bath, in Steuben County, by the route of the valleys of the Rondout Creek, the Beaver Kill, the east branch of the Delaware, and the east and west branches of the Susquehanna.*

Route.	Miles.		Feet.		
Hudson river, at the junction of the Rondout,					
to Kingston village-----		2	rises	188	188
Warwasing-----	21	23	rises	123	311
Sullivan county line on the Rondout-----	10	33	rises	462	773
Height of land between the Rondout and Nev-					
ersink-----	6	39	rises	896	1669
Neversink river-----	2 $\frac{1}{2}$	41 $\frac{1}{2}$	falls	357	1312
Height between the Neversink and Beaver kill.	2 $\frac{1}{2}$	44	rises	768	2080
Junction of the Beaver kill and the east branch					
of the Delaware-----	24	68	falls	1062	1018
Junction of the east and west branches of the					
Delaware-----	7	75	falls	96	922
Deposit on west branch of the Delaware-----	11	86	rises	82	1004
Height of land between the Delaware and the					
Susquehanna-----	6	92	rises	684	1688
Susquehanna at Windsor-----	4	96	falls	775	913
Height across the Great Bend of the Susque-					
hanna-----	5	101	rises	644	1557
Binghamton on the Susquehanna-----	9	110	falls	721	836
Owego on the Susquehanna-----	18	128	falls	32	804
State line above Tioga Point-----	15	143	falls	19	785
Newtown on the Chemung or Tioga-----	13	156	rises	51	836
Painted Post at the junction of Tioga and					
Conhocton-----	14	170	rises	106	942
Bath on the Conhocton-----	17	187	rises	148	1090

NOTE.—The numbers in the first column of figures are the distances from point to point—those in the second, are the total distances. The third column of figures gives the ascents and descents; and the fourth, the elevations of the several points above the level of tide water in the Hudson.

The last six stations in the above table, or those from Binghamton to Bath inclusive, are along the valley of the two great branches of the Susquehanna. The elevations opposite these stations give 900 feet as the mean height of the bottom of this valley, but the mountains on each side rise from five hundred to a thousand feet higher. These mountains are some of the high ridges whose elevations are given in table No. 1, and which here retain about the same elevation.

No. III.—*Table of Ascents and Descents from the valley of the Mohawk, through Otsego Lake, and down the valley of the Susquehanna to the Pennsylvania line.*

Route.	Miles.		Feet.	
Fort Plain, on Erie canal -----				304
Lake Summit, in Springfield -----		16½	rises 1048	1352
Head of Otsego lake -----	3	19½	falls 159	1193
Along Otsego lake to its outlet -----	9	28½	level	1193
Mouth of Oats creek -----	3½	32	falls 12	1181
Crippen's Ville, at the dam -----	12	44	falls 23	1158
Opposite the mouth of Charlotte river -----	7	51	falls 80	1078
Pennsylvania line -----	59	110	falls 178	900

No. IV.—*Table of Ascents and Descents on nearly a direct line from Ogdensburgh, on the St. Lawrence, to Binghamton, on the Susquehanna, by the way of the Black river, and across the valley of the Mohawk; thence to the head of the Chenango river; and down the same to its mouth.*

Route.	Miles.		Feet.	
Ogdensburgh, on the St. Lawrence -----				226
Indian river, near the village of Antwerp -----		34	rises 233	459
Black river, above the falls at the village of Carthage -----	14	48	rises 234	693
Along the valley of Black river to foot of High falls, near mouth of Moosic river -----	27	75	rises 10	703
Summit between Black river and the Mohawk, near Boonville -----	9	84	rises 432	1135
Erie canal at Rome, and highest part of the Mohawk and Oneida lake valley -----	18	102	falls 710	425
Head of Chenango valley, at Hamilton village -----	26	128	rises 730	1155
Along the Chenango river to the forks -----	42	170	falls 208	947
Binghamton, on the Susquehanna -----	10	180	falls 111	836

No. V.—*Table of Ascents and Descents from Lake Ontario along the Oswego River, through the Tully Lakes, and down the Tioughnioga River to the Susquehanna.*

Route.	Miles.		Feet.	
Lake Ontario, at mouth of Oswego river-----	-----	-----	-----	231
Outlet of Onondaga lake-----	-----	28	rises 130	361
Erie canal at Syracuse-----	7	35	rises 38	399
Tully lakes, town of Tully-----	18	53	rises 795	1194
Forks of the streams near Homer village-----	12	65	falls 98	1096
Chenango forks-----	29	94	falls 149	947
Junction of Chenango and Susquehanna, at Binghamton-----	10	104	falls 111	836

No. VI.—*Table of Ascents and Descents from Little Sodus Bay, on Lake Ontario, to Owego, on the Susquehanna, along Cayuga Lake and the valley of Owego Creek.*

Route.	Miles.		Feet.	
Little Sodus bay, on Lake Ontario-----	-----	-----	-----	231
Montezuma, on the Erie canal-----	-----	31	rises 49	380
Outlet of Cayuga lake-----	6	37	rises 7	387
Along the lake to its head-----	36	73	level	387
Summit between Cayuga lake and Owego creek-----	6	79	rises 594	981
Susquehanna, at Owego-----	20	99	falls 185	796

The elevation of Owego, according to table No. 2, is 804 feet, which differs eight feet from that given in the above table. This small discrepancy is owing to the circumstance of the elevations in these two tables being the results of surveys entirely independent of each other, and which intersect at Owego, after a circuit from the Hudson, of more than 300 miles. Table No. 2, also intersects with No. 4, at Binghamton, and with No. 7, at Newtown. At the former place the difference was only the fraction of a foot, and at the latter less than two feet. These facts show with what precision measurements of this kind can be made, and what reliance may be placed on the correctness of the elevations of the several points given in these tables.

No. VII.—*Table of Ascents and Descents from Great Sodus Bay, on Lake Ontario, along Seneca Lake and the route of the Chemung Canal to Newtown, on the Chemung or west branch of the Susquehanna River.*

Route.	Miles.		Feet.	
Lake Ontario, at Great Sodus bay-----				231
Lyons, on the Erie canal-----		15	rises 170	401
Outlet of Seneca lake, near Geneva-----	12	27	rises 46	447
Along the lake to its head-----	34	61	level	447
Summit between the lake and Chemung river-----	8	69	rises 443	890
The Chemung at Newtown-----	10	79	falls 53	837

No. VIII.—*Table of Ascents and Descents from Lake Ontario along the valley of the Genessee River, to the mouth of Black Creek in Allegany County, and thence to Olean, on the Allegany River, along Oil and Black Creeks.*

Route.	Miles.		Feet.	
Mouth of Genessee river-----				231
Erie canal, at Rochester-----		8		506
Squaque hill-----	29	37	rises 68	574
Gardow flats-----	6	43	rises 76	650
Head of the Great Falls in the town of Nunda-----	8	51	rises 453	1103
Mouth of Black creek-----	16	67	rises 162	1265
Summit level between Black and Oil creeks*-----	10	77	rises 221	1486
Olean on the Allegany-----	13	90	falls 78	1408

No. IX.—*Table of Ascents and Descents from the mouth of Oak Orchard Creek, on Lake Ontario, in nearly a direct line to Olean, on the Allegany, by the route of Batavia, the Tonnewanta Creek, Lime Lake, and the valley of Ischua Creek.*

Route.	Miles.		Feet.	
Lake Ontario, at the mouth of Oak Orchard creek-----				231
Albion, on the Erie canal-----		8	rises 275	506
Tonnewanta creek, at Batavia-----	17	25	rises 377	883
Attica, along Tonnewanta creek-----	11	36	rises 71	954
Dividing ridge between Tonnewanta and Cataraugus creeks-----	18	54	rises 526	1480
Lime lake †-----	14	68	rises 143	1623
Olean Point, on the Allegany, along the valley of Ischua and Oil creek-----	27	95	falls 214	1409

* This summit is a marsh—the discharged waters of which find the level of the ocean in the Gulf of St. Lawrence and the Gulf of Mexico.

† This lake, according to Mr. Roberts' report, is 1642 feet above tide. According to the same report, Beaver lake, in the town of China, is 1704 feet,

It is evident from these tables, that the mountain system occupies the entire width of the southern part of the state, between the Hudson and Lake Erie. The section given in Table No. 1, exhibits a mean elevation, after the first 13 miles from the Hudson, of 1400 feet, and presents no height less than 935 feet, except at its extremities, and in the two places where the survey descends into the deep ravines in which are situated Cayuga and Seneca lakes. If this section had passed a few miles to the south of the head of Seneca lake, the lowest point would have been 890 feet, which is the highest part of the bottom of a valley extending from this lake to the Chemung river. The mean elevation of the several ridges, crossed by the same section, is 1700 feet. And as these elevations are the lowest notches near the line of the survey, they may be considered as being but little higher than the general elevation of the surface of the country.

The second division of the mountain district of the state, or that on the north side of the Mohawk and Oneida valley, and between Lake Ontario and Champlain, has not been as minutely explored by topographical surveys for roads and canals, as the division we have already described; but the surface is known to be traversed, in a northeast direction, by at least five or six parallel ridges. The position of the principal one of these, beginning in Oneida county, may be traced on the map, between the heads of streams flowing to the right and left of its course through the middle of Herkimer and Hamilton counties, and the northern part of Essex, near the sources of the Hudson. The lowest pass across this ridge, between the valley of the Black river and the head waters of the Mohawk, is shown in table No. 4, and is elevated 1135 feet above the level of tide water. The lowest notch between West Canada creek and the Black river, is elevated 1226 feet, and between Fish creek and Salmon river, near where the ridge commences, the pass is 659* feet high. One of the peaks of this ridge, called the White Face, rises to the height of 2686 feet; and the general elevation of the country in the middle part of Hamilton county, has been

* Judge Geddes' report.

estimated at from 1800 to 2000 feet above the level of the ocean.

The mountains of this section are often described as an isolated group, entirely disconnected from the Appalachian system, which is generally considered as terminating in New-York, at the valley of the Mohawk river and Oneida lake. But when we view their relative positions, and the general direction of their several ridges, we must at once be convinced that they are, with all the other mountains in this state, only a part of the great chain which traverses the United States from Alabama to Maine. Indeed, the existence of a separate mountain group in any part of our national territory, has been reasonably doubted; and, strictly speaking, such a phenomenon is perhaps not to be found on the surface of the globe.

The third division, or that portion of the state on the east side of the Hudson, is situated principally on the western acclivity of the ridge which has been described as continuing distinct from the other subordinate ridges of the mountain system, and crossing the Hudson in the vicinity of West-Point, forming the Highlands of the river, and afterwards the dividing ridge between the Hudson and the Connecticut. The crest of this ridge passes to the north, on the east side of the boundary of New-York, in New-England, and has a mean elevation of more than 2000 feet. One of the lowest notches yet explored, is at Washington summit, in Massachusetts, on the route of the contemplated rail-way from Boston to Albany, and is elevated 1480 feet above the level of tide water in Boston harbor. This mountain range is known by various names in different parts of its course: before it crosses the Hudson, it is called the Blue Ridge; in Massachusetts and Connecticut, the Taghonnuc Range; and in Vermont, the Green Mountains. But as it lies principally without this state, a more particular description would be foreign to our purpose.

From the foregoing sketch, the truth of our remark must be evident, that the whole surface of the state of New-York is a mountain tract of country, indented in several places

below its general level, by the great depressions, in which are situated the waters of its principal lakes and rivers. The most important depressions, as we have already observed, are the basins of Lake Erie and Ontario, the valley in which is situated the Oneida lake and the Mohawk river, and that which contains the Hudson river and Lake Champlain. The basins of Lake Erie and Ontario are only parts of the immense St. Lawrence basin, which contains the five great western lakes, and bounds a principal part of the northern frontier of the Union. As this interesting depression of country is intimately connected with the topography of this state, we will dwell a few moments on some of its general features. Commencing at the Gulf of St. Lawrence it extends almost to the head waters of the Mississippi, a distance of nearly 1800 miles. In its whole depression it is computed to contain 511,930 square miles of surface, 72,930 of which is covered with water. It may be described as consisting of three great but unequal divisions; the upper, the middle, and the lower sub-basins. The first of these is in the form of a rhomb, and has an area of about 90,000 square miles, more than one-fourth of which is occupied by the waters of Lake Superior. The next, or middle sub-basin, occupies a quadrangular area of at least 160,000 square miles, and contains the three central lakes, viz: Huron, Michigan and Erie, in its lowest depressions. The surface of the lower sub-basin has an area of about 260,000 square miles, and is covered in part by the waters of Lake Ontario and St. Lawrence river.

Lakes Michigan and Huron are immense chasms, the bottoms of which, in some places, sink to the almost incredible depth of 1000 feet below their surface, and more than 300 feet below the level of the ocean. This is an interesting fact in the physical geography of the country; as these lakes are probably the lowest depressions on the continental surface of the earth. The surface of Lake Erie is elevated 565 feet above the level of the Atlantic ocean, 76 below Lake Superior, and 35 lower than the general level of Michigan and Huron. Its bottom, which is seldom depressed more

than 200 feet below its surface, is composed of alluvial deposit, probably washed down from the upper lakes by the continued action of a rapid current. Lake Ontario is elevated 231 feet above the level of the ocean: its mean depth has been estimated at 492 feet, although, in the middle, attempts have been made with 300 fathoms without striking soundings.* The St. Lawrence river, which connects this system of lakes with the Atlantic ocean, is the second river in magnitude in America, being no less than ninety miles wide at its mouth, and navigable for ships of the largest size, 400 miles from the ocean: Its whole length, from Lake Ontario to its mouth, is 692 miles.†

The following table, compiled from Darby's Geographical View of the United States, gives in a connected form, the elevation and extent of the several waters of the St. Lawrence basin.

No. X.—*Table of Elevation, mean Depth, Length, Breadth and Area, of the several collections of Water in the great St. Lawrence basin.*

	Elevation above tide level.	Mean depth.	Mean length.	Mean breadth.	Area.
	<i>Feet.</i>	<i>Feet.</i>	<i>Miles.</i>	<i>Miles.</i>	<i>Sq. miles.</i>
Lake Superior.....	641	900	300	80	24,000
Lake Huron.....	596	900	200	95	19,000
Lake Michigan.....	600	900	300	50	15,000
Lake Erie.....	565	120	230	35	8,030
Lake Ontario.....	231	492	180	30	5,400
River St. Lawrence and smaller lakes.....	-----	20	-----	-----	1,500
Total water surface.....	-----	-----	-----	-----	72,930

The several slopes of the St. Lawrence basin, not covered by water, have been estimated to be sufficient to sustain a population of thirty millions of inhabitants. But the most interesting fact connected with this great depression is the vast quantity of fresh water contained in its several reser-

*Dr. Bigsby's sketch of the topography of Lake Ontario. *Philosoph. Mag. and Annals*, vol. 5, page 4.

†Darby.

voirs. From the data furnished by the above table, which may be considered as an approximation to truth, we find that the whole amount of water is 10,500 cubic miles; more than one half of the fresh water on the surface of the globe.*

The discharged waters of the upper lakes, in passing from the middle to the lower sub-basin of the St. Lawrence, are precipitated over the great falls of Niagara. This celebrated cataract has been rendered so familiar to almost every person, by the pen and pencil of the many travellers who have visited it, that a formal description, in this sketch, would be entirely unnecessary. About 20 miles below Lake Erie the Niagara river narrows, and the rapids commence: these are of such force and velocity, that their noise, agitation and fury constitute an object of as much curiosity as the falls themselves. On the very brink of the precipice, is situated Goat island, which contains about eighty acres, and extending up the stream, divides the waters. At this place the Niagara river, nearly half a mile wide, and flowing with immense velocity, is precipitated headlong over a perpendicular ledge of rocks, into an almost unfathomable abyss below. The height of the falls, from the surface of the water above to that of the water below, is 151 feet on the Canada side, and 164 feet on the American. The descent of the country from Lake Erie to Ontario, is principally by a step, not at the falls, but at Lewiston, several miles below. The surface on each side is a level plain, through which the Niagara river passes below the falls, in a deep chasm, nearly a mile wide, with almost perfect mural sides. In viewing the position of the falls, and the features of the country around, it is impossible not to be impressed with the idea, that this great natural race-way has been formed by the continued action of the irresistible current of the Niagara, and that the falls, beginning at Lewiston, in the course of ages have worn back the rocky strata to their present site.

The distances and descents along the Niagara river, from Lake Erie to Lake Ontario, from actual survey, on the American side, are as follows:

*See Edinburgh Encyclopædia, Article "Phys. Geog.," page 605.

From Lake Erie to the head of the rapids.....	distance 20 miles, fall 15 feet.	
Thence to the falls.....	1	51
The falls.....	--	164
From the falls to Lewiston, at the mouth of the chasm	7	104
Thence to Lake Ontario.....	7	2
Total.....	35 miles, fall 336 feet.	

The annexed table of elevations and distances, through the whole extent of the St. Lawrence basin, in connexion with the tables already given, will show its depression below the mountain surface of the country.

No. XI.—*Table of Ascents and Distances through the St. Lawrence basin, from the Gulf of St. Lawrence to the western angle of Lake Superior.*

Route.	Miles.		Feet.	
Up St. Lawrence river to the head of tide water		450		
Lake Ontario level.....	200	650	rises	231
Lake Erie level.....	175	825	rises 334	565
Lake Huron level.....	340	1165	rises 31	596
Lake Superior level.....	240	1405	rises 45	641
Mouth of St. Louis river into the western angle of Lake Superior	380	1785	level	641

The slopes of the lower subdivision of the St. Lawrence basin, which descend to the shores of Lake Ontario, occupy a considerable portion of the state of New-York. Beginning near the eastern extremity of Lake Erie, the boundary or edge of this sub-basin may be traced on the map along the heads of streams falling into Lake Ontario, through the southern part of the counties of Erie and Genessee, to the valley of the Genessee river, which is an arm of the St. Lawrence basin, stretching up into the high lands of Pennsylvania. From the Genessee river, the edge of the basin curves to the southeast around the southern extremities of Seneca and Cayuga lakes, including the four smaller lakes which lie a little to the west of these. The deep ravines in which are situated Seneca and Cayuga lakes may also be

considered as arms or branches of the principal basin, separated from each other by a high ridge. From the head of Cayuga lake, the edge of the basin turns suddenly to the north along the lake, and passes in a northeasterly direction through the northern part of Cortland county, a little south of Skaneateles lake, in nearly a straight line to the Little Falls on the Mohawk river. Here it suffers, for the first time in the course that we have described, an interruption, and an outlet appears to have been forcibly broken through into the lower valley of the Mohawk, by some tremendous convulsion of nature. From the Little Falls, the edge of the basin may be traced along the sources of the Mohawk river, Fish creek and the Salmon river, to the valley of the Black river, which may be considered a branch of the St. Lawrence basin, extending back almost to the valley of the Mohawk. From the Black river to St. Regis the remaining part of the basin in this state is the narrow slope of land along the St. Lawrence river, and the several valleys through which descend the Grass, the Racket, and the St. Regis rivers.

From the foregoing description of the southern boundary of the lower subdivision of the St. Lawrence basin, it evidently comprises the richest and most fertile part of the state, and includes the minor basins of the Genesee country, of the Oneida lake, and the valley of the Mohawk river as far east as the Little Falls. It is also evident from the data before given, that the mean elevation of the high land, forming the boundary just described, must be at least 1600 feet above the level of the ocean. On the north side of the lake in Canada,* the edge of the basin probably rises to nearly the same height, and as the bottom of Lake Ontario, in the deepest places, sinks 900 feet below its surface, or more than 600 feet below the level of the ocean, it follows that this collection of water occupies the lower part of an immense hollow, the deepest depressions of which are more than two thousand feet below the general level of the surrounding mountain surface. As this hollow is situated with its longer diameter directly across the mountain system, it lays bare to

*See Bigsby's Sketch.

the view on its southern side the different strata of rocks which deeply interlay the surface of the country to the south, and presents a geological section in this state, perhaps not less interesting than that at Paris, London, or Rome.

The lowest pass from the ocean into the St. Lawrence basin throughout its whole extent, except the bed of the St. Lawrence river, is through the valleys of the Hudson and Mohawk rivers. The highest part of this pass is near the Little Falls, and is elevated only 425 feet above the level of tide water.

The elevations of the lowest passes to the south, between the waters of Lake Ontario and those of the Susquhanna and the Allegany rivers, are given in tables Nos. 5, 6, 7, 8 and 9. The lowest of these is shown in table No. 7, where the Seneca lake approaches to within 18 miles of the Chemung river, and is separated from it by an intervening elevation of 443 feet above the lake, or 890 feet above the ocean. The pass through which the Ohio canal is being directed is 395 feet above the level of the lake. But the lowest pass to the south from any of the western lakes is that between the Chicago, a small stream emptying into the southern end of Lake Michigan, and the river Des Plaines, a branch of the Illinois. The summit is here only 17 feet above Lake Michigan, or about 617 feet above the ocean.* This is the most surprising and important hydrographical feature of our country; as, comparatively speaking, it here requires but a slight effort of art to give a new outlet to the upper lakes, and to divert a portion of the waters of Superior and Michigan from their present channel of the St. Lawrence to that of the Mississippi. Indeed, two of the plans reported by the canal commissioners of the State of Illinois, are to cut entirely through the barrier, and to supply the summit of a canal through this pass with water directly from Lake Michigan.

From the elevations of the several notches in the height of land that surround Lake Ontario, we may infer the curious fact, that if a narrow barrier of sufficient height were to exist

* Report of the Canal Commissioners of the state of Illinois, 1825.

across the St. Lawrence river above Quebec, and another at the Little Falls on the Mohawk, Lake Ontario would be on the level of Lake Superior; the falls of Niagara would disappear, and these two lakes would be merged in one immense inland sea. That this has actually been the state of things at some remote period in the history of our globe, is a favorite opinion of many; and indeed the appearance of the two outlets, particularly that at the Little Falls, and the nature of the surface of the different slopes of the lower basin, are not unfavorable to this hypothesis.*

No. XII.—*Table of Ascents and Distances on the line of the Erie Canal, through the Mohawk valley, from the mouth of the river to Little Falls, and thence along the St. Lawrence basin to Lake Erie.*

Route.	Miles.		Feet.		
Mouth of the Mohawk to Schenectady -----	-----	21	rises	226	226
Head of Little Falls -----	58	79	rises	142	368
Beginning of the long level of Utica -----	12	91	rises	57	425
Along that level to its end, near Syracuse -----	69½	160½	level		425
Montezuma, at the Seneca river -----	36½	197	falls	45	380
Beginning of Rochester level -----	64	261	rises	126	506
Along that level to Lockport and Lake Erie level -----	63	324	rises	59	565
Along that level to Lake Erie -----	30	354	level		565
The whole length of the canal, from Albany to Lake Erie, is 363 miles. The junction of the Hudson and Mohawk is nine miles above Albany.					

That part of the above section between Utica and Lake Erie, presents a remarkable uniformity of elevation, with only one intervening depression of 45 feet at the Seneca river. The great length of its levels is also a striking feature of the Erie canal: the Utica level is 69½ miles long, and the Rochester level extends a distance of 63 miles. These facts, however, are both readily explained from a consideration of the circumstance that the canal passes from the Little Falls to Lake Erie along the slope of the St. Lawrence basin, the gradual descent of which to the north is highly favorable to the graduation of a line to the most uniform elevation.

*Appendix to Cuvier's Theory of the Earth, American edit., page 332.

The following are the elevations of the principal lakes in this state, included within the boundaries of the lower sub-basin of the St. Lawrence:

	Above Lake Ontario.	Above tide water.
Crooked lake in Yates and Steuben counties--	487	718
Canandaigua lake-----	437	668
Seneca lake at Geneva -----	216	447
Cayuga lake -----	156	387
Oneida lake-----	144	375
Cross lake-----	139	370
Onondaga or Salt lake -----	130	361

The discharge waters of all these reservoirs pass into Lake Ontario, through the Oswego river.*

After the lower sub-basin of the St. Lawrence, the principal depression of surface connected with the topography of this state, is that containing the Hudson river and Lake Champlain. This depression is a long, deep and narrow vale, extending through the country, in a direct line from the ocean near New-York, to the valley of the St. Lawrence river, a distance of 380 miles. That part north of the Highlands at West-Point, is formed by an opening between two of the Allegany ranges; and is bounded on the one side by the Catskill ridges and the mountains on the north side of the Mohawk, and on the other by the range which we have described as forming the separating ridge between the Hudson and the Connecticut. There are only three lateral passes from this valley. The most important of these is the lower valley of the Mohawk, which may be considered as an arm of the Hudson and Champlain valley, extending back as far as the Little Falls; and thus forming a pass from the Hudson, through the Appalachian mountains, into the great St. Lawrence basin. The highest part of this pass, as we have before observed, is only 425 feet above tide water. The next pass is the valley through which the Delaware and Hudson canal has been constructed. It extends from the Hudson, near the village of Kingston, to the Delaware river; and is elevated in the highest part, 500 feet above the level of the Hudson. The other pass is also between the same rivers,

* It is a curious fact, that this river is the common drain of 15 lakes.

and is through a spacious valley bounded by the Catskill ridge on the one side and the mountains forming the Highlands on the other. The elevation of the summit is 430 feet above the Hudson and 207 above the Delaware.

The most remarkable and peculiar feature of the Hudson and Champlain valley, is its great and uniform depth below the general level of the surface of the adjoining country. The highest part of the bottom of this valley, throughout its whole extent, is on the intervening space between the Hudson and Lake Champlain, and is elevated only 147 feet above the level of tide in the river, and 54 feet above the surface of the lake. From this surprising fact, we learn that an obstruction in the channel of the Hudson at the entrance of the Highlands, near Newburgh, of only 150 feet in height, would turn the current of the river to the north, and cause its waters to descend to the gulf of St. Lawrence, through the outlet of Lake Champlain and the St. Lawrence river. The appearance of the mountain pass at the Highlands, is highly favorable to the supposition, that the Hudson has in reality forced its way through this impeding barrier, and thus gained a more direct passage to the ocean.

It has been justly remarked by an able geographer, that there is but one pass on the earth having a specific resemblance to this valley. Scotland is divided into two unequal sections, by what is well expressed by the term *glen*, signifying a deep vale between high and steep hills. This glen extends from the Atlantic ocean to the German sea, a distance of 120 miles, and has no summit higher than 70 feet, although bounded on each side by high mountains. Each of these passes is occupied by lakes and rivers which follow the general direction of the glen, and both have been rendered navigable by means of canals and other artificial improvements.

Viewed as a whole, the Hudson and Champlain valley may be more minutely described as consisting of two unequal sub-basins: the one containing Lake George, Lake Champlain, and the Chambly river; the other, the Hudson river below Glen's falls. Lake George is a narrow sheet of

water, lying in an apparent rend in the adjacent mountains; is thirty-four miles long, and from one to three miles wide. It discharges its waters into Lake Champlain, through a descent of nearly 200 feet. Lake Champlain, which forms the most important part of the upper sub-basin, is 109 miles long, and from one-half mile to twelve miles wide; its depth nearly corresponds to that of Huron and Michigan; while its surface is elevated only 93 feet above the level of tide water. Surrounded by imposing mountain scenery, the traveller on this lake imagines himself raised to Alpine heights, and can scarcely be convinced that a descent of less than one hundred feet would depress him to the level of the ocean. Lake Champlain is connected with the river St. Lawrence by the Chambly river on the north, and with the Hudson river on the south, by the artificial communication of the Champlain canal. The intervening distance between the Hudson river and the lake is only 22 miles; but the whole length of the canal, from its junction with the Erie canal, is 64 miles, 39 of which is along the side of the river.

The other division of the Hudson and Champlain valley, is the deep basin of the Hudson; and this may again be described as consisting of two subdivisions. The first of these includes the lower valley of the Mohawk, and the slopes of land on each side of the Hudson, from Glen's falls to the entrance of the Highlands near Newburgh. The sandy plain between Albany and Schenectady, is an upper shelf of the lower valley of the Mohawk, the southern boundary of which is a continuation of the Catskill mountains, and is seen in travelling between these cities, stretching along the horizon in a northwesterly direction towards the Mohawk river. This plain has a mean elevation of 320 feet, and suddenly declines into the valley of the Hudson by a precipitous step nearly parallel to the river. The capitol at Albany is built on the very edge of this step; and the Mohawk, in passing over the same depression, forms the Cohoes or great fall of the river. A similar shelf exists on each side of the Hudson, from Albany down to the Highlands. The country rises abruptly from the river to upwards

of two hundred feet, and then sweeps backwards with a very gentle rise to the mountain chain. On this shelf are situated all the cities and villages along the river, with the exception of Troy, which is the only place on the Hudson erected on the alluvial flat.

The lower or southern sub-basin of the Hudson, is a section of country highly interesting to the political geographer. It includes all that part of the state south of the Highlands, (except Long-Island,) as well as a part of New-Jersey. Its greatest width is from the southern sources of the Raritan river, to the eastern head of Croton river, in Putnam county, a distance of about 100 miles.

No. XIII.—*Table of Ascents and Distances through the Hudson and Champlain valley, from the Ocean, at New-York, to the St. Lawrence River.*

Route.	Miles.		Feet.	
New-York to the mouth of the Mohawk-----	-----	154		
Level at Stillwater-----	14	168	rises 99	99
Level at Fort Miller-----	17	185	rises 18	117
Beginning of summit level at Fort Edward, nearly opposite to Glen's Falls-----	8	193	rises 30	147
Along that level to Fort Ann-----	12	205	rises	147
Lake Champlain, at Whitehall-----	12	217	falls 54	93
Along the lake to its outlet, near the 45° of north lat.-----	110	327	falls 33	90
Down the Chamblay or Sorel river to its junc- tion with the St. Lawrence, 40 miles above the head of tide water-----	70	397	falls 55	35

The Hudson river, which occupies so important a part of the Hudson and Champlain valley, is in itself one of the most interesting water courses on the surface of the globe; and as a navigable inlet to the vast and fertile regions of the west, demands a more particular notice than the limit of this article can afford to any other river in the state. It is formed of two principal branches: the Hudson proper, and the Mohawk. Each of these deserves particular attention, as contributing to supply the waters of our northern and western canals.

The Mohawk rises west of Oneida lake, flows south about twenty miles, and then suddenly turns to the southeast at Rome, where it falls on the bottom of what has been called the upper valley of the Mohawk. At this place, in high floods, the waters of the river divide: one part passing down the channel to the Hudson, and the other through Wood creek into Oneida lake, and thence to Lake Ontario. From Rome to the foot of Little Falls, a distance of 37 miles, the river descends 97 feet. Here the river descends through a narrow pass to the lower valley of the Mohawk, and offers incontestible evidence of having forcibly broken its way through the primitive rocks: the ledges on each side bear striking marks of the action of water at a height of more than 40 feet above the present level of the stream. The whole fall of the river, from Rome to its mouth, as may be seen by table No. 5, is 425 feet, in a distance of 116 miles; 78 feet of this descent is passed by the cataract of the Cohoes, one mile above its junction with the Hudson.

The two most remote branches of the Hudson proper, have their sources in the marshy regions of Hamilton and Essex counties. These united with each other, and the Sacandaga river, form a stream of considerable magnitude, which is first precipitated over a ledge of rocks called the Great falls, and afterwards down Glen's falls into the deep valley of the Hudson and Champlain basin. The length of what may be called the upper Hudson, from its extreme source to this place, is about 120 miles; and from here to its junction with the Mohawk is 40 miles, with a fall of 147 feet.

The Hudson, after its reception of the Mohawk, from its peculiar character, has been defined by some geographers as a long narrow bay. The periodical rising of the tides to the height of two feet at Albany—the great volume of water, and the gentleness of the current, which, under ordinary circumstances, is reversed by the ascending tide, are indeed the several characters of a bay; but it nevertheless possesses all the distinctive properties of a river, and when swelled by the spring floods, pours a rapid and immense torrent to the ocean. The oscillation of the tide in this river, is an

interesting phenomenon. It is not caused, as in the main ocean, by the direct action of the sun and moon, but is produced by a vast wave, propelled by the force of the Atlantic tide, along the slightly inclined plane of the bed of the river. The crest of this wave passes through the whole distance of 151 miles, between New-York and Troy, in from seven to nine hours.

The comparative importance of the Hudson, as a great commercial inlet to the western territory of the union, may be inferred from the fact, that it is the only Atlantic river, with the exception of the St. Lawrence, that has not its navigation soon interrupted by a precipitate descent from the mountain chain. At the Highlands the Hudson penetrates the primitive rock, and admits the ocean tide one hundred miles to the interior of the ridge, at whose foot, in every other Atlantic river, it is stopped.* Its tributary, the Mohawk, as we have seen, occupies the bottom of a depression which deeply indents the remaining ridges of the Appalachian mountains, and thus connects by an easy pass the valley of the Hudson with the basin of the St. Lawrence. Nature has thus done more by the valleys of the Hudson and the Mohawk, and that to the south of Lake Michigan, towards uniting the waters of the Atlantic with those of the Mississippi, than the utmost efforts of art can ever hope to accomplish in any other part of the country.

The importance of these peculiar topographical features was duly appreciated by the projectors of our canal policy, and the Erie and Champlain canal, with those in contemplation for uniting the former with the waters of the Susquehanna and Lake Ontario, fully develop the natural facilities for internal navigation possessed by this state.

In a physical point of view, these works produce changes which it could scarcely have been believed that the power of man could have accomplished. The waters of the Tioga river, which now entirely contribute to swell the volume of the Susquehanna, by the construction of the artificial channel of the Chemung canal, will in part be conducted to

* Gallatin's Report.

Seneca lake, and thence with the discharged waters of this reservoir, to the gulf of St. Lawrence. On the summit level of the Champlain canal, the waters of the upper Hudson are turned back to the north, and instead of mingling, as formerly, with the Atlantic ocean in the bay of New-York, now mix with the sea in the straits of Bellisle.

NOTE.—For the accompanying plate of the comparative elevation of the principal mountain ridges and peaks in this state, we are indebted to the politeness of DAVID H. BURR, Esq. It forms a part of a general map of the state, which together with an atlas containing a map and statistical table of each county in the state, has just been published by the above named gentleman.

This work is an important acquisition to the topographical knowledge of our state; and as it is intimately connected with the subject of the preceding article, the following extracts from the author's preface may not be improper in this place. "The legislature of New-York, in 1827, upon the recommendation of Governor Clinton, passed an act directing that whenever a set of maps was compiled on this plan, and delivered to the surveyor-general and comptroller, they should revise and correct the same; and that when they were satisfied with their accuracy, should publish them at the expense of the state. The legislature at the same and subsequent sessions, made liberal appropriations to defray the expenses, at the same time giving the author permission to make use of all documents deposited in any of the public offices of the state, or of the several towns and counties, which he should deem necessary in the completion of the work."

"During its progress, the surveyor-general addressed circulars to the supervisors of the several towns, requiring them to furnish surveys of the same, that their boundaries might be correctly described in the revised statutes. The information so obtained was furnished by the surveyor-general to the author, and has been used in the present work. When

the author had rendered the work as perfect as these authorities and his own personal observations enabled him to do, it was delivered to the surveyor-general and comptroller, for revision and correction, pursuant to the act before mentioned."

"Circulars were again addressed by the surveyor-general to the several supervisors, enclosing maps of their respective towns, and requesting them to point out the errors, if any, and also to suggest such additions as might be necessary to render the work more full and perfect. These circulars were in most instances returned with much useful information, which enabled the surveyor-general, with his previous knowledge, to correct such errors as had escaped the observation of the author. This work, therefore, comprises not alone the geographical knowledge of a single individual, but that of many, and those the best informed by their vocations of any in the state."*

*This article was prepared as an introduction to the atlas of the state of New-York published by David H. Burr. The section of mountains is principally from my own survey. (*MS. note by J. H.*)

[The plate showing the sectional elevations of the principal mountains in New York, (referred to in the preceding page,) has been omitted in the present re-print.]

ON THE APPLICATION OF THE PRINCIPLE OF THE GALVANIC MULTIPLIER TO ELECTRO-MAGNETIC APPARATUS, AND ALSO TO THE DEVELOPMENT OF GREAT MAGNETIC POWER IN SOFT IRON, WITH A SMALL GALVANIC ELEMENT.*

(Silliman's American Jour. of Science, January, 1831, vol. xix, pp. 400-408.)

For a long time after the discovery of the principal facts in electro-magnetism, the experiments in this interesting department of science could be repeated only by those who were so fortunate^o as to possess a large and expensive galvanic apparatus. Mr. Sturgeon, of Woolwich, did much towards making the subject more generally known, by showing that when powerful magnets are used, many of the most interesting experiments can be performed with a very small galvanic combination. His articles of apparatus, constructed on this principle, are of a much larger size, and more convenient, than any before used. They do not, however, form a complete set, as it is evident that strong magnets cannot be applied to every article required, and particularly to those intended to exhibit the action of terrestrial magnetism on a galvanic wire, or the operation of two galvanic wires on each other.

In a paper, published in the *Transactions of the Albany Institute*, June, 1828, I described some modifications of apparatus, intended to supply this deficiency of Mr. Sturgeon, by introducing the spiral coil on the principle of the galvanic multiplier of Prof. Schweigger, and this I think is applicable in every case where strong magnets cannot be used. The coil is formed by covering copper wire, from $\frac{1}{40}$ to $\frac{1}{20}$ of an inch in diameter, with silk; and in every case, which will permit, instead of using a single conducting wire, the effect is multiplied by introducing a coil of this wire, closely turned upon itself. This will be readily understood by an example: thus, in the experiment of Am-

* The term galvanic element is used in this paper to denote a single pair of galvanic plates.

pere, to shew the action of terrestrial magnetism on a galvanic current, instead of using a short single wire suspended on steel points; 60 feet of wire, covered with silk, are coiled so as to form a ring of about 20 inches in diameter, the several strands of which are bound together by wrapping a narrow silk ribbon around them. The copper and zinc of a pair of small galvanic plates are attached to the ends of the coil, and the whole suspended by a silk fibre, with the galvanic-element hanging in a tumbler of diluted acid. After a few oscillations, the apparatus never fails to place itself at right angles to the magnetic meridian. This article is nothing more than a modification of De la Rive's ring on a larger scale.

Shortly after the publication mentioned, several other applications of the coil, besides those described in that paper, were made in order to increase the size of electro-magnetic apparatus, and to diminish the necessary galvanic power. The most interesting of these, was its application to a development of magnetism in soft iron, much more extensively, than to my knowledge had been previously effected by a small galvanic element.

A round piece of iron, about $\frac{1}{4}$ of an inch in diameter, was bent into the usual form of a horse-shoe, and instead of loosely coiling around it a few feet of wire, as is usually described, it was tightly wound with 35 feet of wire, covered with silk, so as to form about 400 turns; a pair of small galvanic plates, which could be dipped into a tumbler of diluted acid, was soldered to the ends of the wire, and the whole mounted on a stand. With these small plates, the horse-shoe became much more powerfully magnetic, than another of the same size, and wound in the usual manner, by the application of a battery composed of 28 plates of copper and zinc, each 8 inches square. Another convenient form of this apparatus was contrived, by winding a straight bar of iron 9 inches long with 35 feet of wire, and supporting it horizontally on a small cup of copper containing a cylinder of zinc,—when this cup, which served the double purpose of a stand and the galvanic element,

was filled with dilute acid, the bar became a portable electro-magnet. These articles were exhibited to the Institute in March, 1829.

The idea afterwards occurred to me, that a sufficient quantity of galvanism was furnished by the two small plates, to develop, by means of the coil, a much greater magnetic power in a larger piece of iron. To test this, a cylindrical bar of iron, $\frac{1}{2}$ an inch in diameter, and about 10 inches long, was bent into the form of a horse-shoe, and wound with 30 feet of wire; with a pair of plates containing only $2\frac{1}{2}$ square inches of zinc, it lifted 14 lbs. avoirdupois. At the same time, a very material improvement in the formation of the coil suggested itself to me, on reading a more detailed account of Prof. Schweigger's galvanometer, and which was also tested with complete success upon the same horse-shoe; it consisted in using several strands of wire, each covered with silk, instead of one:—agreeably to this construction, a second wire, of the same length as the first, was wound over it, and the ends soldered to the zinc and copper in such a manner that the galvanic current might circulate in the same direction in both, or in other words, that the two wires might act as one; the effect by this addition was doubled, as the horse-shoe, with the same plates before used, now supported 28 lbs.

With a pair of plates 4 inches by 6 inches, it lifted 39 lbs., or more than 50 times its own weight.

These experiments conclusively proved that a great development of magnetism could be effected by a very small galvanic element, and also that the power of the coil was materially increased by multiplying the number of wires, without increasing the length of each.

The multiplication of the wires, increases the power in two ways; first, by conducting a greater quantity of galvanism, and secondly, by giving it a more proper direction, for since the action of a galvanic current is directly at right angles to the axis of a magnetic needle, by using several shorter wires, we can wind one on each inch of the length of the bar to be magnetized, so that the magnetism of each

inch will be developed, by a separate wire; in this way the action of each particular coil becomes very nearly at right angles to the axis of the bar, and consequently, the effect is the greatest possible. This principle is of much greater importance when large bars are used. The advantage of a greater conducting power from using several wires might in a less degree be obtained by substituting for them one large wire of equal sectional area, but in this case the obliquity of the spiral would be much greater and consequently the magnetic action less; besides this, the effect appears to depend in some degree on the number of turns which is much increased by using a number of small wires.*

In order to determine to what extent the coil could be applied in developing magnetism in soft iron; and also to ascertain, if possible, the most proper length of the wires to be used—

A series of experiments was instituted jointly by Dr. Philip Ten Eyck and myself. For this purpose 1060 feet (a little more than $\frac{1}{8}$ of a mile) of copper wire of the kind called bell-wire, .045 ($\frac{4.5}{1000}$) of an inch in diameter, were stretched several times across the large room of the Academy.

Experiment 1. A galvanic current from a single pair of plates of copper and zinc two inches square, was passed through the whole length of the wire, and the effect on a galvanometer noted;—From the mean of several observations, the deflection of the needle was 15° .

Exp. 2. A current from the same plates was passed through half the above length (or 530 feet) of wire, the deflection in this instance was 21° .

By a reference to a Trigonometrical table, it will be seen that the natural tangents of 15° and 21° are very nearly in the ratio of the square roots of 1 and 2, or of the relative lengths of the wires in these two experiments.

* Several small wires conduct more common electricity from the machine than one large wire of equal sectional area; the same is probably the case though in a less degree, in galvanism.

The length of the wire forming the galvanometer may be neglected, as it was only 8 feet long. This result agrees remarkably with the law discovered by Mr. Ritchie and published in the last No. of the *Journal of the Royal Institution of Great Britain*.

Exp. 3. The galvanometer was now removed, and the whole length of the wire attached to the ends of the wire of a small soft iron horse-shoe, $\frac{1}{4}$ of an inch in diameter, and wound with about 8 feet of copper wire with a galvanic current from the plates used in Experiments 1 and 2; the magnetism was scarcely observable in the horse-shoe.

Exp. 4. The small plates were removed and a battery composed of a piece of zinc plate 4 inches by 7 inches surrounded with copper, was substituted; when this was attached immediately to the ends of the 8 feet of wire wound round the horse-shoe, the weight lifted was $4\frac{1}{2}$ lbs.: when the current was passed through the whole length of wire (1060 feet) it lifted about half an ounce.

Exp. 5. The current was passed through half the length of wire (550 feet) with the same battery, it then lifted 2 oz.

Exp. 6. Two wires of the same length as in the last experiment were used, so as to form two strands from the zinc and copper of the battery: in this case the weight lifted was 4 oz.

Exp. 7. The whole length of the wire was attached to a small trough on Mr. Cruickshanks' plan, containing 25 double plates, and presenting exactly the same extent of zinc surface to the action of the acid as the battery used in the last experiment. The weight lifted in this case was 8 oz.; when the intervening wire was removed and the trough attached directly to the ends of the wire surrounding the horse-shoe it lifted only 7 oz. From this experiment, it appears that the current from the galvanic trough is capable of producing greater magnetic effect on soft iron after traversing more than $\frac{1}{5}$ of a mile of intervening wire, than when it passes only through the wire surrounding the magnet. It is possible that the different states of the trough, with respect to dryness, may have exerted some influence

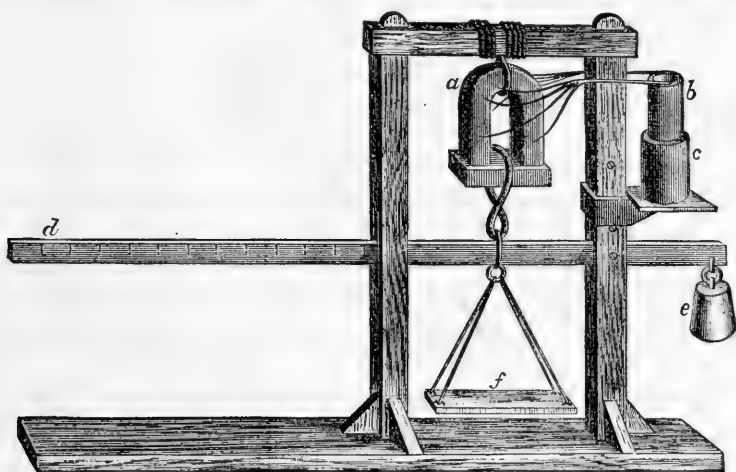
on this remarkable result; but that the effect of a current from a trough, if not increased, is but slightly diminished in passing through a long wire, is certain. A number of other experiments would have been made to verify this had not our use of the room been limited, by its being required for public exercises.

On a little consideration however, the above result does not appear so extraordinary as at the first sight, since a current from a trough possesses more "projectile force," to use Prof. Hare's expression, and approximates somewhat in intensity to the electricity from the common machine. May it not also be a fact that the galvanic fluid, in order to produce the greatest magnetic effect, should move with a small velocity, and that in passing through one fifth of a mile, its velocity is so retarded as to produce a greater magnetic action? But be this as it may, the fact, that the magnetic action of a current from a trough is, *at least*, not sensibly diminished by passing through a long wire, is directly applicable to Mr. Barlow's project of forming an electro-magnetic telegraph;* and it is also of material consequence in the construction of the galvanic coil. From these experiments, it is evident that in forming the coil we may either use one very long wire or several shorter ones as the circumstances may require: in the first case, our galvanic combinations must consist of a number of plates so as to give "projectile force;" in the second, it must be formed of a single pair.

In order to test on a large scale, the truth of these preliminary results, a bar of soft iron, 2 inches square and 20 inches long, was bent into the form of a horse-shoe, $9\frac{1}{2}$ inches high, the sharp edges of the bar were first a little rounded by the hammer, it weighed 21 lbs.; a piece of iron from the same bar weighing 7 lbs. was filed perfectly flat on

*[In a statement made by Prof. Henry, in March, 1857, he says: "Not being familiar with the history of the attempts made in regard to this invention, I called it 'Barlow's project,' while I ought to have stated that Mr. Barlow's investigation merely tended to disprove the possibility of a telegraph."]

one surface, for an armature or lifter; the extremities of the legs of the horse-shoe were also truly ground to the surface of the armature: around this horse-shoe 540 feet of copper bell wire were wound in 9 coils of 60 feet each; these coils were not continued around the whole length of the bar, but each strand of wire, according to the principle before mentioned, occupied about two inches and was coiled several times backward and forward over itself; the several ends of the wires were left projecting and all numbered, so that the first and the last end of each strand might be readily distinguished. In this manner, we formed an experimental magnet on a large scale, with which several combinations of wire could be made by merely uniting the different projecting ends. Thus, if the second end of the first wire be soldered to the first end of the second wire, and so on



[Frame for testing strength of electro-magnet.]

a, the magnet covered with linen, the ends of the wires projecting so as to be soldered to the galvanic element *b*. *c*, a cup with dilute acid on a moveable shelf. *d*, a graduated lever. *e*, a counterpoise. *f*, a scale for supporting weights; when a small sliding weight on the lever is not used, a second galvanic element is attached to the apparatus so that the poles of the magnet can be instantly reversed: this is omitted in the figure.

By inverting the large magnet, it sets in motion a very large revolving cylinder of March and Ampère.

through all the series, the whole will form a continued coil of one long wire. By soldering different ends, the whole may be formed into a double coil of half the length, or into a triple coil of one third the length, &c. The horse-shoe was suspended in a strong rectangular wooden frame 3 feet 9 inches high and 20 inches wide, an iron bar was fixed below the magnet so as to act as a lever of the second order; the different weights supported, were estimated by a sliding weight in the same manner as with a common steelyard. (See the sketch of the magnet.)

In the experiments immediately following,* a small single battery was used, consisting of two concentric copper cylinders, with zinc between them; the whole amount of zinc surface exposed to the acid from both sides of the zinc was $\frac{2}{3}$ of a square foot; the battery required only half a pint of dilute acid for its submersion.

Exp. 8. Each wire of the horse-shoe was soldered to the battery in succession, one at a time; the magnetism developed by each was just sufficient to support the weight of the armature, weighing 7 lbs.

Exp. 9. Two wires, one on each side of the arch of the horse-shoe, were attached; the weight lifted was 145 lbs.

Exp. 10. With two wires, one from each extremity of the legs, the weight lifted was 200 lbs.

Exp. 11. With three wires, one from each extremity of the legs, and the other from the middle of the arch, the weight supported was 300 lbs.

Exp. 12. With four wires, two from each extremity, the weight lifted was 500 lbs. and the armature; when the acid was removed from the zinc, the magnet continued to support, for a few minutes, 130 lbs.

Exp. 13. With six wires, the weight supported was 570 lbs.; in all these experiments, the wires were soldered to the galvanic element; the connexion, in no instance, was formed with mercury.

Exp. 14. When all the wires, (nine in number,) were attached, *the maximum weight lifted was 650 lbs.* and this aston-

* All the weights in this series of experiments are avoirdupois.

ishing result, it must be remembered, was produced by a battery containing only $\frac{2}{5}$ of a square foot of zinc surface, and requiring only half a pint of diluted acid for its submersion.

Exp. 15. A small battery, formed with a plate of zinc 12 inches long and 6 inches wide, and surrounded by copper, was substituted for the galvanic element used in the last experiment; the weight lifted in this case was 750 lbs. This is probably the maximum of magnetic power which can be developed in this horse-shoe, as with a large calorimotor, containing 28 plates of copper and zinc, each 8 inches square, the effect was not increased, and indeed we could not succeed in making it lift as much as with the small battery.

The strongest magnet of which we have any account, is that in the possession of Mr. Peale, of Philadelphia; this weighs 53 lbs. and lifted 310 lbs. or about six times its own weight. Our magnet weighs 21 lbs. and consequently lifts more than thirty-five times its own weight; it is probably, therefore, the most powerful magnet ever constructed.

This, however, is by no means the maximum, which can be produced by a small galvanic element, as in every experiment we have made the power increases by increasing the quantity of iron; with a bar similar to the one used in these experiments, but of double the diameter, or of 8 times the weight, the power would doubtless be quadruple, and that too without increasing the size of the galvanic element.

Exp. 16. In order to ascertain the effect of a very small galvanic element on this large quantity of iron, a pair of plates, exactly one inch square, was attached to all the wires; the weight lifted was 85 lbs.

The following experiments were made with wires of different lengths, on the same horse-shoe.

Exp. 17. With 6 wires, each 30 feet long, attached to the galvanic element, the weight lifted was 375 lbs.

Exp. 18. The same wires used in the last experiment, were united so as to form 3 coils of 60 feet each; the weight supported was 290 lbs. This result agrees nearly with that of Exp. 11, though the same individual wires were not used;

from this it appears, that 6 short wires are more powerful than 3 of double the length.

Exp. 19. The wires used in *Exp. 10*, but united so as to form a *single* coil of 120 feet of wire, lifted 60 lbs.; while in *Exp. 10*, the weight lifted was 200 lbs.: this is a confirmation of the result in the last experiment.

Exp. 20. The same wires used in the last experiment were attached to a small compound battery, consisting of two plates of zinc and two of copper, after the plan of Prof. Hare, and containing exactly the same quantity of zinc surface, as the element in the last experiment; in this case the weight lifted was 110 lbs., or nearly double that in the last. This result is in strict accordance with that of *Exp. 7*; the two plates having more "projectile force," and thus producing a greater effect with a long wire.

In these experiments a fact was observed, which appears somewhat surprising: when the large battery was attached and the armature touching both poles of the magnet, it was capable of supporting more than 700 lbs. but when only one pole is in contact it did not support more than 5 or 6 lbs., and in this case we never succeeded in making it lift the armature (weighing 7 lbs.). This fact may perhaps be common to all large magnets, but we have never seen the circumstance noticed of so great a difference between a single pole and both.

A number of experiments were also made with reference to the best form of the iron to receive magnetism, but no very satisfactory results were obtained; of these however, the following are considered as not uninteresting.

Exp. 21. A cylindrical bar of iron weighing 13 oz. $4\frac{1}{2}$ drachms, and bent into a horse-shoe, was covered with two coils of wire each 60 feet long; with the small battery used in the last experiment, it lifted 42 lbs.

Exp. 22. A rectangular flat bar $\frac{1}{8}$ of an inch wide, and $\frac{1}{2}$ of an inch thick, also bent into a horse-shoe, weighing 9 oz. 3 dr., and of exactly the same surface as the bar used in the last experiment, with the same wires and battery, lifted 35 lbs

Exp. 23. A piece of a gun-barrel, little less than inch in diameter and about 8 inches long, and from $\frac{1}{12}$ to $\frac{1}{18}$ of an inch thick, weighing 8 oz. $3\frac{3}{4}$ dr. (with the wires and battery as before,) lifted 40 lbs.

From the last experiment, it appears that a given quantity of iron in the form of a hollow cylinder, is capable of receiving more magnetism than that of a solid cylinder of less diameter; but it is evident from *Exp. 21*, that a solid bar of the same diameter as the gun-barrel, and of greater weight, would have lifted more: perhaps the gun-barrel was not sufficiently thick for the full development of magnetism, which, according to Barlow's experiments, resides near the surface.*

A series of experiments† was separately instituted by Dr. Ten Eyck in order to determine the maximum development of magnetism in a small quantity of soft iron: from these the following interesting results were obtained.

Experiment 1. A horse-shoe of round iron $\frac{5}{100}$ of an inch in diameter, 4 inches long, weighing 2314 grains and wound with 23 ft. copper-wire, diameter $\frac{4}{100}$ of an inch, with a pair of one inch plates, lifted 19 lbs. 5 oz. 6 dwt. 16 grs.; with a pair of 4 inch plates, lifted 25 lbs. 6 oz. 5 dwt.; with the cylindrical element used in *Exps. 8, 9 and 10* of former series, it lifted 42 lbs. 6 oz. 8 dwt. 8 grs., or 105 times its own weight.

Exp. 2. A horse-shoe of round iron $\frac{1}{4}$ inch in diameter, $3\frac{1}{2}$ inches in length weighing 310 grains, and wound with 15 ft. copper wire, diameter $\frac{4}{100}$ inch, with a pair of one inch plates, lifted 3 lbs. 11 oz. 7 dwt. 22 grs.; with 4 inch plates it lifted 5 lbs. 5 oz. 12 dwt. 12 grs.; with the cylindrical element 8 lbs. 2 oz. 8 dwt. 18 grs., or 152 times its own weight.

Exp. 3. A horse-shoe formed of a flat bar $2\frac{1}{10}$ inches long $\frac{3}{10}$ in. broad and $\frac{6}{100}$ in. thick, weighing 84 grains, and wound with 16 feet of brass wire, $\frac{2}{100}$ of an inch in diameter, with a pair of one inch plates, lifted 5 lbs. 2 oz. 3 dwt. 8 grs.;

* See Barlow's *Essay on Magnetic Attractions*, page 50.

† Troy weight is used in these experiments.

with 4 inch plates, it lifted 2 lbs. 10 oz. 2 dwt. 12 grs.; with the cylindrical element, it lifted 2 lbs. 10 oz. 13 dwt. 2 grs., or 198 times its own weight.

Exp. 4. A horse-shoe of round iron, slightly flattened, one inch in length, diameter, (before flattening) $\frac{6}{100}$ inch, weight 6 grains, and wound with 3 feet brass wire same diameter as that of No. 3, with a pair of one inch plates, lifted 2 oz. 15 dwt. 1 gr.; with four inch plates, lifted 3 oz. 17 dwt. 10 gr.; with the cylindrical element, lifted 5 oz. 5 dwt. 4 grs., or 420 times its own weight.

In this last result the ratio of the weight lifted, to the weight of the magnet is much greater than any we have ever seen noticed; the strongest magnet we can find described is one worn by Sir Isaac Newton in a ring, weighing 3 grains; it is said to have taken up 746 grs. or nearly 250 times its own weight. M. Cavallo has seen one of 6 or 7 grs. weight which was capable of lifting 300 grs. or about 50 times its own weight. From these experiments it is evident, that a much greater degree of magnetism can be developed in soft iron by a galvanic current, than in steel by the ordinary method of touching.

Most of the results given in this paper were witnessed by Dr. L. C. Beck, and to this gentleman we are indebted for several suggestions, and particularly that of substituting cotton well waxed for silk thread, which in these investigations, became a very considerable item of expense; he also made a number of experiments with iron bonnet-wire, which, being found in commerce already wound, might possibly be substituted in place of copper:—the result was that with very short wire the effect was nearly the same as with copper, but in coils of long wire with a small galvanic element, it was not found to answer. Dr. Beck also constructed a horse-shoe of round iron, one inch in diameter, with four coils on the plan before described; with one wire it lifted 30 lbs., with two wires—60 lbs., with three wires—85 lbs., and with four wires—112 lbs.

While we were engaged in these investigations, the last No. of the *Edinburgh Journal of Science* was received, containing

Prof. Moll's paper on Electro-Magnetism. Some of his results are, in a degree, similar to those here described: his object, however, was different, it being only to induce strong magnetism on soft iron with a powerful galvanic battery. The principal object in these experiments was to produce the greatest magnetic force, with the smallest quantity of galvanism. The only effect Prof. Moll's paper has had over these investigations, has been to hasten their publication: the principle on which they were instituted was known to us nearly two years since, and at that time exhibited to the Albany Institute.

AN ACCOUNT OF A LARGE ELECTRO-MAGNET,* MADE FOR THE
LABORATORY OF YALE COLLEGE.

(Extract of a letter to Prof. Silliman, accompanying the Magnet.)

(Silliman's American Journal of Science, April, 1831, vol. xx. pp. 201-203.)

The magnet is constructed on precisely the same principles as that described in the last number of the Journal. It weighs $59\frac{1}{2}$ lbs. avoirdupois, (exclusive of the copper wire which surrounds it,) and was formed from a bar of Swede's

* [This magnet is now arranged in its frame, in the laboratory of Yale College. Being myself out of town when the instrument arrived, the necessary experiments and fixtures were satisfactorily made by C. U. Shepard, (Chemical Assistant) and Dr. Titus W. Powers, of Albany, who was so obliging as to bring the magnet to New Haven. There has not been time (as the magnet came just as this No. was finishing) to do any thing more than make a few trials, which have however fully substantiated the statements of Prof. Henry. He has the honor of having constructed by far, the most powerful magnets that have ever been known, and his last, weighing, armature and all, but $82\frac{1}{2}$ lbs., sustains over a ton. It is eight times more powerful than any magnet hitherto known in Europe, and between six and seven times more powerful than the great magnet in Philadelphia. We understand that the experiments described in the last No. of this Journal, (except those ascribed to Dr. Ten Eyck) were devised by Professor Henry alone, who (except forging the iron) constructed the magnet with his own hand. The plan of the frame, and the fixtures, and the drawing in the last No., were done by Dr. Ten Eyck. In the Yale College magnet, the plan was drawn by Professor Henry, and the iron forged under his direction. The length of the wires being agreed upon, the winding was done by Dr. Ten Eyck, and the experiments were mutually performed.—*Editor of Journal.*]

[It may be worth while to state a single experiment, which I made with a view to learn the chemical effects of this instrument. As its magnetic flow was so powerful, I had strong hopes of being able to accomplish the decomposition of water by its means. My experiment, however, which was made as follows, proved unsuccessful. The battery being immersed, to the extremities of the magnet were applied two broad, polished plates of iron, terminating in flattened wires, which were united with the wires of the ordinary apparatus for decomposing water, and the contact heightened by the use of cups of mercury: not the slightest decomposition was, however, observable. Aware, that had any chemical effect been produced, this arrangement could have decided nothing, (except perhaps from the degree of energy in the decomposition) as respects the point whether simple magnetism is

iron three inches square and thirty inches long. Before bending the bar into the shape of a horse-shoe, it was flattened on the edges, so as to form an octagonal prism, having a perimeter of $10\frac{3}{4}$ inches. The other dimensions of the magnet, as measured before winding it with wire, are as follows:— perpendicular height of the exterior arch of the horse-shoe, $11\frac{1}{4}$ inches; around the outside from one pole to the other, $29\frac{9}{10}$ inches; internal distance between the poles, $3\frac{1}{2}$ inches.

The armature or lifter is formed from a piece of iron from the same bar, not flattened on the edges; it is nearly 3 inches square, $9\frac{1}{2}$ inches long, and weighs 23 lbs. The upper surface is made perfectly flat, except about an inch in the middle where the angles are rounded off so as to form a groove, into which the upper part of a strong iron stirrup, surrounding the armature, fits somewhat loosely. The weight to be supported is fastened to the lower part of the stirrup, and by means of the groove is made to bear directly on the centre of the armature.

For the purpose of suspending the magnet, a piece of round iron with an eye on one end, is firmly screwed into the crown of the arch and is attached to the cross beam of a frame, similar to that figured in the last number of the Journal.

The magnet is wound with 26 strands of copper bell-wire, covered with cotton thread 31 feet long; about 18 inches of the ends are left projecting, so that only 28 feet actually surround the iron; the aggregate length of the coils is therefore 728 feet. Each strand is wound on a little less than an inch; in the middle of the horse-shoe it forms three thick-

adequate to decompose water, since it might under these circumstances be attributed to the electricity from the battery, I had determined in a second experiment, had the first proved successful, to have interrupted the galvanic flow by a non-conductor; in which case, had the decomposition ensued, pure magnetism might have been considered as the decomposing agent. But as my preliminary experiment was unsuccessful, I proceeded no farther; I hope, however, to resume the research hereafter, under more favorable circumstances.

C. U. SHEPARD.]

nesses of wire, and on the ends or near the poles it is wound so as to form six thicknesses.

Two small galvanic batteries are soldered to the wires of the magnet, one on each side of the supporting frame, in such a manner as to cause the poles to be instantaneously reversed, by merely dipping the batteries alternately into acid. To render these as compact as possible, they are formed of concentric copper cylinders, with cylinders of zinc plates interposed, and so united as to form but one galvanic pair. Each of these batteries presents to the action of the acid, (measuring both surfaces of the plate,) $4\frac{7}{8}$ square feet: they are 12 inches high and about 5 inches in diameter.

In experimenting with this magnet, a battery containing $\frac{2}{3}$ of a square foot of zinc surface was first attached to the wires; with this the magnet could not be made to support more than 500 lbs. Another battery was then substituted for the above, containing about three times the same quantity of zinc surface; with this, at the first instant of immersion, the magnet sustained 1600 lbs.; after the acid was removed, it continued to support, for a few minutes, 450 lbs.; and in one experiment, three days after the battery had been excited, more than 150 lbs. were added to the armature* before it fell. It was evident from these experiments, that this magnet required a considerably larger quantity of zinc surface in proportion to its weight, to magnetize it to saturation, than that described in the former paper. Accordingly the two batteries, before mentioned as containing $4\frac{7}{8}$ square feet, were prepared. With one of them, at the first immersion, the magnet readily supported 2000 lbs. A sliding weight was then attached to the bar; the battery was suffered to become perfectly dry, and on immersing it again, the magnet supported 2063 lbs. The effect of a larger battery was not tried.

To test its power of inducing magnetism on soft iron, two

* [The armature of 23 lbs. applied when the battery is immersed, only for an inch and an instant, remains day after day without falling, although the galvanic coils are perfectly dry.—*Editor of Journal.*]

pieces of round iron $1\frac{1}{4}$ inches in diameter and 12 inches long, were interposed between the extremities of the magnet and the armature: with this arrangement, when one of the batteries was immersed, the pieces of iron became so powerfully magnetic as to support 155 lbs.

To exhibit the effects produced by instantaneously reversing the poles, the armature was loaded with 56 lbs., which added to its own weight made 89 lbs.: one of the batteries was then dipped into the acid and immediately withdrawn, when the weight of course continued to adhere to the magnet; the other battery was then suddenly immersed, when the poles were changed so instantaneously that the weight did not fall. That the poles were actually reversed in this experiment, was clearly shown by a change in the position of a large needle placed at a small distance from the side of one extremity of the horse-shoe.

P. S.—Last autumn, I commenced a series of observations on the magnetic intensity of the earth at Albany, and intend to begin a new series next month; the apparatus used was that sent by Capt. Sabine to Prof. Renwick, and was mentioned in the *Journal*, vol. xvii, p. 145. I have constructed a similar apparatus for myself, and intend to pay considerable attention to the subject.

ON A RECIPROCATING MOTION PRODUCED BY MAGNETIC ATTRACTION AND REPULSION.

(Silliman's American Journal of Science, July, 1831, vol. xx, pp. 340-343.)

To the Editor:

SIR:—I have lately succeeded in producing motion in a little machine by a power, which, I believe, has never before been applied in mechanics—by magnetic attraction and repulsion.

Not much importance, however, is attached to the invention, since the article, in its present state, can only be considered a philosophical toy; although, in the progress of discovery and invention, it is not impossible that the same principle, or some modification of it on a more extended scale, may hereafter be applied to some useful purpose. But without reference to its practical utility, and only viewed as a new effect produced by one of the most mysterious agents of nature, you will not, perhaps, think the following account of it unworthy of a place in the *Journal of Science*.

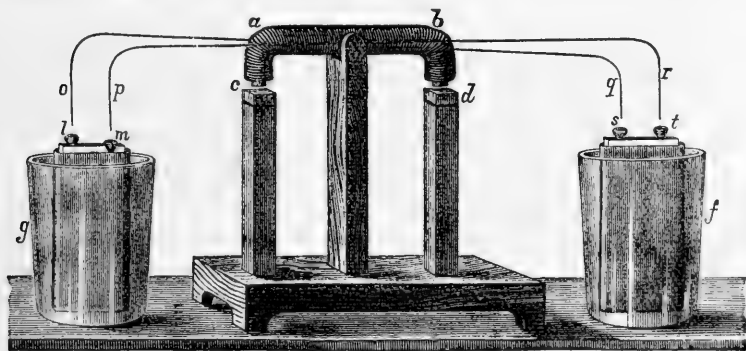
It is well known that an attractive or repulsive force is exerted between two magnets, according as poles of different names, or poles of the same name, are presented to each other.

In order to understand how this principle can be applied to produce a reciprocating motion, let us suppose a bar magnet to be supported horizontally on an axis passing through the centre of gravity, in precisely the same manner as a dipping needle is poised; and suppose two other magnets to be placed perpendicularly, one under each pole of the horizontal magnet, and a little below it, with their north poles uppermost; then it is evident that the south pole of the horizontal magnet will be attracted by the north pole of one of the perpendicular magnets, and its north pole repelled by the north pole of the other: in this state it will remain at rest, but if, by any means, we reverse the polarity of the horizontal magnet, its position will be changed and the extremity,

which was before attracted, will now be repelled; if the polarity be again reversed, the position will again be changed, and so on indefinitely: to produce, therefore, a continued vibration, it is only necessary to introduce, into this arrangement, some means by which the polarity of the horizontal magnet can be instantaneously changed, and that too by a cause which shall be put in operation by the motion of the magnet itself; how this can be effected, will not be difficult to conceive, when I mention that instead of a permanent steel magnet in the moveable part of the apparatus, a soft iron galvanic magnet is used.*

The change of polarity is produced simply by soldering to the extremities of the wires which surround the galvanic magnet, two small galvanic batteries in such a manner that the vibrations of the magnet itself may immerse these alternately into vessels of diluted acid; care being taken that the batteries are so attached that the current of galvanism from each shall pass around the magnet in an opposite direction.

Instead of soldering the batteries to the ends of the wires, and thus causing them at each vibration to be lifted from the acid by the power of the machine, they may be permanently fixed in the vessels, and the galvanic communication formed by the amalgamated ends of the wires dipping into cups of mercury.



[Electro-magnetic Engine.]

* For a method of constructing the galvanic magnet on an improved plan, see my paper in vol. XIX, p. 400 of this Journal. [*Ante*, p. 37.]

The whole will be more readily understood by a reference to the annexed drawing: *a b* is the horizontal magnet, about seven inches long, and movable on an axis at the centre: its two extremities when placed in a horizontal line, are about one inch from the north poles of the upright magnets *c* and *d*. *g* and *f* are two large tumblers containing diluted acid, in each of which is immersed a plate of zinc surrounded with copper. *l, m, s, t*, are four brass thimbles soldered to the zinc and copper of the batteries and filled with mercury.

The galvanic magnet *ab* is wound with three strands of copper bell-wire, each about twenty-five feet long; the similar ends of these are twisted together so as to form two stiff wires, which project beyond the extremity *b*, and dip into the thimbles *s, t*.

To the wires *g, r*, two other wires are soldered so as to project in an opposite direction, and dip into the thimbles *l, m*. The wires of the galvanic magnet have thus, as it were, four projecting ends; and by inspecting the figure it will be seen that the extremity *p*, which dips into the cup *m* attached to the copper of the battery in *g* corresponds to the extremity *r* connecting with the zinc in *f*.

When the batteries are in action, if the end *b* is depressed until *g, r* dips into the cups *s, t*, *ab* instantly becomes a powerful magnet, having its north pole at *b*; this of course is repelled by the north pole *d*, while at the same time it is attracted by *c*, the position is consequently changed, and *o, p* comes in contact with the mercury in *l, m*; as soon as the communication is formed, the poles are reversed, and the position again changed. If the tumblers be filled with strong diluted acid, the motion is at first very rapid and powerful, but it soon almost entirely ceases. By partially filling the tumblers with weak acid, and occasionally adding a small quantity of fresh acid, a uniform motion, at the rate of seventy-five vibrations in a minute, has been kept up for more than an hour: with a large battery and very weak acid, the motion might be continued for an indefinite length of time.

The motion, here described, is entirely distinct from that produced by the electro-magnetic combination of wires and magnets; it results directly from the mechanical action of ordinary magnetism: galvanism being only introduced for the purpose of changing the poles.

My friend, Prof. Green, of Philadelphia, to whom I first exhibited this machine in motion, recommended the substitution of galvanic magnets for the two perpendicular steel ones. If an article of this kind was to be constructed on a large scale, this would undoubtedly be the better plan, as magnets of that kind can be made of any required power; but for a small apparatus, intended merely to exhibit the motion, the plan here described is perhaps the most convenient.

ON A DISTURBANCE OF THE EARTH'S MAGNETISM, IN CONNECTION WITH THE APPEARANCE OF AN AURORA BOREALIS, AS OBSERVED AT ALBANY, APRIL 19TH, 1831.*

(Silliman's American Journal of Science, April, 1832; vol. xxii, pp. 143-155.)

That the aurora has some connection with the magnetism of the earth, was asserted as early as the middle of the last century; and since that time many observations have been recorded tending to confirm this position. 1. It has been observed that when the aurora appears near the northern horizon in the form of an arch the middle of this is not in the direction of the true north, but in that of the magnetic needle at the place of observation, and that when the arch rises towards the zenith it constantly crosses the heavens at right angles, not to the true, but to the magnetic meridian. This fact is most obvious where the variation of the needle is great. 2. When the beams of the aurora shoot up so as to pass the zenith, which is sometimes the case, the point of their convergence is in the direction of the prolongation of the dipping needle at the place of observation. 3. It has also been observed that during the appearance of an active and brilliant aurora the magnetic needle often becomes restless, varies sometimes several degrees, and does not resume its former position until after several hours.

From the above facts, it has been generally inferred that the aurora is in some way connected with the magnetism of the earth; and that the simultaneous appearance of the meteor, and the disturbance of the needle, are either related as cause and effect, or as the common result of some more general and unknown cause.

The subject is however involved in much obscurity; and there are some facts which tend to throw doubt on the connection of the two phenomena. The accurate and valuable observations of Col. Beaufoy in England, continued for several years, add nothing towards establishing the fact of the

* Communicated to the Albany Institute, January 26, 1832.

magnetic influence of the aurora; and in the scientific expeditions under Capt. Parry to the north, in the peculiar regions, as it would appear, of this meteor, no unusual disturbance of the needle was observed to accompany the aurora, although the apparatus was visited every hour in the day, and sometimes oftener, when any thing rendered it desirable. Indeed, so far from producing a disturbing effect, Dr. Brewster concludes, from a comparison of the observations, that the aurora, in the arctic regions, seems rather to exercise a sedative influence.*

On the other hand, Dr. Richardson states, from his own observations, made at Bear Lake, during six successive months of the years 1825-6, and again in 1826-7, that the aurora does influence the magnetic needle. "A careful review of the daily register," says he, "has led me to form the following conclusion: That brilliant and active coruscations cause a deflection of the needle almost invariably, if they appear through a foggy atmosphere, and if prismatic colors are exhibited; on the contrary, when the atmosphere is clear, and the aurora presents a dense steady light of a yellow color, and without motion, the needle is often unaffected." †

In this state of knowledge, every additional fact becomes of some importance. The following communication, it is therefore hoped, may be useful, either in directing the attention of observers in this country to the subject, or in corroborating similar observations made in other quarters of the globe.

In September, 1830, I commenced a series of observations, for Professor Renwick, of Columbia College, to determine the magnetic intensity at Albany. In the course of these, I unexpectedly witnessed a disturbance of the magnetism of the earth, in connection with an appearance of an aurora, which on some accounts appears interesting.

The needles used in these observations were those mentioned in Capt. Sabine's letter to Prof. Renwick, published

* Edinburgh Philosophical Journal of Science, vol. 8.

† Edinburgh New Philosophical Journal, vol. 5.

in the 17th volume of the *American Journal of Science*. One of these, it will be recollected, formerly belonged to Prof. Hansteen, of Norway, and the other to Capt. Sabine. They were suspended, according to the method of Hansteen, in a small mahogany box, by a single fibre of raw silk. The box was furnished with a glass cover, and had a graduated arc of ivory on the bottom to mark the amplitude of the vibrations. It had also two small circular windows, diametrically opposite to each other, through which the oscillations of the needle could be seen.

In using this apparatus, the time of three hundred vibrations was noted by a quarter second watch, well regulated to mean time; a register being made at the end of every tenth vibration, and a mean deduced from the whole, taken as the true time of the three hundred vibrations. Experiments carefully made with this apparatus, were found susceptible of considerable accuracy; as the individual observations, after a small correction for temperature, give a result, except in a few instances, differing from the mean of a number made under similar circumstances, by a quantity not greater than one part in nearly a thousand.

The observations were repeated daily, when the weather would permit, from the latter part of September to the last of November, either at the hours of 12 noon, or between 5 and 6 P. M.* I was always assisted in making them by the same person,—my relative, Mr. Stephen Alexander,—to whose skill and experience I am much indebted for any accuracy they may possess.

In April, 1831, a new series was commenced, to determine if the needles still indicated the same degree of magnetic intensity. No material difference was observed, except in the following instance, when a remarkable anomaly was exhibited.

On the 19th of April, at 12 o'clock noon, an observation was made with the Hansteen needle, the result of which differed only the fractional part of a second from the usual

* These times were chosen only on account of being most convenient.

mean rate of this needle. At 6 o'clock P. M. the same day, another observation was made with the same needle, and apparently under the same circumstances; but a remarkable change was now observed in the time of its making three hundred vibrations, indicating a great increase in the magnetic intensity of the earth. It was at first supposed that the needle had accidentally been placed contiguous to some ferruginous substance; but on a most careful investigation, nothing could be discovered which would tend in the least degree to explain the cause of the phenomenon. The experiment was made at the usual place, with the box containing the needle resting on a post permanently fixed for the purpose, in the Academy Park, at a sufficient distance from every disturbing object, and with the usual precaution of divesting the person of all articles of iron, such as keys, knives, &c.

At about 9 o'clock in the evening, or three hours after the above observation, an unusual appearance was noticed in the *southern* part of the heavens, which was shortly afterwards recognized as an arch of the aurora. It was about nine degrees in breadth, with the vertex of the arch twenty degrees above the horizon. At this time the northern part of the sky was covered with light fleecy clouds. At forty-five minutes past nine, the clouds partially disappeared, and disclosed the whole northern hemisphere entirely occupied with coruscations of the aurora, shooting up past the zenith, and apparently all converging to the same point. The actual formation of a *corona* might probably have been observed, but for a dark cloud which remained stationary a little south of the zenith. The idea for the first time now occurred to me, that this uncommonly brilliant appearance of the aurora might possibly be connected with the magnetic disturbance observed at 6 o'clock; and in order to test this, the apparatus was again placed on the post in the Academy Park, and an observation made during the most active appearance of the meteor.

The result of the observation was however entirely different from that anticipated; for *instead of still indicating, as at 6*

o'clock, an uncommonly high degree of magnetic intensity, it now showed an intensity considerable lower than usual.

Observations were also made on the 20th and 21st, but no disturbance was again noticed; the intensity had resumed its former state.

The following table exhibits the observed times of three hundred vibrations, with the mean temperature and aspect of the weather during each observation:

Day.	Time of 300 vibrations.	Mean temperature.	Weather.
April 19th, 12 h. noon ----	980 ^s .75	66 $\frac{1}{2}$ ° F.	Cloudy, rain A. M.
" 19th, 6 h. P. M. ----	968 ^s .65	61°	Clear.
" 19th, 10 h. P. M. ----	982 ^s .20	52°	Broken clouds.
" 20th, 6 h. P. M. ----	978 ^s .68	51 $\frac{1}{2}$ °	Clear.

The above observations may be reduced approximately to the uniform temperature of 60°, by the formula,

$$T = T[1 \pm 0.000165(t' \pm t)],*$$

(T being time, t temperature in degrees of Fahrenheit,) which was deduced from experiments on a similar needle. The relative intensities may also be readily calculated, since they are reciprocally as the squares of the times of the vibrations. In this way, by assuming as unity the time observed on the 20th, we have the following results:

Day.	Time of 300 vibrations at temperature of 60.	Relative intensities.
April 19th, 12 h. noon ----	979 ^s .94	1.00022
" 19th, 6 h. P. M. ----	968 ^s .49	1.02401
" 19th, 10 h. P. M. ----	983 ^s .50	0.99299
" 20th, 6 h. P. M. ----	980 ^s .05	1.00000

From the mean of several observations made with this needle in April, I consider its time of three hundred vibrations for this month, and in an undisturbed state of terres-

* This formula was obtained by Hansteen.

trial magnetic intensity, to be nine hundred and seventy nine seconds. The accidental errors in the above observations do not probably exceed in any case one second.

At the time of registering the above observations, I had not seen the following remark of Prof. Hansteen, which was subsequently met with in the 12th volume of the *Edinburgh Philosophical Journal*:—"A short time before the aurora borealis appears," says Prof. Hansteen, "the intensity of the magnetism of the earth is apt to rise to an uncommon height; but so soon as the aurora begins, in proportion as its force increases, the intensity of the magnetism of the earth decreases, recovering its former strength by degrees, often not till the end of twenty-four hours."* This statement, founded on observations made in Norway, is a precise description of the phenomenon observed in Albany; and should it be found a general, or even a frequent occurrence, that a great increase of intensity precedes the appearance of the aurora, it would perhaps reconcile many apparent discrepancies in the different accounts of magnetic influence of the meteor.

Prof. Hansteen also remarks, in the same paper, that "The polar lights seem to be the effect of an uncommonly high magnetic intensity, which lets itself off, as it were, by the aurora, and thus sinks under its common strength." Nothing however can with certainty be deduced from these observations, in reference to this supposition; since the magnetic intensity at any place, as exhibited by the vibrations of the horizontal needle, may change while the absolute force or intensity of the whole earth remains the same. If we represent by F the whole force in the direction of the dipping needle, by δ the dip in degrees, and by H the horizontal force, we shall have, by a well known law,

$$F = \frac{H}{\cos \delta}$$

*I find the same observation has also been made by Humboldt; and also a similar one by Van Swinden, who remarks, that the variation of the needle increases when the aurora borealis is approaching. *Journal Royal Institution. Young's Natural Philosophy*, vol. 2, p. 442.

In this formula it is evident that F may remain constant, although H is caused to vary by a change in the value of $\cos \delta$. The fact therefore of a variation in the absolute intensity, can only be determined by combining the observations of the vibrations of the horizontal needle with simultaneous observations on the dipping needle.

If we suppose F constant during the change of horizontal intensity as observed at Albany, we may, by means of the above formula, calculate the change in declination or dip required to produce the observed difference in the horizontal intensity. Assuming $\delta = 75^\circ$, (the dip at Albany nearly,) and $H =$ to the horizontal intensity observed at 6 o'clock, we can readily find the value of F ; and since this value is supposed constant, by substituting it in the expression

$$\cos \delta = \frac{H'}{F}$$

in which H' represented the intensity observed at 10 o'clock, we shall have the value of δ (the dip) corresponding to the latter intensity. In this way, the change observed in the horizontal intensity at the time of the aurora, gives $28' 48''$ as the deviation of the needle in the plane of the dip.

The aurora which appeared in connection with this magnetic disturbance, was probably one of the most interesting ever observed in this country, particularly from the circumstance of the actual formation of a *corona*, which was seen in several parts of this State. My friend Prof. Joslin, of Union College, who happened to be in New York at the time, has furnished me with the following account:

"The aurora borealis of 19th April, as it appeared in the city of New York at 9 p. m., was peculiarly interesting, on account of the meeting of the luminous columns in the magnetic meridian, at the point in the direction of the dipping needle towards which they usually tend. The luminous matter occupied the whole northern half of the visible celestial hemisphere, and was very much condensed near the point of convergence. Some of the eastern coruscations were at times transiently curved, as though their middle parts (as was probably the case) were driven eastward by the

impulse of the westerly breeze which was blowing at the time. A luminous band was at one time extended across the heavens, at right angles to the meridian, and 30° south of the zenith. This had at times an oscillatory motion in a north and south direction. It passed near the moon, around which was one of the large halos. The sky had been previously clear. The converging rays appeared to meet at the star δ Leonis."

By computing the position of δ Leonis for 9 o'clock on the evening of the 19th, its altitude was found to be $70^{\circ} 25'$, and its azimuth $11^{\circ} 27'$ east. A small error in time however would make a great difference in the azimuth. The dip of the needle at New York is 73° , and the variation probably between 4° and 5° , as it is $6\frac{3}{4}^{\circ}$ at Albany.

The aurora was also seen by Dr. William Campbell, at Cherry Valley. He describes it as very brilliant, and assuming a variety of forms; at one time appearing as a stupendous arch, crossing the heavens from east to west; at another, radiating from a point south of the zenith. The Rev. Mr. Thummel, of the Hartwick Seminary, at his residence in Otsego county, likewise observed the same aurora. He describes it as radiating in every direction from a nucleus near the zenith, which appeared clear and compact for some time, when it began to move, and darted forth rays in every direction like crystals.

MARCH 6, 1832.

Since the foregoing was communicated to the Institute, several particulars have been learned in reference to the subject, which, on some accounts, are deemed interesting. The Annual Meteorological Reports of the different Academies in the State of New York, to the Regents of the University, have been received; and from them it appears that the aurora of the 19th of April was visible over the whole extent of the State, and probably considerably west of it. It is described as being very brilliant at Lewiston on the Niagara river, extending high, and farther to the south than any before observed. In the eastern part of the State, it was seen at most of the Academies along the Hudson, and at Eras-

mus Hall, on Long Island. It also appeared brilliant at Potsdam in St. Lawrence county, the most northern Academy in the State. It was probably not seen very extensively in the States east of New York, as I am informed the weather in the eastern part of New England was cloudy at the time, accompanied with rain. The aurora is described as shooting up to the zenith at North Salem; and at Middlebury as consisting of coruscations in almost every part of the visible heavens. At Fairfield, it illuminated nearly the whole heavens; a number of bows, commencing in the northwest, passed south of the zenith, and terminated in the northeast. An interesting account is given of its appearance at Utica, where it is described as rising at one time in streams of light, of purple, yellow, green, and other colors, and exhibiting a rapid horizontal motion, passing and repassing like a company of dancers. The actual intersection of the beams so as to form the appearance called the *corona*, is mentioned as having been seen in the city of New York, at Hartwick, Cherry Valley, Hudson, and Prattsburg in Steuben county.

The only plausible explanation of the formation of the *corona*, is that which supposes the beams of the aurora to consist of cylindrical portions of some kind of matter, which becomes luminous as it passes into the higher regions of the atmosphere; and that the cylindrical beams shoot up from many points of the earth's surface, nearly parallel to each other, and in the direction of the dipping needle. Being at different distances from the observer, they appear of different elevations; and sometimes, when seeming to overlap each other, they form continued streaks of light in every part of the visible heavens. The *corona*, according to this hypothesis, is the perspective projection on the sky, of the beams which are shooting up at the same instant on all sides of the observer, and which, being all parallel to the dipping needle, appear to converge as it were to a vanishing point, situated, in the State of New York, about 15° south of the zenith. If this hypothesis be correct, (and it seems a strict geometrical deduction from actual appearances,) it would follow that on the evening of the 19th of April,

beams of auroral matter, were shooting up from every part of the surface of the State of New York.

But the most interesting circumstance in reference to this aurora, is that which I have learned from the December number of the *Journal of the Royal Institution of Great Britain*, viz., the fact of a disturbance of terrestrial magnetism being observed by Mr. Christie in England, on the same evening, and at nearly the same time the disturbance was witnessed in Albany, and that too in connection with the appearance of an aurora.

Mr. Christie had adjusted a magnetic needle for the express purpose of observing the effect when an aurora should appear, but was not so fortunate as to be able to make any observations with it until the evening of the 19th of April. His apparatus consisted of a light needle six inches long, suspended within a compass box by a fine brass wire $\frac{1}{600}$ of an inch in diameter, and twenty-three inches long. The needle was deflected from the magnetic meridian by the repulsive action of two bar magnets placed on opposite sides of it; so that instead of pointing to the magnetic north, it settled in the direction of N. 37° W. As the needle assumed this position in consequence of the attractive force of the earth, and the repulsive force of the magnets, a deviation from the north towards the west would indicate a diminution of the terrestrial horizontal intensity, and a deviation towards the north an increase in that intensity, the intensity of the magnets remaining the same. At 10 o'clock P. M. on the evening of the 19th, during the appearance of the aurora, Mr. Christie found the needle vibrating between N. $43^{\circ} 40'$ W. and N. $42^{\circ} 40'$ W. At 10h. 15m. its direction was N. 34° W. It continued to approach the north until 10h. $37\frac{1}{2}$ m. when it pointed N. $33^{\circ} 30'$ W. It again receded from the pole, and at 10h. 40m. vibrated between N. 37° W. and N. 36° W. The next morning at 7h. 20m. the needle pointed N. 40° W. From this brief abstract of Mr. Christie's observations, it will be seen that the horizontal intensity was less than usual at 10 o'clock; that it increased until 10h. $37\frac{1}{2}$ m. when it was greater than in its undisturbed state;

and that it again decreased, and was less than usual the next morning at 7h. 20m.

By adding five hours to the time of the observations made at Albany, we shall have nearly the corresponding time at Mr. Christie's residence in Woolwich. These times being 6h. and 10h. P. M. will therefore correspond with 11h. P. M. and 3h. A. M. of time at Woolwich. From this it appears that the observations at Albany were made at a period of absolute time between the last observation of Mr. Christie on the evening of the 19th, and the morning of the 20th. The only interesting result however which apparently can be drawn from a comparison of the observations, is that at both places there was a disturbance of terrestrial intensity at the same time; the intensity rising above and sinking below its usual state at each, although these changes did not occur in the same order at both places.

I am not aware that a simultaneous disturbance of terrestrial magnetism, in connexion with an aurora, has ever before been noted at two places so distant from each other. Nor do I think the co-incidence in this case in the least degree accidental. On the contrary, it appears to me highly probable that the disturbing cause was not only common to both places, but was also active at the same time in a great portion of the northern part of the globe. A brilliant aurora is by no means a local phenomenon. That of the 28th of August, 1827, was visible over nearly the whole of the northern States, in Canada, and also from some part of the Atlantic ocean. But what places the extensive and simultaneous appearance of the aurora in a more striking point of view than any in which it perhaps was ever before exhibited, is the comparison of the notices of the aurora given under the monthly meteorological reports in the *Annals of Philosophy* for 1830 and 1831, and the Reports of the Regents of the University of the State of New York for the same period. By inspecting these two publications, it will be seen that from April 1830 to April 1831 inclusive, the aurora borealis was remarkably frequent and brilliant, both in Europe and in this country; and *that most of the auroras*

described in the *Annals* for this time, particularly the brilliant ones, were seen on the same evening in England and in the State of New York.

The particular days on which the aurora appeared in England, are not mentioned in the *Annals*, except when the aurora is considered on some accounts interesting. By comparing those which are thus noticed with the Regents' Reports, the following results are obtained:

The first aurora mentioned in the *Annals* for 1830, occurred on the 19th of April. A particular description is given of its appearance in England, and also a notice of its being seen in Scotland. In the State of New York, a brilliant aurora was extensively seen on the same evening. Accounts are given of it from Auburn, Cambridge, Canajoharie, Cayuga, Franklin, Hudson, Lansingburgh, Lowville, Oxford, Pompey, Rochester, Union, Cazenovia, and Utica.

The second aurora noticed in the *Annals*, is that of the 20th of August. An aurora was also seen in the State of New York, at Lowville, Pompey, Cazenovia, and is particularly described as presenting an unusual appearance at Utica.

The next aurora which appeared worthy of a particular notice in the *Annals*, happened on the 7th of September; and the same evening an aurora was seen at Lewiston in Niagara county. On the 17th of the same month an aurora was also observed in England, and the same time at Pompey, St. Lawrence and Utica.

Under the report of the meteorology for the month of October, in the *Annals*, two auroras are described as appearing, one on the evening of the 5th, and the other on that of the 16th. These were both seen in the State of New York, the first at Utica, and the second at Lowville.

Two auroras are particularly mentioned as appearing in England in November; but no corresponding ones are noticed in the Report of the Regents, as having been seen in the State of New-York.

In the meteorological reports for the month of December, in the *Annals*, there are five auroras mentioned. The most

interesting of these happened on the 11th, and exhibited peculiar appearances. At one time, from a segment of the horizon of 70 degrees in extent, there emanated several flame-colored perpendicular columns, some of which were 2 degrees wide and 30 in altitude: these were succeeded by others, which ultimately exhibited red and purple tints. Many persons in England saw the aurora, and described it as exhibiting an awful appearance from a mixture of the colors. The most brilliant aurora which appeared in the State of New York during 1830, happened on the same evening. At Albany, it extended nearly 90 degrees around the northern horizon; and at one time, a row of bright columns rose from an arch, and extended upwards, some of them nearly to the north star. The columns from the western limb of the arch were slightly tinged with redness; all the others were white. At Lowville, flashes of light are described as arising from the north to the zenith, and thence descending half-way to the southern horizon. It was brilliant at Auburn, Dutchess, Erasmus Hall, Lansingburgh, Hartwick, Lewiston, North Salem, Plattsburgh, Rochester, St. Lawrence, Union, and Utica. An aurora also appeared on the 12th of the same month, and a brilliant one was likewise seen in the State of New York, at Auburn, Dutchess, Franklin, Fredonia, Ithaca, Lansingburgh, Lewiston, Middlebury, North-Salem, Plattsburgh, Pompey, St. Lawrence, Utica. Faint auroras are also mentioned as appearing in England on the 13th and 14th, and another on the evening of the 25th; but no corresponding ones are described in the Regents' Report.

In 1831, the first aurora described in the *Annals* is that of the 7th of January; "and of all the auroræ boreales," says the author, "that have been observed here (in England) the last twenty years, (some say forty,) this was the most extensive, the most beautiful in colors, and the most interesting on account of the singular phenomena which it displayed, in the number of distinct luminous bows which were presented in the course of the night." Several communications are given on the subject of this aurora, in the *Annals of Phi-*

losophy, and the *Journal of the Royal Institution*. It was seen at Paris, and at Brussels. A particular description is given of its appearance in Utrecht, by Prof. Moll. On inspecting the Reports for 1831, I find that an aurora was seen in the State of New-York, at places in the extreme east and west part of the State—at North-Salem on the east side of the Hudson river, and Fredonia near Lake Erie; and intermediate to these places, at Utica, and Pompey. The *Annals* also mention that faint auroras were seen on the evening preceding and following, and also an aurora on the 11th. An aurora was noticed at several places in New York on the evening of the 6th, but none on that of the 8th or 11th.

No auroras are mentioned in the *Annals* under the meteorology for February, but three are noticed for March; the first, an interesting one, appeared on the 7th; the second, on the 8th; and the third, a bright one, on the 11th. By referring to the Reports of the Regents, it will be seen that auroras were observed on the same evening in several places in the State of New York.

The next aurora mentioned in the *Annals* is that of the 19th of April, which has been the principal subject of this paper. An interesting account is given of its appearance in England, which states that at one time there was a grand display of about ten long active streamers along an arch of the aurora, several of which ascended to an altitude of sixty degrees; and when most active, many passed beyond the zenith, exhibiting at the same time several prismatic colors. At 10 o'clock, the arch of the aurora extended 150 degrees. The extensive appearance of this aurora in the State of New York, and the magnetic disturbance accompanying it, have already been sufficiently described.

The above co-incidences appear too numerous to admit the supposition that they are merely accidental, particularly when it is recollected that there are many causes to prevent the co-temporaneous appearance of an aurora being recorded at two distant places, although it exists at both. While it is observed at one place, it may be obscured by clouds, or may escape the notice of the meteorological observer, at the other.

Besides this, the co-incidences occurred on the evenings when the aurora was most brilliant, and consequently when its action might be supposed most extensive. These simultaneous appearances of the meteor in Europe and America would therefore seem to warrant the conclusion, that the aurora borealis cannot be classed among the ordinary local meteorological phenomena, but that it must be referred to some cause connected with the general physical principles of the globe; and that the more energetic actions of this cause, whatever it may be, affects simultaneously a great portion of the northern hemisphere.

ON THE PRODUCTION OF CURRENTS AND SPARKS OF ELECTRICITY FROM MAGNETISM.

(Silliman's American Journal of Science, July, 1832; vol. xxii, pp. 403-408.)

Although the discoveries of Oersted, Arago, Faraday, and others, have placed the intimate connection of electricity and magnetism in a most striking point of view, and although the theory of Ampere has referred all the phenomena of both these departments of science to the same general laws, yet until lately one thing remained to be proved by experiment, in order more fully to establish their identity; namely, the possibility of producing electrical effects from magnetism. It is well known that surprising magnetic results can readily be obtained from electricity, and at first sight it might be supposed that electrical effects could with equal facility be produced from magnetism; but such has not been found to be the case, for although the experiment has often been attempted, it has nearly as often failed.

It early occurred to me, that if galvanic magnets on my plan were substituted for ordinary magnets, in researches of this kind, more success might be expected. Besides their great power, these magnets possess other properties, which render them important instruments in the hands of the experimenter; their polarity can be instantaneously reversed, and their magnetism suddenly destroyed or called into full action, according as the occasion may require. With this view, I commenced, last August, the construction of a much larger galvanic magnet than, to my knowledge, had before been attempted, and also made preparations for a series of experiments with it on a large scale, in reference to the production of electricity from magnetism. I was however at that time accidentally interrupted in the prosecution of these experiments, and have not been able since to resume them, until within the last few weeks, and then on a much smaller scale than was at first intended. In the mean time, it has been announced in the 117th number of the *Library of Useful Knowledge*, that the result so much sought after

has at length been found by Mr. Faraday of the Royal Institution. It states that he has established the general fact, that when a piece of metal is moved in any direction, in front of a magnetic pole, electrical currents are developed in the metal, which pass in a direction at right angles to its own motion, and also that the application of this principle affords a complete and satisfactory explanation of the phenomena of magnetic rotation. No detail is given of the experiments, and it is somewhat surprising that results so interesting, and which certainly form a new era in the history of electricity and magnetism, should not have been more fully described before this time in some of the English publications; the only mention I have found of them is the following short account from the *Annals of Philosophy* for April, under the head of Proceedings of the Royal Institution:

"Feb. 17.—Mr. Faraday gave an account of the first two parts of his researches in electricity; namely, Volta-electric induction and magneto-electric induction. If two wires, *A* and *B*, be placed side by side, but not in contact, and a Voltaic current be passed through *A*, there is instantly a current produced by induction in *B*, in the opposite direction. Although the principal current in *A* be continued, still the secondary current in *B* is not found to accompany it, for it ceases after the first moment, but when the principal current is stopped then there is a second current produced in *B*, in the opposite direction to that of the first produced by the inductive action, or in the same direction as that of the principal current.

"If a wire, connected at both extremities with a galvanometer, be coiled in the form of a helix around a magnet, no current of electricity takes place in it. This is an experiment which has been made by various persons hundreds of times, in the hope of evolving electricity from magnetism, and as in other cases in which the wishes of the experimenter and the facts are opposed to each other, has given rise to very conflicting conclusions. But if the magnet be withdrawn from or introduced into such a helix, a current of electricity is produced *whilst the magnet is in motion*, and is rendered evident by the deflection of the galvanometer. If a single wire be passed by a magnetic pole, a current of electricity is induced through it which can be rendered sensible."*

*[Phil. Mag.; and Annals of Philosophy; April, 1832: vol. xi, p. 300.]

Before having any knowledge of the method given in the above account, I had succeeded in producing electrical effects in the following manner, which differs from that employed by Mr. Faraday, and which appears to me to develop some new and interesting facts. A piece of copper wire, about thirty feet long and covered with elastic varnish, was closely coiled around the middle of the soft iron armature of the galvanic magnet, described in vol. xix of the *American Journal of Science*, and which, when excited, will readily sustain between six hundred and seven hundred pounds. The wire was wound upon itself so as to occupy only about one inch of the length of the armature which is seven inches in all. The armature, thus furnished with the wire, was placed in its proper position across the ends of the galvanic magnet, and there fastened so that no motion could take place. The two projecting ends of the helix were dipped into two cups of mercury, and there connected with a distant galvanometer by means of two copper wires, each about forty feet long. This arrangement being completed, I stationed myself near the galvanometer and directed an assistant at a given word to immerse suddenly, in a vessel of dilute acid, the galvanic battery attached to the magnet. At the instant of immersion, the north end of the needle was deflected 30° to the west, indicating a current of electricity from the helix surrounding the armature. The effect however appeared only as a single impulse, for the needle, after a few oscillations, resumed its former undisturbed position in the magnetic meridian, although the galvanic action of the battery, and consequently the magnetic power was still continued. I was however much surprised to see the needle suddenly deflected from a state of rest to about 20° to the east, or in a contrary direction when the battery was withdrawn from the acid, and again deflected to the west when it was re-immersed. This operation was repeated many times in succession, and uniformly with the same result, the armature the whole time remaining immovably attached to the poles of the magnet, no motion being required to produce the effect, as it appeared to take place only in consequence of the instantaneous development

of the magnetic action in one, and the sudden cessation of it in the other.

This experiment illustrates most strikingly the reciprocal action of the two principles of electricity and magnetism, if indeed it does not establish their absolute identity. In the first place, magnetism is developed in the soft iron of the galvanic magnet by the action of the currents of electricity from the battery, and secondly the armature, rendered magnetic by contact with the poles of the magnet, induces in its turn currents of electricity in the helix which surrounds it; we have thus as it were electricity converted into magnetism and this magnetism again into electricity.

Another fact was observed which is somewhat interesting inasmuch as it serves in some respects to generalize the phenomena. After the battery had been withdrawn from the acid, and the needle of the galvanometer suffered to come to a state of rest after the resulting deflection, it was again deflected in the same direction by partially detaching the armature from the poles of the magnet to which it continued to adhere from the action of the residual magnetism, and in this way, a series of deflections, all in the same direction, was produced by merely slipping off the armature by degrees until the contact was entirely broken. The following extract from the register of the experiments exhibits the relative deflections observed in one experiment of this kind.

At the instant of immersion of the battery,	deflection	40° west.
" " emersion "	" "	18° east.
Armature partially detached,	" "	7° east.
Armature entirely detached,	" "	12° east.

The effect was reversed in another experiment, in which the needle was turned to the west in a series of deflections by dipping the battery but a small distance into the acid at first and afterwards immersing it by degrees.

From the foregoing facts, it appears that a current of electricity is produced, for an instant, in a helix of copper wire surrounding a piece of soft iron whenever magnetism is induced in the iron; and a current in an opposite direction when the magnetic action ceases; also that an instantaneous current in one or the other direction accompanies every change in the magnetic intensity of the iron.

Since reading the account before given of Mr. Faraday's method of producing electrical currents I have attempted to combine the effects of motion and induction; for this purpose a rod of soft iron ten inches long and one inch and a quarter in diameter, was attached to a common turning lathe, and surrounded with four helices of copper wire in such a manner that it could be suddenly and powerfully magnetized, while in rapid motion, by transmitting galvanic currents through three of the helices; the fourth being connected with the distant galvanometer was intended to transmit the current of induced electricity; all the helices were stationary while the iron rod revolved on its axis within them. From a number of trials in succession, first with the rod in one direction then in the opposite, and next in a state of rest, it was concluded that no perceptible effect was produced on the intensity of the *magneto-electric* current by a rotary motion of the iron combined with its sudden magnetization.

The same apparatus however furnished the means of measuring separately the relative power of motion and induction in producing electrical currents. The iron rod was first magnetized by currents through the helices attached to the battery and while in this state one of its ends was quickly introduced into the helix connected with the galvanometer; the deflection of the needle in this case was seven degrees. The end of the rod was next introduced into the same helix while in its natural state and then suddenly magnetized; the deflection in this instance amounted to thirty degrees, showing a great superiority in the method of induction.

The next attempt was to increase the *magneto-electric* effect while the magnetic power remained the same, and in this I was more successful. Two iron rods six inches long and one inch in diameter, were each surrounded by two helices and then placed perpendicularly on the face of the armature, and between it and the poles of the magnet, so that each rod formed as it were a prolongation of the poles, and to these the armature adhered when the magnet was excited. With this arrangement, a current from one helix produced a deflection of thirty-seven degrees; from two helices both on the same

rod fifty two degrees, and from three fifty nine degrees; but when four helices were used, the deflection was only fifty five degrees, and when to these were added the helix of smaller wire around the armature, the deflection was no more than thirty degrees. This result may perhaps have been somewhat affected by the want of proper insulation in the several spires of the helices, it however establishes the fact that an increase in the electric current is produced by using at least two or three helices instead of one. The same principle was applied to another arrangement which seems to afford the maximum of electric development from a given magnetic power; in place of the two pieces of iron and the armature used in the last experiments, the poles of the magnet were connected by a single rod of iron, bent into the form of a horse-shoe, and its extremities filed perfectly flat so as to come in perfect contact with the faces of the poles; around the middle of the arch of this horse-shoe, two strands of copper wire were tightly coiled one over the other. A current from one of these helices deflected the needle one hundred degrees, and when both were used the needle was deflected with such force as to make a complete circuit. But the most surprising effect was produced when instead of passing the current through the long wires to the galvanometer, the opposite ends of the helices were held nearly in contact with each other, and the magnet suddenly excited; in this case a small but vivid spark was seen to pass between the ends of the wires and this effect was repeated as often as the state of intensity of the magnet was changed.

In these experiments the connection of the battery with the wires from the magnet was not formed by soldering, but by two cups of mercury which permitted the galvanic action on the magnet to be instantaneously suspended and the polarity to be changed and rechanged without removing the battery from the acid; a succession of vivid sparks was obtained by rapidly interrupting and forming the communication by means of one of these cups; but the greatest effect was produced when the magnetism was entirely destroyed and instantaneously reproduced by a change of polarity.

It appears from the May No. of the *Annals of Philosophy* that I have been anticipated in this experiment of drawing sparks from the magnet by Mr. James D. Forbes of Edinburgh, who obtained a spark on the 30th of March; my experiments being made during the last two weeks of June. A simple notification of his result is given, without any account of the experiment, which is reserved for a communication to the Royal Society of Edinburgh; my result is therefore entirely independent of his and was undoubtedly obtained by a different process.

Electrical self-induction in a long helical wire.

I have made several other experiments in relation to the same subject, but which more important duties will not permit me to verify in time for this paper. I may however mention one fact which I have not seen noticed in any work, and which appears to me to belong to the same class of phenomena as those before described; it is this: when a small battery is moderately excited by diluted acid, and its poles which should be terminated by cups of mercury, are connected by a copper wire not more than a foot in length, no spark is perceived when the connection is either formed or broken; but if a wire thirty or forty feet long be used instead of the short wire, though no spark will be perceptible when the connection is made, yet when it is broken by drawing one end of the wire from its cup of mercury, a vivid spark is produced. If the action of the battery be very intense, a spark will be given by the short wire; in this case it is only necessary to wait a few minutes until the action partially subsides, and until no more sparks are given from the short wire; if the long wire be now substituted a spark will again be obtained. The effect appears somewhat increased by coiling the wire into a helix; it seems also to depend in some measure on the length and thickness of the wire. I can account for these phenomena only by supposing the long wire to become charged with electricity, which by its re-action on itself projects a spark when the connection is broken.

CONTRIBUTIONS TO ELECTRICITY AND MAGNETISM. No. I.
DESCRIPTION OF A GALVANIC BATTERY FOR PRODUCING ELECTRICITY OF DIFFERENT INTENSITIES.

(Transactions American Philosophical Society, n. s., vol. v, pp. 217-222.)*

Read January 16th, 1835.†

The following account of a Galvanic Battery, constructed under my direction for the Physical Department of the College of New Jersey, is submitted to the American Philosophical Society, with the intention of referring to it in some communications which I purpose making on the subject of Electricity and Magnetism. It is hoped however that the arrangement and details of the instrument, in themselves, will be found to possess some interest, since they have been adopted in most cases after several experiments and much personal labor.

The apparatus is intended to exhibit most of the phenomena of Galvanism and all those of Electro-Magnetism, on a large scale, with one battery. It was constructed to illustrate the several facts of these branches of science to my class, and also to be used as a convenient instrument of research in all cases where no very great degree of intensity is required.

The several parts of this battery are not soldered together forming one permanent galvanic arrangement, but are only temporarily connected by means of movable conductors and cups of mercury. The whole is constructed with reference to the principle well understood of producing electricity of greater or less intensity, by a change in the method of uniting the several elements with each other.

The apparatus consists of eighty-eight elements or pairs, composed of plates of rolled zinc nearly one eighth of an inch thick, nine inches wide, and twelve inches long, inserted into copper cases open at top and bottom. Eleven of these

*[The title-page of this volume bears date 1837.]

†[The date given in the "Transactions" is January 14. This appears from the Minutes to be a typographic error.]

elements are suspended together from two cross pieces of wood, and the whole number is thus arranged in eight sets, of eleven in each. These are supported by the ends of the cross pieces in a strong wooden frame, so as to be immersed in eight separate troughs: they thus form as many independent batteries, which can be used separately or together as the occasion may require. Each trough is divided into eleven cells by wooden partitions coated with cement. If one of the cells be charged with dilute acid, a single element may be excited without producing action in any other part of the battery. Each set or battery may also be lifted separately from the frame by its cross pieces, without disturbing the other parts of the apparatus.

The elements remain stationary, while the troughs are raised to them on a movable platform by the common application of a wheel and pinion.

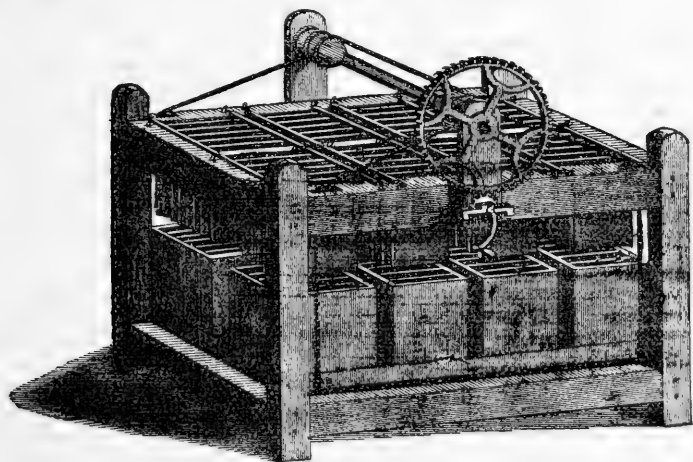


FIG. 1.—Galvanic Battery.

The general arrangement of the whole may be seen at once by a reference to the perspective drawing, fig. 1: *a a*, &c., represent the cross pieces resting on the upper part of the frame of the machine; *c c* is the movable platform.

A perspective view of one of the elements on a larger scale is given in fig. 3. *a a* are two cups of cast copper, with

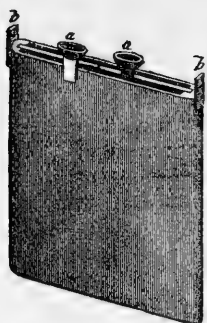


FIG. 3.

a broad stem on the bottom; one soldered to the zinc plate, and the other to the copper case. The cavity in these cups is about three eighths of an inch wide, a little more than an inch long, and half an inch deep. The cups being well amalgamated and partially filled with mercury, receive the ends of the copper conductors which unite the several elements.

For the purpose of suspension, a slip of copper, *b b*, with a hole in it, is soldered to each upper corner of the copper case; these fit loosely into a mortice or narrow groove in the cross pieces, and are secured by a pin of copper wire. When the pins are withdrawn, a single element may be removed from any part of the series, without disturbing the remainder.

The zinc plate is fastened into its copper case, without touching, by a piece of wood at each corner with a groove in it to receive the edge of the plate. The grooves in the two lower pieces of wood terminate at about a quarter of an inch from the lower end, and thus form shoulders, which prevent the plate from slipping down; while the wood itself is supported by a flange, formed by bending in the lower edges of the corner of the case.

There are two principal sets of connectors; the first is formed of bars of cast copper thirteen inches long, an inch wide, and about an eighth of an inch thick. On the lower side of these are eleven broad projections, which fit loosely



FIG. 4.—Homogeneous Connector.

into a row of cups on the plates of zinc or copper. Fig. 4 represents one of these connectors with a thimble soldered on the upper side for the purpose of attaching a conductor, which may serve as a pole.

There are two of these for each of the eight batteries, and when in their places, one unites all the zinc, and the other all the copper, so that the battery becomes a "calorimotor" of a single element or pair. If with this arrangement the several batteries be connected, zinc to zinc and copper to copper, by conductors reaching from one to the other, the whole apparatus of eighty-eight elements becomes a large "calorimotor" of a single pair; but if the copper of the first be united to the zinc of the second, and so on, it then forms a "calorimotor" of eight elements, and by a simple change may be reduced to one of four, or of two elements.

The other set of connectors consists of short pieces of thick copper plate, the ends of which are bent down at right angles, so as to dip into the cups of mercury: they connect the copper of one element with the zinc of the next. Ten of these, intended to unite the elements of one battery, are shown in



FIG. 5.—Alternate or Serial Connector.

fig. 5. They are attached crosswise to a slip of harness leather, which, by its pliability, permits them to fit loosely into the cups, while it enables the whole set to be removed as one piece. When these connectors are in their places, and the several batteries united, the copper pole of the one, with the zinc pole of another, and so on, the whole series forms a "deflagrator" of eighty-eight elements.

The different arrangements of the several connectors will be readily understood by a reference to the plan drawing, fig. 2, which exhibits one-half of the whole apparatus arranged as a "deflagrator" of forty-four elements, and the other half as a "calorimotor" of four pairs. By closely inspecting the drawing, it will be seen that the connexion in the upper half of the figure is from the copper of the first element to the zinc of the next, and so on through the entire series of forty-four elements. In the lower half the union of copper and zinc takes place only between the poles of the dif-

ferent batteries; the several elements of which are united so as to act as one plate of copper and one of zinc. The four

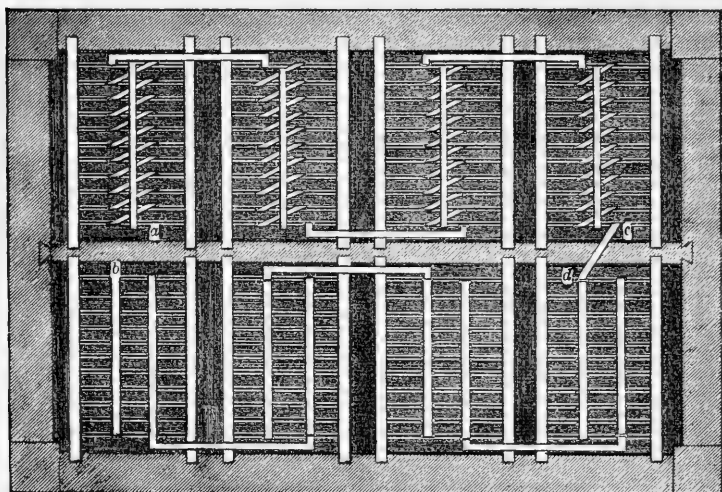


FIG. 2.—Plan of Battery.

batteries therefore will act together as a “calorimotor” of four elements. The arrangement, as given in the drawing, is intended to illustrate by one figure the two sets of connectors; but such an arrangement becomes interesting in practice in determining the effect of the conjoined actions of batteries producing electricity of different intensities.

The circuit of the connections as given in the figure is complete except at *a b*; the two plates at this point form the poles of the battery. A set of poles however may be formed at any other point of the circuit, by making an interruption at that place. In the same way two or more sets may be formed. It furnishes an interesting and instructive experiment to place a pair of large decomposing plates at *a b* and another at *c d*. When only one of these is plunged into a saline solution, the circuit being interrupted at the other pair, no effect is produced; but as soon as this other is plunged into a similar solution, a copious decomposition simultaneously takes place at both. Also the co-temporaneous action in each element of the battery is pleasingly shown by placing

at the same time several large magnetic needles on the different parts of the apparatus. These instantly change their direction when the second pair of decomposing plates touch the solution.

At first sight it might be supposed that there would be some difficulty in entering the several plates into their respective cells, but this is obviated by the precise movement of the platform on which the troughs stand. Its horizontal position is adjusted by four screws (*c c* fig. 1), and its corners slide in grooves in the upright posts of the large frame. Besides this, when the plates are once entered, they are not required to be entirely withdrawn from the cells until the end of the series of experiments; since the acid descends as the plates are withdrawn, and finally fills but little more than three-fourths of the capacity of the cells. When a plate accidentally catches on the side of the cell, the battery to which it belongs is gently raised in its place and the plate adjusted.

This apparatus readily furnishes the means of making comparative experiments on the difference produced by partial and perfect insulation. When no higher degree of intensity is required than that afforded by eight pairs of plates, perfect insulation is obtained by the eight separate troughs. In higher degrees of intensity the partitions in the troughs furnish the means of perfectly insulating forty-eight of the elements: this is effected by simply charging with acid every other cell in each of the troughs, and connecting the corresponding element by conductors, which pass over the intermediate elements without touching them: with this arrangement we have six cells in each trough separated from one another by a cell without acid, or in effect by a stratum of air. For comparison with these a set of troughs has been constructed without partitions.

The want of perfect insulation is not very perceptible in the common experiments of the deflagration of large and perfect conductors; but where the decomposition of a liquid is attempted, or the battery required to act on a small or imperfect conductor, the loss of power is very great, the apparatus

partially discharging itself through its own liquid, and the intensity at the poles does not increase with a short interruption of the current.

There is also considerable loss on account of imperfect insulation even in the case of low intensity, and when the poles are connected by a perfect conductor. In one experiment with an arrangement of five pairs, and the poles united by a conductor composed of thirty strands of copper bell-wire, each forty feet long, the loss was found to be at least one-seventh, as measured by the quantity of zinc surface required to be immersed in order to produce the same magnetic effect. I would infer from this that the most perfect of all Dr. Hare's ingenious galvanic arrangements is that in which the elements dip into separate glass vessels, as this combines perfect insulation with the power of instantaneous immersion.

A variety of experiments have been made during the past year with this instrument on several points of Galvanism and Electro-magnetism, which will be communicated to the Society as soon as my engagements will permit me to repeat and arrange them for publication .

FACTS IN REFERENCE TO THE SPARK, ETC., FROM A LONG CONDUCTOR UNITING THE POLES OF A GALVANIC BATTERY.*

(Journal of the Franklin Institute, March, 1835, vol. xv, pp. 169, 170.)

Extract from the proceedings of the stated meeting of the American Philosophical Society, January 16, 1835.†

The following facts in reference to the spark, shock, &c., from a galvanic battery of a single pair when the poles are united by a long conductor, were communicated by Prof. Joseph Henry, and those relating to the spark were illustrated experimentally:

1. A long wire gives a more intense spark than a short one. There is however with a given surface of zinc a length beyond which the effect is not increased; a wire of one hundred and twenty feet gave about the same intensity of spark as one of two hundred and forty feet.

2. A thick wire gives a larger spark than a smaller one of the same length.

3. A wire coiled into a helix gives a more vivid spark than the same wire when uncoiled.

4. A ribbon of copper, coiled into a flat spiral, gives a more intense spark than any other arrangement yet tried.

* TO THE COMMITTEE ON PUBLICATIONS.

GENTLEMEN: The American Philosophical Society, at their last stated meeting, authorized the publication of the following abstract of a verbal communication made to the Society by Professor Henry on the sixteenth of January last. A memoir on this subject has been since submitted to the Society containing an extension of the subject, the primary fact in relation to which was observed by Professor Henry as early as 1832, and announced by him in the *American Journal of Science* (vol. xxii, p. 408). Mr. Faraday having recently entered upon a similar train of observations, the immediate publication of the accompanying is important, that the prior claims of our fellow-countryman may not be overlooked.

Very respectfully, yours,

A. D. BACHE,

One of the Secretaries Am. Philos. Soc.

PHILADELPHIA, Feb. 7th, 1835.

†[The Minutes of the Am. Phil. Soc. from 1743-1837 have only recently been published, (1885,) the first volume of the published "Proceedings" commencing with 1838.]

5. The effect is increased, by using a longer and wider ribbon, to an extent not yet determined. The greatest effect has been produced by a coil ninety-six feet long and weighing 15 lbs. ; a larger conductor has not been received.

6. A ribbon of copper, first doubled into two strands and then coiled into a flat spiral, gives no spark, or a very feeble one.

7. Large copper handles, soldered to the ends of a coil of ninety-six feet, and these both grasped, one by each hand, a shock is felt at the elbows, when the contact is broken in a battery of a single pair with one and a half feet of zinc surface.

8. A shock is also felt when the copper of the battery is grasped with one hand and one of the handles with the other ; the intensity however is not as great as in the last case. This method of receiving the shock may be called the direct method ; the other, the lateral one.

9. The decomposition of a liquid is effected by the use of the coil with a battery of a single pair, by interrupting the current and introducing a pair of decomposing wires.

10. A mixture of oxygen and hydrogen is also exploded by means of the coil, and breaking the contact, in a bladder containing the mixture.

11. The property of producing an intense spark is induced, on a short wire, by introducing at any point of a compound galvanic current a large flat spiral, and joining the poles by the short wire.

12. A spark is produced when the plates of a single battery are separated by a foot or more of diluted acid.

13. Little or no increase in the effect is produced by inserting a piece of soft iron into the centre of a flat spiral.

14. The effect produced by an electro-magnet, in giving the shock, is due principally to the coiling of the long wire which surrounds the soft iron.

[The foregoing article was re-printed in Silliman's American Journal of Science, July, 1835, vol. XXVIII, pp. 327-329.]

APPENDIX TO THE ABOVE—ACTION OF A SPIRAL CONDUCTOR.

(Silliman's Am. Journal of Science, July, 1835, vol. XXVIII, pp. 329-331.)

To Prof. Silliman.

SIR: With this I send you a copy of a paper communicated by me to the American Philosophical Society, on the influence of a spiral conductor in increasing the intensity of electricity from a galvanic arrangement of a single pair. As the part of the Transactions which contains the paper has not yet been distributed, I regret that I am not at liberty to request you to insert the article for more general diffusion in your valuable Journal. An abstract however of the principal facts was ordered to be published, and appeared in the March number of the *Franklin Journal*. A copy was also sent by Prof. Bache for insertion in the *American Journal*; but as it did not appear in the last number, you will confer a favor by inserting it in the next.*

Should you wish to repeat the experiments, you will find them most interestingly exhibited with one of Dr. Hare's "calorimotors." If a galvanic current of very low intensity from this instrument be transmitted through a spiral conductor formed of copper ribbon about one inch wide, from sixty to one hundred feet long, well covered with silk, and the several spires closely wound on each other, the "calorimotor" will be almost converted into a "deflagrator." One end of the conductor being attached to a pole of the battery, and the other brought in contact with or rubbed along the edge of a plate of metal attached to the other pole, a vivid deflagration will be produced, even when the plates are immersed in a mixture containing not more than one part of acid to five hundred parts of water.

If a copper cylinder of about two inches in diameter and four or five inches long, to serve as a handle, be attached to each end of the spiral by an intervening piece of copper wire and these cylinders grasped with moistened hands, a series of shocks will be felt when one end of the conductor

* [Then mislaid, but now inserted; as above.—*Ed.*]

is drawn across the edges of the zinc plates, the other end being in contact with the copper pole.

Another method of producing the shocks is to place the spiral between two batteries, each of a single pair, so as to connect the copper of one with the zinc of the other. If the extreme poles of this compound arrangement be terminated by the copper handles, and these be brought in contact, holding one in each hand, a deflagration of the metal will be produced, and a thrilling sensation, scarcely supportable, felt in each arm. The effect is much increased if the handles are rough. Two cylinders of cast zinc terminating the poles were found to produce the greatest effect when rubbed on each other.

To exhibit these phenomena in a striking manner a galvanic battery of considerable size is required. I have used one for the purpose containing about forty feet of zinc surface, estimating both sides of the plate. This battery was first immersed for a short time in a strong solution of acid, to dissolve the coating of oxide, and then removed to a vessel containing pure water. The small quantity of acid adhering to the plates was sufficient to produce, by means of the spiral, the deflagration of the metals, which would shock and snap for many hours in succession, while with a short conductor the battery in the same state gave no signs of electricity.

This will be found an economical method of exhibiting some very interesting experiments with the calorimotor. After having shown the ordinary heating powers of the instrument with strong acid, transfer the plates to a trough containing pure water, and the action of the coil may be shown for an almost indefinite time, at little or no expense of zinc or acid.

The spiral produces no increased effect when applied to a galvanic trough of one hundred four-inch plates. If however a coil of five or six hundred feet of wire be substituted, an increase of action will be manifest. The length of the coil must be in some ratio to the "projectile" force of the electricity, and also the quantity to the thickness of the conductor, in order to produce a maximum result. Thus, when

a small battery is used with a large conductor, it must be charged with strong acid.

The action of the spiral conductor depends on the inductive principle of an electric current discovered by Mr. Faraday, and is consequently intimately connected with the whole subject of magneto-electricity.

If a magnet be fitted up in the ordinary manner, with a spool of wire covered with silk around the keeper, the intensity of the shock will be astonishingly increased if the current generated in the spool be transmitted through a coil of several hundred feet of fine wire surrounding the legs of the magnet. To produce this effect however it is necessary that the wire on the spool and that around the magnet should at first form a continuous closed circuit, and that this be interrupted at the same instant that the keeper is detached, so that the induced current may pass entirely through the body.

The intense shock may also be given by generating a current with one magnet, and accelerating it by passing it around a second magnet.

Professor Emmet, of the University of Virginia, more than two years since, made the interesting discovery that the magneto-electric current is much increased in intensity by passing it through a portion of the generating magnet. This interesting fact, which he has applied with much success to improve the magneto-electric machine, may undoubtedly be referred to the same cause as the action of the spiral, and I have succeeded in modifying the application of it in several ways.

These magnetic experiments were made on the first or second day of May last, while on a visit to Philadelphia, with the large magnet belonging to the museum, and kindly lent to me for the purpose by Mr. Peale. They were made with the assistance of my friend, Mr. Lukens; but as I have not had an opportunity of verifying them, I cannot at present give a more detailed account. I have also made some preparations for applying the same principle to increase the action of a thermo-electric current.

CONTRIBUTIONS TO ELECTRICITY AND MAGNETISM. No. II.

ON THE INFLUENCE OF A SPIRAL CONDUCTOR IN INCREASING THE INTENSITY OF ELECTRICITY FROM A GALVANIC ARRANGEMENT OF A SINGLE PAIR, ETC.

(Transactions American Philosophical Society, n. s., vol. v, pp. 223-231.)

Read February 6th, 1835.

In the *American Journal of Science* for July, 1832, I announced a fact in Galvanism which I believe had never before been published. The same fact however appears to have been since observed by Mr. Faraday, and has lately been noticed by him in the November number of the *London and Edinburgh Journal of Science* for 1834.

The phenomenon as described by me is as follows: "When a small battery is moderately excited by diluted acid, and its poles, terminated by cups of mercury, are connected by a copper wire not more than a foot in length, no spark is perceived when the connection is either formed or broken; but if a wire thirty or forty feet long be used instead of the short wire, though no spark will be perceptible when the connection is made, yet when it is broken by drawing one end of the wire from its cup of mercury, a vivid spark is produced. If the action of the battery be very intense, a spark will be given by a short wire; in this case it is only necessary to wait a few minutes until the action partially subsides, and until no more sparks are given from the wire; if the long wire be now substituted a spark will be again obtained. The effect appears somewhat increased by coiling the wire into a helix; it seems also to depend in some measure on the length and thickness of the wire. I can account for these phenomena only by supposing the long wire to become charged with electricity, which by its re-action on itself projects a spark when the connection is broken."*

The above was published immediately before my removal from Albany to Princeton, and new duties interrupted for

*Silliman's *Journal of Science*, vol. 22, page 408. [See *ante*, p. 79.]

a time the further prosecution of the subject. I have however been able during the past year to resume in part my investigations, and among others, have made a number of observations and experiments which develop some new circumstances in reference to this curious phenomenon.

These, though not as complete as I could wish, are now presented to the Society, with the belief that they will be interesting at this time on account of the recent publication of Mr. Faraday on the same subject.

The experiments are not given in the precise order in which they were first made, but in that which I deem best suited to render them easily understood; they have however been repeated for publication in almost the same order in which they are here given.

1. A galvanic battery, consisting of a single plate of zinc and copper, and exposing one and a half square feet of zinc surface, including both sides of the plate, was excited with diluted sulphuric acid, and then permitted to stand until the intensity of the action became nearly constant. The poles connected by a piece of copper bell-wire of the ordinary size and five inches long, gave no spark when the contact was broken.

2. A long portion of wire, from the same piece with that used in the last experiment, was divided into equal lengths of fifteen feet, by making a loop at each division, which could be inserted into the cups of mercury on the poles of the battery. These loops being amalgamated and dipped in succession into one of the cups while the first end of the wire constantly remained in the other, the effect was noted. The first length, or fifteen feet, gave a very feeble spark, which was scarcely perceptible. The second, or thirty feet, produced a spark a little more intense, and the effect constantly increased with each additional length until one hundred and twenty feet were used; beyond this there was no perceptible increase; and a wire of two hundred and forty feet gave a spark of rather less intensity. From other observations I infer that the length necessary to produce a maximum result, varies with the intensity of the action of the battery, and also with its size.

3. With equal lengths of copper wire of unequal diameters, the effect was greater with the larger; this also appears to depend in some degree on the size of the battery.

4. A length of about forty feet of the wire used in experiments first and second was covered with silk and coiled into a cylindrical helix of about two inches in height and the same in diameter. This gave a more intense spark than the same wire when uncoiled.

5. A ribbon of sheet copper nearly an inch wide, and twenty-eight and a half feet long, was covered with silk, and rolled into a flat spiral similar to the form in which woollen binding is found in commerce. With this a vivid spark was produced, accompanied by a loud snap. The same ribbon uncoiled gave a feeble spark similar in intensity to that produced by the wire in experiment third. When coiled again the snap was produced as at first. This was repeated many times in succession, and always with the same result.

6. To test still farther the influence of coiling, a second ribbon was procured precisely similar in length and in all other respects to the one used in the last experiment. The effect was noted with one of these coiled into a flat spiral and the other uncoiled, and again with the first uncoiled and the second coiled. When uncoiled each gave a feeble spark of apparently equal intensity, when coiled, a loud snap. One of these ribbons was next doubled into two equal strands, and then rolled into a double spiral with the point of doubling at the centre. By this arrangement the electricity, in passing through the spiral, would move in opposite directions in each contiguous spire, and it was supposed that in this case the opposite actions which might be produced would neutralize each other. The result was in accordance with the anticipation; the double spiral gave no spark whatever, while the other ribbon coiled into a single spiral produced as before a loud snap. Lest the effect might be due to some accidental touching of the different spires, the double spiral was covered with an additional coating of silk, and also the other ribbon was coiled in the same manner; the effect with both was the same.

7. In order to increase if possible the intensity of the spark while the battery remained the same, larger spirals were applied in succession. The effect was increased until one of ninety-six feet long, an inch and a half wide and weighing fifteen pounds, was used. The snap from this was so loud that it could be distinctly heard in an adjoining room with the intervening door closed. Want of materials has prevented me from trying a larger spiral conductor than this, but it is probable that there is a length which, with a given quantity and intensity of galvanism, would produce a maximum effect. When the size of the battery is increased, a much greater effect is produced with the same spiral. Thus when the galvanic apparatus, described in the first article, is arranged as a "calorimotor" of eight pairs, the snap produced on breaking contact, with the spiral last described, resembled the discharge of a small Leyden jar highly charged.

8. A handle of thick copper was soldered on each end of the large spiral at right angles to the ribbon similar to those attached to the wires in Pixii's magneto-electric machine for giving shocks. When one of these was grasped by each hand, and the contact broken, a shock was received which was felt at the elbows, and this was repeated as often as the contact was broken. This shock is rather a singular phenomenon, since it appears to be produced by a lateral discharge, and it is therefore important to determine its direction in reference to the primary current.

9. A shock is also received when the copper of the battery is grasped by one hand, and the handle attached to the copper pole of the ribbon with the other. This may be called the direct shock, since it is produced by a part of the direct current. It is however far less intense than that produced by the lateral discharge.

10. When the poles were joined by two coils, connected by a cup of mercury between them, a spark was produced by breaking the circuit at the middle point, and when a pair of platina wires was introduced into the circuit with the large coil and immersed in a solution of acid, decomposition took place in the liquid at each rupture of contact, as was shown

by a bubble of gas given off at each wire. It must be recollected that the shocks and the decomposition here described were produced by the electricity from a single pair of plates.

11. The contact with the poles of the battery and the large spiral being broken in a vessel containing a mixture of hydrogen and atmospheric air, an explosion was produced.

I should also mention that the spark is generally attended with a deflagration of the mercury, and that when the end of the spiral is brought in contact with the edge of the copper cup or the plate of the battery, a vivid deflagration of the metal takes place. The sides of the cup sometimes give a spark when none can be drawn from the surface of the mercury. This circumstance requires to be guarded against when experimenting on the comparative intensities of sparks from different arrangements. If the battery formerly described [fig. 1, page 81] be arranged as a "calorimotor," and one end of a large spiral conductor be attached to one pole, and the other end drawn along the edge of the connector, fig. 4, a series of loud and rapid explosions is produced, accompanied by a brilliant deflagration of the metal, and this takes place when the excitement of the battery is too feeble to heat to redness a small platina wire.

12. A number of experiments were made to determine the effect of introducing a cylinder of soft iron into the axis of the flat spiral, in reference to the shock, the spark, &c.; but no difference could be observed with the large spiral conductor; the effect of the iron was merged in that of the spiral. When however one of the smaller ribbons was formed into a hollow cylindrical helix of about nine inches long, and a cylinder of soft iron an inch and a half in diameter was inserted, the spark appeared a little more intense than without the iron. The obliquity of the spires in this case was unfavorable to their mutual action, while the magnetism was greater than with the flat spiral, since the conductor closely surrounded the whole length of the cylinder.

I would infer from these experiments, that some effects heretofore attributed to magneto-electric action are chiefly due to the re-action on each other of the several spires of the coil which surround the magnet.

13. One of the most singular results in this investigation was first obtained in operating with the large galvanic battery [fig. 2, page 84]. The whole instrument was arranged as a "calorimotor" of eight pairs, and a large spiral conductor introduced into the circuit at *c d*, while a piece of thick copper wire, about five inches long, united the poles at *a b*. In this state an explosion or loud snap was produced, not only when the contact was broken at the spiral, but also when one end of the short wire, at the other extremity of the apparatus, was drawn from its cup. All the other short movable connectors of the battery gave a similar result. When the spiral was removed from the circuit, and a short wire substituted, no effect of the kind was produced. From this experiment it appears that the influence of the spiral is exerted through at least eight alternations of zinc, acid, and copper, and thus gives to a short wire, at the other extremity of the circuit, the power of producing a spark.

14. The influence of the coil was likewise manifest when the zinc and copper plates of a single pair were separated from each other to the distance of fourteen inches in a trough without partitions, filled with diluted acid. Although the electrical intensity in this case must have been very low, yet there was but little reduction in the apparent intensity of the spark.

The spiral conductor produces however little or no increase of effect when introduced into a galvanic circuit of considerable intensity. Thus when the large spiral used in experiments seven, eight, &c., was made to connect the poles of two Cruickshanks troughs, each containing fifty-six four inch plates, no greater effect was perceived than with a short thick wire; in both cases in making the contact a feeble spark was given, attended with a slight deflagration of the mercury. The batteries at the same time were in sufficiently intense action to give a disagreeable shock. It is probable however that if the length of the coil were increased in some proportion to the increase of intensity, an increased effect would still be produced.

In operating with the apparatus described in the last exper-

iment, a phenomenon was observed in reference to the action of the battery itself, which I do not recollect to have seen mentioned, although it is intimately connected with the facts of Magneto-electricity, as well as with the subject of these investigations, viz.: When the body is made to form a part of a galvanic circuit composed of a number of elements, a shock is of course felt at the moment of completing the circuit. If the battery be not very large, little or no effect will be perceived during the uninterrupted circulation of the galvanic current; but if the circuit be interrupted by breaking the contact at any point, a shock will be felt at the moment, nearly as intense as that given when the contact was first formed. The secondary shock is rendered more evident, when the battery is in feeble action, by placing in the mouth the end of one of the wires connected with the poles; a shock and flash of light will be perceived when the circuit is completed, and also the same when the contact is broken at any point, but nothing of the kind will be perceived in the intermediate time, although the circuit may continue uninterrupted for some minutes. This I consider an important fact in reference to the action of the voltaic current.

The phenomena described in this paper appear to be intimately connected with those of Magneto-electricity, and this opinion I advanced with the announcement of the first fact of these researches in the *American Journal of Science*. They may I conceive be all referred to that species of dynamical *Induction* discovered by Mr. Faraday, which produces the following phenomenon, namely: when two wires, *A* and *B*, are placed side by side, but not in contact, and a voltaic current is passed through *A*, there is a current produced in *B*, but in an opposite direction. The current in *B* exists only for an instant, although the current in *A* may be indefinitely continued; but if the current in *A* be stopped, there is produced in *B* a second current, in an opposite direction however to the first current.

The above fundamental fact in Magneto-electricity appears to me to be a direct consequence of the statical principles of "Electrical *Induction*" as mathematically investigated by

Cavendish, Poisson, and others. When the two wires *A* and *B* are in their natural state, an equilibrium is sustained by the attractions and repulsions of the two fluids in each wire; or, according to the theory of Franklin and Cavendish, by the attractions and repulsions of the one fluid, and the matter of the two wires. If a current of free electricity be passed through *A*, the natural equilibrium of *B* will be disturbed for an instant, in a similar manner to the disturbance of the equilibrium in an insulated conductor, by the sudden addition of fluid to a contiguous conductor. On account of the repulsive action of the fluid, the current in *B* will have an opposite direction to that in *A*; and if the intensity of action remains constant, a new state of equilibrium will be assumed. The second state of *B* however may perhaps be regarded as one of tension, and as soon as the extra action ceases in it, the fluid in *B* will resume its natural state of distribution, and thus a returning current for an instant be produced.

The action of the spiral conductor in producing sparks, is but another case of the same action; for since action and reaction are equal and in contrary directions, if a current established in *A* produces a current in an opposite direction in *B*, then a current transmitted through *B* should accelerate or increase the intensity of a current already existing in the same direction in *A*. In this way the current in the several successive spires of the coil may be conceived to accelerate, or to tend to accelerate each other; and when the contact is broken, the fluid of the first spire is projected from it with intensity by the repulsive action of the fluid in all the succeeding spires.

In the case of the double spiral conductor, in experiment six, the fluid is passing in an opposite direction; and according to the same views, a retardation or decrease of intensity should take place.

The phenomenon of the secondary shock with the battery, appears to me to be a consequence of the law of Mr. Faraday. The parts of the human body contiguous to those through which the principal current is passing, may be considered as in the state of the second wire *B*; when the principal current

ceases, a shock is produced by the returning current of the natural electricity of the body.

If this explanation be correct, the same principle will readily account for a curious phenomenon discovered several years since by Savary, but which I believe still remains an isolated fact. When a current is transmitted through a wire, and a number of small needles are placed transverse to it, but at different distances, the direction of the magnetic polarity of the needles varies with their distance from the conducting wire. The action is also periodical; diminishing as the distance increases, until it becomes zero; the polarity of the needles is then inverted, acquires a maximum, decreases to zero again, and then resumes the first polarity; several alternations of this kind being observed.* Now this is precisely what would take place if we suppose that the principal current induces a secondary one in an opposite direction in the air surrounding the conductor, and this again another in an opposite direction at a great distance, and so on. The needles at different distances would be acted on by the different currents, and thus the phenomena described be produced.

The action of the spiral is also probably connected with the fact in common electricity called the lateral discharge: and likewise with an appearance discovered some years since by Nobili, of a vivid light, produced when a Leyden jar is discharged through a flat spiral.

The foregoing views are not presumed to be given as exhibiting the actual operation of nature in producing the phenomena described, but rather as the hypotheses which have served as the basis of my investigations, and which may further serve as formulæ from which to deduce new consequences to be established or disproved by experiment.

Many points of this subject are involved in an obscurity which requires more precise and extended investigation; we may however confidently anticipate much additional light from the promised publication of Mr. Faraday's late researches in this branch of science.

* Cumming's *Demonferrand*, page 247; also *Edinburgh Journal of Science*, October, 1826.

NOTICE OF ELECTRICAL RESEARCHES—THE LATERAL
DISCHARGE.

(Report of British Association, 1837, vol. VI, part ii, pp. 22-24.)*

September, 1837.

The primary object of these investigations was to detect, if possible, an inductive action in common electricity analogous to that discovered in a current of galvanism. For this purpose an analysis was instituted of the phenomena known in ordinary electricity by the name of the lateral discharge. Professor Henry was induced to commence with this from some remarks by Dr. Roget on the subject. The method of studying the lateral spark consisted in catching it on the knob of a small Leyden phial, and presenting this to an electrometer. The result of the analysis was in accordance with an opinion of Biot that the lateral discharge is due only to the escape of the small quantity of redundant electricity which always exists on one or the other side of a jar, and not to the whole discharge. The Professor then stated several consequences which would flow from this, namely, that we could increase or diminish the lateral action by the several means which would affect the quantity of redundant or as it may be called free electricity, such as an increase of the thickness of the glass, or by substituting for the small knob of the jar a large ball. But the arrangement which produces the greatest effect is that of a long fine copper wire, insulated parallel to the horizon, and terminated at each end by a small ball. When sparks are thrown on this from a globe of about a foot in diameter, the wire at each discharge becomes beautifully luminous from one end to the other, even if it be a hundred feet long. Rays are given off on all sides perpendicular to the axis of the wire. In this arrangement the electricity of the globe may be considered nearly all as free electricity; and as the insulated

* [Re-printed in Silliman's American Journal of Science, April, 1838, pp. 16-18.]

wire contains its natural quantity, the whole spark is thrown off in the form of a lateral discharge. But to explain these phenomena more fully, Professor Henry remarked that it appeared necessary to add an additional postulate to our theory of the principle of electricity, namely, a kind of momentum, or inertia without weight. By this he would only be understood to express the classification or generalization of a number of facts which would otherwise be insulated. To illustrate this, he stated that the same quantity of electricity could be made to remain on the wire if gradually communicated; but when thrown on in the form of a spark it is dissipated, as before described. Other facts of the same kind were mentioned; and also that we could take advantage of the principle to procure a greater effect in the decomposition of water by ordinary electricity. The fact of a wire becoming luminous by a spark was noticed by the celebrated Van Marum more than fifty years ago; but he ascribed it to the immense power of the great Haarlem machine. The effect however can be produced, as before described, by a cylinder of Nairn's construction of seven inches in diameter, a globe of a foot in diameter being placed in connection with the prime conductor to increase its capacity.

Some experiments were next described in reference to the induction of the lateral action of different discharges on each other. When the long wire is arranged in two parallel but continuous lines by bending the wire the outer side of each wire only becomes luminous. When formed into three parallel lines by a double bend the middle portion of the wire does not become luminous, the outer sides only of the outer lines of wire exhibit the rays. When the wire is formed into a flat spiral the outer spiral alone exhibits the lateral discharge, but the light in this case is very brilliant; the inner spirals appear to increase the effect by induction.

Professor Henry stated that a metallic conductor intimately connected with the earth at one end, does not silently conduct the electricity thrown in sparks on the other end.

In one experiment described a copper wire one-eighth of an inch in diameter was plunged at its lower end into the water of a deep well, so as to form as perfect a connection with the earth as possible. A small ball being attached to the upper end and sparks passed on to this from the globe before mentioned a lateral spark could be drawn from any part of the wire, and a pistol of Volta fired even near the surface of the water. This effect was rendered still more striking by attaching a ball to the middle of the perpendicular part of a lightning rod, put up according to the directions given by Gay-Lussac. When sparks of about an inch and a half in length were thrown on the ball corresponding lateral sparks could be drawn not only from the parts of the rod between the ground and the ball, but from the part above even to the top of the rod. Some remarks were then made on the theory of thunder-storms, as given by the French writers, in which the cloud is considered as analogous in action to one coating of a charged glass, the earth the other coating, and the air between as the non-conducting glass. One very material circumstance has been overlooked in this theory, namely, the great thickness of the intervening stratum and the consequent great quantity of free or redundant electricity in the cloud. This must modify the nature of the discharge from the thunder-cloud, and lead to doubt if it be perfectly analogous to the discharge from an ordinary Leyden jar, since the great quantity of redundant electricity must produce a comparatively greater lateral action; and hence, possibly, the ramifications of the flash and other similar phenomena may be but cases of the lateral discharge.

Some facts were then mentioned on the phenomena of the spark from a long wire charged with common or atmospheric electricity. It is well known that the spark in this case is very pungent, resembling a shock from a Leyden jar. The effect does not appear to be produced, as is generally supposed, by the high intensity of the electricity at the ends of the wire by mere distribution, since this is incompatible with the shortness of the spark. In one experiment fifteen persons, joining hands, received a severe shock, (while stand-

ing on the grass,) from a long wire. One of the number only touched the conductor. The spark in this case was not more than a quarter of an inch long. Several other analogous facts were mentioned, and the suggestion made that the whole were probably the result of an inductive action in the long wire, similar to that observed in a long galvanic current. The subject now required further investigation.

Professor Henry concluded by observing that the facts he had given in this communication were such as must have been noticed by every person who is in the habit of experimenting on ordinary electricity; but he believed these had never been studied in this connection. He was anxious to direct the attention of the Section to the subject as one which appeared to afford an interesting field of research, particularly in connection with the recent discoveries of the surprising inductive actions of galvanic currents.

THE LATERAL DISCHARGE OF ELECTRICITY.

(Proceedings of the American Philosophical Society, vol. I, p. 6.)*

February 16, 1838.

Professor Henry made a verbal communication on the lateral discharge of electricity while passing along a wire, as in the Leyden experiment, or communicated directly to an insulated wire, or to a wire connected with the earth, and detailed various experiments proving that free electricity is not, under any circumstances, conducted silently to the earth.

INDUCTION CURRENTS FROM ORDINARY ELECTRICITY.

(Proceedings of the American Philosophical Society, vol. I, p. 14.)

May 4, 1838.

Dr. Patterson read a letter from Professor Henry, of Princeton, dated May 4, 1838, announcing that in recent experiments he has produced directly from ordinary electricity currents by induction analogous to those obtained from galvanism, and that he has ascertained that these currents possess some peculiar properties; that they may be increased in intensity to an indefinite degree, so that if a discharge from a Leyden jar be sent through a good conductor a shock may be obtained from a contiguous but perfectly insulated conductor more intense than one directly from the jar. Professor Henry remarks that he has also found that all conducting substances screen the inductive action, and that he has succeeded in referring this screening process to currents induced for a moment in the interposed body.

*[The title-page of vol. I (comprising the proceedings from Jan., 1838, to Dec., 1840,) bears date 1840.]

INDUCED CURRENTS FROM ORDINARY ELECTRICITY.

(Proceedings of the American Philosophical Society, vol. I, pp. 54-56.)

November 2, 1838.

Professor Henry read a paper entitled "Contributions to Electricity and Magnetism, No. 3. On the Phenomena of Electro-dynamic Induction."

The primary object of the investigation undertaken by the author was the discovery of induced currents from ordinary electricity similar to those produced by galvanism. Preparatory to this, a new investigation was instituted of the phenomena of galvanic induction, and the result of this, forms perhaps the most important part of the communication.

The first section of the paper refers to the conditions which influence the induction of a current on itself, as in the case of a long wire and a spiral conductor. These are shown to depend on the intensity and quantity of the battery current, and on the length, thickness, and form of the conductor.

The next section examines the conditions necessary to the production of powerful secondary currents, and also the changes which take place in the same when the form of the battery and the size and form of the conductor are varied.

The important fact is shown that not only a current of intensity can be induced by one of quantity, but also the converse—that a current of quantity can be produced by one of intensity.

The third section relates to the effect of interposing different substances between the conductor which transmits the current from the battery, and that which is arranged to receive the induced current. All good conducting substances are found to screen the inducing action, and this screening effect is shown, by the detail of a variety of experiments, to be the result of the neutralizing action of a current induced in the interposed body. This neutralizing

current is separately examined, and its direction found to be the same as that of the battery current. The question is then raised, how two currents in the same direction can counteract each other? An answer to this question is given in a subsequent part of the paper.

The fourth section relates to the discovery of induced currents of the third, fourth, and fifth orders; that is, to the fact that the second current is found capable of inducing a third current, and this latter again another, and so on. The properties of these new currents are next examined, and the screening influence is found to take place between them; quantity is induced from intensity, and conversely; magnetism is developed in soft iron, decomposition is effected, and intense shocks are obtained, even from the current of the fourth order. A remarkable and important fact is stated in reference to the direction of these currents. If the direction of the battery current and that of the second be called *plus*, then the direction of the third current will be *minus*, of the fourth current *plus*, of the fifth *minus*, and so on. The application of the fact of these alternations is made to the explanation of the phenomenon of screening before mentioned, and also to the improvement of the magneto-electrical machine.

The last part of the paper relates to the discovery of secondary currents, and of currents of the several orders, in the discharge of ordinary electricity. Shocks are obtained from these, the screening influence of good conductors is shown to take place, magnetism is developed, and the alternations in the direction are found to exist as in the currents from galvanic induction. Some remarkable results are given in reference to the great distance at which the induction takes place. Experiments are detailed in which needles were made magnetic when the conductors were removed to the distance of twelve feet from each other.

Prof. Henry made a verbal communication during the course of which he illustrated experimentally the phenomena developed in his paper.

CONTRIBUTIONS TO ELECTRICITY AND MAGNETISM. No. III.

ON ELECTRO-DYNAMIC INDUCTION.

(Transactions American Philosophical Society, n. s., vol. VI, pp. 303-337.)*

Read November 2, 1838.

INTRODUCTION.

1. Since my investigations in reference to the influence of a spiral conductor, in increasing the intensity of a galvanic current, were submitted to the Society, the valuable paper of Dr. Faraday, on the same subject, has been published, and also various modifications of the principle have been made by Sturgeon, Masson, Page, and others, to increase the effects. The spiral conductor has likewise been applied by Cav. Antinori to produce a spark by the action of a thermo-electrical pile; and Mr. Watkins has succeeded in exhibiting all the phenomena of hydro-electricity by the same means. Although the principle has been much extended by the researches of Dr. Faraday, yet I am happy to state that the results obtained by this distinguished philosopher are not at variance with those given in my paper.

2. I now offer to the Society a new series of investigations in the same line, which I hope may also be considered of sufficient importance to merit a place in the Transactions.

3. The primary object of these investigations was to discover, if possible, inductive actions in common electricity analogous to those found in galvanism. For this purpose a series of experiments was commenced in the spring of 1836, but I was at that time diverted, in part, from the immediate object of my research, by a new investigation of the phenomenon known in common electricity by the name of the lateral discharge. Circumstances prevented my doing anything further, in the way of experiment, until April last, when most of the results which I now offer to the Society were obtained. The investigations are not as complete in several points as I could wish, but as my duties will not per-

* [The title page of vol. VI bears date 1839.]

mit me to resume the subject for some months to come, I therefore present them as they are; knowing, from the interest excited by this branch of science in every part of the world, that the errors which may exist will soon be detected, and the truths be further developed.

4. The experiments are given nearly in the order in which they were made; and in general they are accompanied by the reflections which led to the several steps of the investigation. The whole series is divided, for convenience of arrangement, into six sections, although the subject may be considered as consisting, principally, of two parts. The first relating to a new examination of the induction of galvanic currents; and the second to the discovery of analogous results in the discharge of ordinary electricity.

5. The principal articles of apparatus used in the experiments, consist of a number of flat coils of copper ribbon, which will be designated by the names of coil No. 1, coil No.

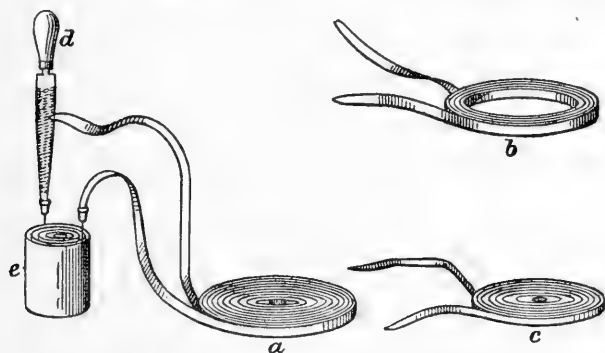


FIG. 1.—*a* represents coil No. 1, *b* coil No. 2, and *c* coil No. 3; *e* the battery, *d* the rasp.

2, &c.; also of several coils of long wire; and these, to distinguish them from the ribbons, will be called helix No. 1, helix No. 2, &c.

6. Coil No. 1 is formed of thirteen pounds of copper plate, one inch and a half wide and ninety-three feet long. It is well covered with two coatings of silk, and was generally used in the form represented in Fig. 1, which is that of a flat spiral sixteen inches in diameter. It was however some-

times formed into a ring of larger diameter, as is shown in Fig. 4, Section III.

7. Coil No. 2 is also formed of copper plate, of the same width and thickness as coil No. 1. It is however only sixty feet long. Its form is shown at *b*, Fig. 1. The opening at the centre is sufficient to admit helix No. 1. Coils No. 3, 4, 5, 6, &c., are all about sixty feet long, and of copper plate of the same thickness, but of half the width of coil No. 1.

8. Helix No 1 consists of sixteen hundred and sixty yards of copper wire, $\frac{1}{49}$ th of an inch in diameter. No. 2, of nine



FIG. 2.—*a* represents helix No. 1, *b* helix No. 2, *c* helix No. 3.

hundred and ninety yards; and No. 3, of three hundred and fifty yards, of the same wire. These helices are shown in Fig. 2, and are so adjusted in size as to fit into each other; thus forming one long helix of three thousand yards: or, by using them separately, and in different combinations, seven helices of different lengths. The wire is covered with cotton thread, saturated with beeswax, and between each stratum of spires a coating of silk is interposed.

9. Helix No. 4 is shown at *a*, Fig. 4, Section III; it is formed of five hundred and forty-six yards of wire, $\frac{1}{49}$ th of an inch in diameter, the several spires of which are insulated by a coating of cement. Helix No. 5 consists of fifteen hundred yards of silvered copper wire, $\frac{1}{125}$ th of an inch in diameter, covered with cotton, and is of the form of No. 4.

10. Besides these I was favored with the loan of a large spool of copper wire, covered with cotton, $\frac{1}{16}$ th of an inch in diameter, and five miles long. It is wound on a small axis of iron, and forms a solid cylinder of wire, eighteen inches long, and thirteen in diameter.

11. For determining the direction of induced currents, a magnetizing spiral was generally used, which consists of about thirty spires of copper wire, in the form of a cylinder, and so small as just to admit a sewing needle into the axis.

12. Also a small horseshoe is frequently referred to, which is formed of a piece of soft iron, about three inches long, and $\frac{3}{8}$ ths of an inch thick; each leg is surrounded with about five feet of copper bell wire. This length is so small, that only a current of electricity of considerable quantity can develop the magnetism of the iron. The instrument is used for indicating the existence of such a current.

13. The battery used in most of the experiments is shown in Fig. 1. It is formed of three concentric cylinders of copper, and two interposed cylinders of zinc. It is about eight inches high, five inches in diameter, and exposes about one square foot and three quarters of zinc surface, estimating both sides of the metal. In some of the experiments a larger battery was used, weakly charged, but all the results mentioned in the paper except those with a Cruickshank trough, can be obtained with one or two batteries of the above size, particularly if excited by a strong solution. The manner of interrupting the circuit of the conductor by means of a rasp *b*, is shown in the same Figure.

SECTION I.

Conditions which influence the induction of a Current on itself.

14. The phenomenon of the spiral conductor is at present known by the name of the induction of a current on itself, to distinguish it from the induction of the secondary current, discovered by Dr. Faraday. The two however belong to the same class, and experiments render it probable that the spark given by the long conductor is, from the natural electricity of the metal, disturbed for an instant by the induction of the primary current. Before proceeding to the other parts of these investigations, it is important to state the results of a number of preliminary experiments, made to determine more definitely the conditions which influence the action of the spiral conductor.

15. When the electricity is of low intensity, as in the case of the thermo-electrical pile, or a large single battery weakly excited with dilute acid, the flat ribbon coil No. 1, ninety-

three feet long, is found to give the most brilliant deflagrations, and the loudest snaps from a surface of mercury. The shocks, with this arrangement, are however very feeble, and can only be felt in the fingers or through the tongue.

16. The induced current in a short coil, which thus produces deflagration, but not shocks, may, for distinction, be called one of quantity.

17. When the length of the coil is increased, the battery continuing the same, the deflagrating power decreases, while the intensity of the shock continually increases. With five ribbon coils, making an aggregate length of three hundred feet, and the small battery, Fig. 1, the deflagration is less than with coil No. 1, but the shocks are more intense.

18. There is however a limit to this increase of intensity of the shock, and this takes place when the increased resistance or diminished conduction of the lengthened coil begins to counteract the influence of the increasing length of the current. The following experiment illustrates this fact. A coil of copper wire $\frac{1}{8}$ th of an inch in diameter, was increased in length by successive additions of about thirty-two feet at a time. After the first two lengths, or sixty-four feet, the brilliancy of the spark began to decline, but the shocks constantly increased in intensity, until a length of five hundred and seventy-five feet was obtained, when the shocks also began to decline. This was then the proper length to produce the maximum effect with a single battery, and a wire of the above diameter.

19. When the intensity of the electricity of the battery is increased, the action of the short ribbon coil decreases. With a Cruickshank's trough of sixty plates, four inches square, scarcely any peculiar effect can be observed, when the coil forms a part of the circuit. If however the length of the coil be increased in proportion to the intensity of the current, then the inductive influence becomes apparent. When the current, from ten plates of the above mentioned trough, was passed through the wire of the large spool (10), the induced shock was too severe to be taken through the body. Again, when a small trough of twenty-five one-inch plates, which

alone would give but a very feeble shock, was used with helix No. 1, an intense shock was received from the induction, when the contact was broken. Also a slight shock in this arrangement is given when the contact is formed, but it is very feeble in comparison with the other. The spark however with the long wire and compound battery is not as brilliant as with the single battery and the short ribbon coil.

20. When the shock is produced from a long wire, as in the last experiments, the size of the plates of the battery may be very much reduced, without a corresponding reduction of the intensity of the shock. This is shown in an experiment with the large spool of wire (10). A very small compound battery was formed of six pieces of copper bell-wire, about one inch and a half long, and an equal number of pieces of zinc of the same size. When the current from this was passed through the five miles of the wire of the spool, the induced shock was given at once to twenty-six persons joining hands. This astonishing effect placed the action of a coil in a striking point of view.

21. With the same spool and the single battery used in the former experiments, no shock, or at most a very feeble one, could be obtained. A current however was found to pass through the whole length, by its action on the galvanometer; but it was not sufficiently powerful to induce a current which could counteract the resistance of so long a wire.

22. The induced current in these experiments may be considered as one of *considerable* "intensity," and *small* "quantity."

23. The form of the coil has considerable influence on the intensity of the action. In the experiments of Dr. Faraday, a long cylindrical coil of thick copper wire, inclosing a rod of soft iron, was used. This form produces the greatest effect when magnetic reaction is employed; but in the case of simple galvanic induction, I have found the form of the coils and helices represented in the figures most effectual. The several spires are more nearly approximated, and therefore they exert a greater mutual influence. In some cases, as will be seen hereafter, the ring form, shown in Fig. 4, is most effectual.

24. In all cases the several spires of the coil should be well insulated, for although in magnetizing soft iron, and in analogous experiments, the touching of two spires is not attended with any great reduction of action; yet in the case of the induced current, as will be shown in the progress of these investigations, a single contact of two spires is sometimes sufficient to neutralize the whole effect.

25. It must be recollected that all the experiments with these coils and helices, unless otherwise mentioned, are made without the reaction of iron temporarily magnetized; since the introduction of this would in some cases interfere with the action, and render the results more complex.

SECTION II.

Conditions which influence the production of Secondary Currents.

26. The secondary currents, as it is well known, were discovered in the introduction of magnetism and electricity, by Dr. Faraday, in 1831. But he was at that time urged to the exploration of new, and apparently richer veins of science, and left this branch to be traced by others. Since then however attention has been almost exclusively directed to one part of the subject, namely, the induction from magnetism, and the perfection of the magneto-electrical machine: and I know of no attempts, except my own, to review and extend the purely electrical part of Dr. Faraday's admirable discovery.

27. The energetic action of the flat coil, in producing the induction of a current on itself, led me to conclude that it would also be the most proper means for the exhibition and study of the phenomena of the secondary galvanic currents.

28. For this purpose coil No. 1 was arranged to receive the current from the small battery, and coil No. 2 placed on this, with a plate of glass interposed to insure perfect insulation; as often as the circuit of No. 1 was interrupted, a powerful secondary current was induced in No. 2. The arrangement is the same as that exhibited in Fig. 3, with the exception that in this the compound helix is represented as receiving the induction, instead of coil No. 2.

29. When the ends of the second coil were rubbed together, a spark was produced at the opening. When the same ends

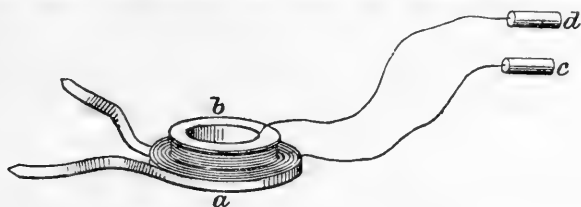


FIG. 3.—*a* represents coil No. 1, *b* helix No. 1, and *c*, *d*, handles for receiving the shock.

were joined by the magnetizing spiral (11), the enclosed needle became strongly magnetic. Also when the secondary current was passed through the wires of the iron horseshoe (12), magnetism was developed; and when the ends of the second coil were attached to a small decomposing apparatus, of the kind which accompanies the magneto-electrical machine, a stream of gas was given off at each pole. The shock however from this coil is very feeble, and can scarcely be felt above the fingers.

30. This current has therefore the properties of one of moderate "intensity," but considerable "quantity."

31. Coil No. 1 remaining as before, a longer coil, formed by uniting Nos. 3, 4, and 5, was substituted for No. 2. With this arrangement, the spark produced when the ends were rubbed together, was not as brilliant as before; the magnetizing power was much less; decomposition was nearly the same, but the shocks were more powerful, or in other words the "intensity" of the induced current was increased by an increase of the length of the coil, while the "quantity" was apparently decreased.

32. A compound helix, formed by uniting Nos. 1 and 2, and therefore containing two thousand six hundred and fifty yards of wire, was next placed on coil No. 1. The weight of this helix happened to be precisely the same as that of coil No. 2, and hence the different effects of the same quantity of metal in the two forms of a long and short conductor, could be compared. With this arrangement the magnetizing effects,

with the apparatus before mentioned, disappeared. The sparks were much smaller, and also the decomposition less, than with the short coil; but the shock was almost too intense to be received with impunity, except through the fingers of one hand. A circuit of fifty-six of the students of the senior class, received it at once from a single rupture of the battery current, as if from the discharge of a Leyden jar weakly charged. The secondary current in this case was one of small quantity, but of great intensity.

33. The following experiment is important in establishing the fact of a limit to the increase of the intensity of the shock, as well as the power of decomposition, with a wire of a given diameter. Helix No. 5, which consists of wire only $\frac{1}{125}$ th of an inch in diameter, was placed on coil No. 2, and its length increased to about seven hundred yards. With this extent of wire, neither decomposition nor magnetism could be obtained, but shocks were given of a peculiarly pungent nature; they did not however produce much muscular action. The wire of the helix was further increased to about fifteen hundred yards; the shock was now found to be scarcely perceptible in the fingers.

34. As a counterpart to the last experiment, coil No. 1 was formed into a ring of sufficient internal diameter to admit the great spool of wire (11), and with the whole length of this (which, as has before been stated, is five miles) the shock was found so intense as to be felt at the shoulder, when passed only through the forefinger and thumb. Sparks and decomposition were also produced, and needles rendered magnetic. The wire of this spool is $\frac{1}{16}$ th of an inch thick, and we therefore see from this experiment, that by increasing the diameter of the wire, its length may also be much increased, with an increased effect.

35. The fact (33) that the induced current is diminished by a further increase of the wire, after a certain length has been attained, is important in the construction of the magneto-electrical machine, since the same effect is produced in the induction of magnetism. Dr. Goddard of Philadelphia, to whom I am indebted for coil No. 5, found that when its

whole length was wound on the iron of a temporary magnet, no shocks could be obtained. The wire of the machine may therefore be of such a length, relative to its diameter, as to produce shocks, but no decomposition; and if the length be still further increased, the power of giving shocks may also become neutralized.

36. The inductive action of coil No. 1, in the foregoing experiments, is precisely the same as that of a temporary magnet in the case of the magneto-electrical machine. A short thick wire around the armature gives brilliant deflagrations, but a long one produces shocks. This fact, I believe, was first discovered by my friend Mr. Saxton, and afterwards investigated by Sturgeon and Lenz.

37. We might, at first sight, conclude, from the perfect similarity of these effects, that the currents which, according to the theory of Ampere, exist in the magnet, are like those in the short coil, of great quantity and feeble intensity; but succeeding experiments will show that this is not necessarily the case.

38. All the experiments given in this section have thus far been made with a battery of a single element. This condition was now changed, and a Cruickshanks trough of sixty pairs substituted. When the current from this was passed through the ribbon coil No. 1, no indication, or a very feeble one, was given of a secondary current in any of the coils or helices, arranged as in the preceding experiments. The length of the coil, in this case, was not commensurate with the intensity of the current from the battery. But when the long helix, No. 1, was placed instead of coil No. 1, a powerful inductive action was produced on each of the articles, as before.

39. First, helices No. 2 and 3 were united into one, and placed within helix No. 1, which still conducted the battery current. With this disposition a secondary current was produced, which gave intense shocks but feeble decomposition, and no magnetism in the soft iron horseshoe. It was therefore one of intensity, and was induced by a battery current also of intensity.

40. Instead of the helix used in the last experiment for receiving the induction, one of the coils (No. 3) was now placed on helix No. 1, the battery remaining as before. With this arrangement the induced current gave no shocks, but it magnetized the small horseshoe; and when the ends of the coil were rubbed together, produced bright sparks. It had therefore the properties of a current of quantity; and it was produced by the induction of a current, from a battery, of intensity.

41. This experiment was considered of so much importance, that it was varied and repeated many times, but always with the same result; it therefore establishes the fact *that an "intensity" current can induce one of "quantity,"* and, by the preceding experiments, the converse has also been shown, *that a "quantity" current can induce one of "intensity."*

42. This fact appears to have an important bearing on the law of the inductive action, and would seem to favor the supposition that the lower coil, in the two experiments with the long and short secondary conductors, exerted the same amount of inductive force, and that in one case this was expended (to use the language of theory) in giving a great velocity to a small quantity of the fluid, and in the other in producing a slower motion in a larger current; but in the two cases, were it not for the increased resistance to conduction in the longer wire, the quantity multiplied by the square of the velocity would be the same. This however is as yet a hypothesis, but it enables us to conceive how intensity and quantity may both be produced from the same induction.

43. From some of the foregoing experiments we may conclude, that the quantity of electricity in motion in the helix is really less than in the coil, of the same weight of metal; but this may possibly be owing simply to the greater resistance offered by the longer wire. It would also appear, if the above reasoning be correct, that to produce the most energetic physiological effects, only a small quantity of electricity, moving with great velocity, is necessary.

44. In this and the preceding section, I have attempted

to give only the general conditions which influence the galvanic induction. To establish the law would require a great number of more refined experiments, and the consideration of several circumstances which would affect the results, such as the conduction of the wires, the constant state of the battery, the method of breaking the circuit with perfect regularity, and also more perfect means than we now possess of measuring the amount of the inductive action; all these circumstances render the problem very complex.

SECTION III.

On the Induction of Secondary Currents at a distance.

45. In the experiments given in the two preceding Sections, the conductor which received the induction, was separated from that which transmitted the primary current by the thickness only of a pane of glass; but the action from this arrangement was so energetic, that I was naturally led to try the effect at a greater distance.

46. For this purpose coil No. 1 was formed into a ring of about two feet in diameter, and helix No. 4 placed as is shown

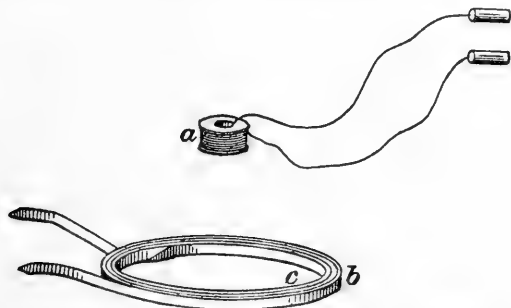


FIG. 4.—*a* represents helix No. 4, *b* coil No. 1, in the form of a ring.

in the figure. When the helix was at the distance of about sixteen inches from the middle of the plane of the ring, shocks could be perceived through the tongue, and these rapidly increased in intensity as the helix was lowered, and when it reached the plane of the ring they were quite severe. The effect however was still greater, when the helix was

moved from the centre to the inner circumference, as at *c*: but when it was placed without the ring, in contact with the outer circumference, at *b*, the shocks were very slight; and when placed within, but its axis at right angles to that of the ring, not the least effect could be observed.

47. With a little reflection, it will be evident that this arrangement is not the most favorable for exhibiting the induction at a distance, since the side of the ring, for example, at *c*, tends to produce a current revolving in one direction in the near side of the helix, and another in an opposite direction in the farther side. The resulting effect is therefore only the difference of the two, and in the position as shown in the figure; this difference must be very small, since the opposite sides of the helix are approximately at the same distance from *c*. But the difference of action on the two sides constantly increases as the helix is brought near the side of the ring, and becomes a maximum when the two are in the position of internal contact. A helix of larger diameter would therefore produce a greater effect.

48. Coil No. 1 remaining as before, helix No. 1, which is nine inches in diameter, was substituted for the small helix of the last experiment, and with this the effect at a distance was much increased. When coil No. 2 was added to coil No. 1, and the currents from two small batteries sent through these, shocks were distinctly perceptible through the tongue, when the distance of the planes of the coils and the three helices, united as one, was increased to thirty-six inches.

49. The action at a distance was still further increased by coiling the long wire of the large spool into the form of a ring of four feet in diameter, and placing parallel to this another ring, formed of the four ribbons of coils No. 1, 2, 3, and 4. When a current from a single battery of thirty-five feet of zinc surface was passed through the ribbon conductor, shocks through the tongue were felt when the rings were separated to the distance of four feet. As the conductors were approximated, the shocks became more and more severe; and when at the distance of twelve inches, they could not be taken through the body.

50. It may be stated in this connection, that the galvanic induction of magnetism in soft iron, in reference to distance, is also surprisingly great. A cylinder of soft iron, two inches in diameter and one foot long, placed in the centre of the ring of copper ribbon, with the battery above mentioned, becomes strongly magnetic.

51. I may perhaps be excused for mentioning in this communication that the induction at a distance affords the means of exhibiting some of the most astonishing experiments, in the line of *physique amusante*, to be found perhaps in the whole course of science. I will mention one which is somewhat connected with the experiments to be described in the next section, and which exhibits the action in a striking manner. This consists in causing the induction to take place through the partition wall of two rooms. For this purpose coil No. 1 is suspended against the wall in one room, while a person in the adjoining one receives the shock, by grasping the handles of the helix, and approaching it to the spot opposite to which the coil is suspended. The effect is as if by magic, without a visible cause. It is best produced through a door, or thin wooden partition.

52. The action at a distance affords a simple method of graduating the intensity of the shock in the case of its application to medical purposes. The helix may be suspended by a string passing over a pulley, and then gradually lowered down towards the plane of the coil, until the shocks are of the required intensity. At the request of a medical friend, I have lately administered the induced current precisely in this way, in a case of paralysis of a part of the nerves of the face.

53. I may also mention that the energetic action of the spiral conductors enables us to imitate, in a very striking manner the inductive operation of the magneto-electrical machine, by means of an uninterrupted galvanic current. For this purpose it is only necessary to arrange two coils to represent the two poles of a horseshoe magnet, and to cause two helices to revolve past them in a parallel plane. While a constant current is passing through each coil, in opposite

directions, the effect of the rotation of the helices is precisely the same as that of the revolving armature in the machine.

54. A remarkable fact should here be noted in reference to helix No. 4, which is connected with a subsequent part of the investigation. This helix is formed of copper wire, the spires of which are insulated by a coating of cement instead of thread, as in the case of the others. After being used in the above experiments, a small discharge from a Leyden jar was passed through it, and on applying it again to the coil, I was much surprised to find that scarcely any signs of a secondary current could be obtained.

55. The discharge had destroyed the insulation in some part, but this was not sufficient to prevent the magnetizing of a bar of iron introduced into the opening at the centre. The effect appeared to be confined to the inductive action. The same accident had before happened to another coil of nearly the same kind. It was therefore noted as one of some importance. An explanation was afterwards found in a peculiar action of the secondary current.

SECTION IV.

On the Effects produced by interposing different Substances between the Conductors.

56. Sir H. Davy found, in magnetizing needles by an electrical discharge, that the effect took place through interposed plates of all substances, conductors and non-conductors.* The experiment which I have given in paragraph 51 would appear to indicate that the inductive action which produces the secondary current might also follow the same law.

57. To test this the compound helix was placed about five inches above coil No. 1, Fig. 5, and a plate of sheet iron, about $\frac{1}{10}$ th of an inch thick, interposed. With this arrangement no shocks could be obtained; although, when the plate was withdrawn, they were very intense.

58. It was at first thought that this effect might be pe-

* Philosophical Transactions, 1821.

cular to the iron, on account of its temporary magnetism ; but this idea was shown to be erroneous by substituting a

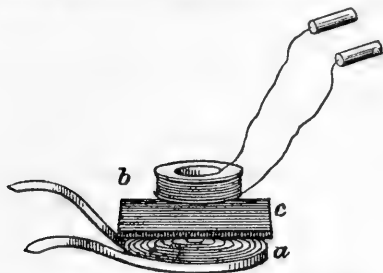


FIG 5.—*a* represents coil No. 1, *b* helix No. 1, and *c* an interposed plate of metal.

plate of zinc of about the same size and thickness. With this the screening influence was exhibited as before.

59. After this a variety of substances was interposed in succession, namely, copper, lead, mercury, acid, water, wood, glass, &c.; and it was found that all the perfect conductors, such as the metals, produced the screening influence; but nonconductors, as glass, wood, &c., appeared to have no effect whatever.

60. When the helix was separated from the coil by a distance only equal to the thickness of the plate, a slight sensation could be perceived even when the zinc of $\frac{1}{10}$ of an inch in thickness was interposed. This effect was increased by increasing the quantity of the battery current. If the thickness of the plate was diminished, the induction through it became more intense. Thus a sheet of tinfoil interposed produced no perceptible influence; also four sheets of the same were attended with the same result. A certain thickness of metal is therefore required to produce the screening effect, and this thickness depends on the quantity of the current from the battery.

61. The idea occurred to me that the screening might, in some way, be connected with an instantaneous current in the plate, similar to that in the induction by magnetic rotation, discovered by M. Arago. The ingenious variation of this principle by Messrs. Babbage and Herschel, furnished me with a simple method of determining this point.

62. A circular plate of lead was interposed, which caused the induction in the helix almost entirely to disappear. A slip of the metal was then cut out in the direction of a radius of the circle, as is shown in Fig. 6. With the plate in this condition, no screening was produced; the shocks were as intense as if the metal were not present.



FIG. 6.—*a* represents a lead plate, of which the sector *b* is cut out.

63. This experiment however is not entirely satisfactory, since the action might have taken place through the opening of the lead; to obviate this objection, another plate was cut in the same manner, and the two interposed with a glass plate between them, and so arranged that the opening in the one might be covered by the continuous part of the other. Still shocks were obtained with undiminished intensity.

64. But the existence of a current in the interposed conductor was rendered certain by attaching the magnetizing spiral by means of two wires to the edge of the opening in the circular plate, as is shown in Fig. 7. By this arrange-



FIG. 7.—*a* represents a lead plate, the spiral at *b*.
b the magnetizing spiral.

65. This current was a secondary one, and its direction, in conformity with the discovery of Dr. Faraday, was found to be the same as that of the primary current.

66. That the screening influence is in some way produced by the neutralizing action of the current thus obtained, will be clear, from the following experiment. The plate of zinc before mentioned, which is nearly twice the diameter of the helix, instead of being placed between the conductors, was put on the top of the helix, and in this position, although the neutralization was not as perfect as before, yet a great reduction was observed in the intensity of the shock.

67. But here a very interesting and puzzling question occurs. How does it happen that two currents, both in the same direction, can neutralize each other? I was at first

disposed to consider the phenomenon as a case of real electrical interference, in which the impulses succeed each other by some regular interval. But if this were true the effect should depend on the length and other conditions of the current in the interposed conductor. In order to investigate this, several modifications of the experiments were instituted.

68. First a flat coil (No. 3) was interposed instead of the plates. When the two ends of this were separated, the shocks were received as if the coil were not present; but when the ends were joined, so as to form a perfect metallic circuit, no shocks could be obtained. The neutralization with the coil in this experiment was even more perfect than with the plate.

69. Again, coil No. 2, in the form of a ring, was placed not between the conductors, but around the helix. With this disposition of the apparatus, and the ends of the coil joined, the shocks were scarcely perceptible, but when the ends were separated, the presence of the coil has no effect.

70. Also when helix No. 1 and 2 were together submitted to the influence of coil No. 1, the ends of the one being joined, the other gave no shock.

71. The experiments were further varied by placing helix No. 2 within a hollow cylinder of sheet brass, and this again within coil No. 2 in a manner similar to that shown in Fig. 12, which is intended to illustrate another experiment. In this arrangement the neutralizing action was exhibited, as in the case of the plate.

72. A hollow cylinder of iron was next substituted for the one of brass, and with this also no shocks could be obtained.

73. From these experiments it is evident that the neutralization takes place with currents in the interposed or adjoining conductors of all lengths and intensities, and therefore cannot, as it appears to me, be referred to the interference of two systems of vibrations.

74. This part of the investigation was, for a time, given up almost in despair, and it was not until new light had been obtained from another part of the inquiry, that any further advances could be made towards a solution of the mystery.

75. Before proceeding to the next Section, I may here state that the phenomenon mentioned, paragraph 54, in reference to helix No. 4, is connected with the neutralizing action. The electrical discharge having destroyed the insulation at some point, a part of the spires would thus form a shut circuit, and the induction in this would counteract the action in the other part of the helix; or in other words, the helix was in the same condition as the two helices mentioned in paragraph 70, when the ends of the wire of one were joined.

76. Also the same principle appears to have an important bearing on the improvement of the magneto-electrical machine: since the plates of metal which sometimes form the ends of the spool containing the wire, must necessarily diminish the action, and also from experiment of paragraph 72 the armature itself may circulate a closed current which will interfere with the intensity of the induction in the surrounding wire. I am inclined to believe that the increased effect observed by Sturgeon and Calland, when a bundle of wire is substituted for a solid piece of iron, is at least in part due to the interruption of these currents. I hope to resume this part of the subject, in connection with several other points, in another communication to the Society.

77. The results given in this Section may, at first sight, be thought at variance with the statements of Sir H. Davy, that needles could be magnetized by an electrical discharge with conductors interposed. But from his method of performing the experiment, it is evident that the plate of metal was placed between a straight conductor and the needle. The arrangement was therefore similar to the interrupted circuit in the experiment with the cut plate (62), which produces no screening effect. Had the plate been curved into the form of a hollow cylinder, with the two ends in contact, and the needle placed within this, the effect would have been otherwise.

SECTION V.

On the Production and Properties of induced Currents of the Third, Fourth, and Fifth order.

78. The fact of the perfect neutralization of the primary current by a secondary, in the interposed conductor, led me to conclude that if the latter could be drawn out, or separated from the influence of the former, it would itself be capable of producing a new induced current in a third conductor.

79. The arrangement exhibited in Fig. 8 furnishes a ready

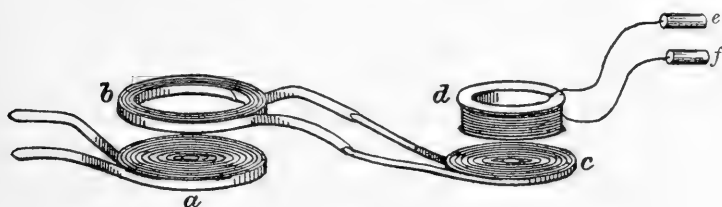


FIG. 8.—*a* coil No. 1, *b* coil No. 2, *c* coil No. 3, *d* helix No. 1.

means of testing this. The primary current, as usual, is passed through coil No. 1, while coil No. 2 is placed over this to receive the induction, with its ends joined to those of coil No. 3. By this disposition the secondary current passes through No. 3; and since this is at a distance, and without the influence of the primary, its separate induction will be rendered manifest by the effects on helix No. 1. When the handles *e, f*, are grasped a powerful shock is received, proving the induction of a tertiary current.

80. By a similar but more extended arrangement, as shown in Fig. 9, shocks were received from currents of a fourth and fifth order; and with a more powerful primary current, and additional coils, a still greater number of successive inductions might be obtained.

81. The induction of currents of different orders, of sufficient intensity to give shocks, could scarcely have been anticipated from our previous knowledge of the subject. The secondary current consists as it were of a single wave

of the natural electricity of the wire, disturbed but for an instant by the induction of the primary; yet this has the power of inducing another current, but little inferior in energy to itself, and thus produces effects apparently much greater in proportion to the quantity of electricity in motion than the primary current.

82. Some difference may be conceived to exist in the action of the induced currents, and that from the battery, since they are apparently different in nature; the one consisting, as we may suppose, of a single impulse, and the other of a succession of such impulses, or a continuous action. It was therefore important to investigate the properties of these currents, and to compare the results with those before obtained.

83. First, in reference to the intensity, it was found that with the small battery a shock could be given from the current of the third order to twenty-five persons joining hands; also shocks perceptible in the arms were obtained from a current of the fifth order.

84. The action at a distance was also much greater than could have been anticipated. In one experiment shocks from the tertiary current were distinctly felt through the tongue, when helix No. 1 was at the distance of eighteen inches above the coil transmitting the secondary current.

85. The same screening effects were produced by the interposition of plates of metal between the conductors of the different orders, as those which have been described in reference to the primary and secondary currents.

86. Also when the long helix is placed over a secondary current generated in a short coil, and which is therefore, as we have before shown, one of quantity, a tertiary current of intensity is produced.

87. Again, when the intensity current of the last experiment is passed through a second helix, and another coil is placed over this, a quantity current is again produced. Therefore in the case of these currents, as in that of the primary, *a quantity current can be induced from one of intensity, and the converse.* By the arrangement of the apparatus as shown in Fig. 9, these different results are exhibited at once.

The induction from coil No. 3 to helix No. 1 produces an intensity current, and from helix No. 2 to coil No. 4 a quantity current.

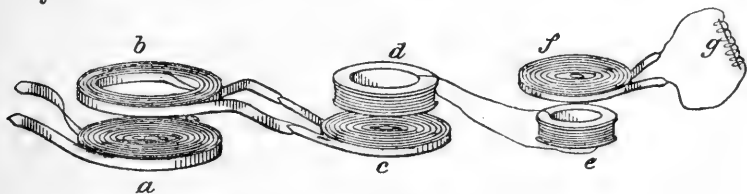


FIG. 9.—*a* coil No. 1, *b* coil No. 2, *c* coil No. 3, *d* helix No. 1, *e* helix No. 2 and 3, *f* coil No. 4, and *g* magnetizing spiral.

88. If the ends of coil No. 2, as in the arrangement of Fig. 8, be united to helix No. 1 instead of coil No. 3, no shocks can be obtained; the quantity current of coil No. 2 appears not to be of sufficient intensity to pass through the wire of the long helix.

89. Also, no shocks can be obtained from the handles attached to helix No. 2, in the arrangement exhibited in Fig.

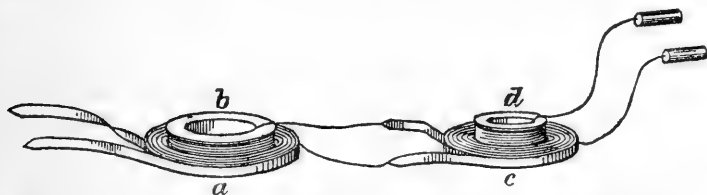


FIG. 10.—*a* coil No. 1, *b* helix No. 1, *c* coil No. 3, and *d* helix No. 3.

10. In this case the quantity of electricity in the current from the helix appears to be too small to produce any effect, unless its power is multiplied by passing it through a conductor of many spires.

90. The next inquiry was in reference to the direction of these currents, and this appeared important in connection with the nature of the action. The experiments of Dr. Faraday would render it probable, that at the beginning and ending of the secondary current, its induction on an adjacent wire is in contrary directions, as is shown to be the case in the primary current. But the whole action of a secondary current is so instantaneous, that the inductive effects at the

beginning and ending cannot be distinguished from each other, and we can only observe a single impulse, which however may be considered as the difference of two impulses in opposite directions.

91. The first experiment happened to be made with a current of the fourth order. The magnetizing spiral (11) was attached to the ends of coil No. 4, Fig. 9, and by the polarity of the needle it was found that this current was in the same direction with the secondary and primary currents.* By a too hasty generalization, I was led to conclude, from this experiment, that the currents of all orders are in the same direction as that of the battery current, and I was the more confirmed in this from the results of my first experiments on the currents of ordinary electricity. The conclusion however caused me much useless labor and perplexity, and was afterwards proved to be erroneous.

92. By a careful repetition of the last experiment, in reference to each current, the important fact was discovered, that *there exists an alternation in the direction of the currents of the several orders, commencing with the secondary.* This result was so extraordinary, that it was thought necessary to establish it by a variety of experiments. For this purpose the direction was determined by decomposition, and also by the galvanometer, but the result was still the same; and at this stage of the inquiry I was compelled to the conclusion that the directions of the several currents were as follows:

Primary current.....	+
Secondary current.....	+
Current of the third order.....	—
Current of the fourth order	+
Current of the fifth order	—

93. In the first glance at the above table, we are struck with the fact that the law of alternation is complete, except between the primary and secondary currents, and it appeared

* It should be recollected that all the inductions which have been mentioned were produced at the moment of breaking the circuit of the battery current. The induction at the formation of the current is too feeble to produce the effects described.

that this exception might possibly be connected with the induced current which takes place in the first coil itself, and which gives rise to the phenomena of the spiral conductor. If this should be found to be *minus*, we might consider it as existing between the primary and secondary, and the anomaly would thus disappear. Arrangements were therefore made to fully satisfy myself on this point. For this purpose the decomposition of dilute acid and the use of the galvanometer were resorted to, by placing the apparatus between the ends of a cross wire attached to the extremities of the coil, as in the arrangement described by Dr. Faraday (ninth series;) but all the results persisted in giving a direction to this current the same as stated by Dr. Faraday, namely, that of the primary current. I was therefore obliged to abandon the supposition that the anomaly in the change of the current is connected with the induction of the battery current on itself.

94. Whatever may be the nature or causes of these changes in the direction, they offer a ready explanation of the neutralizing action of the plate interposed between two conductors, since a secondary current is induced in the plate; and although the action of this, as has been shown, is in the same direction as the current from the battery, yet it tends to induce a current in the adjacent conducting matter of a contrary direction. The same explanation is also applicable to all the other cases of neutralization, even to those which take place between the conductors of the several orders of currents.

95. The same principle explains some effects noted in reference to the induction of a current on itself. If a flat coil be connected with the battery, of course sparks will be produced by the induction, at each rupture of the circuit. But if in this condition another flat coil, with its ends joined, be placed on the first coil, the intensity of the shock is much diminished, and when the several spires of the two coils are mutually interposed by winding the two ribbons together into one coil, the sparks entirely disappear in the coil transmitting the battery current, when the ends of the other are joined. To understand this, it is only necessary to mention

that the induced current in the first coil is a true secondary current, and it is therefore neutralized by the action of the secondary in the adjoining conductor; since this tends to produce a current in the opposite direction.

96. It would also appear from the perfect neutralization which ensues in the arrangement just before described, that the induced current in the adjoining conductor is more powerful than that of the first conductor; and we can easily see how this may be. The two ends of the second coil are joined, and it thus forms a perfect metallic circuit; while the circuit of the other coil may be considered as partially interrupted, since to render the spark visible the electricity must be projected as it were through a small distance of air.

97. We would also infer that two contiguous secondary currents, produced by the same induction, would partially counteract each other. Moving in the same direction, they would each tend to induce a current in the other of an opposite direction. This is illustrated by the following experiment: helix No. 1 and 2 were placed together, but not united, above coil No. 1, so that they each might receive the induction; the larger was then gradually removed to a greater distance from the coil, until the intensity of the shock from each was about the same. When the ends of the two were united, so that the shock would pass through the body from the two together, the effect was apparently less than with one helix alone. The result however was not as satisfactory as in the case of the other experiments; a slight difference in the intensity of two shocks could not be appreciated with perfect certainty.

SECTION VI.

The production of induced Currents of the different Orders from ordinary Electricity.

98. Dr. Faraday, in the ninth series of his researches, remarks that "the effect produced at the commencement and the end of a current (which are separated by an interval of time when that current is supplied from a voltaic apparatus) must occur at the same moment when a common electrical

discharge is passed through a long wire. Whether if it happen accurately at the same moment they would entirely neutralize each other, or whether they would not still give some definite peculiarity to the discharge, is a matter remaining to be examined."

99. The discovery of the fact that the secondary current, which exists but for a moment, could induce another current of considerable energy, gave some indication that similar effects might be produced by a discharge of ordinary electricity, provided a sufficiently perfect insulation could be obtained.

100. To test this a hollow glass cylinder, Fig. 11, of about

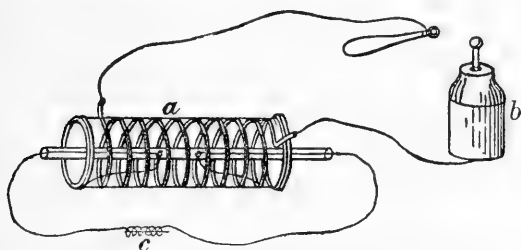


FIG. 11.—*a* glass cylinder, *b* Leyden jar, *c* magnetizing spiral.

six inches in diameter, was prepared with a narrow ribbon of tinfoil, about thirty feet long, pasted spirally around the outside, and a similar ribbon of the same length, pasted on the inside; so that the corresponding spires of the two were directly opposite each other. The ends of the inner spiral passed out of the cylinder through a glass tube, to prevent all direct communication between the two. When the ends of the inner ribbon were joined by the magnetizing spiral (11), containing a needle, and a discharge from a half gallon jar sent through the outer ribbon, the needle was strongly magnetized in such a manner as to indicate *an induced current through the inner ribbon in the same direction as that of the current of the jar*. This experiment was repeated many times, and always with the same result.

101. When the ends of one of the ribbons were placed very nearly in contact, a small spark was perceived at the

opening, the moment the discharge took place through the other ribbon.

102. When the ends of the same ribbon were separated to a considerable distance, a larger spark than the last could be drawn from each end by presenting a ball, or the knuckle.

103. Also if the ends of the outer ribbon were united, so as to form a perfect metallic circuit, a spark could be drawn from any point of the same, when a discharge was sent through the inner ribbon.

104. The sparks in the two last experiments are evidently due to the action known in ordinary electricity by the name of the lateral discharge. To render this clear, it is perhaps necessary to recall the well known fact, that when the knob of a jar is electrified positively, and the outer coating is connected with the earth, then the jar contains a small excess of positive electricity beyond what is necessary to perfectly neutralize the negative surface. If the knob be put in communication with the earth, the extra quantity, or the free electricity, as it is sometimes called, will be on the negative side. When the discharge took place in the above experiments, the inner ribbon became for an instant charged with this free electricity, and consequently threw off from the outer ribbon, by ordinary induction, the sparks described. It therefore became a question of importance to determine whether the induced current described in paragraph 100 was not also a result of the lateral discharge, instead of being a true case of a secondary current analogous to those produced from galvanism. For this purpose the jar was charged, first with the outer coating in connection with the earth, and again with the knob in connection with the same, so that the extra quantity might be in the one case *plus* and in the other *minus*; but the direction of the induced current was not affected by these changes; it was always the same, namely, from the positive to the negative side of the jar.

105. When however the quantity of free electricity was increased, by connecting the knob of the jar with a globe about a foot in diameter, the intensity of magnetism ap-

peared to be somewhat diminished, if the extra quantity was on the negative side; and this might be expected, since the free electricity, in its escape to the earth through the ribbon, in this case would tend to induce a feeble current in the opposite direction to that of the jar.

106. The spark from an insulated conductor may be considered as consisting almost entirely of this free or extra electricity, and it was found that this was also capable of producing an induced current, precisely the same as that from the jar. In the experiment which gave this result, one end of the outer ribbon of the cylinder (100) was connected with the earth, and the other caused to receive a spark from a conductor fourteen feet long, and nearly a foot in diameter. The direction of the induced current was the same as that of the spark from the conductor.

107. From these experiments it appears evident that the discharge from the Leyden jar possesses the property of inducing a secondary current precisely the same as the galvanic apparatus, and also that this induction is only so far connected with the phenomenon of the lateral discharge as this latter partakes of the nature of an ordinary electrical current.

108. Experiments were next made in reference to the production of currents of the different orders by ordinary electricity. For this purpose a second cylinder was prepared with ribbons of tinfoil, in a similar manner to the one before described. The two were then so connected that the secondary current from the first would circulate around the second. When a discharge was passed through the outer ribbon of the first cylinder, a tertiary current was induced in the inner ribbon of the second. This was rendered manifest by the magnetizing of a needle in a spiral joining the ends of the last mentioned ribbon.

109. Also by the addition, in the same way, of a third cylinder, a current of the fourth order was developed. The same result was likewise obtained by using the arrangement of the coils and helices shown in Fig. 9. For these experiments however the coils were furnished with a double coat-

ing of silk, and the contiguous conductors separated by a large plate of glass.

110. Screening effects precisely the same as those exhibited in the action of galvanism were produced by interposing a plate of metal between the conductors of different orders, Figures 8 and 9. The precaution was taken to place the plate between two frames of glass, in order to be assured that the effect was not due to a want of perfect insulation.

111. Also analogous results were found when the experiments were made with coils interposed instead of plates, as described in paragraph 68. When the ends of the interposed coils were separated, no screening was observed, but when joined, the effect was produced. The existence of the induced current, in all these experiments, was determined by the magnetism of a needle in a spiral attached to one of the coils.

112. Likewise shocks were obtained from the secondary current by an arrangement shown in Fig. 12. Helices No.

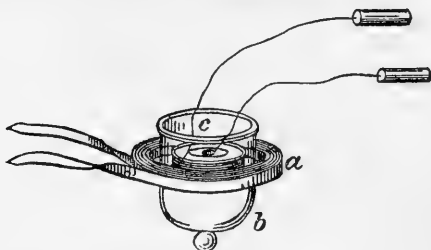


FIG. 12.—*a* coil No. 2, *b* an inverted bell glass, *c* helices No. 2 and 3.

2 and No. 3 united are put within a glass jar, and coil No. 2 is placed around the same. When the handles are grasped, a shock is felt at the moment of the discharge, through the outer coil. The shocks however were very different in intensity with different discharges from the jar. In some cases no shock was received, when again, with a less charge, a severe one was obtained. But these irregularities find an explanation in a subsequent part of the investigation.

113. In all these experiments, the results with ordinary and galvanic electricity are similar. But at this stage of the investigation there appeared what at first was considered a

remarkable difference in the action of the two. I allude to the direction of the currents of the different orders. These, in the experiments with the glass cylinders, instead of exhibiting the alternations of the galvanic currents (92), were all in the same direction as the discharge from the jar, or in other words, they were all *plus*.

114. To discover, if possible, the cause of this difference, a series of experiments was instituted; but the first fact developed, instead of affording any new light, seemed to render the obscurity more profound. When the directions of the currents were taken in the arrangement of the coils (Fig. 9) the discrepancy vanished. *Alternations were found the same as in the case of galvanism.* This result was so extraordinary that the experiments were many times repeated, first with the glass cylinders, and then with the coils; the results however were always the same. The cylinders gave currents all in one direction; the coils in alternate directions.

115. After various hypotheses had been formed, and in succession disproved by experiment, the idea occurred to me that the direction of the currents might depend on the distance of the conductors, and this appeared to be the only difference existing in the arrangement of the experiments with the coils and the cylinders.* In the former the distance between the ribbons was nearly one inch and a half, while in the latter it was only the thickness of the glass, or about $\frac{1}{20}$ th of an inch.

116. In order to test this idea, two narrow slips of tinfoil, about twelve feet long, were stretched parallel to each other, and separated by thin plates of mica to the distance of about $\frac{1}{60}$ th of an inch. When a discharge from the half gallon jar was passed through one of these, an induced current in the same direction was obtained from the other. The ribbons were then separated, by plates of glass, to the distance of $\frac{1}{20}$ th of an inch; the current was still in the same direction, or *plus*. When the distance was increased to about $\frac{1}{8}$ th of an inch, no induced current could be obtained; and when they

* This idea was not immediately adopted, because I had previously experimented on the direction of the secondary current from galvanism, and found no change in reference to distance.

were still further separated the current again appeared, but was now *found to have a different direction, or to be minus*. No other change was observed in the direction of the current; the intensity of the induction decreased as the ribbons were separated. The existence and direction of the current, in this experiment, were determined by the polarity of the needle in the spiral attached to the ends of one of the ribbons.

117. The question at this time arose, whether the direction of the current, as indicated by the polarity of the needle, was the true one, since the magnetizing spiral might itself, in some cases, induce an opposite current. To satisfy myself on this point a series of charges, of various intensity and quantity, from a single spark of the large conductor to the full charge of nine jars, were passed through the small spiral, which had been used in all the experiments, but they all gave the same polarity. The interior of this spiral is so small, that the needle is throughout in contact with the wire.

118. The fact of a change in the direction of the induced current by a change in the distance of the conductors, being thus established, a great number and variety of experiments were made to determine the other conditions on which the change depends. These were sought for in a variation of the intensity and quantity of the primary discharge, in the length and thickness of the wire, and in the form of the circuit. The results were however in many cases anomalous, and are not sufficiently definite to be placed in detail before the Society. I hope to resume the investigation at another time, and will therefore at present briefly state only those general facts which appear well established.

119. With a single half gallon jar, and the conductors separated to a distance less than $\frac{1}{10}$ th of an inch, the induced current is always in the same direction as the primary. But when the conductors are gradually separated, there is always found a distance at which the current begins to change its direction. This distance depends certainly on the amount of the discharge, and probably on the intensity, and also on the length and thickness of the conductors. With a battery of eight half gallon jars, and parallel wires of about ten feet long, the change in the direction did not

take place at a less distance than from twelve to fifteen inches, and with a still larger battery and longer conductors, no change was found, although the induction was produced at the distance of several feet.

120. The facts given in the last paragraph relate to the inductive action of the primary current; but it appears from the results detailed in paragraphs 110 and 114, that the currents of all the other orders also change the direction of the inductive influence with a change of the distance. In these cases however the change always takes place at a very small distance from the conducting wire; and in this respect the result is similar to the effect of a *primary current* from the discharge of a small jar.

121. The most important experiments, in reference to distance, were made in the lecture room of my respected friend Dr. Hare of Philadelphia, with the splendid electrical apparatus described in the Fifth Volume (new series) of the Transactions of this Society. The battery consists of thirty-two jars, each of the capacity of a gallon. A thick copper wire of about $\frac{1}{10}$ th of an inch in diameter and eighty feet in length, was stretched across the lecture room, and its ends brought to the battery, so as to form a trapezium, the longer side of which was about thirty-five feet. Along this side a wire was stretched of the ordinary bell size, and the extreme ends of this joined by a spiral, similar to the arrangement shown in Fig. 13. The two wires were at first placed within

the distance of about an inch, and afterwards constantly separated after each discharge of the whole battery through the thick wire. When a break was made in the second wire at *a*, no magnetism was developed in a needle in the spiral at *b*, but when the circuit was complete, the needle at each discharge indicated a current in the same direction as that of the battery. When the distance of the two

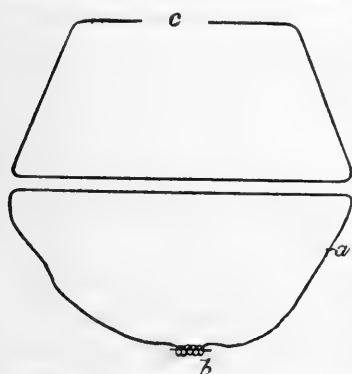


FIG. 13.—*c* place of the battery, *b* spiral.

wires was increased to sixteen inches, and the ends of the second wire placed in two glasses of mercury, and a finger of each hand plunged into the metal, a shock was received. The direction of the current was still the same, but the magnetism not as strong as at a less distance.

122. The second wire was next arranged around the other, so as to enclose it. The magnetism by this arrangement appeared stronger than with the last; the direction of the current was still the same, and continued thus, until the two wires were at every point separated to the distance of twelve feet, except in one place where they were obliged to be crossed at the distance of seven feet, but here the wires were made to form a right angle with each other, and the effect of the approximation was therefore (46) considered as nothing. The needle at this surprising distance was tolerably strongly magnetized, as was shown by the quantity of filings which would adhere to it. The direction of the current was still the same as that of the battery. The form of the room did not permit the two wires to be separated to a greater distance. The whole length of the circuit of the interior large wire was about eighty feet; that of the exterior one hundred and twenty. The two were not in the same plane, and a part of the outer passed through a small adjoining room.

123: The results exhibited in this experiment are such as could scarcely have been anticipated by our previous knowledge of the electrical discharge. They evince a remarkable inductive energy, which has not before been distinctly recognized, but which must perform an important part in the discharge of electricity from the clouds. Some effects which have been observed during thunder storms, appear to be due to an action of this kind.

124. Since a discharge of ordinary electricity produces a secondary current in an adjoining wire, it should also produce an analogous effect in its own wire; and to this cause may be now referred the peculiar action of a long conductor. It is well known that the spark from a very long wire, although quite short, is remarkably pungent. I was so fortunate as to witness a very interesting exhibition of

this action during some experiments on atmospheric electricity made by a committee of the Franklin Institute, in 1836. Two kites were attached, one above the other, and raised with a small iron wire in place of a string. On the occasion at which I was present, the wire was extended by the kites to the length of about one mile. The day was perfectly clear, yet the sparks from the wire had so much projectile force (to use a convenient expression of Dr. Hare) that fifteen persons joining hands and standing on the ground, received the shock at once, when the first person of the series touched the wire. A Leyden jar being grasped in the hand by the outer coating, and the knob presented to the wire, a severe shock was received, as if by a perforation of the glass, but which was found to be the result of the sudden and intense induction.

125. These effects were evidently not due to the accumulated intensity at the extremities of the wire, on the principles of ordinary electrical distribution, since the knuckle required to be brought within about a quarter of an inch before the spark could be received. It was not alone the quantity, since the experiments of Wilson prove that the same effect is not produced with an equal amount of electricity on the surface of a large conductor. It appears evidently therefore a case of the induction of an electrical current on itself. The wire is charged with a considerable quantity of feeble electricity, which passes off in the form of a current along its whole length, and thus the induction takes place at the end of the discharge, as in the case of a long wire transmitting a current of galvanism.

126. It is well known that the discharge from an electrical battery possesses great divellent powers; that it entirely separates, in many instances, the particles of the body through which it passes. This force acts, in part, at least, in the direction of the line of the discharge, and appears to be analogous to the repulsive action discovered by Ampere, in the consecutive parts of the same galvanic current. To illustrate this, paste on a piece of glass a narrow slip of tinfoil, cut it through at several points, and loosen the ends from the glass

at the places so cut. Pass a discharge through the tinfoil from about nine half gallon jars; the ends, at each separation, will be thrown up, and sometimes bent entirely back, as if by the action of a strong repulsive force between them.

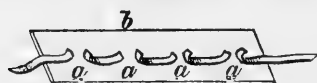


FIG 14.—*b* glass plate; *a, a, a, a,* openings in tinfoil.

This will be understood by a reference to Fig. 14; the ends are shown bent back at *a, a, a, a*. In the popular experiment of the pierced card, the bur on each side

appears to be due to an action of the same kind.

127. It now appears probable, from the facts given in paragraphs 119 and 120, that the table in paragraph 92 is only an approximation to the truth, and that each current from galvanism, as well as from electricity, first produces an inductive action in the direction of itself, and that the inverse influence takes place at a little distance from the wire.

128. To test this the compound helix was placed on coil No. 1, to receive the induction, and its ends joined to those of the outer ribbon of tinfoil of the glass cylinder, while the magnetizing spiral was attached to the ends of the inner riband. A feeble tertiary current was produced by this arrangement, which in two cases gave a polarity to the needle indicating a direction the same as that of the primary current. In other cases the magnetism was either imperceptible or *minus*. With an arrangement of two coils of wires around two glass cylinders, one within the other, the same effect was produced. The magnetism was less when the distance of the two sets of spires was smaller, indicating, as it would appear, an approximation to a position of neutrality. These results are rather of a negative kind, yet they appear to indicate the same change with distance in the case of the galvanic currents as in that of the discharge of ordinary electricity. The distance however at which the change takes place would seem to be less in the former than in the latter.

129. There is a perfect analogy between the inductive action of the primary current from the galvanic apparatus and of that from the larger electrical battery. The point of change, in each, appears to be at a great distance.

130. The neutralizing effect described in Section IV may now be more definitely explained by saying that when a third conductor is acted on at the same time by a primary and secondary current (unless it be very near the second wire) it will fall into the region of the *plus* influence of the former, and into that of the *minus* influence of the latter; and hence no induction will be produced.

131. This will be rendered perfectly clear by Fig. 15, in which *a* represents the conductor of the primary current, *b* that of the secondary, and *c* the third conductor. The characters + + +, &c., beginning at the middle of the first conductor and extending downwards, represent the constant *plus* influence of the primary current, and those + 0 —, &c., beginning at the

FIG. 15.

second conductor, indicate its inductive influence as changing with the distance. The third conductor, as is shown by the figure, falls in the *plus* region of the primary current, and in the *minus* region of the secondary, and hence the two actions neutralize each other, and no apparent result is produced.

132. Fig. 16 indicates the method in which the neutralizing effect is produced in the case of the secondary and tertiary currents. The wire conducting the secondary current is represented by *b*, that conducting the tertiary by *c*, and the other wire, to receive the induction from these, by *d*. The direction of the influence, as before, is indicated by + 0 —,

FIG. 16.

&c., and the third wire is again seen to be in the *plus* region of the one current, and in the *minus* of the other. If however *d* is placed sufficiently near *c*, then neutralization will

not take place, but the two currents will conspire to produce in it an induction in the same direction. A similar effect would also be produced were the wire *c*, in Fig. 15, placed sufficiently near the conductor *b*.

133. Currents of the several orders were likewise produced from the excitation of the magneto-electrical machine. The same neutralizing effects were observed between these as in the case of the currents from the galvanic battery, and hence we may infer that also the same alternations take place in the direction of the several currents.

134. In conclusion, I may perhaps be allowed to state, that the facts here presented have been deduced from a laborious series of experiments, and are considered as forming some addition to our knowledge of electricity, independently of any theoretical considerations. They appear to be intimately connected with various phenomena, which have been known for some years, but which have not been referred to any general law of action. Of this class are the discoveries of Savary on the alternate magnetism of steel needles placed at different distances from the line of a discharge of ordinary electricity,* and also the magnetic, screening influence of all metals, discovered by Dr. Snow Harris, of Plymouth.† A comparative study of the phenomena observed by these distinguished *savants*, and those given in this paper, would probably lead to some new and important developments. Indeed every part of the subject of electro-dynamic induction appears to open a field for discovery, which experimental industry cannot fail to cultivate with immediate success.

NOTE.

On the evening of the meeting at which my investigations were presented to the Society, my friend, Dr. Bache of the Girard College, gave an account of the investigations of Professor Ettingshausen, of Vienna, in reference to the improvement of the magneto-electric machine, some of the results of which he had witnessed at the University of Vienna,

* *Annales de Chimie et de Physique*, 1827.

† *Philosophical Transactions*, 1831.

about a year since. No published account of these experiments has yet reached this country, but it appears that Professor Ettingshausen had been led to suspect the development of a current in the metal of the keeper of the magneto-electric machine, which diminished the effect of the current in the coil about the keeper, and hence to separate the coil from the keeper by a ring of wood of some thickness, and afterwards, to prevent entirely the circulation of currents in the keeper, by dividing it into segments, and separating them by a non-conducting material. I am not aware of the result of this last device, nor whether the mechanical difficulties in its execution were fully overcome. It gives me pleasure to learn that the improvements, which I have merely suggested as deductions from the principles of the interference of induced currents (76), should be in accordance with the experimental conclusions of the above named philosopher.*

*[Re-printed in Silliman's American Journal of Science, March, 1840, vol. xxxviii, pp. 209-243.]

CAPILLARY TRANSMISSION THROUGH SOLIDS.

(Proceedings of the American Philosophical Society, vol. I, pp. 82, 83.)*

March 15, 1839.

Professor Henry made a verbal communication relating to a phenomenon of capillary action which had fallen under his notice.

A lead tube of about half an inch in diameter and eight inches long happened to be left with one end immersed in a cup of mercury, and on inspection a few days afterwards it was observed that the mercury had disappeared from the cup, and was found on the floor at the other end of the tube. Struck with the phenomenon, I again filled the cup with mercury; the next morning the same effect was exhibited. The mercury had again passed over through the tube, apparently like water through a capillary siphon, and was again found on the floor.

On cutting the tube into pieces, it was evident that the mercury had not passed along the hollow axis, but had apparently been transmitted through the pores of the solid metal. To determine this, a lead rod of about seven inches long and a quarter of an inch in diameter was bent into the form of a siphon. The shorter leg was immersed in a watch-glass filled with mercury, and a similar glass placed under the end of the longer leg, to receive the metal which might pass over. At the end of twenty-four hours a globule of mercury was perceived at the lower end; and in the course of five or six days all the mercury passed over, leaving a crop of beautiful arborescent crystals of an amalgam of lead in the upper glass.

The mercury did not pass along the surface of the wire, since the lead exhibited, externally, but little change of appearance, although the progress of the penetration could be traced by a slight variation of the color of the oxide on the surface.

The action is much influenced by the texture of the lead. When a rod of cast lead, of the same size and form, was

* [Re-printed in Silliman's American Journal of Science, December, 1839, vol. xxxviii, pp. 180, 181.]

substituted for the one before described, the globule of mercury did not make its appearance at the lower end until about forty days, and all the mercury of the upper glass had not yet (after three months) entirely disappeared.

The penetration takes place much more readily in the direction of the laminæ of the metal than across them. A plate of thick sheet lead was formed into a cup, and mercury poured into this; and it was found that before a drop had passed directly through, the mercury oozed out all around the edge of the plate.

Professor Henry stated that he had in progress a variety of experiments to investigate this action, and if any results of importance were obtained he would communicate them to the Society.

LETTER ON ELECTRICAL INDUCTION.

(Proceedings of the American Philosophical Society, vol. I, pp. 134-136.)

October 18, 1839.

The following extract from a letter addressed by Professor Henry to Professor Bache was read, announcing the discovery of two distinct kinds of dynamic induction by a galvanic current.

"Since the publication of my last paper, I have received through the kindness of Dr. Faraday a copy of his fourteenth series of experimental researches, and in this I was surprised to find a statement directly in opposition to one of the principal results given in my paper. It is stated in substance in the 59th paragraph of my last communication to the American Philosophical Society, that when a plate of metal is interposed between a galvanic current and a conductor the secondary shock is neutralized. Dr. Faraday finds, on the contrary, under apparently the same circumstances, that no effect is produced by the interposition of the metal. As the fact mentioned forms a very important part of my paper, and is connected with nearly all the phenomena described subsequently to it, I was anxious to investigate the cause of the discrepancy between the results obtained by Dr. Faraday, and those found by myself. My

experiments were on such a scale, and the results so decided, that there could be no room for doubt as to their character; a secondary current of such intensity as to paralyze the arms having been so neutralized by the interposition of a plate and ribbon of metal, as not to be perceptible through the tongue. I was led by a little reflection to conclude that there might exist a case of induction similar to that of magnetism, in which no neutralization would take place; and I thought it possible that Dr. Faraday's results might have been derived from this. I have now however found a solution to the difficulty in the remarkable fact that an electrical current from a galvanic battery exerts two distinct kinds of dynamic induction. One of these produces, by means of a helix of long wire, intense secondary shocks at the moment of breaking the contact, and feeble shocks at the moment of making the contact. This kind of induction is capable also of being neutralized by the interposition of a plate of metal between two conductors. The other kind of induction is produced at the same time from the same arrangement, and does not give shocks, but affects the needle of the galvanometer. It is of equal energy at the moment of making contact and of breaking contact, and is not affected by the introduction of a plate of copper or zinc between the conductors.* The phenomena produced by the first kind of induction form the subject of my last paper, as well as that of the previous one; while it would appear from the arrangement of Dr. Faraday's experiments that the results detailed in his first series and those in the fourteenth were principally produced by the second kind of induction. Although I may be too sanguine in reference to the results of the discovery, yet I cannot refrain from adding that it appears to lead to a separation of the electrical induction of a galvanic current from the magnetical, and that it is a step of some importance towards a more precise knowledge of the phenomena of magneto-electricity."

* Since writing the account of the two kinds of induction I have found that the second kind, although not screened by a plate of copper or zinc, is affected by the introduction of a plate of iron. In the cases of the first kind of induction iron acts as any other metal.

CONTRIBUTIONS TO ELECTRICITY AND MAGNETISM. No. IV.

ON ELECTRO-DYNAMIC INDUCTION. (CONTINUED.)

(Transactions American Philosophical Society, n. s., vol. VIII, pp. 1-35.)*

Read June 19, 1840.

INTRODUCTION.

1. In the course of my last paper, (No. III,) it was stated that the investigations which it detailed were not as complete in some parts as I could wish, and that I hoped to develop them more fully in another communication. After considerable delay, occasioned by alterations in the rooms of the physical department of the college, I was enabled to resume my researches; and since then I have been so fortunate as to discover a series of new facts belonging to different parts of the general subject of my contributions. These I have announced to the Society at different times, as they were discovered, and I now purpose to select from the whole such portions as relate particularly to the principal subject of my last paper, namely, the induction at the beginning and ending of a galvanic current, and to present them as a continuation, and in a measure as the completion of this part of my researches. The other results of my labors in this line will be arranged for publication as soon as my duties will permit me to give them a more careful examination.

2. In the course of the experiments I am about to describe, I have had occasion to repeat and vary those given in my last paper, and I am happy to be able to state, in reference to the results, that except in some minor particulars which will be mentioned in the course of this paper, I have found no cause to desire a change in the accounts before published. My views however of the connection of the phenomena have been considerably modified, and I think rendered much more definite by the additional light which the new facts have afforded.

3. The principal articles of apparatus used in these experiments are nearly the same as those described in my last

*[The title-page of vol. VIII bears date 1843.]

paper, namely, several flat coils, and a number of long wire helices. (No. III, 6, 7, 8.*) I have however added to these a constant battery, on Prof. Daniell's plan, the performance of which has fully answered my expectations, and confirmed the accounts given of this form of the instrument by its author. It consists of thirty elements, formed of as many copper cylinders, open at the bottom, each five inches and a half in height, three inches and a half in diameter, and placed in earthen cups. A zinc rod is suspended in each of these, of the same length as the cylinders, and about one inch in diameter. The several elements are connected by a thick copper wire, soldered to the copper of one element, and dipping into a cup of mercury on the zinc of the next. The copper and zinc as usual are separated by a membrane, on both sides of which is placed a solution of one part of sulphuric acid in ten parts of water; and to this is added, on the side next the copper, as much sulphate of copper as will saturate the solution. The battery was sometimes used as a single series, with all its elements placed consecutively, and at others in two or three series, arranged collaterally, so as to vary the quantity and intensity of the electricity as the occasion might require.

4. The galvanometers mentioned in this paper, and referred to in the last, are of two kinds; one, which is used with a helix, to indicate the action of an induced current of intensity, consists of about five hundred turns of fine copper wire, covered with cotton thread, and more effectually insulated by steeping the instrument in melted cement, which was drawn into the spaces between the spires by capillary attraction. The other galvanometer is formed of about forty turns of a shorter and thicker wire, and is always used to indicate an induced current, of considerable quantity, but of feeble intensity. The needle of both these instruments is suspended by a single fibre of raw silk.

5. I should also state, that in all cases where a magnetizing spiral is mentioned in connection with a helix, the

*The numerals II or III included in parentheses refer to the corresponding Nos. of my previous Contributions.

article is formed of a long, fine wire, making about one hundred turns around the axis of a hollow piece of straw about two inches and a half long: also the spiral mentioned in connection with a coil, is formed of a short wire which makes about twenty turns around a similar piece of straw. The reason of the use of the two instruments in these two cases is the same as that for the galvanometers, under similar circumstances, namely, the helix gives a current of intensity, but of small quantity, while the coil produces one of considerable quantity, but of feeble intensity.

SECTION I.

On the Induction produced at the moment of the Beginning of a Galvanic Current, &c.

6. It will be recollected that the arrangement of apparatus employed in my last series of experiments gave a powerful induction at the moment of breaking the galvanic circuit, but the effect at making the same was so feeble as scarcely to be perceptible. I was unable in any case to get indications of currents of the third or fourth orders from the beginning induction, and its action was therefore supposed to be so feeble as not materially to affect the results obtained.

7. Subsequent reflection however led me to conclude that in order to complete this part of my investigations, a more careful study of the induction at the beginning of the current would be desirable; and accordingly on resuming the experiments, my attention was first directed to the discovery of some means by which the intensity of this induction might be increased. After some preliminary experiments, it appeared probable that the desired result could be obtained by using a compound galvanic battery, instead of the single one before employed. In reference to this conjecture, the constant battery before mentioned (3) was constructed, and a series of experiments instituted with it, the results of which agreed with my anticipation.

8. In the first experiment, coil No. 2, which it will be remembered (No. III, 7,) consists of a copper ribbon about sixty feet long, coiled on itself like the main spring of a watch,

was connected with the compound battery, and helix No. 1, (No. III, 8,) formed of one thousand six hundred and sixty yards of fine copper wire, was placed on the coil to receive the induction, as is shown in figure 3, which is again inserted here for the convenience of the reader.

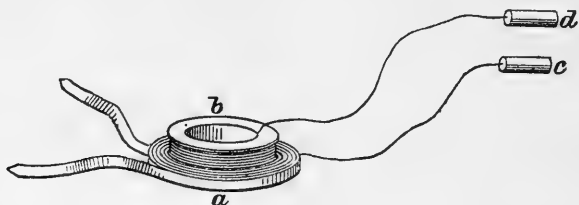


FIG. 3.—*a* represents coil No. 1, *b* helix No. 1, and *c*, *d*, handles for receiving the shock.

This arrangement being made, currents of increasing intensity were passed through the coil by constantly retaining one of its ends in the cup of mercury forming one extremity of the battery, and successively plunging the other end into the cups which served to form the connections of the several elements of the battery. With the current from one element, the shock at breaking the circuit was quite severe; but at making the same it was very feeble, and could be perceived in the fingers only, or through the tongue. With two elements in the circuit, the shock at the beginning was slightly increased; with three elements the increase was more decided, while the shock at breaking the circuit remained nearly of the same intensity as at first, or was comparatively but little increased. When the number of elements was increased to *ten*, the shock at making contact was found fully equal to that at breaking, and by employing a still greater number, the former was decidedly stronger than the latter, the difference continually increasing until all the thirty elements were introduced into the circuit.

9. In my last paper, a few experiments are mentioned as being made with a compound battery of Cruickshank's construction; but from the smallness of its plates, and the rapidity with which its power declined, I was led into the error of supposing that the induction at the ending of

the current, in the case of a short coil, was diminished by increasing the intensity of the battery; (see paragraph 19, of No. III;) but by employing the more perfect instrument of Professor Daniell in the arrangement of the last experiment, I am enabled to correct this error, and to state that the induction at the ending remains nearly the same, when the intensity of the battery is increased. If the induction depends in any degree on the quantity of current electricity in the conductor, then a slight increase in the induction should take place, since according to theory the current is somewhat increased in quantity, in the case of a long coil, by the increase of the intensity of the battery. Although very little, if any, difference could be observed in the intensity of the shock from the secondary current, yet the snap and deflagration of the mercury appeared to be greater from the primary current, when *ten* elements of the battery were included in the circuit, than with a single one. The other results which are mentioned in my last paper in reference to the compound battery are I believe correctly given.

10. The intensities of the different shocks in the foregoing experiments were compared by gradually raising the helix from the coil, (see Fig. 3,) until on account of the distance of the conductors, the shock in one case would be so much reduced as to be scarcely perceptible through the fingers or the tongue, while the shock from another arrangement, but with the same distance of the conductors, would be evident perhaps in the hands. The same method was generally employed in the experiments in which shocks are mentioned as being compared, in the other parts of this paper.

11. Experiments were next made to determine the influence of a variation in the length of the coil, the intensity of the battery remaining the same. For this purpose, the battery consisting of a single element, and the arrangement of the apparatus as represented in Fig. 3, the coil was diminished in length from sixty feet to forty-five, then to thirty, and so on. With the first mentioned length the shock, at making contact with the battery, was of course very feeble, and could be felt only in the tongue; with the

next shorter length it was more perceptible, and increased in intensity with each diminution of the coil, until a length of about fifteen feet appeared to give a maximum result.

12. The diminution of the intensity of the shock in the last experiment, after the length of the coil was diminished below fifteen feet, was due to the diminution of the number of spires of the coil, each of which, by acting on the helix, tends to increase the intensity of the secondary current, unless the combined length of the whole is too great for the intensity of the battery. That this is the fact is shown by the following experiment: the helix was placed on a single spire or turn of the coil, and the length of the other part of the copper ribbon, which did not act on the helix, was continually shortened, until the whole of it was excluded from the circuit; in this case the intensity of the shock at the beginning was constantly increased. We may therefore state generally, that at the beginning of the battery current, the induction of a unit of its length is increased by every diminution of the length of the conductor.

13. In the experiment given in paragraph 11, the intensity of the shock at the *ending* of the battery current diminishes with each diminution of the length of the coil; and this is also due to the decrease of the number of the spires of the coil, as is evident from an experiment similar to the last, in which the helix was placed on a coil consisting of only two turns or spires of copper ribbon; the shock at the ending, with this arrangement, was comparatively feeble, but could be felt in the hands. Different lengths of coil No. 2 were now introduced into the same circuit, but not so as to act on the helix; but although these were varied from four or five feet to the whole length of the coil, (sixty feet,) not the least difference in the intensity of the shock could be perceived. We have therefore the remarkable result, that the intensity of the ending induction of each unit of length of the battery current is not materially altered, at least within certain limits, by changing the length of the whole conductor. From this we would infer that the shock depends more on the intensity of the action than on the quantity of

the current, since we know that the latter is diminished in a given unit of the conductor by increasing the length of the whole.

14. We have seen (8) that with a circuit composed of ten elements of the compound battery and the coil No. 2, the shock, at the beginning of the current, was fully equal to that at the ending. It was however found that if in this case the length of the coil was increased, this shock was diminished; and we may state as an inference from several experiments, that however great may be the intensity of the electricity from the battery, the shock at the beginning may be so reduced, by a sufficient increase of the length of the primary circuit, as to be scarcely perceptible.

15. It was also found that when the thickness of the coil was increased, the length and intensity of the circuit remaining the same, the shock at the beginning of the battery current was somewhat increased. This result was produced by using a double coil; the electricity was made to pass through one strand, and immediately afterwards through both; the shock from the helix in the latter case was apparently the greater.

16. By the foregoing results we are evidently furnished with two methods of increasing at pleasure the intensity of the induction at the beginning of a battery current;—the one consisting in increasing the intensity of the source of the electricity, and the other in diminishing the resistance to conduction of the circuit while the intensity remains the same.

17. The explanation of the effects which we have given, relative to the induction at the beginning, is apparently not difficult. The resistance to conduction in the case of a long conductor and a battery of a single element is so great that the full development of the primary current may be supposed not to take place with sufficient rapidity to produce the instantaneous action on which the shock from the secondary current would seem to depend. But when a battery of a number of elements is employed, the poles of this, previous to the moment of completing the circuit, are in a state

of electrical tension; and therefore the discharge through the conductor may be supposed to be more sudden, and hence an induction of more intensity is produced.

18. That the shock at both making and breaking the circuit in some way depends on the rapidity of formation and diminution of the current is shown by the following experiment, in which the tension just mentioned does not take place, and in which also the current appears to diminish more slowly. The two ends of the coil were placed in the two cups which formed the poles of the battery, and permanently retained there during the experiment; also, at the distance of about six inches from—say the right hand end of the coil, a loop was made in the ribbon, which could be plunged into the cup containing the left hand end. With this arrangement, and while only the two extreme ends of the coil were in connection with the cups of mercury, of course the current passed through the entire length of the ribbon of the coil; but by plunging the loop into the left hand cup, the whole length of the coil, except the six inches before mentioned, was excluded from the battery circuit. And again, when the loop was lifted out of the cup, the whole length was included. In this way the current in the coil could be suddenly formed and interrupted, while the poles of the battery were continually joined by a conductor, but no shock with either a single or a compound battery could be obtained by this method of operation.

19. The feebleness of the shock at the beginning of the current, with a single battery and a long coil, is not entirely owing to the cause we have stated, (17,) namely the resistance to conduction offered by the long conductor, but also depends in a considerable degree (if not principally) on the adverse influence of the secondary current, induced in the primary conductor itself, as is shown by the result of the following experiment. Helix No. 1 was placed on a coil consisting of only three spires or turns of copper ribbon; with this, the shock both at making and breaking the circuit with a single battery could be felt in the hands. A compound coil was then formed of the copper ribbons of coils No. 3 and 4 rolled

together so that the several spires of the two alternated with each other, and when this was introduced into the circuit so as not to act on the helix by its induction, and the battery current passed through (for example) coil No. 3, the shock at making contact with the pole of the battery was so much reduced as to be imperceptible in the hands, while the shock at breaking the contact was about the same as before this addition was made to the length of the circuit. The ends of coil No. 4 were now joined so as to produce a closed circuit, the induced current in which would neutralize the secondary current in the battery conductor itself; and now the shock at making the contact was nearly as powerful as in the case where the short conductor alone formed the circuit with the battery. Hence the principal cause of the feebleness of the effect at the beginning of the battery current is the adverse action on the helix of the secondary current produced in the conductor of the battery circuit itself. The shock at the breaking of the circuit in this experiment did not appear affected by joining or separating the ends of coil No. 4.

20. Having investigated the conditions on which the inductive action at the beginning of a battery current depends, experiments were next instituted to determine the nature of the effects produced by this induction: and first, the coils were arranged in the manner described in my last paper, (No. III, 79,) for producing currents of the different orders. The result with this arrangement was similar to that which I have described in reference to the ending induction, namely, currents of the third, fourth, and fifth orders were readily obtained.

21. Also, when an arrangement of apparatus was made similar to that described in paragraph 87 of my last paper, it was found that a current of intensity could be induced from one of quantity and the converse.

22. Likewise, the same screening or rather neutralizing effect was produced, when a plate of metal was interposed between two consecutive conductors of the series of currents, as was described (No. III section iv) in reference to the ending

induction. In short, the series of induced currents produced at the beginning of the primary current appeared to possess all the properties belonging to those of the induction at the ending of the same current.

23. I may mention in this place that I have found in the course of these experiments that the neutralizing power of a plate of metal depends in some measure on its superficial extent. Thus a broad plate which extends in every direction beyond the helix and coil, produces a more perfect screening than one of the same metal and of the same thickness, but of a diameter only a little greater than that of the coil.

24. The next step in the investigation was to determine the direction of the currents of the different orders produced by the beginning induction; and for this purpose the magnetizing spirals (5) were used, and the results obtained by these verified by the indications of the galvanometer. It should be stated here, as a fact which was afterwards found of some importance, that although the needle of the galvanometer was powerfully deflected when the instrument was placed in the circuit of the secondary current, yet a very feeble effect was produced on it by the action of a current of the third, fourth, or fifth order. The directions however of these currents, as indicated by the feeble motions of the needle, were the same as those given by the magnetizing spiral.

25. The direction of the different currents produced at the making of the battery current, as determined by these instruments, is as follows, namely: the direction of the secondary current is, as stated by Dr. Faraday, adverse to that of the primary current, and also the direction of each succeeding current is opposite to that of the one which produced it. We have therefore from these results, and those formerly obtained, (No. III, 92,) the following series of directions of currents, one produced at the moment of beginning, and the other at that of ending of the battery current.

	At the Beginning.	At the Ending.
Primary current.....	+	+
Secondary current.....	—	+
Current of the third order.....	+	—
Current of the fourth order.....	—	+
Current of the fifth order.....	+	—

26. These two series, at first sight, may appear very different, but with a little attention, they will be seen to be of the same nature. If we allow that the induction at the ending of a galvanic battery should be opposite to that at the beginning of the same, then the sign at the top of the second column may be called minus instead of plus, and we shall have the second series — + — + alternating precisely like the first.

27. In connection with the results given in the last two paragraphs, it is due to Mr. Sturgeon that I should state, that in a letter addressed to me and published in the *Annals of Electricity*, he has predicted from his theory, that I would find on examination the series of alternation of currents for the beginning induction which I have here given. I may however here add, it appears to me that this result might have been predicted without reference to any theory. There was no reason to suppose the induction at the beginning would be different in its nature from that at the ending, and therefore the series which would be produced from the former might be immediately inferred from that belonging to the latter, by recollecting that the direction of the induction at the beginning should be opposite to that at the ending. I do not wish it to be supposed however from this remark, that I had myself drawn any inference from my experiments as to the alternations of currents which might be produced by the beginning induction; the truth is, that this action was so feeble with the arrangement of apparatus I employed, that I supposed it could not produce a series of currents of the different orders.

28. In the course of the experiments given in this section, I have found that a shock can be produced without using a coil, by arranging about ten elements of the battery in the form of a circle, and placing the helix within this. The

shock was felt in the hands at the moment of closing the circuit, but the effect at opening the same was scarcely perceptible through the tongue. An attempt was also made to get indications of induction by placing the helix within a circle of dilute acid, connected with a battery instead of a coil, but the effect if any was very feeble.

29. I have shown, in the second number of my Contributions, that if the body be introduced into a circuit with a battery of one hundred and twenty elements, without a coil, a thrilling sensation will be felt during the continuance of the current, and a shock will be experienced at the moment of interrupting the current by breaking the circuit at any point. This result is evidently due to the induction of a secondary current in the battery itself, and on this principle the remarkable physiological effects produced by Dr. Ure, on the body of a malefactor, may be explained. The body, in these experiments, was made to form a part of the circuit, with a compound galvanic apparatus in which a series of interruptions was rapidly made by drawing the end of a conductor over the edges of the plates of the battery. By this operation a series of induced currents must have been produced in the battery itself, the intensity of which was greater than that of the primary current.

30. In this connection I may mention that the idea has occurred to me that the intense shocks given by the electrical fish may possibly be from a secondary current, and that the great amount of nervous organization found in these animals may serve the purpose of a long conductor.* It appears to me, that in the present state of knowledge, this is the only way in which we can conceive of electricity so intense being produced in organs imperfectly insulated and immersed in a conducting medium. But we have seen that an original current of feeble intensity can induce, in a long wire, a secondary current capable of giving intense shocks, although the several strands of the wire are separated from each other only by a covering of cotton thread. Whatever

*Since writing the above, I have found that M. Masson has suggested the same idea, in an interesting thesis lately published.

may be the worth of this suggestion, the secondary current affords the means of imitating the phenomena of the shock from the electrical eel, as described by Dr. Faraday. By immersing the apparatus (Fig. 3) in a shallow vessel of water, the handles being placed at the two extremities of the diameter of the helix, and the hands plunged into the water parallel to a line joining the two poles, a shock is felt through the arms; but when the contact with the water is made in a line at right angles to the last, only a slight sensation is felt in each hand, but no shock.

31. Since the publication of my last paper, I have exhibited to my class the experiment (No. III, Sec. III) relative to the induction at a distance on a much larger scale. All my coils were united so as to form a single length of conductor of about four hundred feet, and this was rolled into a ring of five and a half feet in diameter, and suspended vertically against the inside of the large folding doors which separate the laboratory from the lecture room. On the other side of the doors, in the lecture room, and directly opposite the coil, was placed a helix, formed of upwards of a mile of copper wire, one sixteenth of an inch in thickness, and wound into a hoop of four feet in diameter. With this arrangement, and a battery of one hundred and forty-seven square feet of zinc surface divided into eight elements, shocks were perceptible in the tongue, when the two conductors were separated, to the distance of nearly seven feet; at the distance of between three and four feet, the shocks were quite severe. The exhibition was rendered more interesting by causing the induction to take place through a number of persons standing in a row between the two conductors.

SECTION II.

On apparently two kinds of Electro-dynamic Induction.

32. The investigations arranged under this head had their origin in the following circumstances. After the publication of my last paper, I received, through the kindness of Dr. Faraday, a copy of the fourteenth series of his Researches, and in this I was surprised to find a statement which ap-

peared in direct opposition to one of the principal facts of my communication. In paragraph 59, I state in substance that when a plate of metal is interposed between the coil transmitting a galvanic current, and the helix placed above it to receive the induction, the shock from the secondary current is almost perfectly neutralized. Dr. Faraday, in the extension of his new and ingenious views of the agency of the intermediate particles in transmitting induction, was led to make an experiment on the same point, and apparently, under the same circumstances, he found that it "makes not the least difference whether the intervening space between the two conductors is occupied by such insulating bodies as air, sulphur, and shell-lac, or such conducting bodies as copper and other non-magnetic metals."

33. As the investigation of the fact mentioned above forms an important part of my paper, and is intimately connected with almost all the phenomena subsequently described in the communication I was of course anxious to discover the cause of so remarkable a discrepancy. There could be no doubt of the truth of my results, since a shock from a secondary current which would paralyze the arms was so much reduced by the interposition of plates of metal as scarcely to be felt through the tongue.

34. After some reflection however the thought occurred to me that induction might be produced in such a way as not to be affected by the interposition of a plate of metal. To understand this, suppose the end of a magnetic bar placed perpendicularly under the middle of a plate of copper, and a helix suddenly brought down on this; an induced current would be produced in the helix by its motion towards the plate, since the copper, in this case, could not screen the magnetic influence. Now, if we substitute for the magnet a coil through which a galvanic current is passing, the effect should be the same. The experiment was tried by attaching the ends of the helix to a galvanometer,* and the

*The arrangement will be readily understood by supposing in Fig. 3, the handles removed, and the ends of the helix joined to the ends of the wire of a galvanometer; also, by a plate of metal interposed between the helix and the coil.

result was as I expected: when the coil was suddenly brought down on the plate the needle swung in one direction and when lifted up, in the other; the amount of deflection being the same, whether the plate was interposed or not.

35. It must be observed in this experiment, that the plate was at rest, and consequently did not partake of the induction produced by the motion of the helix. From my previous investigations, I was led to conclude that a different result would follow, were a current also generated in the plate by simultaneously moving it up and down with the helix. This conclusion however was not correct, for on making the experiment, I found that the needle was just as much affected when the plate was put in motion with the helix as when the latter alone was moved.

36. This result was so unexpected and remarkable, that it was considered necessary to repeat and vary the experiment in several ways. First, a coil was interposed instead of the plate, but whether the coil was at rest or in motion with the helix, with its ends separated or joined, the effect on the galvanometer was still the same; not the least screening influence could be observed. In reference to the use of the coil in this experiment, it will be recollected that I have found this article to produce a more perfect neutralization than a plate.

37. Next, the apparatus remaining the same, and the helix at rest during the experiment, currents were induced in it by moving the battery attached to the coil up and down in the acid. But in this case as in the others the effect on the galvanometer was the same, whether the plate or the coil was interposed or not.

38. The experiment was also tried with magneto-electricity. For this purpose, about forty feet of copper wire, covered with silk, were wound around a short cylinder of stiff paper, and into this was inserted a hollow cylinder of sheet copper, and into this again, a short rod of soft iron; when the latter was rendered magnetic, by suddenly bringing in contact with its two ends the different poles of two magnets, a current was of course generated in the wire, and this as before

was found to affect the galvanometer to the same degree, when the copper cylinder was interposed, as when nothing but the paper intervened.

39. The last experiment was also varied by wrapping two copper wires of equal length around the middle of the keeper of a horse-shoe magnet, leaving the ends of the inner one projecting, and those of the outer attached to a galvanometer. A current was generated in each by moving the keeper on the ends of the magnet, but the effect on the galvanometer was not in the least diminished by joining the ends of the inner wire.

40. At first sight, it might appear that all these results are at variance with those detailed in my last paper, relative to the effect of interposed coils and plates of metal. But it will be observed that in all the experiments just given, the induced currents are not the same as those described in my last communication. They are all produced by motion, and have an appreciable duration, which continues as long as the motion exists. They are also of low intensity, and thus far I have not been able to get shocks by any arrangement of apparatus from currents of this kind. On the other hand, the currents produced at the moment of *suddenly* making or breaking a galvanic current, are of considerable intensity, and exist but for an instant. From these and other facts presently to be mentioned, I was led to suppose that there are two kinds of electro-dynamic induction; one of which can be neutralized by the interposition of a metallic plate between the conductors, and the other not.

41. In reference to this surmise, it became important to examine again all the phenomena of induction at suddenly making and breaking a galvanic current.* And in connection with this part of the subject, I will first mention a fact which was observed in the course of the experiments given in the last section, on the direction of the induced currents of different orders. It was found that though the indications of the galvanometer were the same as those of the spiral, in reference to the direction of the induced currents,

*See my last paper. (No. III.)

yet they were very different in regard to the intensity of the action. Thus, when the arrangement of the apparatus was such that the induction at making the battery circuit was so feeble as not to give the least magnetism to the needle, and so powerful at the ending as to magnetize it to saturation, the indication of the galvanometer was the same in both cases.

42. Also, similar results were obtained in comparing the shock and the deflection of the galvanometer. In one experiment for example the shock was so feeble at making contact that it could scarcely be perceived in the fingers, but so powerful at the breaking of the circuit as to be felt in the breast; yet the galvanometer was deflected about thirty-five degrees to the right, at the beginning of the current, and only an equal number of degrees to the left, at the ending of the same.

43. In another experiment, the apparatus being the same as before, the magnetizing spiral and the galvanometer were both at once introduced into the circuit of the helix. A sewing needle being placed in the spiral, and the contact with the battery made, the needle showed no signs of magnetism, although the galvanometer was deflected thirty degrees. The needle being replaced, and the battery circuit broken, it was now found strongly magnetized, while the galvanometer was moved only about as much as before in the opposite direction.

44. Also, effects similar to those described in the last two paragraphs were produced when the apparatus was so arranged as to cause the induction at the beginning of the battery current to predominate. In this case the galvanometer was still almost equally affected at making and breaking battery contact, or any difference which was observed could be referred to a variation in the power of the battery during the experiment.

45. Another fact of importance belonging to the same class has been mentioned before, (24,) namely, that the actions of the currents of the third, fourth, and fifth orders produce a very small effect on the galvanometer, compared with that

of the secondary current; and this is not on account of the diminishing power alone of the successive inductions, as will be evident from the following experiment: By raising the helix from the coil, in the arrangement of the apparatus for the secondary current, the shock was so diminished as to be inferior to one produced by the arrangement for a tertiary current, yet while with the secondary current the needle was deflected twenty-five degrees, with the tertiary it moved scarcely more than one degree; and with the currents of the fourth and fifth orders the deflections were still less, resembling the effect of a slight impulse given to the end of the needle.

46. With the light obtained from the foregoing experiments, I was the more fully persuaded that some new and interesting results might be obtained by a re-examination of my former experiments, on the phenomena of the interposed plate of metal, in the case where the induction was produced by making and breaking the circuit with a cup of mercury; and in this I was not disappointed. The coil (Fig. 3) being connected with a battery of ten elements, the shocks, both at making and breaking the circuit, were very severe; and these as usual were almost entirely neutralized by the interposition of a zinc plate. But when the galvanometer was introduced into the circuit instead of the body, its indications were the same whether the plate was interposed or not; or in other words the galvanometer indicated no screening, while, under the same circumstances, the shocks were neutralized.

47. A similar effect was observed when the galvanometer and the magnetizing spiral were together introduced into the circuit. The interposition of the plate entirely neutralized the magnetizing power of the spiral, in reference to tempered steel, while the deflections of the galvanometer were unaffected.

48. In order to increase the number of facts belonging to this class, the last experiments were varied in several ways; and first, instead of the hard steel needle, one of soft iron wire was placed in the spiral, with a small quantity of iron filings almost in contact with one of its ends. The plate

being interposed, the small particles of iron were attracted by the end of the needle, indicating a feeble, temporary development of magnetism. Hence the current which moves the needle, and is not neutralized by the interposed plate, also feebly magnetizes soft iron, but not hard steel.

49. Again, the arrangement of apparatus being as in paragraph 46, instead of a plate of zinc, one of cast iron, of about the same superficial dimensions, but nearly half an inch thick, was interposed; with this, the magnetizing power of the spiral, in reference to tempered steel, was neutralized; and also the action of the galvanometer was much diminished.

50. Another result was obtained by placing in the circuit of the helix, (Fig. 3,) at the same time, the galvanometer, the spiral, and a drop of distilled water; with these the magnetizing power of the spiral was the same as without the water, but the deflection of the galvanometer was reduced from ten to about four degrees. In addition to these the body was also introduced into the same circuit; the shocks were found very severe, the spiral magnetized needles strongly, but the galvanometer was still less moved than before. The current of low intensity, which deflects the needle of the galvanometer in these instances, was partially intercepted by the imperfect conduction of the water and the body.

51. To exhibit the results of these experiments with still more precision, an arrangement of apparatus was adopted similar to that used by Dr. Faraday, and described in the fourteenth series of his Researches, namely, a double galvanometer was formed of two separate wires of equal length and thickness, and wound together on the same frame; and also a double magnetizing spiral was prepared by winding two equal wires around the same piece of hollow straw. Coil No. 1, connected with the battery, was supported perpendicularly on a table, and coils Nos. 3 and 4 were placed parallel to this, one on each side, to receive the induction, the ends of these being so joined with those of the galvanometer and the spiral that the induced current from the one

coil would pass through the two instruments, in an opposite direction to that of the current from the other coil. The two outside coils were then so adjusted, by moving them to and from the middle coil, that the induced currents perfectly neutralized each other in the two instruments, and the needle of the galvanometer and that in the spiral were both unaffected when the circuit of the battery was made and broken. With this delicate arrangement the slightest difference in the action of the two currents would be rendered perceptible; but when a zinc plate was introduced so as to screen one of the coils, the needle of the galvanometer still remained perfectly stationary, indicating not the least action of the plate, while the needle in the spiral became powerfully magnetic. When however a plate of iron was interposed instead of the one of zinc, the needle of the galvanometer was also affected.

52. From the foregoing results it would seem that the secondary current, produced at the moment of the sudden beginning or ending of a galvanic current, by making and breaking contact with a cup of mercury, consists of two parts, which possess different properties. One of these is of low intensity, can be interrupted by a drop of water, does *not* magnetize hardened steel needles, and is *not* screened by the interposition of a plate of any metal, except iron, between the conductors. The other part is of considerable intensity, is *not* intercepted by a drop of water, develops the magnetism of hardened steel, gives shocks, and *is* screened or neutralized by a closed coil, or a plate of any kind of metal. Also, the induced current produced by moving a conductor towards or from a battery current, and that produced by the movement up and down of a battery in the acid, are of the nature of the first mentioned part, while the currents of the third, fourth, and fifth orders partake almost exclusively of the properties of the second part.*

* [The above paper was reprinted in Silliman's American Journal of Science, April, 1841, vol. xli, pp. 117-152. Also, in Sturgeon's Annals of Electricity, etc., vol. vii, pp. 21-56. Also, in the London and Edinburgh Philosophical Magazine, June, 1841, vol. xviii, pp. 482-514.]

Postscript.

53. The principal facts and conclusions of this section were announced to the Society in October, 1839, and again, in June last, presented in the form in which they are here detailed. Since then however I have had leisure to examine the subject more attentively, and after a careful comparison of these results with those before given, I have obtained the more definite views of the phenomena which are given in the following section.

SECTION III.

Theoretical Considerations relating to the Phenomena described in this and the preceding Communications.

Read November 20, 1840.

54. The experiments given in number III of my Contributions were merely arranged under different heads, and only such inferences drawn from them as could be immediately deduced without reference to a general explanation. The addition however which I have since made to the number of facts, affords the means of a wider generalization; and after an attentive consideration of all the results given in this and the preceding papers, I have come to the conclusion that they can all be referred to the simple laws of the induction at the beginning and the ending of a galvanic current.

55. In the course of these investigations the limited hypotheses which I have adopted have been continually modified by the development of new facts, and therefore my present views, with the further extension of the subject, may also require important corrections. But I am induced to believe, from its exact accordance with all the facts, so far as they have been compared, that if the explanation I now venture to give be not absolutely true, it is so at least in approximation, and will therefore be of some importance in the way of suggesting new forms of experiment, or as a first step towards a more perfect generalization.

56. To render the laws of induction at the beginning and the ending of a galvanic current more readily applicable to

the explanation of the phenomena, they may be stated as follows: 1. During the time a galvanic current is increasing in quantity in a conductor, it induces, or tends to induce, a current in an adjoining parallel conductor in an opposite direction to itself. 2. During the continuance of the primary current in full quantity, no inductive action is exerted. 3. But when the same current begins to decline in quantity, and during the whole time of its diminishing, an induced current is produced in an opposite direction to the induced current at the beginning of the primary current.

57. In addition to these laws, I must frequently refer to the fact, that *when the same quantity of electricity in a current of short duration is passed through a galvanometer, the deflecting force on the needle is the same, whatever be the intensity of the electricity.* By intensity is here understood the ratio of a given quantity of force to the time in which it is expended;* and according to this view, the proposition stated is an evident inference from dynamic principles. But it does not rest on considerations of this kind alone, since it has been proved experimentally by Dr. Faraday, in the third series of his Researches.

58. In order to form a definite conception of the several conditions of the complex phenomena which we are about to investigate, I have adopted the method often employed in physical inquiries, of representing the varying elements of action by the different parts of a curve. This artifice has been of much assistance to me in studying the subject, and without the use of it at present, I could scarcely hope to present my views in an intelligible manner to the Society.

59. After making these preliminary statements, we will now proceed to consider the several phenomena; and first, let us take the case in which the induction is most obviously produced in accordance with the laws as above stated (56), namely, by immersing a battery into the acid, and also by withdrawing it from the same. During the time of the descent of the battery into the liquid, the conductor connected with it is constantly receiving additional quantities

*Or, more correctly speaking, the ratio of two quantities of the same species, representing the force and time.

62. The sum of the several increments of the battery current, up to its full development, will be expressed by the ordinate $c B$, and this will therefore also represent the whole amount of inductive action exerted in one direction at the beginning of the primary current; and, for the same reason, the equal ordinate, $C d$, will represent the whole induction in the other direction at the ending of the same current. Also, the whole time of continuance of the inductive action at the beginning and ending will be represented by $A c$ and $d D$.

63. If we suppose the battery to be plunged into the acid to the same depth, but more rapidly than before, then the time represented by $A c$ will be diminished, while the whole amount of inductive force expended remains the same; hence, since the same quantity of force is exerted in a less time, a greater intensity of action will be produced (57), and consequently a current of more intensity, but of less duration, will be generated in the secondary conductor. The intensity of the induced currents will therefore evidently be expressed by the ratio of the ordinate $c B$ to the abscissa $A c$. Or, in more general and definite terms, the intensity of the inductive action at any moment of time will be represented by the ratio of the rate of increase of the ordinate to that of the abscissa for that moment.*

64. It is evident from the last paragraph, that the greater or less intensity of the inductive action will be immediately presented to the eye by the greater or less obliquity of the several parts of the curve to the axis. Thus, if the battery be suddenly plunged into the acid for a short distance, and then gradually immersed through the remainder of the depth, the varying action will be exhibited at once by the form of $A B$, the first part of the curve, Fig. 17. The steepness of the part $A g$ will indicate an intense action for a

* According to the differential notation, the intensity will be expressed by $\frac{dy}{dx}$. In some cases the effect may be proportional to the intensity multiplied by the quantity, and this will be expressed by $\frac{dy^2}{dx}$, x and y representing as usual the variable abscissa and ordinate.

short time $A a$, while the part $g B$ denotes a more feeble induction during the time represented by $a c$. In the same way, by drawing up the battery suddenly at first, and afterwards slowly, we may produce an inductive action such as would be represented by the parts between C and D of the ending of the curve.

65. Having thus obtained representations of the different elements of action, we are now prepared to apply these to the phenomena. And first, however varied may be the intensity of the induction expressed by the different parts of the two ends of the curve, we may immediately infer that a galvanometer, placed in the circuit of the secondary conductor, will be equally affected at the beginning and ending of the primary current; for, since the deflection of this instrument is due to the whole amount of a current, whatever may be its intensity (57), and since the ordinates $c B$ and $C d$, which represent the quantity of induction in the two directions, are equal, and consequently the amount of the secondary current, therefore the deflection at the beginning and ending of the battery current will in all cases be equal. This inference is in strict accordance with the results of experiment; for however rapidly or slowly we may plunge the battery into the acid, and however irregular may be the rate at which it is drawn out, still, if the whole effect be produced within the time of one swing of the needle, the galvanometer is deflected to an equal degree.

66. Again, the intensity of one part of the inductive action, for example that represented by $A g$, may be supposed to be so great as to produce a secondary current capable of penetrating the body, and of thus producing a shock* while the other parts of the action, represented by $g B$ and $C D$, are so feeble as to effect the galvanometer only. We would then have a result the same as one of those given in the last section (42), and which was supposed to be produced by two kinds of induction; for if the shock were referred to as the test of the existence of an induced current, one would be

* The shock depends more on the intensity than on the quantity. See paragraph 13.

found at the beginning only of the battery current, while, if the galvanometer were consulted, we would perceive the effects of a current as powerful at the ending as at the beginning.

67. The results mentioned in the last paragraph cannot be obtained by plunging a battery into the acid; the formation of the current in this way is not sufficiently rapid to produce a shock. The example was given to illustrate the manner in which the same effect is supposed to be produced, in the case of the more sudden formation of a current, by plunging one end of a conductor into a cup of mercury permanently attached to a battery already in the acid, and in full operation. The current in this case—rapid as may be its development, cannot be supposed to assume *per saltum* its maximum state of quantity; on the contrary, from the general law of continuity, we would infer that it passes through all the intermediate states of quantity, from that of no current, (if the expression may be allowed,) to one of full development; there are however considerations of an experimental nature which would lead us to the same conclusion, (18,) (90,) and also to the further inference that the *decline* of the current is not instantaneous. According to this view therefore the inductive action at the beginning and the ending of a primary current, of which the formation and interruption are effected by means of the contact with a cup of mercury, may also be represented by the several parts of the curve, Fig. 17.

68. We have now to consider how the rate of increase or diminution of the current, in the case in question, can be altered by a change in the different parts of the apparatus; and first, let us take the example of a single battery and a short conductor, making only one or two turns around the helix; with this arrangement a feeble shock, as we have seen, (11,) will be felt at the making, and also at the breaking of the circuit. In this case it would seem that almost the only impediment to the most rapid development of the current would be the resistance of the metal to conduction; and this we might suppose would be more rapidly overcome

by increasing the tension of the electricity; and accordingly we find that if the number of elements of the battery be increased, the shock at making the circuit will also be increased, while that at breaking the circuit will remain nearly the same. To explain however this effect more minutely, we must call to mind the fact before referred to, (17,) that when the poles of a compound battery are not connected, the apparatus acquires an accumulation of electricity, which is discharged at the first moment of contact, and which in this case would more rapidly develop the full current, and hence produce the more intense action on the helix at making the circuit.

69. The shock, and also the deflection of the needle, at breaking the circuit with a compound battery and a short coil, (9,) appear nearly the same as with a battery of a single element, because the accumulation just mentioned, in the compound battery, is discharged almost instantly, and according to the theory (71) of the galvanic current, leaves the constant current in the conductor nearly in the same state of quantity as that which would be produced by a battery of a single element; and hence the conditions of the ending of the current are the same in both cases. Indeed, in reference to the ending induction, it may be assumed as a fact which is in accordance with all the experiments, (9, 13, 73, 74, 75, 76, &c.,) as well as with theoretical considerations,* *that when the circuit is broken by a cup of mercury, the rate of the diminution of the current, within certain limits, remains the same, however the intensity of the electricity or the length of the conductor may be varied.*

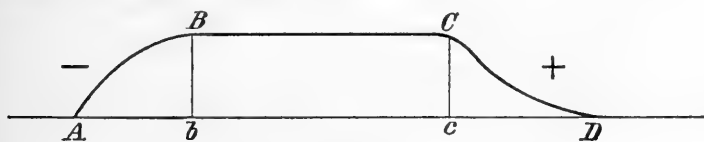


FIG. 18.

70. The several conditions of the foregoing examples are exhibited by the parts of the curves, Figs. 18 and 19. The

* Adopting here the theory of Ohm.

gradual development of the current in the short conductor, with a single battery, and the gradual decline of the same, are represented by the gentle rise of AB and fall of CD , Fig. 18; while, in the next Fig., (19,) the sudden rise of AB indicates the intensity which produces the increased shock, after the number of elements of the battery has been increased. The accumulation of the electricity, which almost instantly subsides, is represented by the part Bce ,



FIG. 19.

Fig. 19, and from this we see, at once, that although the shock is increased by using the compound battery, yet the needle of the galvanometer will be deflected only to the same number of degrees, since the parts Bc and ce give inductive actions in contrary directions, and both within the time of the single swing of the needle, and consequently they will neutralize each other. The resulting deflecting force will therefore be represented by ef , which is equal to Ck , or to bB , in Fig. 19. The intensity of the shock at the breaking is represented as being the same in the two figures, by the similarity of the rate of descent of the part CD of the curve in each.

71. We have said (69) that the quantity of current electricity in a short conductor and a compound battery, after the first discharge, is nearly the same as with a single battery. The exact quantity, according to the theory of Ohm, in a unit of length of the conductor, is given by the formula—

$$\frac{nA}{rn + R}.$$

In this, n represents the number of elements; A , the electro-motive force of one element; r , the resistance to conduction of one element; and R , the length of the conductor, or rather, its resistance to conduction in terms of r . Now when R is very small in reference to rn , as is the case with a very

short metallic conductor, it may be neglected, and then the expression becomes

$$\frac{n A}{r n} \text{ or } \frac{A}{r};$$

and since this expresses the quantity of current electricity in a unit of the length of the circuit, with either a single or a compound battery, therefore with a short conductor the quantity of current electricity in the two cases is nearly the same.

72. Let us next return to the experiment with a battery of a single element, (68,) and instead of increasing the intensity of the apparatus, as in the last example, let the length of the conductor be increased; then the intensity of the shock at the beginning of the current, as we have seen, (14,) will be diminished, while that of the one at the ending will be increased. That the shock should be lessened at the beginning, by increasing the length of the conductor, is not surprising, since as we might suppose, the increased resistance to conduction would diminish the rapidity of the development of the current. But the secondary current, which is produced in the conductor of the primary current itself, as we have seen, (19,) is the principal cause which lessens the intensity of the shock; and the effect of this, as will be shown hereafter, may also be inferred from the principles we have adopted.

73. The explanation of the increased shock at the moment of breaking the circuit with the long conductor, rests on the assumption before mentioned, (69,) that the velocity of the diminution of a current is nearly the same in the case of a long conductor as in that of a short one. But to understand the application of this principle more minutely, we must refer to the change which takes place in the quantity of the current in the conductor by varying its length; and this will be given by another application of the formula before stated, (71.) This, in the case of a single battery, in which n equals unity, becomes

$$\frac{A}{r + R};$$

and since this, as will be recollected, represents the quantity

of current electricity in a unit of length of the conductor, we readily infer from it that by increasing the length of the conductor, or the value of R , the quantity of current in a unit of the length is lessened. And if the resistance of a unit of the length of the conductor were very great in comparison with that of r , (the resistance of one element of the battery,) then the formula would become

$$\frac{A}{R},$$

or the quantity in a single unit of the conductor would be inversely as its entire length, and hence the amount of current electricity in the whole conductor would be a constant quantity, whatever might be its length. This however can never be the case in any of our experiments, since in no instance is the resistance of R very great in reference to r , and, therefore, according to the formula, (73,) the whole quantity of current electricity in a long conductor is always somewhat greater than in a short one.

74. Let us however in order to simplify the conditions of the induction at the ending of a current, suppose that the quantity in a unit of the conductor is inversely at its whole length, or in other words that the quantity of current electricity is the same in a long conductor as in a short one; and let us also suppose for an example that the length of the spiral conductor, (Fig. 3,) was increased from one spire to twenty spires; then, if the velocity of the diminution of the section of the current is the same (69) in the long conductor as in the short one, the shock which would be received by submitting the helix to the action of one spire of the long coil would be nearly of the same intensity as that from one spire of the short conductor; the quantity of induction however as shown by the galvanometer, should be nearly twenty times less; and these inferences I have found in accordance with the results of experiments, (75.) If however instead of placing the helix on one spire of the long conductor, it be submitted at once to the influence of all the twenty spires, then the intensity of the shock should be twenty times greater, since twenty times the quantity of

current electricity collapses (if we may be allowed the expression) in the same time, and exerts at once all its influence on the helix. If in addition to this we add the consideration that the whole quantity of current electricity in a long conductor is greater than that in a short one, (73,) we shall have a further reason for the increase of the terminal shock, when we increase the length of the battery conductor.

75. The inference given in the last paragraph relative to the change in the quantity of the induction, but not in the intensity of the shock from a single spire, by increasing the whole length of the conductor, is shown to be true by repeating the experiment described in paragraph 13. In this, as we have seen, the intensity of the shock remained the same, although the length of the circuit was increased by the addition of coil No. 2. When however the galvanometer was employed in the same arrangement, the whole quantity of induction, as indicated by the deflection of the needle, was diminished almost in proportion to the increased length of the circuit. I was led to make this addition to the experiment (13) by my present views.

76. The explanation given in paragraph 74 also includes that of the peculiar action of a long conductor, either coiled or extended, in giving shocks and sparks from a battery of a single element, discovered by myself in 1832; (see No. II.) The induction in this case takes place in the conductor of the primary current itself, and the secondary current which is produced is generated by the joint action of each unit of the length of the primary current. Let us suppose for illustration that the conductor was at first one foot long, and afterwards increased to twenty feet. In the first case, because the short conductor would transmit a greater quantity of electricity, the secondary current produced by it would be one of considerable quantity, or power to deflect a galvanometer; but it would be of feeble intensity, for although the primary current would collapse with its usual velocity, (69,) yet, acting on only a foot of conducting matter, the effect (74) would be feeble. In the second case, each foot of the twenty feet of the primary current would severally

produce an inductive action of the same intensity as that of the short conductor, the velocity of collapse being the same; and as they are all at once exerted on the same conductor, a secondary current would result of twenty times the intensity of the current in the former case.

77. To render this explanation more explicit, it may be proper to mention that a current produced by an induction on one part of a long conductor of uniform diameter, must exist of the same intensity in every other part of the conductor; hence, the action of the several units of length of the primary current must re-enforce each other, and produce the same effect on its own conductor that the same current would if it were in a coil, and acting on a helix. I need scarcely add, that in this case, as in that given in paragraph 74, the whole amount of induction is greater with the long conductor than with the short one, because the quantity of current electricity is greater in the former than in the latter.

78. We may next consider the character of the secondary current, in reference to its action in producing a tertiary current in a third conductor. The secondary current consists (as we may suppose) in the disturbance for an instant of the natural electricity of the metal, which subsiding leaves the conductor again in its natural state; and whether it is produced by the beginning or ending of a primary current, its nature, as we have seen, (22,) is the same. Although the time of continuance of the secondary current is very short, still we must suppose it to have some duration, and that it increases, by degrees, to a state of maximum development, and then diminishes to the normal condition of the metal of the conductor; the velocity of its development, like that of the primary current, will depend on the intensity of the action by which it is generated, and also perhaps in some degree, on the resistance of the conductor; while, agreeably to the hypothesis we have assumed, (69,) the velocity of its diminution is nearly a constant quantity, and is not affected by changes in these conditions; hence, if we suppose the induction which produces the secondary current to be sufficiently intense, the velocity of its development will ex-

ceed that of its diminution, as in the example of the primary current from the intense source of the compound battery of many elements. Now this is the case with the inductions which produce currents of the different orders, capable of giving shocks or of magnetizing steel needles; the secondary currents from these are always of considerable intensity, and hence their rate of development must be greater than that of their diminution, and, consequently, they may be represented by a curve of the form exhibited in Fig. 20, in which

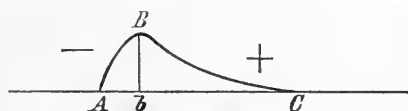


FIG. 20.

there is no constant part, and in which the steepness of $A B$ is greater than that of $B C$. There are however other considerations, which will be noticed hereafter, (89,) which may affect the form of the part $B C$ of the curve, rendering it still more gradual in its descent, or in other words which tend to diminish the intensity of the ending induction of the secondary current.

79. It will be seen at once, by an inspection of the curve, that the effect produced in a third conductor, and which we have called a tertiary current, is not of the same nature as that of a secondary current. Instead of being a single development in one direction, it consists of two instantaneous currents, one produced by the induction of $A B$, and the other by that of $B C$, in opposite directions, of equal quantities, but of different intensities. The whole quantity of induction in the two directions, will each be represented by the ordinate $B b$, and hence they will nearly neutralize each other, in reference to their action on the galvanometer, in the circuit of the third conductor. I say they will *nearly* neutralize each other, because, although they are equal in quantity, they do not both act in absolutely the same moment of time. The needle will therefore be slightly affected; it will be impelled in one direction—say to the right, by the induction of $A B$, but, before it can get fairly under

way, it will be arrested, and turned in the other direction, by the action of $B C$. This inference is in strict accordance with observation; the needle, as we have seen, (24,) starts from a state of rest, with a velocity which apparently would send it through a large arc, but before it has reached perhaps more than half a degree, it suddenly stops, and turns in the other direction. As the needle is first affected by the action of $A B$, it indicates a current in the adverse direction to the secondary current.

80. Although the two inductions in the tertiary conductor nearly neutralize each other, in reference to the indications of the galvanometer, yet this is far from being the case with regard to the shocks, and the magnetization of steel needles. These effects may be considered as the results alone of the action of $A B$; the induction of $B C$ being too feeble in intensity to produce a tertiary current of sufficient power to penetrate the body, or overcome the coercive power of the hardened steel. Hence, in reference to the shock, and magnetization of the steel needle, we may entirely neglect the action of $B C$, and consider the tertiary excitement as a single current, produced by the action $A B$; and because this is the beginning induction, (56,) the tertiary current must be in an opposite direction to the secondary. For a similar reason, a current of the third order should produce in effect a single current of the fourth order, in a direction opposite to that of the current which produced it, and so on; we have here therefore a simple explanation of the extraordinary phenomenon of the alternation of the directions of the currents of the different orders, as given in this and the preceding paper. (See paragraph 25.)

81. The operation of the interposed plate, (32, 47, 48, &c.,) in neutralizing the shock, and not affecting the galvanometer, can also be readily referred to the same principles. It is certain, that an induced current is produced in the plate (No. III, 64,) and that this must re-act on the secondary, in the helix; but it should not alter the total amount of this current, since for example at the ending induction, the same quantity of current is added to the helix while the current

in the plate is decreasing, as is subtracted while the same current is increasing. To make this more clear, let the inductive actions of the interposed current be represented by the parts of the curve, Fig. 20. The induction represented by $A B$ will re-act on the current in the helix, and diminish its quantity, by an amount represented by the ordinate $b B$; but the induction represented by $B C$, will act in the next moment, on the same current, and increase its quantity by an equal amount, as represented by the same ordinate $B b$; and since both actions take place within a small part of the time of a single swing of the needle, the whole deflection will not be altered, and consequently, as far as the galvanometer is concerned, the interposition of the plate will have no perceptible effect.

82. But the effect of the plate on the shock, and on the magnetization of tempered steel, should be very different; for, although the quantity of induction in the helix may not be changed, yet its intensity may be so reduced, by the adverse action of the interposed current, as to fall below that degree which enables it to penetrate the body, or overcome the coercive force of the steel. To understand how this may be, let us again refer for example to the induction which takes place at the ending of a battery current; this will produce, in both the helix and the plate, a momentary current in the direction of the primary current, which we have called *plus*; the current in the plate will re-act on the helix, and tend to produce in it two inductions, which as before may be represented by $A B$, and $B C$, of the curve, Fig. 20; the first of these, $A B$, will be an intense action, (78,) in the *minus* direction, and will therefore tend to neutralize the intense action of the primary current on the helix; the second, ($B C$), will add to the helix an equal quantity of induced current, but of a much more feeble intensity, and hence the resulting current in the helix will not be able to penetrate the body; no shock will be perceived, or at least a very slight one, and the phenomena of screening will be exhibited.

83. When the plate of metal is placed between the con-

ductors of the second and third orders, or between those of the third and fourth, the action is somewhat different, although the general principle is the same. Let us suppose the plate interposed between the second and third conductors; then the helix, or third conductor, will be acted on by four inductions, two from the secondary current and two from the current in the plate. The direction and character of these will be as follows, on the supposition that the direction of the secondary current is itself *plus*:

The beginning secondary -----	intense and -----	<i>minus.</i>
The ending secondary -----	feeble and -----	<i>plus.</i>
The beginning interposed -----	intense and -----	<i>plus.</i>
The ending interposed -----	feeble and -----	<i>minus.</i>

Now if the action, on the third conductor, of the first and third of the above inductions be equal in intensity and quantity, they will neutralize each other; and the same will also take place with the action of the second and fourth, if they be equal, and hence in this case, neither shock nor motion of the needle of the galvanometer would be produced. If these inductions be not precisely equal, then only a partial neutralization will take place, and the shock will be merely diminished in power; and also the needle will perhaps be very slightly affected.

84. If in the foregoing exposition we throw out of consideration the actions of the feeble currents which cannot pass the body, and which consequently are not concerned in producing the shock, then the same explanation will still apply which was given in the last paper, (No. III, 94,) namely, in the above example, the helix is acted on by the minus influence of the secondary, and the plus influence of the interposed current.

85. We are now prepared to consider the effect on the helix (Fig. 3,) of the induced currents produced in the conductor of the primary current itself. These are true secondary currents, and are almost precisely the same in their action as those in the interposed plate. Let us first examine the induced currents at the beginning of the primary, in the case of a long coil and a battery of a single element.

Its action on the helix may be represented by the parts of the curve, Fig. 20. The first part, *A B*, will produce an intense induction opposite to that of the primary current; and hence the action of the two will tend to neutralize each other, and no shock, or a very feeble one, will be produced. The ending action of the same induced current, which is represented by *B D*, restores to the helix the same quantity of current electricity (but in a feeble state) which was neutralized by *A B*, and hence the needle of the galvanometer will be as much affected as if this current did not exist. These inferences perfectly agree with the experiment given in paragraph 19. In this, when the ends of the interposed coil were joined so as to neutralize the induced current in the long conductor, the shock at the beginning of the primary current was nearly as powerful as with a short conductor, while the amount of deflection of the galvanometer was unaffected by joining the ends of the same coil.

86. At first sight it might appear that any change in the apparatus which may tend to increase the induction of the primary current (16) would also tend to increase in the same degree the adverse secondary in the same conductor; and that hence the neutralization mentioned in the last paragraph would take place in all cases; but we must recollect that if a more full current be suddenly formed in a conductor of a given thickness, the adverse current will not have as much space as it were for its development, and therefore will have less power in neutralizing the induction of the primary than before. But there is another and perhaps a better reason, in the consideration that in the case of the increase of the number of elements of the battery, although the rapidity of the development of the primary current is greater, yet the increased resistance which the secondary meets with, in its motion against the action of the several elements, will tend to diminish its effect. Also by diminishing the length of the primary current, we must diminish (76) the intensity of the secondary, so that it will meet with more resistance in passing the acid of the single battery, and thus its effects be diminished.

87. The action of the secondary current in the long coil at the *ending* of the primary current, should also at first sight produce the same screening influence as the current in the interposed plate; but on reflection it will be perceived that its action in this respect must be much more feeble than that of the similar current at the beginning; the latter is produced at the moment of making contact, and hence it is propagated in a continuous circuit of conducting matter, while the other takes place at the *rupture* of the circuit, and must therefore be rendered comparatively feeble by being obliged to pass through a small portion of heated air; very little effect is therefore produced on the helix by this induction, (19.) The fact that this current is capable of giving intense shocks, when the ends of a long wire which is transmitting a primary current, are grasped at the time of breaking the circuit, is readily explained, since in this case the body forms with the conductor a closed circuit, which permits the comparatively free circulation of the induced current.

88. It will be seen that I have given a peculiar form to the beginning and ending of the curves, Figs. 17, 18, &c. These are intended to represent the variations which may be supposed to take place in the rate of increase and decrease of the quantity of the current, even in the case where the contact is made and broken with mercury. We may suppose, from the existence of analogous phenomena in magnetism, heat, &c., that the development of the current would be more rapid at first than when it approximates what may be called the state of current saturation, or when the current has reached more nearly the limit of capacity of conduction of the metal. Also, the decline of the current may be supposed to be more rapid at the first moment, than after it has lost somewhat of its intensity, or sunk more nearly to its normal state. These variations are indicated by the rapid rise of the curve, Fig. 17, from *A* to *g*, and the more gradual increase of the ordinates from *h* to *B*; and by the rapid diminution of the ordinates between *C* and *l*, and the gradual decrease of those towards the end of the curve.

89. These more minute considerations, relative to the form of the curve, will enable us to conceive, how the time of the ending of the secondary current, as we have suggested, (78,) may be prolonged beyond that of the natural subsidence of the disturbance of the electricity of the conductor on which this current depends. If the development of the primary current is produced by equal increments in equal times, as would be the case in plunging the battery (59) into the acid with a uniform velocity, then the part AB of the curve Fig. 17 would be a straight line, and the resulting secondary current, after the first instant, would be one of constant quantity during nearly the whole time represented by Ac ; but if the rate of the development of the primary current be supposed to vary in accordance with the views we have given in the last paragraph, then the quantity of the secondary current will begin to decline before the termination of the induction, or as soon as the increments of the primary begin to diminish; and hence the whole time of the subsidence of the secondary will be prolonged, or the length of bC , Fig. 20, will be increased, the descent of BC be more gradual, and the intensity of the ending induction of the secondary current be diminished, (see last part of paragraph 78.)

90. Besides the considerations we have mentioned, (88,) there are others of a more obvious character, which would also appear to affect the form of particular parts of the curve. And first we might perhaps make a slight correction in the drawing of Figs. 17, 18, &c., at the point A , in consideration of the fact that the very first contact of the end of the conductor with the surface of the mercury is formed by a point of the metal, and hence the increment of development should be a little less rapid at the first moment than after the contact has become larger; or in other words, the curve should perhaps start a little less abruptly from the axis at the point A . Also, Dr. Page has stated* that he finds the shock increased by spreading a stratum of oil over the surface of the mercury; in this case it is probable that the ter-

*Silliman's American Journal of Science.

mination of the current is more sudden, on account of the prevention of the combustion of the metal by means of the oil, and the fact that the end of the conductor is drawn up into a non-conducting medium.

91. The time of the subsidence of the current, when the circuit is broken by means of a surface of mercury, is very small, and probably does not exceed the ten-thousandth part of a second, but even this is an appreciable duration, since I find that the spark at the ending presents the appearance of a band of light of considerable length, when viewed in a mirror revolving at the rate of six hundred times in a second; and I think the variations in the time of the ending of a current under different conditions may be detected by means of this instrument.

92. Before concluding this communication, I should state that I have made a number of attempts to verify the suggestion given in my last paper, (No. III, 127,) that an inverse induction is produced by a galvanic current by a change in the distance of the conductors, but without success. These attempts were made before I had adopted the views given in this section, and since then I have found (80) a more simple explanation of the alternation of the currents.

93. In this number of my Contributions, the phenomena exhibited by the galvanic apparatus have alone been discussed. I have however made a series of experiments on the induction from ordinary electricity, and the re-action of soft iron on currents; and I think that the results of these can also be referred to the simple principles adopted in this paper; but they require further examination before being submitted to the public.

ON A RECIPROCATING MOTION PRODUCED BY GALVANIC ATTRACTION AND REPULSION.

(Proceedings of the American Philosophical Society, vol. I, p. 301.)

November 20, 1840.

Prof. Henry described an apparatus for producing a reciprocating motion by the repulsion in the consecutive parts of a conductor, through which a galvanic current is passing, and made some remarks in reference to the electro-magnetic engine invented by him in 1831,* and subsequently described by Dr. Ritchie, of London. The machine referred to had been applied recently by Prof. Henry in his experiments.

ON THE EVOLUTION OF ELECTRICITY FROM STEAM, ETC.

(Proceedings of the American Philosophical Society, vol. I, pp. 322, 324.)

December 18, 1840.

[Dr. Patterson called the attention of the Society to the subject of the evolution of electricity from steam, mentioned at the last meeting, and stated that the experiments made lately in England had been successfully repeated by Mr. Peale, Mr. Saxton, and himself, at the United States Mint. - - - - He thought it most probable that the electricity, in these experiments, was evolved by the condensation of the steam. - - - -]

Prof. Henry stated that he had not seen the sparks from steam; but that he had obtained feeble electricity from a small ball, partly filled with water, and heated by a lamp. He agreed with Dr. Patterson in the opinion that the source of the electricity was the change of state,—but from water to vapor. There was however some doubt on the subject. Pouillet had denied the evolution of electricity from the evaporation of pure water. The facts were interesting, particularly on account of the great intensity of the electricity.

*[Silliman's American Journal of Science, July, 1831, vol. xx, p. 340.—*Ante*, page 54.]

The results obtained by the philosophers, which had been mentioned, indicated electricity of very feeble tension, which could only be observed by the most delicate instruments, but here the sparks were an inch in length.

If the vaporization of the water were shown to be the source of the electricity, Prof. Henry thought the phenomena might be readily explained by the beautiful theory of Becquerel, in regard to the production of the great intensity of the electricity in the thunder cloud. According to this theory, each particle of the vapor carries up with it into the atmosphere the free electricity which it receives at the moment of the change of state: this being diffused through the whole capacity of the air is of very feeble intensity although of great quantity; but the condensation of the vapor in a cloud affords a continuous conductor, and consequently the electricity of all the particles of the interior, according to the well known principles of distribution, rushes to the surface of the cloud, and hence the great intensity of the lightning. Agreeably with this hypothesis, the insulated conductor, placed in the steam, would act not only as a collector, but also as a condenser of the free but feeble electricity of the vapor.

Prof. Henry further stated, in relation to this subject, that he had been informed by several persons, that they had obtained sparks of electricity from a coal stove during the combustion of anthracite. A case had been stated to him several years ago, which he mentioned to his friend Professor Bache, who informed him that a similar one had fallen under his own notice, in which however Prof. Bache had succeeded in tracing the electricity to the silk shirt of the person who drew the spark. Another case had lately been reported to him by an intelligent gentleman, of a stove burning bituminous coal on board of a steamboat on the Ohio, which afforded amusement to all the passengers, during the voyage, by giving sparks of electricity whenever it was touched.

In connection with the facts that had been stated of the production of electricity from steam, Prof. Henry observed that he was now inclined to believe that electricity may also

be evolved during the combustion of coal in a stove. But what (he asked) is the source of electricity in this case? Is it combustion, the evaporation of the moisture, or the friction of the hot air on the interior of the pipe?

EXPERIMENTS ON PHOSPHORESCENCE.

(Proceedings of the American Philosophical Society, vol. II, p. 46.)*

April 16, 1841.

Professor Henry mentioned that he had recently repeated some experiments of Becquerel and Biot on phosphorescence, the results of which demonstrate the existence of an emanation from incandescent bodies, particularly when in an electrical state, of a character not heretofore known. He promised to give a more full account of these at a future meeting of the Society.

[*The title-page of vol. II, (comprising the proceedings from Jan., 1841, to May, 1843,) bears date 1844.]

ON A SIMPLE FORM OF HELIOSTAT.

(Proceedings of the American Philosophical Society, vol. II, pp. 97, 98.)

September 17, 1841.

Professor Henry exhibited to the Society a simple form of the Heliostat, or instrument for throwing a stationary beam of light into a darkened room.

He stated that this article of apparatus, which is indispensable in delicate experiments on light, is in its usual form a very complex instrument and consequently very expensive, while the one to which the attention of the Society was directed is very simple, and cost scarcely more than the tenth part of the price of one of the old form.

It was made in accordance with the plan given by Dr. Thomas Young in the first volume of his *Lectures on Natural Philosophy*, which consists in reflecting a beam of light into the room in a line parallel to the axis of the earth, and then causing it to retain this direction by giving the reflector a rotary motion equal to the apparent motion of the sun. The instrument consists of a flat block of mahogany, about nine inches long and five inches wide, on which is placed in an inclined position, the wheel-work of a common pocket watch. This serves to give rotary motion to a brass wheel of about five inches diameter, which is so geared into the large wheel of the watch as to make one turn in twenty-four hours. The axis of this wheel is a steel rod, carrying on its upper end a small mirror, which can be set in any position by means of a universal joint. The watch-work and the wheel are attached to the mahogany block by a hinge, so that the axis of the wheel can be inclined to the horizon at an angle precisely equal to the latitude of the place where the instrument is to be used.

The adjustment of the instrument is very simple. It is placed on the outside of the window, with the axis of the wheel parallel to the axis of the earth; a meridian line having been traced on the window-sill for this purpose.

The mirror is then set so that the beam of light is thrown into the room in a line forming the prolongation of the axis of the wheel, which is readily effected by means of a mark previously made on the opposite wall. The beam will preserve this direction during the day, since the mirror and the sun revolve with the same velocity, and are therefore comparatively at rest. The only motion of the beam in reference to terrestrial objects is one of rotation on its own axis. If the required direction of the beam is different from that of the first reflection, a second mirror is used.

Professor Henry's object in exhibiting this article to the Society, was to render this simple contrivance more generally known in our country. He stated that the original invention probably belongs to Dr. Young; that it was at least published by him in 1807, although an account of the same instrument is given in the *London Philosophical Magazine* for 1833, as a new invention by Mr. Potter. The details of the instrument exhibited differ from those proposed by Mr. Potter, in the addition of a hinge and clamp-screw, by which the axis may be adjusted to the angle of the latitude. The instrument was constructed by an ingenious watch-maker at Princeton; and its whole cost, including the watch-work, was but sixteen dollars.

ON THE EFFECTS OF A THUNDER-STORM.

(Proceedings of the American Philosophical Society, vol. II, p. 111-116.)

November 5, 1841.

Professor Henry gave an account of some observations he had made on the effects of a thunder-storm which visited Princeton on the evening of the 14th of July, 1841.

Storms of this kind (he said) are not very frequent at Princeton; but two severe ones have passed immediately over the place within the last nine years, and the lightning has struck but twice in the village during that time. It is thought by some of the inhabitants that damage by lightning was more frequent some years ago than it has been

lately; and the idea has been suggested that the water of the canal, which passes to the south of the place, may have had some effect in determining the course of the cloud. Be this as it may, the thunder-storm generally comes from the south-west, and before it reaches the village it usually divides into two parts, one of which passes along the edge of the Rocky Hill, and the other along the valley of Stonybrook, so that the principal part of the storm seldom passes immediately over the village; and when it does thus pass it is generally at a great elevation, and the thunder is not so loud as that which the observer has been in the habit of hearing at the north. In connection with this remark, Professor Henry mentioned that he has several times observed the lightning assume a beautiful violet color, similar to that of the vapor of iodine, and this was particularly the case during a storm which occurred during the 12th of April, 1840. On this occasion, although the cloud and the flashes appeared directly overhead, yet the sound of the thunder seemed to come from a distance. The peculiar color may perhaps receive a sufficient explanation by referring it to the fact of the discharge taking place at a great altitude, and consequently in comparatively rarified air, as in the case of the color exhibited by the spark through a vessel partially exhausted.

The storm of the evening of the 14th of July, was said to be more severe than any which had visited Princeton for twenty years before. It commenced between 7 and 8 o'clock, and lasted about three hours: the thunder was almost continuous, but except in two or three cases it was not very near. Several buildings and other objects were struck in the vicinity of Princeton; and also Mrs. Hamilton's house, which is situated in the village, about twenty rods west of the college, on the opposite side of the way. It seemed a little surprising that this house should be singled out, since the buildings on either side are considerably higher, although at a few rods distance, and in front of the one to the west is a number of tall trees. The house is also furnished with a lightning rod; but this, like most of the rods erected in the country, is not formed in accordance with the most scientific

principles. The front of Mrs. Hamilton's house is parallel with the main street, and is nearly in an east and west direction. The building is of brick, with a shingle roof, and is two stories high; it has on the front, three upper windows, and two windows and a door below; the latter being immediately under the western upper window. The chimney is on the eastern end, and the lightning conductor is supported against this. The rod is formed of round iron, three-eighths of an inch thick, and the several parts of it are imperfectly connected by hooks and eyes. It appears to be merely thrust into the ground to the depth of about two feet, and is terminated above by three prongs instead of one, the points of which are blunted by long exposure, but do not exhibit any appearance of fusion. The top of the rod is not more than six feet above the ridge of the roof; and since the house is about thirty feet long, the farther end of the ridge is unprotected. A point, according to the experiments of Mr. Charles, can only protect a circular space, the radius of which is not greater than twice the height of the point above the plane to be protected.

The lightning, according to the accounts of several persons, came from a cloud situated to the southwest, and the discharge did not strike the most elevated part of the building, but the western end of the horizontal wooden gutter which extends along the front of the house under the eaves. This point is at the greatest possible distance from the extremity of the lightning rod, and perhaps was as near to the cloud as any other part of the building. The discharge immediately divided itself into two parts: one of these, and probably the larger, passed along the gutter, which must have been filled with water at the time, to the eastern end of the same, and then down to the earth along an ordinary tinned iron pipe or conductor, which conveys the water from the gutter to the pavement below. Marks of its passage were observed along the gutter, and particularly near the end next the metallic conductor. The other part of the discharge passed immediately downward through the end of the gutter which first received the shock, to the casing of the window

below ; and was probably thus deflected out of its course by the attraction of the iron hinges and bolts of the shutters. Its course to the ground was further traced along the casings on each side of the front door. The wood was cracked at every place where a nail happened to be in the line of the discharge, and at some places the lightning appeared merely to pass along the surface making a groove in the wood of about one-eighth of an inch in width, and six or seven inches long ; several of these grooves were observed on the side casings of the door. Three panes of glass were broken in the window above the door, and the pieces were thrown inward. The entrance within the door was filled with dust, and a strong sulphurous odor was preceptible for an hour or more after. No marks of a discharge were found at the foot of the lightning rod.

During the storm, several women were alone in the house, and at the time it was struck three of these were in the front room in the second story, and consequently near the line of the discharge along the gutter. Two of them were on a bed placed against the partition wall, opposite to the front, and the third one was standing on the floor about eight feet from the front window, with her face to the same. Those on the bed were unaffected ; but the one on the floor stated that she felt a sensation on her right ear, as if it had been touched with a live coal ; at the same time she felt a rushing sensation down her side and perceived a flash at her foot, and a forked spark in the air between her and the nearest window. One of the persons on the bed also stated that she saw the forked spark in the air, and that the one standing on the floor appeared to her for an instant as if surrounded with light. The outside shutters of the window opposite to which she was standing, were closed, and also one leaf of the shutters of the window farther east. The western window, or that from which the glass was broken, was not in the same room, but in a small adjoining one, over the main entrance from the front door. The chamber door was shut at the time, and no marks of the entrance of the electricity into the room could be found on the walls or on the casings of the two windows.

The principal facts here detailed, although perhaps not unusual occurrences, afford interesting illustrations of the action of electrical induction. First, the horizontal gutter and the vertical tin pipe, both filled with water, formed a long continuous electrical conductor, extending from the point where the lightning first struck to the lower farther corner of the front of the house; and this conductor, on account of its length, would be intensely affected by the induction of the distant cloud, or rather by that of the approaching discharge. If the electricity of the cloud, were positive, then that of the water in the nearest end of the gutter would be negative, and consequently a powerful attraction would determine the lightning on the point where it struck. The house, under these circumstances, might have been damaged even had the rod been much higher than it was, and its connection with the earth much more perfect.

Again, the phenomena exhibited to the women in the upper chamber were also most probably due to inductive action. After a proper allowance for imperfect observation, occasioned by the fright and confusion of the moment, it is still evident that the one on the floor was in some degree affected by the discharge, although none of the electricity of the cloud actually entered the room, since no traces of it were to be found on the walls or other parts. The effects may therefore be referred to the inductive action of the lightning at a distance and through the wall as it passed along the gutter across the front of the house. When a shock of electricity from a Leyden jar is passed through a slip of tinfoil pasted on one side of a pane of glass, the hand on the other side will receive a slight sensation from the lateral induction through the glass. In the same way, it may be supposed that the effects perceived by these persons were due to the disturbance for an instant of the natural electricity of the chamber by the passage of a large charge along the outside of the house.

The discharge, as has before been stated, came from the southwest, and in its passage it crossed obliquely some houses on the opposite side of the street. In one of these, two persons

were sensibly affected by the shock ; and another, in a room with the windows closed, according to her own statement, saw sparks of electricity on the floor. The same explanation will also apply to these effects.

During the same storm another house,* about three miles southwest of the village, was struck, and this also was furnished with an imperfect conductor. The upper part of the rod had been broken, and it hung down, so that no part was above the chimney. The lightning struck the eastern chimney, which was on the end of the house opposite to that to which the rod was attached, and passed down the inside of the flue to the kitchen fire-place, in which wood was burning at the time. It threw down a great quantity of soot, filled the lower rooms with smoke, and diffused, according to the account, a strong smell of gunpowder.

A part of the charge passed to the outside through the thick stone wall which forms the back of the chimney, and was evidently attracted by the iron hoop of a large cask which was nearly against the wall. It made a triangular hole, as if the stone and mortar had been burst outwards by an explosive force, and this was directly opposite the nearest part of the hoop. It then descended along the cask to the ground, breaking off all the wooden hoops in its course, while those of iron were undisturbed. The house is about sixty feet long ; and from the state of the rod the greater part of this distance might be considered as unprotected. The stroke fell on the end most remote from the approaching storm, and probably the lightning was drawn to this chimney rather than the other on account of the heated air which was escaping from it at the time.

Effects were also produced in this case which can only be explained on the principles of induction. Three persons, the man of the house, his wife, and son, all took refuge on a bed in a room separated from that through which the chimney passes, and upwards of twenty feet from the line of the electrical discharge. They were all lying across the bed, with their feet hanging down the side, and they each received

* The dwelling-house of Mr. Henry Philip.

a shock in the knees and lower joints of the legs. The wife stated that the feeling was precisely like that which she had experienced from a shock from an electrical jar. No marks of the entrance of any part of the discharge from the cloud were found on the plastering or any other parts of the room; the effect can therefore only be accounted for by a sudden disturbance of the equilibrium of the natural electricity of the space within the room.

The induction of an electrical cloud is often exerted at an astonishing distance. It has long been known that a delicate gold-leaf electrometer is sometimes affected by the presence of an electrical cloud immediately overhead; but Dr. Ellet, professor of chemistry in the college of South Carolina, has informed him that if one of Dr. Hare's single-leaf electrometers be furnished with a pointed metal rod attached to the cap, and then placed on the sill of an open window in the upper story, the leaf will be seen to touch the ball at the moment of a flash, although the lightning is several miles distant.

CONTRIBUTIONS TO ELECTRICITY AND MAGNETISM. No. V.

ON INDUCTION FROM ORDINARY ELECTRICITY; AND ON THE
OSCILLATORY DISCHARGE.*

(Proceedings of the American Philosophical Society, vol. II, pp. 193-196.)

June 17, 1842.

Professor Henry presented the record of a series of experiments on induction from ordinary electricity, as the fifth number of his Contributions to Electricity and Magnetism. Of these experiments he gave an oral account, of which the following is the substance.

In the third number of his Contributions he had shown on this subject: 1. That the discharge of a Leyden battery through a conductor, developed in an adjoining parallel conductor an induced current, analogous to that which, under similar circumstances, is produced by a galvanic current. 2. That the direction of the induced current, as indicated by the polarity given to a steel needle, changes its sign with a change of distance of the two conductors, and also with a change in the quantity of the discharge of electricity. 3. That when the induced current is made to act on a third conductor, a second induced current is developed, which can again develop another, and so on through a series of successive inductions. 4. That when a plate of metal is interposed between any two of the consecutive conductors, the induced current is neutralized by the adverse action of a current in the plate.

The direction of the induced currents in all the author's experiments was indicated by the polarity given to steel needles enclosed in a spiral, the wire of which formed a part of the circuit. But some doubts were reasonably entertained of the true indications of the direction of a current by this means, since M. Savary had announced in 1826, that when several needles are placed at different distances above a

* [The full Memoir was not printed in the "Transactions of the Am. Philosophical Society."]

wire through which the discharge of a Leyden battery is passed, they are magnetized in different directions, and that by constantly increasing the discharge through a spiral, several reversions of the polarity of the contained needles are obtained.

It was therefore very important before attempting further advances in the discovery of the laws of the phenomena, that the results obtained by M. Savary should be carefully studied; and accordingly the first experiments of the new series relate to the repetition of them. The author first attempted to obtain them by using needles of a larger size, Nos. 3, and 4, such as he had generally employed in all his previous experiments; but although nearly a thousand needles were magnetized in the course of the experiments, he did not succeed in getting a single change in the polarity. The needles were always magnetized in a direction conformable to the direction of the electrical discharge. When however very fine needles were employed he did obtain several changes in the polarity in the case of the spiral, by merely increasing the quantity of the electricity, while the direction of the discharge remained the same.

This anomaly which has remained so long unexplained, and which at first sight appears at variance with all our theoretical ideas of the connection of electricity and magnetism, was after considerable study satisfactorily referred by the author to an action of the discharge of the Leyden jar which had never before been recognized. The discharge, whatever may be its nature, is not correctly represented (employing for simplicity the theory of Franklin) by the single transfer of an imponderable fluid from one side of the jar to the other; the phenomena require us to admit *the existence of a principal discharge in one direction, and then several reflex actions backward and forward, each more feeble than the preceding, until the equilibrium is obtained.* All the facts are shown to be in accordance with this hypothesis, and a ready explanation is afforded by it of a number of phenomena which are to be found in the older works on electricity, but which have until this time remained unexplained.

The same action is evidently connected with the induction of a current on its own conductor, in the case of an open circuit, such as that of the Leyden jar, in which the two ends of the conductor are separated by the thickness of the glass. And hence, if an induced current could be produced in this case, one should also be obtained in that of a second conductor, the ends of which are separated; and this was detected by attaching to the ends of the open circuit a quantity of insulated metal, or by connecting one end with the earth.

The next part of the research relates to a new examination of the phenomena of the change in the direction of the induced currents, with a change of distance, &c. These are shown to be due to the fact that the discharge from a jar does not produce a single induced current in one direction, but several successive currents in opposite directions. The effect on the needle is principally produced by two of these: the first is the more powerful, and in the adverse direction with that of the jar; the second is less powerful, and in the same direction with that of the jar. To explain the change of polarity, let us suppose the capacity of the needle to receive magnetism to be represented by ± 10 , while the power of the first induced current to produce magnetism is represented by -15 , and that of the second by $+12$; then the needle will be magnetized to saturation or to -10 , by the first induced current, and immediately afterwards all this magnetism will be neutralized by the adverse second induction, and a power of $+2$ will remain; so that the polarity of the needle in this case will indicate an induced current in the same direction as that of the jar. Next, let the conductors be so far separated, or the charge so much diminished, that the power of the first current to develop magnetism may be reduced to -8 , while that of the second current is reduced to $+6$, the magnetic capacity of the needle remaining the same. It is evident then that the first current will magnetize the needle to -8 , and that the second current will immediately afterwards neutralize 6 of this, and consequently the needle will retain a magnetism of -2 , or will indicate an induced current in an opposite direction to that of the jar.

In extending the researches relative to this part of the investigations, a remarkable result was obtained in regard to the distance at which inductive effects are produced by a very small quantity of electricity; a single spark from the prime conductor of the machine, of about an inch long, thrown on the end of a circuit of wire in an upper room, produced an induction sufficiently powerful to magnetize needles in a parallel circuit of wire placed in the cellar beneath, at a perpendicular distance of thirty feet with two floors and ceilings, each fourteen inches thick, intervening. The author is disposed to adopt the hypothesis of an electrical *plenum*, and from the foregoing experiment it would appear that the transfer of a single spark is sufficient to disturb perceptibly the electricity of space throughout at least a cube of 400,000 feet of capacity; and when it is considered that the magnetism of the needle is the result of the difference of two actions, it may be further inferred that the diffusion of motion in this case is almost comparable with that of a spark from a flint and steel in the case of light.

The author next alludes to a proposition which he advanced in the second number of his Contributions, namely, that the phenomena of dynamic induction may be referred to the known electrical laws, as given by the common theories of electricity; and he gives a number of experiments to illustrate the connection between statical and dynamical induction.

The last part of the series of experiments relates to induced currents from atmospheric electricity. By a very simple arrangement, needles are strongly magnetized in the author's study, even when the flash is at the distance of seven or eight miles, and when the thunder is scarcely audible. On this principle, he proposes a simple self-registering electrometer, connected with an elevated exploring rod.

EXPERIMENTS ON PHOSPHORESCENCE.

(Proceedings of the American Philosophical Society, vol. III, page 38-44.)*

May 26, 1843.

Professor Henry presented a communication "On Phosphorogenic Emanation," and illustrated by numerous diagrams the experiments which he had made on the subject.

It has long been known, that when the diamond is exposed to the direct light of the sun, and then removed to a dark place, it shines with a pale bluish light, which has received the name of phosphorescence. The effect is not peculiar to the diamond, but is common to a long list of substances, among which the sulphuret of lime (Homberg's phosphorus) is the most prominent. It is also an old fact, mentioned by Canton, that the phosphorescence is excited by exposing the substance to the light of the electrical discharge.

About three years ago, M. Becquerel, of the French Institute, repeated the experiment of Canton, and discovered the remarkable fact, that the phosphorescence is excited in a very feeble degree, or not at all, when a plate of glass or mica is interposed between the spark and the sulphuret of lime, although the effect is not apparently diminished when a plate of rock crystal or one of sulphate of lime is similarly interposed. Or in other words he found that substances equally transparent do not equally well transmit the exciting cause of the phosphorescence. Hence the old explanation of the glowing of the diamond, namely, that it is owing to the light which has been absorbed and is again given off in the dark, could no longer be admitted; and Becquerel inferred from his experiments, that the exciting cause of the

* [The title-page of vol. III, (comprising the proceedings only from May 25 to May 30,) bears date 1843. The proceedings occupying a series of special meetings held during the mornings and evenings, were appointed to commemorate the hundredth anniversary of the American Philosophical Society: and the volume containing them was published in advance of the preceding volume II.]

phosphorescence was due to an impression made on the lime by a radiation from the electrical spark, differing essentially from light, and to which he gave the name of the "phosphorogenic emanation."

Biot afterwards made a series of experiments on the "permeability" of different substances, in reference to this emanation as it exists in the beams of the sun; and still later, Matteucci, the celebrated Italian experimental philosopher, has investigated and extended the same subject. The younger Becquerel also has published a memoir on the constitution of solar spectrum, including its phosphorogenic properties. From the notices of the labors of these *savans* on this subject, as they were adverted to, it appears that all their experiments (with the exception of those before mentioned as made by M. Becquerel) were confined to the solar radiation, and consequently they do not lessen the importance of a careful examination of the properties of the same emanation, as derived from a different source, and having a different intensity.

The investigations detailed in this communication relate almost exclusively to the emanation as derived from the electrical spark. The apparatus employed in the experiments was a Leyden jar, of the capacity of about half a gallon; and this was charged each time, so as to give a spark between the rounded ends of two thick wires of about an inch in length. The sulphuret of lime was exposed to the light of the spark at different distances, in shallow leaden pans. The first experiments relate to an examination of a considerable number of substances, in regard to their permeability by the emanation. The results of these, which were given at the close of the communication, will serve to corroborate the inference of M. Becquerel, that the exciting cause of luminous appearance of the lime is not identical with ordinary light.

The next experiments are in reference to the propagation of this emanation. Two slits, of about the one-twelfth of an inch wide and an inch long, were made in two screens of sheet brass, and these slits were placed in the same plane

with the path of the spark. After the discharge, the sulphuret of lime under the opening was observed to be marked with a narrow line of light well defined at its edges, and shaded off at its ends into a penumbra; the appearance being precisely in accordance with the laws of a radiation in straight lines from a narrow line of emanation.

Experiments were next made to determine whether the radiation of the emanation takes place with the same intensity from every point of the length of the spark, or whether it is confined to the two extremities, or the poles of the discharging wires. For this purpose the slits were turned at right angles to their former position, so that the emanation could only reach the lime from a single point of the spark. The experiments with this arrangement showed that the radiation is from each point of the line of the spark, but that it is much more intense from the two extremities. This curious result was verified by another arrangement, which allowed the impressions from different points of the spark to be at once compared with each other. Three slips were cut in a thick plate of mica, and this was placed immediately above the line, so that one of the slits was directly under the end of each wire, and the other midway between the other two. When the discharge was passed over the plate, the lime under the middle slit exhibited a feeble phosphorescence for two or three seconds, and then became dark, while that under the slits at the end of the spark continued to glow for more than a minute. This effect did not appear to be due to the diffusion of the spark at the middle of its course, since the discharge was from a Leyden jar, and the spark as is usual in this case, appeared as a single line of light, of the same intensity and width throughout its whole length.

The phosphorescence was excited at a much greater distance than was at first thought possible. In a perfectly dark room, the light was observed for a few moments when the pan containing the lime was removed to the distance of ten feet from the point of discharge. The intensity of the light and the time of continuance however diminished very rapidly with an increase of distance.

To determine whether the emanation obeys the laws of the reflection of light, a piece of common looking-glass was so arranged with the path of the spark, the slits in the screens, and the pan of lime, that the angle of reflection could be compared with the angle of incidence: but with this arrangement no impression on the lime could be obtained; the want of permeability in the glass apparently preventing any reflection from the silvered side of the mirror. A plate of polished black glass was next used, so as to get the reflection from the anterior surface: the result however was of the same negative character as before. It would therefore appear, that glass neither reflects nor transmits the phosphorogenic emanation, except in a very small degree. When a metallic mirror was employed, a well defined line of light was impressed on the lime from the reflected emanation, and from the position of this it was found that the two angles were equal.

The refraction and dispersion of the emanation were readily obtained, by employing for the purpose a prism of rock salt, instead of one of glass. The dispersion was shown by the conversion of the narrow line of light, by means of the prism, into a broad band.

The next question was in reference to the polarization of the phosphorogenic emanation; and in obtaining a satisfactory answer to this, several difficulties were encountered. Attempts were first made to polarize the beam by passing it through tourmaline; but it was found that this substance is less permeable to the emanation than even glass or mica. Nicol's polarizing prisms were next employed, but no impression could be made on the lime through two of them; and since the emanation is not reflected by glass, and the polarization from polished metal is very feeble, these substances could not be employed in the process. At length an indirect method was adopted which gave positive results. This was founded on an experiment of Melloni, in his interesting researches on radiant heat. A pile of exceedingly thin plates of mica, prepared according to the method of Professor Forbes, of Edinburgh, was placed between the spark

and the pan containing the lime, with its plane at right angles to the line joining the middle of the two. In this position of the pile, no impression was made on the lime by the electrical discharge; but when the plane of the pile was inclined to the line just mentioned, so as to form with it the polarizing angle, a luminous spot was excited.

By this change of the position of the pile, the thickness of the path to be traversed by the phosphorogenic beam was considerably lengthened; and yet the permeability was much increased. This remarkable result could only be the effect of the successive polarization of the several parts of the beam as they passed the several films of mica, and were thus prepared for a more ready transmission by the succeeding films.

After the emanation was found to be polarizable, it was important to determine if the intensity of the action on the lime would be different, in case the beam were transmitted through crystals in different directions in reference to their optical axis, but no difference could be observed, when the beam was passed through crystals of carbonate of lime, and of quartz—parallel, and perpendicular to the axis.

From the foregoing results it is evident that the exciting cause of the phosphorescence of the sulphuret of lime is an emanation possessing the mechanical properties of light, and yet so different in other respects as to prove the want of identity. That the same emanation also differs from heat is manifest from the fact that the lime becomes as luminous under a plate of alum as under a plate of rock salt, although these substances are almost entirely opposite in their property of transmitting heat.

Some experiments were also made to compare the phosphorogenic emanation with the chemical radiation. For this purpose a sensitive daguerreotype plate and a pan of sulphuret of lime were exposed together to the light of the sky for five seconds. The plate by this exposure was marked with a photographic impression, but little or no effect was produced on the lime. Another sensitive plate and the same pan of lime were similarly exposed to the light of an electrical discharge; the lime was now observed to glow, while

no impression was produced on the plate. When however the plate was exposed very near to a succession of sparks, continued for ten minutes, with a plate of mica interposed, an impression was made.

The sulphuret of lime was also exposed for several minutes to the direct light of the full moon, without any phosphorescent effect. A sensitive plate, similarly exposed, according to the statement of Dr. Draper, receives a photographic impression. These experiments, although not sufficiently extensive, appear to indicate that the phosphorogenic emanation is distinct from the chemical, and that it exists in a much greater quantity in the electrical spark than either the luminous or the chemical emanation.

Professor Henry remarked that in considering these emanations as distinct, he had reference only to the classification of the phenomena, for if they be viewed in accordance with the undulatory hypothesis they may all be considered as the results of waves, differing in length and amplitude, and possibly also slightly differing in the direction of vibration.

The phosphorescence of the lime may also be excited by exposure to the light of a burning coal, and in this case the emanation is also screened by a plate of mica. It was also found that the magneto-electrical spark from a surface of mercury excites the luminous condition of the sulphuret, and it has long been known that heat, applied to the bottom of the vessel containing the article, produces the same effect.

To determine whether the phosphorescence could be excited by electro-dynamic induction, a quantity of the sulphuret was placed between two plates of quartz, and a covered copper wire was wound around the whole, so that the lime occupied the axis of a spiral. But when a discharge of electricity was passed through the wire the lime gave no indications of phosphorescence; the same negative result was also obtained when the sparks were passed through the bottom of the leaden pan.

It has been supposed that the phosphorescence of the lime is due to the disturbance of the electricity of the mass of the substance, and the continuance of the light to the subse-

quently slow restoration of the equilibrium. The result however of the following experiment would seem to be at variance with this explanation. The lime was thrown into a tumbler of water, and sank to the bottom, but in this situation, when the spark was passed over the surface of the liquid, it became as luminous and the effect appeared to remain as long as when the exposure took place in the air.

The author stated that some of the experiments described by him can be repeated with common chalk, although it is not as sensitive as the sulphuret of lime. Some pieces of it however become luminous at a considerable distance, and it is not improbable that the chalk cliffs of England are sometimes rendered phosphorescent by flashes of lightning during a thunder storm.

But the substance which gives the most brilliant light, although the light does not continue so long, and is not as easily excited as that from the lime, is the sulphate of potassa. When exposed to the discharge of a jar highly charged, at the distance of a few inches below the spark, it glows for a few seconds with a beautiful azure light; and as this salt is not readily acted on by liquids, it was used to determine the permeability of different substances, by placing a crystal of the salt in the liquid to be tested.

It has long been known that the sulphate of potassa often emits flashes of light during the progress of its crystallization; and it is probable that other substances, which are known to emit light under the same circumstances, may also be rendered phosphorescent at a distance by the electrical emanation.

The following is a list of the substances which have been examined by Professor Henry, with reference to their permeability by the phosphorogenic emanation :

TRANSPARENT SOLIDS.

Permeable.

Ice,	Sulphate of baryta,
Sulphate of lime,	Sulphate of potassa,
Quartz,	Sulphate of soda,

TRANSPARENT SOLIDS. (CONTINUED.)

Permeable.

Borax,	Alum,
Citric acid,	Horn (pellucid),
Rochelle salt,	Wax, do.
Common salt,	

Imperfectly permeable.

Tourmaline,	Tartaric acid,
Mica,	Hyposulphate of soda,
Flint glass,	Copal,
Crown glass,	Camphor.
Saltpetre,	

TRANSPARENT LIQUIDS.

Permeable.

Water,	Sulphate of magnesia,
Solution of alum,	Nitrate of ammonia,
Solution of ammonia,	And all weak solutions.

Imperfectly permeable.

Muriatic acid,	Arsenious acid,
Sulphuric acid,	Ammonia,
Nitric acid,	Spirits of turpentine,
Phosphoric acid,	Alcohol,
Sulphate of zinc,	Ether,
Sulphate of lead,	Oil of aniseed,
Acetate of zinc,	Acetate of lead.

ON A METHOD OF DETERMINING THE VELOCITY OF PROJECTILES.

(Proceedings of the American Philosophical Society, vol. III, pp. 165-167.)*

May 30, 1843.

Professor Henry read a communication "On a New Method of determining the Velocity of Projectiles."

The new method proposed by the author, consists in applying the instantaneous transmission of an electrical action, to determine the time of the passage of the ball between two screens, placed at a short distance from each other, in the path of the projectile. For this purpose the observer is provided with a revolving cylinder, moved by clock-work at the rate of at least ten turns in a second; and of which the convex surface is divided into a hundred equal parts, each part therefore indicating in the revolution the thousandths part of a second. Close to the surface of this cylinder which revolves horizontally, are placed two galvanometers, one at each extremity of a diameter, the needles of these being furnished at one end with a pen for making a dot with printers' ink on the revolving surface.

To give motion to the needles at the proper moment, each galvanometer is made to form a part of the circuit of a galvanic current, which is completed by a long copper wire passing to one of the screens, and crossing it several times, so as to form a grating, through which the ball cannot pass without breaking the wire, and thus stopping the current. During the continuance of the galvanic action, the marking end of the needle is turned from the revolving cylinder a few degrees, and pressed immovably against a "steady pin" by the well known deflecting power of the electrical current; but the moment the current is stopped by the breaking of the long conductor, in the passage of the ball through the screen, the marking end of the needle is projected against the cylinder by the action of a fine spiral spring, similar to

*[Re-printed in Walker's Electrical Magazine, 1845, vol. I, pp. 350-352.]

the hair spring of a watch, coiled around the centre pin which supports the needle, and having an elastic force a little less than the deflecting power of the electrical current. The relative position of the dots thus formed gives the time of the passage of the ball through the space between the screens, and indicates the velocity at this part of the course.

The degree of deflection of the needle can be increased or diminished by turning a screw, which alters the position of the "steady pin," and the tension of the spiral spring can also be changed by an arrangement like that of the regulator of a watch.

In order that the position of the dots on the surface of the cylinder may exactly indicate the required interval of time, it is necessary that the time occupied by each needle in starting from rest and moving across the small arc to strike against the cylinder, should be precisely equal. If this be not the case, then the difference of these times will be the error of the instruments. This must however be exceedingly small, since the whole range of the end of the needle need not be more than the one-twentieth of an inch—and the precise amount of error can readily be determined by experiment.

To adjust the apparatus for use, the galvanometers must be so placed that the two dots may be impressed on the cylinder, diametrically opposite each other when the instrument is at rest. The cylinder being then put in motion, the two circuits of long wire are placed together, so that they can be broken at the same instant by lifting a wire common to both from a cup of mercury. If, after breaking the circuits, the dots are still found in the same relative position, no further adjustment or correction will be required: but if this is not the case, then the springs may be altered until the dots are found in their proper positions; or the difference may be noted, and this constantly applied in each actual experiment as an index error.

To prevent the dot from the first galvanometer being confounded with that from the second, the two instruments are placed one below the other in different horizontal planes.

In order that the pen may not describe a line on the cylinder, re-entering into itself, and thus obliterate the dot first impressed, it may be found necessary to give the cylinder a slow ascending motion, so that a spiral instead of a circle would be marked on its surface. A chronometer for measuring minute portions of time, with a motion of this kind is described in Young's *Natural Philosophy*, vol. I, page 191.

To prevent agitations of the air, the whirling apparatus with the galvanometer may be placed in the vacuum of an air pump; and that part of the conducting wire which crosses the screen may be separated at each crossing, the ends being again united by slightly twisting them together, and the conduction being preserved by proper amalgamation, so that the force necessary to break the circuit may not sensibly lessen the velocity of the ball.

Various other methods may be devised for impressing a mark on the revolving cylinder, at the moment of the rupture of the galvanic current by the passage of the ball through the screen. But the following, which has suggested itself to Professor Henry since the meeting of the Society, and has been communicated by him to the Reporter, may be regarded as among the best. It dispenses with the galvanometers, and produces the mark by a direct electrical action.

A part of the long wire which leads to the screen is coiled around a bundle of soft iron wire; and over this is coiled another long wire, so as to produce an intense secondary current, on the principle of the common coil machine. One extremity of the secondary circuit is connected with the axis of the cylinder, and the other is made to terminate almost in contact with the revolving surface, which in this modification of the instrument is surrounded by a ruled or graduated paper. It is obvious that the secondary current which is induced by the interruption of the primary circuit, will pierce or mark the paper band at the moment of the screen being broken. There is no difficulty in effecting

such a current of sufficient intensity to mark the paper, since in some of his experiments on induction, he has developed one which gave a spark between a point and a surface of nearly a fourth of an inch in length.

The terminal points of the wires from the two screens may be placed very near each other in the same horizontal plane: if then the cylinder revolving horizontally has at the same time a slow ascending motion, the relative position of the dots on the paper will give the number of whole turns and parts of a turn, made by the cylinder while the ball is passing between the two screens. In the same way the terminal points of wires from a number of different pairs of screens may be made to impress their marks on the surface of the same cylinder, and the velocity of the ball at the different points of its path may in this way be determined by a single experiment.

ON THE APPLICATION OF THE THERMO-GALVANOMETER TO
METEOROLOGY, ETC.

(Proceedings of the American Philosophical Society, vol. iv, pp. 22, 23.)*

November 3, 1843.

Professor Henry made an oral communication in regard to the application of Melloni's thermo-electric apparatus to meteorological purposes, and explained a modification of the parts connected with the pile, to which he had been led in the course of his researches. He had found the vapors near the horizon powerful reflectors of heat; but in the case of a distant thunderstorm he had found that the cloud was colder than the adjacent blue space.

* [The title-page of vol. iv, (comprising the proceedings from June, 1843, to December, 1847,) bears date 1847.]

Theory of the discharge of the Leyden jar.

Referring to the theory of the discharge of the Leyden jar, which he had submitted to the Society some time since,* Professor Henry examined some apparent objections to it, resulting from the researches of Matteucci. The effect produced on the galvanometer by the discharge of a battery is due to the retardation of the lesser waves of electricity, a fact which indicates the cause of Matteucci's results, when a card was pierced by the currents induced in a neighboring wire conductor forming an open circuit.

The speaker described several experiments on the direct and return stroke, showing that equilibrium was restored by the same succession of oscillations; large and small needles placed in spirals forming part of an electrical circuit, being magnetized in different directions. The disturbance of the electrical *plenum* by a discharge of electricity was referred to as explanatory of the induction which takes place, and the subject was applied to the explanation of various phenomena; among others, the light appearing in well authenticated cases about persons and objects in the neighborhood of a discharge of lightning in its direct passage, and suggestions were made as to the most effectual mode of protecting powder houses, etc., from the effects of lightning.

Professor Henry examined in the same connection whether currents or ordinary electricity pass actually at the surface, or like galvanic electricity, through the mass of the conductor, and he concluded that the law of conduction developed by Ohm cannot apply to the case of surface passages, as these are indicative of ordinary electricity.

* Contributions No. V, June 17, 1842.

ON THE COHESION OF LIQUIDS.

(Proceedings of the American Philosophical Society, vol. *iv*, pp. 56, 57.)

April 5, 1844.

Professor Henry made a verbal communication relative to "the cohesion of liquids."

He stated that very erroneous ideas are given as to the constitution of matter in the ordinary books on Natural Philosophy. The passage of a body from a solid to a liquid state is generally attributed to the neutralization of the attraction of cohesion by the repulsion of the increased quantity of heat, the liquid being supposed to retain a small portion of its original attraction, which is shown by the force necessary to separate a surface of water from water, in the well known experiment of a plate suspended from a scale beam over a vessel of the liquid. It is however more in accordance with all the phenomena of cohesion to suppose, instead of the attraction of the liquid being neutralized by the heat, that the effect of this agent is merely to neutralize the polarity of the molecules so as to give them perfect freedom of motion around every imaginable axis. The small amount of cohesion (53 grains to the square inch) exhibited in the foregoing experiment, is due, according to the theory of capillarity of Young and Poisson, to the tension of the exterior film of the surface of water drawn up by the elevation of the plate. This film gives way first, and the strain is thrown on an inner film, which in turn is ruptured, and so on until the plate is entirely separated, the whole effect being similar to that of tearing water apart atom by atom.

Reflecting on this subject, he had thought that a more correct idea of the magnitude of the molecular attraction might be obtained by studying the tenacity of a more viscid liquid than water. For this purpose he had recourse to soap-water, and attempted to measure the tenacity of this liquid by means of weighing the quantity of water which adhered to a bubble of this substance just before it burst, and by determining the thickness of the film from an obser-

vation of the color it exhibited in comparison with Newton's scale of thin plates. Although experiments of this kind could only furnish approximate results, yet they showed that the molecular attraction of water for water, instead of being only about 53 grains to the square inch, is really several hundred pounds, and is probably equal to that of the attraction between the molecules of ice. The effect of dissolving the soap in the water is not (as might at first appear,) to increase the molecular attraction, but to diminish the mobility of the molecules, and thus to render the liquid more viscid.

ON THE COHESION OF LIQUIDS. (CONTINUED.)

(Proceedings of the American Philosophical Society, vol. iv, pp. 84, 85.)

May 17, 1844.

Professor Henry made a second communication on the subject of cohesion.

He had prosecuted his experiments on the soap-bubble to a further extent, and had arrived at a number of results which appeared to him of some interest in reference to capillarity, a subject which had given rise to a greater diversity of opinion than any other part of natural philosophy. As an evidence of its present unsettled state, he mentioned the fact that the last edition of the *Encyclopædia Britannica* contained two articles on this subject under different names; one by Dr. Young, and the other by Mr. Ivory, which explain the phenomena on entirely different physical principles.

According to the theory of Young and Poisson, many of the phenomena of liquid cohesion, and all those of capillarity, are due to a contractile force existing at the free surface of the liquid, and which tends in all cases to urge the liquid in the direction of the radius of curvature towards the centre, with a force inversely as this radius. According to this theory the spherical form of a dew drop is not the effect of the attraction of each molecule of the water on every

other, as in the action of gravitation in producing the globular form of the planets, (since the attraction of cohesion extends only to an unappreciable distance,) but is due to the contractile force which tends constantly to enclose the given quantity of water within the smallest surface, namely that of a sphere. Professor Henry finds a contractile force perfectly similar to that assumed by this theory in the surface of the soap-bubble; indeed, the bubble may be considered a drop of water with the internal liquid removed, and its place supplied by air. The spherical form in the two cases is produced by the operation of the same cause. The contractile force in the surface of the bubble is easily shown by blowing a large bubble on the end of a wide tube, say an inch in diameter; as soon as the mouth is removed, the bubble will be seen to diminish rapidly, and at the same time quite a forcible current of air will be blown through the tube against the face. This effect is not due to the ascent of the heated air from the lungs with which the bubble was inflated, for the same effect is produced by inflating with cold air, and also when the bubble is held perpendicularly above the face, so that the current is downward.

Many experiments were made to determine the amount of this force, by blowing a bubble on the larger end of a glass tube in the form of the letter **U**, and partially filled with water; the contractile force of the bubble, transmitted through the enclosed air, forced down the water in the larger leg of the tube, and caused it to rise in the smaller. The difference of level observed by means of a microscope, gave the force in grains per square inch, derived from the known pressure of a given height of water. The thickness of the film of soap water which formed the envelope of the bubble, was estimated as before by the color exhibited just before bursting. The results of these experiments agree with those of weighing the bubble, in giving a great intensity to the molecular attraction of the liquid, equal at least to several hundred pounds to the square inch. Several other methods were employed to measure the tenacity of the film, the general results of which were the same; the

numerical details of these are reserved however until the experiments can be repeated with a more delicate balance.

The comparative cohesion of pure water and soap water was determined by the weight necessary to detach the same plate from each; and in all cases the pure water required the greater force. The want of permanency in the bubble of pure water is therefore not due to feeble attraction, but to the perfect mobility of the molecules, which causes the equilibrium, as in the case of the arch without friction of parts, to be destroyed by the slightest extraneous force.

Several other experiments with films of soap water were also described, which afford striking illustrations of the principles of capillarity, and which apparently have an important bearing on the whole subject of cohesion.*

ON THE ORIGIN AND CLASSIFICATION OF THE NATURAL MOTORS.
(Proceedings of the American Philosophical Society, vol. iv, pp. 127-129.)

December 20, 1844.

Professor Henry made an oral communication in regard to some speculations in which he had indulged relative to the classification and origin of mechanical power.

He stated that he was indebted for the origin of this train of thought to some remarks made by Mr. Babbage in his work on the economy of machinery, and to the late researches of the German and French chemists on the subject of vital chemistry; indeed all the views contained in the communication might perhaps be found in detached portions in different works, but he believed that they had never before been brought together and presented as a whole.

He defined mechanical power to be that which is capable of overcoming a constant resistance, and of producing a continued motion; or in the language of the engineer, it is that which can be employed to "do work." It is here used in a

*[Reprinted in Silliman's *American Journal of Science*, October, 1844. Vol. XLVIII, pp. 215-217. Also in the *London and Edinburgh Philosophical Magazine*, June, 1845. Vol. XXVI, pp. 541-543.]

more restricted sense than force, which is applied as a more general term, to whatever tends to produce or resist motion. The following list of mechanical powers he believed would be found to include all the prime movers employed at the present time, either directly or indirectly, in producing mechanical changes in matter, and all these could be referred to two sources:

Class 1st	{ Water power Tide power Wind power }	Referable to celestial disturbance.
Class 2d	{ Steam and other powers developed by combustion. Animal power. }	Referable to that which is called vital or or- ganic action.

These natural motive principles are not always directly employed in producing work, but are sometimes used to develop other power by disturbing the natural equilibrium of other forces, and in this way they give rise to a class of mechanical motors which may be called intermediate powers. It will be evident on a little reflection that the forces of gravity, cohesion and chemical attraction, with those of the "imponderable" agents of nature, so far as they belong to the earth, all tend to produce a state of stable or permanent equilibrium at the surface of our planet—that in all cases before the energies of these forces can be exhibited, the disturbing effect of some extraneous force is required, hence these principles in themselves are not the primary sources of power, but are merely secondary agents in producing mechanical effects, or in other words it will be found that while the approximate source of every power is the force exerted by matter in its passage from an unstable to a stable state of equilibrium, yet in all cases it may be referred beyond this to a force which disturbed a previously existing quiescence. As an example we may take the case of water power, in which the mechanical effects are approximately due to the return of the water to a state of stable equilibrium on the surface of the ocean, but the cause of the continued motion is the force which produced the original disturbance, and which elevates the liquid in the form of vapor. Also in the

phenomena of combustion the immediate source of the power evolved in the form of heat, is the passage from an unstable state into one of stable combination of the carbon and hydrogen of the fuel and the oxygen of the atmosphere, but this power may ultimately be resolved into the force which caused the separation of these elements from their previous combination in the state of carbonic acid and water.

Now the only forces of any importance which operate at the surface of the earth to counteract the tendency to a general state of stable equilibrium are those derived from two sources, namely, *celestial disturbance*, and what is called *vital action*; and hence all mechanical power, as well as all activity on the surface of the globe, may be referred to these two sources. The only exception to this generalization is the comparatively limited effect of volcanic action, which is a power, from whatever source it may be derived, that must tend to exhaust itself.

Thus far the author considered his conclusions founded on well-established physical law, and perhaps here the true spirit of inductive philosophy would admonish him to stop; but they who are disposed to continue the speculation, and to consider the results of the late researches of the German and French chemists as well-established truths, may extend the generalization so as to reduce all mechanical motion on the surface of the earth to a source from without. Thus, according to Liebig, Dumas and Boussingault, the mechanical power exerted by animals is due to the passage of organized matter in the body from an unstable to a stable equilibrium; and as this matter is derived in an unstable state from vegetables, and the elements of these again from the atmosphere, it would therefore appear to follow, that animal power is referable to the same sources as that from the combustion of fuels, namely, the original force which separates the elements of the plants from their stable and original combination with the oxygen of the atmosphere. But what is this power which furnishes the plant with the material of its growth? Is it due to a constantly created vital power; or since its effects are never directly exhibited but in the presence of light, may

not the opinion of many chemists of the present day be adopted, namely, that it is due to the decomposing energy of the sun's rays, which are found to exhibit a wonderful decomposing effect in cases where no vital phenomena are present.

If this hypothesis be adopted, it must be supposed that vitality is that mysterious principle which propagates a form and arranges the atoms of organizable matter, while the power with which it operates, as well as that developed by the burning fuel and the moving animal, is a separate force, derived from the divellent power of the sunbeam. It is true that this is as yet little more than a mere hypothesis, and as such forms no part of positive science, but it appears to be founded on a clear physical analogy, and may therefore form the basis of definite philosophical research.

ON A PECULIAR ACTION OF FIRE ON IRON NAILS.

(Proceedings of the American Philosophical Society, vol. iv, p. 173.)

June 20, 1845.

[Dr Patterson exhibited a mass of nails melted together at the fire in Pittsburg, presenting a series of united tubes.]

Professor Henry stated that he had received a similar mass from the New York fire, and found that the action of the fire had changed the nails to a certain depth, leaving a core unchanged, which had afterwards fallen or been drawn out, leaving the hollow tube.

OBSERVATIONS ON THE RELATIVE RADIATION OF THE SOLAR SPOTS.

(Proceedings of the American Philosophical Society, vol. iv, pp. 173-176.)

June 20, 1845.

Professor Henry made a verbal communication of a series of experiments made by himself and Professor Alexander, relative to the spots on the sun.

His attention was directed to the subject by an article in the September number of the *Annales de Chimie*, by M. Gautier, upon the influence of the spots on the sun on terrestrial temperature. It is well known that Sir William Herschel entertained the idea that the appearance of solar spots was connected with a more copious emission of heat, and that the seasons during which they were most abundant were most fruitful in vegetable productions, and pursuing this idea he was led to trace an analogy between the price of corn and the number of solar spots during several successive periods. The result of this investigation, so far as it was extended, seemed to favor the views of this distinguished philosopher. A mode of investigation of this kind however is not susceptible of any great degree of accuracy; the price of corn is subject to so many other causes of variation besides that of solar temperature that little reliance can be placed on this condition alone.

M. Gautier has attempted to investigate the influence of solar spots on terrestrial temperature, by comparing the temperature of several places on the earth's surface during the years in which the spots were most abundant with those in which the smallest number were perceptible. From all the observations collected it seems to be indicated that during the years in which the spots were the greatest in number the heat had been a trifle less; but the results are far from being sufficiently definite to settle the question; and M. Gautier remarks that a greater number of years of observation at a greater number of stations will be necessary to establish a permanent connection between these phenomena.

The idea occurred to Professor Henry that much interesting information relative to the sun might be derived from the application of a thermo-electric apparatus to a picture of the solar disc produced by a telescope on a screen in a dark room. This idea was communicated to Professor Alexander, who readily joined in the plan for reducing it to practice. It was agreed that they should first attempt to settle the question of the relative heat of the spots as compared with the surrounding luminous portions of the sun's disc. The first experiments were made on the 4th of January, 1845. Mr. Alexander had observed a few days previous a very large spot, more than 10,000 miles in diameter, near the middle of the disc. To produce the image of this spot a telescope of four inches aperture and four and a half feet focus was placed in the window of a dark room with a screen behind it, on which the image of the spot was received. The instrument was placed behind the screen with the end slightly projecting through a hole made for the purpose, and a small motion of the telescope was sufficient to throw the image of the spot off or on the end of the pile. The spot was very clearly defined, and might have been readily daguerretyped had the telescope been furnished with an equatorial movement. The form of the penumbra of the spot as it appeared on the screen was that of an irregular oblong about two inches in one direction and an inch and a half in the other. The dark central spot within the penumbra

was nearly square, of about three-fourths of an inch on the side, and a little larger than the end of the thermo-pile.

The method of observation consisted in first placing, for example, a portion of the picture of the luminous surface of the sun in connection with the face of the pile, and after noting the indication of the needle of the galvanometer the telescope was then slightly moved so as to place the dark part of the spot directly on the face of the pile, the indication of the needle being again noted. In the next set of experiments the order was reversed, the picture of the spot at the beginning was placed in connection with the pile and afterwards a new part of the luminous portion of the disc was made to occupy the same place.

The thermo-electrical apparatus used in these experiments, was made by Ruhmkorff, of Paris; and in order to render the galvanometer more sensitive, two bar magnets, arranged in the form of the legs of a pair of dividers, were placed with the opening downward, in a vertical plane, above the needle, so that by increasing or diminishing the angle the directive power of the needle could be increased or diminished, and consequently the sensibility of the instrument could be varied, and the zero point changed at pleasure.

In the present experiments, in order to mark more definitely the difference in temperature, after the needle had been deflected by the heat of the sun, the magnetic bars above mentioned were so arranged as to repel it back to near zero point, so that it might in this position receive the maximum effect of any variation in the electrical current.

Twelve sets of observations were made on the first day, all of which, except one, gave the same indication, namely, that *the spot emitted less heat than the surrounding parts of the luminous disc*. The following is a copy of the record made at the time of observation. The degrees are those marked on the card of the galvanometer and are of course arbitrary:

Spot, $3^{\circ} \frac{1}{4}$.	Sun, $5^{\circ} \frac{1}{4}$.
Sun, $4^{\circ} \frac{1}{2}$.	Spot, 4° .
Sun, 3° .	Spot, $4^{\circ} \frac{1}{2}$.
Spot, $1^{\circ} \frac{3}{4}$.	Sun, 5° .

Spot, 2° .	Sun, $4^{\circ} \frac{1}{2}$.
Sun, 3° .	Spot, $3^{\circ} \frac{3}{4}$.
Sun, $2^{\circ} \frac{1}{2}$.	Sun, 2° .
Spot, 2° .	Spot, $3^{\circ} \frac{1}{4}$. *
Spot, 2° .	Spot, $0^{\circ} \frac{3}{4}$.
Sun, $2^{\circ} \frac{1}{4}$.	Sun, $2^{\circ} \frac{1}{2}$.
Spot, $4^{\circ} \frac{3}{4}$.	Sun, $1^{\circ} \frac{1}{4}$.
Sun, 5° .	Spot, 0° .

The change in the temperature during the intervals of observation, is due to the variations in the temperature of the room differently affecting the two extremities of the pile.

In consequence of cloudy weather, another set of observations was not obtained until the 10th of January, and at this time the spot had very much changed its appearance; the penumbra, while it retained its dimensions in one direction, was much narrowed in the other, and the dark part was separated into two small ones; also the sky was not perfectly clear and therefore the results were not as satisfactory as those of the previous observations; the indications were however the same as in the other sets, exhibiting a less degree of heat from the spots.

Cloudy weather prevented other observations on the heat of different parts of the sun, particularly a comparison between the temperature of the centre and the circumference of the disc, which would have an important bearing on the question of an atmosphere of the sun. The observations will be continued, and any results of interest which may be obtained, will be communicated to the Society.

* At this observation a slight cloud probably passed over the sun's disc.

ON THE CAPILLARITY OF METALS.

(Proceedings of the American Philosophical Society, vol. iv, page 176-178.)

June 20, 1845.

Professor Henry gave an account of some observations he had made on capillarity, in addition to those he had before communicated to the Society on the same subject.

In 1839* he presented the results of some experiments on the permeability of lead to mercury; and subsequent observation had led him to believe that the same property was possessed by other metals in reference to each other. His first attempt to verify this conjecture was made with the assistance of Dr. Patterson, at the United States Mint. For this purpose a small globule of gold was placed on a plate of sheet iron, and submitted to the heat of an assaying furnace; but the experiment was unsuccessful; for although the gold was heated much above melting point it exhibited no signs of sinking into the pores of the iron. The idea afterward suggested itself that a different result would have been obtained had the two metals been made to adhere previous to heating, so that no oxide could have been formed between the surfaces. In accordance with this view he inquired of Mr. Cornelius, of Philadelphia, if in the course of his experience in working silver-plated copper in his extensive manufactory of lamps he had ever observed the silver to disappear from the copper when the metal was heated. The answer was that the silver always disappears when the plate is heated above a certain temperature, leaving a surface of copper exposed; and that it was generally believed by the workmen that the silver evaporates at this temperature.

Professor Henry suggested that the silver, instead of evaporating, merely sunk into the pores of the copper, and that by carefully removing the surface of the latter, by the action of an acid the silver would re-appear. To verify this by ex-

*[Proc. Am. Phil. Soc. vol. i, p. 82. See *ante*, page 146.]

periment, Mr. Cornelius heated one end of a piece of thick plated copper to nearly the melting point of the metal; the silver at this end disappeared, and when the metal was cleaned by a solution of dilute sulphuric acid, the end which had been heated presented a uniform surface of copper, whilst the other end exhibited its proper coating of silver. The unsilvered end of the plate was next placed, for a few minutes, in a solution of muriate of zinc, by which the exterior surface of copper was removed, and the surface of silver was again exposed. This method of recovering the silver (before the process of plating silver by galvanism came into use) would have been of much value to manufacturers of plated ware, since it often happened that valuable articles were spoiled, in the process of soldering, by heating them to a degree at which silver disappears.

It is well known to the jeweller that articles of copper, plated with gold, lose their brilliancy after a time, and that it can be restored by boiling them in ammonia; this effect is probably produced by the ammonia acting on the copper, and dissolving off its surface so as to expose the gold, which, by diffusion, has entered into the copper.

A slow diffusion of one metal through another probably takes place in cases of alloys. Silver coins, after having lain long in the earth, have been found covered with a salt of copper. This may be explained by supposing that the alloy of copper, at the surface of the coin, enters into combination with the carbonic acid of the soil, and being thus removed, its place is supplied by a diffusion from within; and in this way it is not improbable that a considerable portion of the alloy may be exhausted in the process of time; and the purity of the coin be considerably increased.

Perhaps also the phenomenon of what is called *segregation*, or the formation of nodules of flint in masses of carbonated lime, and of indurated marl in beds of clay, may be explained on the same principle. In breaking up these masses it is almost always observed that a piece of shell, or some extraneous matter, occupies the middle, and probably formed the nucleus around which the matter was accumulated by attrac-

tion. The difficulty consists in explaining how the attraction of cohesion, which becomes insensible at sensible distances, should produce this effect. To explain this let us suppose two substances uniformly diffused through each other by a slight mutual attraction, as in the case of a lump of sugar dissolved in a large quantity of water, every particle of the water will attract to itself its proportion of sugar, and the whole will be in a state of equilibrium. If the diffusion at its commencement had been assisted by heat, and this cause of the separation of the homogeneous particles no longer existed, the diffusion might be one of unstable equilibrium; and the slightest extraneous force, such as the attraction of a minute piece of shell, might serve to disturb the quiescence, and draw to itself the diffused particles which were immediately contiguous to it. This would leave a vacuum of the atoms around the attracting mass, for example, as in the case of the sugar, there would be a portion of the water around the nucleus deprived of sugar; this portion of the water would attract its portion of sugar from the layer without, and into this layer the sugar from the layer next without would be diffused, and so on until, through all the water, the remaining sugar would be uniformly diffused. The process would continue to be repeated, by the nucleus again attracting a portion of the sugar from the water immediately around it, and so on until a considerable accumulation would be formed around the foreign substance.

We can in this way conceive of the manner by which the molecular action, which is insensible at perceptible distances, may produce results which would appear to be the effect of attraction acting at a distance.

ON THE PROTECTION OF HOUSES FROM LIGHTNING.

(Proceedings of the American Philosophical Society, vol. iv, pp. 179, 180.)

June, 20, 1845.

Professor Henry made a communication relative to a simple method of protecting from lightning buildings covered with metallic roofs.

On the principle of electric induction, houses thus covered are evidently more liable to be struck than those furnished either with shingle or tile. Fortunately however they admit of very simple means of perfect protection. It is evident, from well-established principles of electrical action, that if the outside of a house were encased entirely in a coating of metal, the most violent discharge which might fall upon it from the clouds would pass silently to the earth without damaging the house, or endangering the inmates. It is also evident, that if the house be merely covered with a roof of metal, without projecting chimneys, and this roof were put in metallic connection with the ground, the building would be perfectly protected. To make a protection therefore of this kind the Professor advises that the metallic roof be placed in connection with the ground by means of the tin or copper gutters which serve to lead the water from the roof to the earth. For this purpose it is sufficient to solder to the lower end of the gutter a ribbon of sheet copper, two or three inches wide, surrounding it with charcoal, and continuing it out from the house until it terminates in moist ground. The upper ends of these gutters are generally soldered to the roof; but if they are not in metallic contact, the two should be joined by a slip of sheet copper. The only part of the house unprotected by this arrangement will be the chimneys; and in order to secure these it will only be necessary to erect a short rod against the chimney, soldered at its lower end to the metal of the roof, and extending fifteen or twenty inches above the top of the flue.

Considerable discussion in late years has taken place in reference to the transmission of electricity along a conductor;—whether it passes through the whole capacity of the rod, or

is principally confined to the surface. From a series of experiments presented to the American Philosophical Society, by Professor Henry, on this subject, it appears that the electrical discharge passes, or tends to pass, principally at the surface; and as an ordinary sized house is commonly furnished with from two to four perpendicular gutters (generally two in front and two in the rear), the surface of these will be sufficient to conduct silently, the most violent discharge which may fall from the clouds.

Professor Henry also stated that he had lately examined a house struck by lightning, which exhibited some effects of an interesting kind. The lightning struck the top of the chimney, passed down the interior of the flue to a point opposite a mass of iron placed on the floor of the garret, where it pierced the chimney; thence it passed explosively, (breaking the plaster,) into a bedroom below, where it came in contact with a copper bell-wire, and passed along this horizontally and silently for about six feet; thence it leaped explosively through the air a distance of about ten feet, through a dormer window, breaking the sash, and scattering the fragments across the street. It was evidently attracted to this point by the upper end of a perpendicular gutter, which was near the window. It passed silently down the gutter, exhibiting scarcely any mark of its passage until it arrived at the termination, about a foot from the ground. Here again an explosion appeared to have taken place, since the windows of the cellar were broken. A bed in which a man was sleeping at the time, was situated against the wall, immediately under the bell-wire; and although his body was parallel to the wire, and not distant from it more than four feet, he was not only uninjured, but not sensibly affected. The size of the hole in the chimney, and the fact that the lightning passed along the copper wire without melting it, show that the discharge was a small one, and yet the mechanical effects, in breaking the plaster, and projecting the window frame across the street, were astonishingly great.

These effects the Professor attributes to a sudden repulsive energy, or expansive force developed in the air along the

path of the discharge. Indeed, he conceives that most of the mechanical effects often witnessed in cases of buildings struck by lightning, may be referred to the same cause. In the case of a house struck within a few miles of Princeton, the discharge entered the chimney, burst open the flue, and passed along the *cock-loft* to the other end of the house; and such was the explosive force in this confined space, that nearly the whole roof was blown off. This effect was in all probability due to the same cause which suddenly expands the air in the experiment with Kinnersly's electrical air thermometer.

ON COLOR-BLINDNESS.*

(From the Princeton Review, vol. XVII, pp. 483-489.)

July, 1845.

It is an interesting fact in reference to the dependence of at least one class of our knowledge—on sensation, that many persons are born with defective vision and yet remain for years of their lives without being conscious of the deficiency. We know a gentleman who had probably been always near sighted, but who did not discover the peculiarity of his vision until the age of twenty-five, when it was accidentally made known by looking at a distant object through a concave lens. Many persons whose eyes are sound and capable of exercising the most delicate functions, are permanently unable to distinguish certain colors. And the number of such persons is much more considerable than we would be led to imagine from the little attention this defect of vision has excited. It is often unknown to the individual himself, and indeed only becomes revealed by comparing his powers of discriminating different colors, with those of other persons.

* 1. Observations on color-blindness, or insensibility to the impression of certain colors. By Sir David Brewster. Philosophical Magazine.

2. Memoir on Daltonism, (or color-blindness.) By M. Elie Wartmann, Professor of Natural Philosophy in the Academy of Lausanne, &c. Scientific Memoirs.

The eye also under some circumstances may lose its sensibility for particular colors, or be thrown into such an unusual state as to present all objects to the mind under the appearance of a false color. Thus, if a person looks fixedly for a time at a bright red object and then turns his eye to a white wall, he will perceive a green image of the red object depicted on the white surface. A lady of our acquaintance was once thrown into alarming but laughable paroxysm of terror by an effect of this kind. She had been for some hours attentively sewing on a bright crimson dress, when her attention was directed towards her child, who in its sport had thrown itself on the carpet; its face appeared of the most ghastly hue, and the affrighted mother screamed in agony, that her child was in convulsions: the other inmates of the house hastened to her assistance, but they were surprised to find the little one smiling in perfect health. The sanity of the mother became the natural object of solicitude, until the effect was properly referred to the impression made on her eye by the crimson cloth.

Phenomena of this kind are known by the name of accidental colors; they have long attracted the attention of the natural philosopher, but the explanation of them is still involved in considerable uncertainty. The hypothesis which has been most generally adopted is that the eye by long attention to a particular color becomes fatigued with this and is incapable after a time of distinctly perceiving it; while it retains its full power of perception in reference to a fresh color. The consequence of this is that when the eye is directed to a white surface, after having attentively regarded a red object, green must appear; because white may be considered as a compound of red and green, and when the perception of the red is destroyed, the green must become visible. This explanation, however well it may apply to some of the phenomena, is not sufficient for the whole. Accidental colors can be perceived in the eye itself in perfect darkness. This is shown by steadily regarding for a short time a brilliant lamp, and then covering the eyes with the hands so as to exclude all external light, a luminous spot

will be perceived which passes in succession through all the colors of the rainbow.

Of the real cause of these appearances we are as yet almost entirely ignorant. Professor Plateau, of Ghent, has indeed referred them all to a few simple principles; but these appear to us rather expressions of the law of succession of the phenomena, than physical explanations of them. We do not however at this time intend to dwell on this class of phenomena, but to give a succinct account of those peculiarities of vision, in which abnormal perceptions of color are permanent, and which are fully treated of in the memoirs, the titles of which stand at the head of this article.

The peculiarity of vision called *color-blindness*, and sometimes *Daltonism*, may generally be referred to two classes. 1. Those in which all impression of color, except white and black, are wanting. 2. Those in which the individual can perceive certain simple colors, but is not able properly to distinguish between them. There are persons, strange as it may appear, in whom the sense of primary color is entirely deficient, and who, in place of red, yellow, and blue, see nothing but different degrees of white and black. Professor Wartmann gives a number of cases of this kind. The most ancient of those he finds described is that by Dr. Tuberville, in 1684, of a woman about 32 years of age, who came to consult the Dr. about her sight, which though excellent in other respects, gave her no impression in reference to color, except white and black. Spurzheim mentions a family, all the members of which could distinguish only different shades of white and black. An account is given by Mr. Huddart of a shoemaker, in Cumberland, who could distinguish in different colors only a greater or less intensity of light, calling all bright tints white and all dull ones black. His peculiarity of vision was unknown to him until one day, while a boy, playing in the street, he found a stocking, and for the first time was struck with the fact that it was called by his companions red, whereas to his mind it was capable of no farther description than that designated by the word stocking; he was thus led to conclude that there was some-

thing else besides the form and position in the leaves and fruit of a cherry tree, perceived by his playmates but not seen by himself. Two of his brothers had the same imperfection, while two other brothers, his sisters, and other relatives, had the usual condition of vision.

Of the other class the cases are much more numerous; we shall however give only a few examples. Mr. Harvey, of Plymouth, mentions a tailor who could see in the rainbow but two tints, namely, yellow and bright blue. Black appeared to him in general—green, sometimes crimson; light blue appeared like dark blue, crimson, or black; green was confounded with black and brown; carmine, red, lake, and crimson,—with blue.

But the most interesting case of this kind is that of the celebrated chemical philosopher, Dr. Dalton, of England. He published an account of his own case and that of several others in the Transactions of the Manchester Society, in 1794. Of the seven colors of the rainbow he could distinguish but two, yellow and blue; or at most, three, yellow, blue, and purple. He saw no difference between red and green; so that he thought the color of a laurel leaf the same as that of a stick of red sealing wax. A story is told of his having, on one occasion, appeared at the Quaker meeting, of which he was a member, in the usual drab coat and small-clothes of the sect, with a pair of flaming red-colored stockings to match. Whatever may be the truth in reference to this story, we have the assertion of Professor Whewell, that when Dr. Dalton was asked with what he would compare the scarlet gown with which he had been invested by the university, he pointed to the trees, and declared that he perceived no difference between the color of his robe and that of their foliage. Dr. Dalton found nearly twenty persons possessed of the same peculiarity of vision as himself; and among the number the celebrated metaphysician, Dugald Stewart, who could not distinguish a crimson fruit, like the Siberian crab-apple, from the leaves of the tree on which it grew otherwise than by the difference in its form.

On account of the prominence conferred on this defect of vision by Mr. Dalton's publication, the continental philoso-

phers gave it the name of *Daltonism*. To this name however several British writers have strongly objected. If this system of names were once allowed, say they, there is no telling where it would stop: the names of celebrated men would be connected, not with their superior gifts or achievements, but with the personal defects which distinguish them from their more favored but less meritorious contemporaries. Professor Whewell proposed the term *Idiopts*, signifying peculiarity of vision; but to this name Sir David Brewster properly objected, that the important consonant *p* would be very apt to be omitted in ordinary pronunciation, and so the last state of the Idiopt would be worse than the first. The name *color-blindness*, suggested by Sir David, although not in all cases free from objection, is perhaps better than any we have seen proposed.

It has already been stated that the number of persons affected with color-blindness is much more considerable than is generally imagined. They are often themselves ignorant of their imperfection of vision, particularly when it is restricted to the want of power to discriminate between colors nearly related to each other. Professor Seebeck found five cases among the forty boys who composed the two upper classes of a gymnasium of Berlin. Professor Prevost, of Geneva, stated that they amounted to one in twenty; and Professor Wartmann does not think this estimate much exaggerated.

Observations on this peculiarity of vision have as yet been confined, so far as we know, to Europe, with the exception of two cases described by Dr. Hays, of Philadelphia, in the Proceedings of the American Philosophical Society. It has also as yet been found only among the white race, although sufficient observations have not been made to render it probable that it is confined to this variety of the human family. The question has been asked, whether there is any external sign by which to detect, with simple inspection of the visual organ, a case of color-blindness. Professor Wartmann remarks, that he would not venture to give an answer to this question in all cases in the negative. I have observed, says

he, in the case of *Daltonians* whose eyes are brown, of the color which the English call hazel, a golden lustre of a peculiar tint, when the eye was viewed under an incidence of some obliquity.

Color-blindness is found much more common among men than women. Out of one hundred and fifty registered cases, there are but six of females, and one of these is doubtful. It has been conjectured that needle-work on a variety of colored articles might be the means of counteracting the tendency to this defect, as well as to produce a delicacy of discrimination of different shades of color not possessed by those otherwise employed. But in answer to this it has been remarked, that in the case of "Daltonians" engaged in painting, there has been found but little, if any, improvement of the condition of vision; and the very employment of the females on works which require a constant comparison of color would daily reveal cases of blindness of this kind did it frequently exist in the female sex. This peculiarity of vision is principally congenital. Professor Wartmann has found but two exceptions. In one of these, colors were perceived in the usual manner, until at the ninth year; when the boy received a violent blow on the head, which fractured the skull, and rendered a surgical operation necessary. The fact however that three of the brothers of this individual were affected with the same kind of vision renders it probable that he was constitutionally pre-disposed to this peculiarity.

With regard to hereditary pre-disposition there are some persons in whom this defect of vision occurs, whose relatives have never been known to be affected with it; others appear to have inherited it from their fathers through several generations, both on the maternal and paternal side. The boy before mentioned, as becoming color-blind at the age of nine years, was the eldest of eleven children,—seven males and four females; these were singularly divided into two sets, one of which consisted of individuals with blonde hair, and all the males with defective vision; the other, of those with red hair and ordinary power of vision.

Dr. Seebeck, as well as Professor Wartmann, has made a series of experiments to determine whether a person of this peculiarity of vision possesses the power of perceiving differences in colors which appear identical to us. The result of the investigations of both these philosophers was that he does not. Another problem has also been solved by the last-mentioned gentleman, in reference to the difference between a person with this defective vision and one of ordinary conditioned sight, in the perception of complementary colors. He found that colors which we regard as complementary, or such as when mingled together produce white, do not appear as such to those affected with this abnormal vision. They are not however insensible to accidental colors, but the feeling which results from the fatigue of attempting to produce these appears to be more painful in them than in us.

Various hypotheses have been advanced by different persons for the explanation of color-blindness. Mr. Dalton supposed that his peculiarity of vision, as well as that of those whom he had examined, depended on the fact that the vitreous or principal humour of the eye, in these cases, instead of being colorless and transparent was tinged with a blue. After his death, in obedience to his own instruction, his eyes were examined by his medical attendant, Mr. Ransome, but the vitreous humour was not found to exhibit any tinge of blue; on the contrary, it was of a pale yellow color. Objects viewed through it were not changed in color as they should have been had the hypothesis been true. Indeed, were the supposition correct, the same effect should be produced by blue spectacles, which is known not to be the case.

Stewart, Herschel, and others are of the opinion that this malady of vision is attributable to a defect in the *sensorium* itself, which renders it incapable of appreciating the differences between the rays on which the sensation of color depends. Sir David Brewster conceives that the eye, in the case of color-blindness, is insensible to the colors at one end of the spectrum, just as the ear of certain persons is insensible to sounds at one extremity of the scale of musical notes, while it is perfectly sensible to all other sounds. He knows

nothing about the *sensorium* or its connection with, or mode of operation upon, the nerves of sensation; and from the analogy of sight and hearing he has no hesitation in predicting that there may be found persons whose color-blindness is confined to one eye, or at least is greater in one eye than in the other. "Nor is this (says he) wholly a conjecture from analogy, for my own right eye, though not a better one than the left, which has no defect whatever, is more sensible to red light than the left eye." The case is precisely analogous with respect to his ears, for certain sounds; and no person, it is presumed, will maintain that there is a *sensorium* for each ear and each eye.

Whatever may be the cause of the inferiority, there exists a very easy means of compensating it to a certain extent. This method, first used by Dr. Seebeck, consists in viewing colored objects through colored media. Suppose the medium to be a piece of red glass; the impression of a red body and a green one on the eye of a person like Dr. Dalton, would be different, although with the naked eye they would be the same. The red glass would intercept much more of the light of the green object than of the red one, and hence the two would be readily distinguishable by a difference in the intensity of the illumination of the two objects. Nothing can equal the surprise, says Professor Wartmann, of a *Daltonian* when the errors which he commits every day in the appreciation of colors are thus disclosed to him.

EXPERIMENTS ON ELECTRICAL DISCHARGE.

(Proceedings of the American Philosophical Society, vol. iv, pages 208, 209.)

November 7, 1845.

Professor Henry communicated the result of a series of experiments on electricity made last winter. They had reference, first, to the discharge of electricity through a long wire connected with the earth at the farther end; secondly, to the discharge of a jar through a wire; and, thirdly, to an attempt to account for the phenomena of dynamic induction.

He first showed that when a charge of electricity is given

to one end of a wire, the different parts of the wire become charged successively, as though a wave of electricity passed along it. He then showed that the charge passed along the surface of the wire, and not through its whole mass, as was supposed from the analogy of galvanic conduction. Hence he inferred that dynamical electricity obeys the same laws as the statical. He then detailed some experiments upon the passage of electricity through plates, and showed that when a charge was transmitted across a plate the tension was greatest at the edges, the electricity apparently exercising a self-repelling action; while if the charge were passed through two pieces of tinfoil, these slips attract each other.

Professor Henry believes that it may be justly inferred from these experiments, that the attraction is due to ponderable matter, while the repulsion is due to electricity; thus showing that electricity is a separate principle, and not a mere property of matter.

He next passed to the subject of the discharge of a jar. It was necessary, in his experiments, to get rid of the free electricity arising from the thickness of the glass, and it occurred to him that this might be done by removing the knob, and making the coating upon the inside of less area than that upon the outside. With this arrangement, when the discharge was made through a long wire, and a test jar brought near it during discharge, a bright spark passed; but upon approaching the jar to a delicate electrometer it gave no indications of free electricity. Reflecting upon this, and upon an experiment of Professor Wheatstone's, he was led to believe that the jar is discharged by two waves, a negative and a positive one, starting simultaneously from the two ends of the wire. To prove this he broke the wire, and interposed a pane of glass dusted with red lead and sulphur; two figures of positive and negative electricity were produced. He made several other experiments tending to prove this same fact. He showed how these experiments serve to explain that of Dr. Priestly, where a spark was found to pass between the ends of a long bent wire, the ends being brought within a few inches of each other.

He next passed to the connection between statical and dynamical induction. Statical induction has heretofore been observed only at short distances. His first experiment proved that it could be observed at the distance of nineteen feet, the floor of a chamber intervening, showing that statical induction takes place at great distances, though not at so great distances as the dynamical. He then explained his views of the nature of dynamical induction. When a spark is thrown upon a wire it passes in a wave, whose length might be determined if we knew the velocity of electricity. Now, if we have another parallel wire, a negative wave will be formed in this, and the two waves will travel simultaneously in the same direction. But this is equivalent to a positive induced wave in the opposite direction. In this way the phenomena accompanying the discharge of a jar are easily explained. Again, if we conceive that in a galvanic battery the discharge consists of a series of such waves, we may very simply explain the phenomena of galvanic induction.

EXPERIMENT ON THE MAGNETIC POLARIZATION OF LIGHT.

(Proceedings of American Philosophical Society, vol. iv. pp. 229, 230.)

January 16, 1846.

Dr. Patterson read a portion of a letter from Professor Henry in which he describes the manner in which he had repeated the experiment of Mr. Faraday on the magnetic polarization of light.

" - - - This consists in producing in pure water and other liquids a new arrangement of particles by which they become possessed of the property of circular polarization during the time a current of galvanism is circulating around them. The arrangement I employed was as follows: A tube of glass was filled with pure water and the ends closed with plates of glass; this was placed in the axis of an iron tube, and this again inserted into the axis of a coil consisting of about eight hundred feet of copper wire. The ends of the iron tube were closed with corks, through one of which was passed a Nicol's prism, and in the axis of the other was fastened a plate of tourmaline. This tube being directed to the clear sky, and the tourmaline, which was placed next the eye, so turned that it presented a dark field of view, a current of galvanism from twenty-two cups of a Daniell's battery was passed through the coil. At the moment of making the communication with the battery the field became light, and when the circuit was broken it again appeared dark. A slight rotation of the tourmaline also produced darkness while the galvanic current was passing, which indicated a twist in the plane of polarization of the prolonged beam. The same effect was produced without the iron tube but not to the same extent."

ON THE RELATION OF TELEGRAPH LINES TO LIGHTNING.

(Proceedings of the American Philosophical Society, vol. iv, pp. 260-268.)

June 19, 1846.

[A letter from S. D. Ingham to Dr. Patterson was read, detailing cases in which the telegraph wires were struck by lightning, and asking the attention of the Society to some interesting questions connected with the mode in which the wires may be affected by electricity.]

Professor Henry, to whom the letter was referred, made the following report:

The action of the electricity of the atmosphere on the wires of the electrical telegraph is at the present time a subject of much importance, both on account of its practical bearing, and the number of purely scientific questions which it involves. I have accordingly given due attention to the letter referred to me, and have succeeded in collecting a number of facts in reference to the action in question. Some of these are from the observations of different persons along the principal lines, and others from my own investigations during a thunder storm on the 19th of June, when I was so fortunate as to be present in the office of the telegraph in Philadelphia, while a series of very interesting electrical phenomena was exhibited. In connection with the facts derived from these sources, I must ask the indulgence of the Society in frequently referring, in the course of this communication, to the results of my previous investigations in dynamic electricity, accounts of which are to be found in the Proceedings and Transactions of this Institution.

From all the information on the subject of the action of the electricity of the atmosphere on the wires of the telegraph, it is evident that effects are produced in several different ways.

1. The wires of the telegraph are liable to be struck by a direct discharge of lightning from the clouds, and several cases of this kind have been noticed during the present season. About the 20th of May the lightning struck the

elevated part of the wire which is supported on a high mast at the place where the telegraph crosses the Hackensack river. The fluid passed along the wire each way from the point which received the discharge, for several miles, striking off at irregular intervals down the supporting poles. At each place where the discharge to a pole took place a number of sharp explosions were heard in succession, resembling the rapid reports of several rifles. During another storm, the wire was struck in two places in Pennsylvania, on the route between Philadelphia and New York; at one of these places twelve poles were struck, and at the other eight. In the latter case the remarkable fact was observed that every other pole escaped the discharge; and the same phenomenon was observed, though in a less marked degree, near the Hackensack river. In some instances the lightning has been seen coursing along the wire in a stream of light; and in another case it is described as exploding from the wire at certain points, though there were no bodies in the vicinity to attract it from the conductor.

In discussing these, and other facts to be mentioned hereafter, we shall for convenience adopt the principles and language of the theory which refers the phenomena of electricity to the action of a fluid of which the particles repel each other, and are attracted by the particles of other matter. Although it cannot be affirmed that this theory is an actual representation of the cause of the phenomena, as they are produced in nature, yet it may be asserted that it is, in the present state of science, an accurate mode of expressing the laws of electrical action, so far as they have been made out; and that though there are a number of phenomena which have not as yet been referred to this theory, there are none which are proved to be directly at variance with it.

That the wires of the telegraph should be frequently struck by a direct discharge of lightning is not surprising when we consider the great length of the conductor, and consequently the many points along the surface of the earth through which it must pass, peculiarly liable to receive the discharge from the heavens. Also, from the great length of the con-

ductor, the more readily must the repulsive action of the free electricity of the cloud drive the natural electricity of the conductor to the farther end of the line, thus rendering more intense the negative condition of the nearer part of the wire, and consequently increasing the attraction of the metal for the free electricity of the cloud. It is not however probable that the attraction, whatever may be its intensity, of so small a quantity of matter as that of the wire of the telegraph, can of itself produce an electrical discharge from the heavens, although, if the discharge were started by some other cause, such as the attraction of a large mass of conducting matter in the vicinity, the attraction of the wire might be sufficient to change the direction of the descending bolt, and draw it, in part or in whole, to itself. It should also be recollected, that on account of the perfect conduction, a discharge on any part of the wire must affect every other part of the connected line, although it may be hundreds of miles in length.

That the wire should give off a discharge to a number of poles in succession is a fact I should have expected from my previous researches on the lateral discharge of a conductor transmitting a current of free electricity. In a paper on this subject, presented to the British Association in 1837,* I showed that when electricity strikes a conductor explosively it tends to give off sparks to all bodies in the vicinity, however intimately the conductor may be connected with the earth. In an experiment in which sparks from a small machine were thrown on the upper part of a lightning rod, erected in accordance with the formula given by the French Institute, corresponding sparks could be drawn from every part of the rod, even from that near the ground. In a communication since made to this Society, I have succeeded in referring this phenomenon to the fact, that during the transmission of a quantity of electricity along a rod, the surface of the conductor is charged in succession, as it were, by a wave of the fluid, which, when it arrives opposite a given point, tends to give off a spark to a neighboring body for the same reason

* [Report of British Association, 1837. See *ante*, page 101.]

that the charged conductor of the machine gives off a spark under the same circumstances.

It might at first be supposed that the redundant electricity of the conductor would exhaust itself in giving off the first spark, and that a second discharge could not take place; but it should be observed that the wave of free electricity, in its passage, is constantly attracted to the wire by the portion of the uncharged conductor which immediately precedes its position at any time; and hence but a part of the whole redundant electricity is given off at one place, the velocity of transmission of the wave as it passes the neighboring body, and its attraction for the wire, preventing a full discharge at any one place. The intensity of the successive explosions is explained by referring to the fact, that the discharge from the clouds does not generally consist of a single wave of electricity, but of a number of discharges along the same path in rapid succession, or of a continuous discharge which has an appreciable duration; and hence the wire of the telegraph is capable of transmitting an immense quantity of the fluid thus distributed over a great length of the conductor.

The remarkable facts of the explosions of the electricity into the air, and of the poles being struck in interrupted succession, find a plausible explanation in another electrical principle which I have established, namely, in all cases of the disturbance of the equilibrium of the electrical *plenum* which we must suppose to exist throughout all terrestrial space, the state of rest is attained by a series of diminishing oscillations. Thus, in a discharge of a Leyden jar, I have shown that the phenomena exhibited cannot be explained by merely supposing the transfer of a quantity of fluid from the inner to the outer side of the jar; but in addition to this we are obliged to admit the existence of several waves, backwards and forwards, until the equilibrium is attained. In the case of the discharge from the cloud, a wave of the natural electricity of the metal is repelled each way from the point on which the discharge falls, to either end of the wire, is then reflected, and in its reverse passage meets in

succession the several waves which make up the discharge from the cloud. These waves will therefore interfere at certain points along the wire, producing, for a moment, waves of double magnitude, and will thus enhance the tendency of the fluid at these points to fly from the conductor. I do not say that the effects observed were actually produced in this way; I merely wish to convey the idea that known principles of electrical action might, under certain circumstances, lead us to anticipate such results.

2. The state of the wire may be disturbed by the conduction of a current of electricity from one portion of space to another, without the presence of a thunder-cloud; and this will happen in case of a long line, when the electrical condition of the atmosphere which surrounds the wire at one place is different from that at another. Now it is well known that a mere difference in elevation is attended with a change in the electrical state of the atmosphere. A conductor, elevated by means of a kite, gives sparks of positive electricity on a perfectly clear day; hence, if the line of the telegraph passes over an elevated mountain ridge, there will be continually, during clear weather, a current from the more elevated to the lower points of the conductor.

A current may also be produced in a long level line by the precipitation of vapor, in the form of a fog, at one end, while the air remains clear at the other; or by the existence of a storm of rain or snow at any point along the line, while the other parts of the wire are not subjected to the same influence.

Currents of sufficient power to set in motion the marking machine of the telegraph have been observed, which must have been produced by some of these causes. In one case the machine spontaneously began to operate without the aid of the battery while a snow-storm was falling at one end of the line, and clear weather existed at the other. On another occasion a continued stream of electricity was observed to pass between two points at a break in the wire, presenting the appearance of a gaslight almost extinguished. A con-

stant effect of this kind indicates a constant accession of electricity at one part of the wire, and a constant discharge at the other.

3. The natural electricity of the wire of the telegraph is liable to be disturbed by the ordinary electrical induction of a distant cloud. Suppose a thunder-cloud driven by the wind in such a direction as to cross one end of the line of the telegraph at the elevation—say of a mile; during the whole time of the approach of the cloud to the point of its path directly above the wire, the repulsion of the redundant electricity with which it is charged would constantly drive more and more of the natural electricity of the wire to the farther end of the line, and would thus give rise to a current. When the cloud arrived at the point nearest to the wire, the current would cease for a moment; and as the repulsion gradually diminished by the receding of the cloud, the natural electricity of the wire would gradually return to its normal state, giving rise to a current in an opposite direction. If the cloud were driven by the wind parallel to the line of the telegraph a current would be produced towards each end of the wire, and these would constantly vary in intensity with the different positions of the cloud. Although currents produced in this way may be too feeble to set in motion the marking apparatus, yet they may have sufficient power to influence the action of the current of the battery so as to interfere with the perfect operation of the machine.

4. Powerful electrical currents are produced in the wires of the telegraph by every flash of lightning which takes place within many miles of the line, by the action of dynamic induction; which differs from the action last described in being the result of the influence of electricity *in motion* on the natural electricity of the conductor. The effect of this induction, which is the most fruitful source of disturbance, will be best illustrated by an account of some experiments of my own, presented to the Society in 1843. A copper wire was suspended by silk strings around the ceiling of an upper

room so as to form a parallelogram of about sixty feet by thirty on the sides; and in the cellar of the same building, immediately below, another parallelogram of the same dimensions was placed. When a spark from an electrical machine was transmitted through the upper parallelogram an induced current was developed in the lower one, sufficiently powerful to magnetize needles, although two floors intervened, and the conductors were separated to the distance of thirty feet. In this experiment no electricity passed through the floors from one conductor to the other; the effect was entirely due to the repulsive action of the electricity in motion in the upper wire on the natural electricity of the lower. In another experiment two wires, about 400 feet long, were stretched parallel to each other between two buildings; a spark of electricity sent through one produced a current in the other, though the two were separated to the distance of 300 feet; and from all the experiments it was concluded that the distance might be indefinitely increased, provided the wires were lengthened in a corresponding ratio.

That the same effect is produced by the repulsive action of the electrical discharge in the heavens is shown by the following modification of the foregoing arrangement. One of the wires was removed and the other so lengthened at one end as to pass into my study and thence through a cellar window into an adjacent well. With every flash of lightning, which took place in the heavens within at least a circle of twenty miles around Princeton, needles were magnetized in the study by the induced current developed in the wire. The same effect was produced by soldering a wire to the metallic roof of the house, and passing it down into the well; at every flash of lightning a series of currents, in alternate directions, was produced in the wire.

I was also led, from these results, to infer that induced currents must traverse the line of a railroad, and this I found to be the case. Sparks were seen at the breaks in the continuity of the rail with every flash of a distant thunder cloud.

Similar effects, but in a greater degree, must be produced on the wire of the telegraph, by every discharge in the

heavens ; and the phenomena which I witnessed on the 19th of June in the telegraph office in Philadelphia were, I am sure, of this kind. In the midst of the hurry of the transmission of the congressional intelligence from Washington to Philadelphia, and thence to New York, the apparatus began to work irregularly. The operator at each end of the line announced at the same time a storm at Washington, and another at Jersey City. The portion of the circuit of the telegraph which entered the building, and was connected with one pole of the galvanic battery, happened to pass within the distance of less than an inch of the wire which served to form the connection of the other pole with the earth. Across this space, at an interval of every few minutes, a series of sparks in rapid succession was observed to pass ; and when one of the storms arrived so near Philadelphia that the lightning could be seen, each series of sparks was found to be simultaneous with a flash in the heavens. Now we cannot suppose, for a moment, that the wire was actually struck at the time each flash took place ; and indeed it was observed that the sparks were produced when the cloud and flash were at a distance of several miles to the east of the line of the wire. The inevitable conclusion is that all the exhibition of electrical phenomena witnessed during the afternoon was purely the effects of induction, or the mere disturbance of the natural electricity of the wire at a distance, without any transfer of the fluid from the cloud to the apparatus.

The discharge between the two portions of the wire continued for more than an hour, when the effect became so powerful that the superintendent, alarmed for the safety of the building, connected the long wire with the city gas pipes, and thus transmitted the current silently to the ground. I was surprised at the quantity and intensity of the current ; it is well known that to affect a common galvanometer with ordinary electricity requires the discharge of a large battery ; but such was the quantity of the induced current exhibited on this occasion that the needle of an ordinary vertical galvanometer, with a short wire, and apparently of little sensibility, was moved several degrees.

The pungency of the spark was also, as might have been expected, very great. When a small break was made in the circuit, and the parts joined by the forefinger and thumb the discharge transmitted through the hand affected the whole arm up to the shoulder. I was informed by the superintendent that on another occasion a spark passed over the surface of the spool of wire surrounding the legs of the horse-shoe magnet at right angles to the spires; and such was its intensity and quantity that all the wires across which it passed were melted at points in the same straight line as if they had been cut in two by a sharp knife.

The effects of the powerful discharges from the clouds may be prevented, in a great degree, by erecting at intervals along the line, and aside of the supporting poles, a metallic wire connected with the earth at the lower end, and terminating above at the distance of about half an inch from the wire of the telegraph. By this arrangement the insulation of the conductor will not be interfered with, while the greater portion of the charge will be drawn off. I think this precaution of great importance at places where the line crosses a river, and is supported on high poles. Also in the vicinity of the office of the telegraph where a discharge, falling on the wire near the station, might send a current into the house of sufficient quantity to produce serious accidents. The fate of Professor Richman, of St. Petersburg, should be recollected, who was killed by a flash from a small wire, which entered his house from an elevated pole, while he was experimenting on atmospheric electricity.

The danger however which has been apprehended from the electricity leaving the wire and discharging itself into a person on the road is, I think, very small; electricity, of sufficient intensity to strike a person at the distance of eight or ten feet from the wire, would, in preference, be conducted down the nearest pole. It will however in all cases be most prudent to keep at a proper distance from the wire during the existence of a thunder storm in the neighborhood.

It may be mentioned as an interesting fact, derived from two independent sources of information, that large numbers of small birds have been seen suspended by the claws from the wire of the telegraph. They had, in all probability, been instantaneously killed, either by a direct discharge, or an induced current from a distant cloud while they were resting on the wire.

Though accidents to the operators, from the direct discharge, may be prevented by the method before mentioned, yet the effect on the machine cannot be entirely obviated; the residual current which escapes the discharge along the perpendicular wires must neutralize, for a moment, the current of the battery, and produce irregularity of action in the apparatus.

The direct discharge from the cloud on the wire is, comparatively, not a frequent occurrence, while the dynamic inductive influence must be a source of constant disturbance during the seasons of thunder storms; and no other method presents itself to my mind at this time for obviating the effect, but that of increasing the size of the battery, and diminishing the sensibility of the magnet so that at least the smaller induced currents may not be felt by the machine. It must be recollected that the inductive influence takes place at a distance through all bodies, conductors and non-conductors; and hence no coating that can be put upon the wire will prevent the formation of induced currents.

I think it not improbable, since the earth has been made to act the part of the return conductor, that some means will be discovered for insulating the single wire beneath the surface of the earth; the difficulty in effecting this is by no means as great as that of insulating two wires, and preventing the current striking across from one to the other. A wire buried in the earth would be protected in most cases from the effect of a direct discharge; but the inductive influence would still be exerted, though perhaps in a less degree.

The wires of the telegraph are too small and too few in number to affect, as some have supposed, the electrical con-

dition of the atmosphere by equalizing the quantity of the fluid in different places, and thus producing a less changeable state of the weather. The feeble currents of electricity which must be constantly passing along the wires of a long line may however, with proper study, be the means of discovering many interesting facts relative to the electrical state of the air over different regions.*

ON THE "FOUNTAIN-BALL," AND ON THE INTERFERENCE OF
HEAT.

(Proceedings of the American Philosophical Society, vol. iv, p. 285.)

October 16, 1846.

Professor Henry laid before the Society the results of some investigations that he had lately made on two questions in physical science, and a theory of the causes of the phenomena observed.

The well known phenomenon of a ball resting on a jet of water he ascribed to the action of three different causes: 1st, to the adhesion of the water to the ball: 2d, to the adhesion of the water to itself: 3d, to the tendency of water to move in a straight line and also to the principle of action and re-action.

He had also made experiments in regard to the interference of heat for the purpose of discovering whether certain phenomena of interference of light were exhibited as well in the case of heat. He found it to be so, and that two rays of heat may be thrown on each other so as to produce a reduction of temperature.

*[Re-printed in Silliman's American Journal of Science, 1847, vol. III, pp. 25-32. Also in the London and Edinburgh Philosophical Magazine, 1847, vol. xxx, pp. 186-194.]

ON THE ATOMIC CONSTITUTION OF MATTER.

(Proceedings of the American Philosophical Society, vol. iv, pp. 287-290.)

November 6, 1846.

The reference to a paper presented at the preceding meeting of the Society led Professor Henry to make some remarks on the corpuscular hypothesis of the constitution of matter.

He stated that this subject has occupied attention at every period of the history of science, and though at first sight speculations of this kind might appear to belong exclusively to the province of the imagination, yet in reality he considered this hypothesis a fruitful source of valuable additions to our knowledge of the actual phenomena of the physical world. Though simple insulated facts may occasionally be stumbled upon by a lucky accident, the discovery of a series of facts or of a general scientific principle is in almost all cases the result of deductions from a rational antecedent hypothesis, the product of the imagination—founded it is true on a clear analogy with modes of physical action the truth of which have been established by previous investigation.

In constructing an hypothesis of the constitution of matter the simplest assumption, and indeed the only one founded on a proper physical analogy, is that the same laws of force and motion which govern the phenomena of the action of matter in masses pertains to the minutest atoms of these masses.

It is a well established fact that portions of matter at a distance tend to approach each other, and when they are brought very near, to separate, and still nearer, again to approach, and so on through several alternations. In the present state of science we consider these actions as ultimate facts to which we give the name of attracting and repelling forces, and without attempting to go behind them we may study their laws of variation as to intensity and direction under different circumstances and particularly in reference to a change of distance. Bodies or masses of matter are also subjected to fixed laws of motion which have been classed

under three heads, namely, the law of inertia or tendency to resist a change of state and to move in a straight line with a constant velocity, the law of the co-existence of separate motions, and the law of the equality of action and reaction.

The explanation of a mechanical phenomenon consists in its analysis and the reference of its several parts to the foregoing laws of force and motion, and as no phenomenon, whether it relates to masses or the minutest portions of matter is fully explained until it can be referred to one or more of these laws it follows that any corpuscular hypothesis which does not ascribe to each atom of matter the property of obedience to the same laws must be defective. It was for this reason that in printing a syllabus of his lectures about two years ago he was induced to make some additions to the assumptions on which the corpuscular hypothesis of Boscovich is founded. According to this celebrated hypothesis, a portion of matter consists of an assemblage in space of an indefinite number of points kept at a given distance by attracting and repelling forces: these points have relative position but not magnitude, and are merely centers of action of the forces which affect our senses, and since all our knowledge of matter is derived from the action of these forces, to infer that these points are anything more than the centers of forces is going beyond our premises.

This hypothesis readily explains the statical properties of bodies, such as elasticity, porosity, impenetrability, solidity, liquidity, crystallization, resistance to compression when a force is applied to either side of the body, etc.; but it fails to account for the dynamic phenomena of *masses* of matter, or those which are referable to the three laws of motion. It is not therefore enough that we assume, as the elements of matter, an assemblage of points in space from which merely emanate attracting and repelling forces; we must also suppose these points to be endowed with *inertia*, or a tendency to resist a change of state, whether of rest or motion, and a tendency to move in a straight line; also to possess the property of preserving the effects of a number of impulses, as well as that of transferring motion from one point

to another, the one losing as much motion as the other gains. But the admission of the existence of points with such qualities brings us back to the Newtonian hypothesis of matter.

According to the view we have given, a portion of matter consists of an assemblage of indivisible and indestructible atoms endowed with attracting and repelling forces, and with the property of obedience to the three laws of motion. All the other properties, and indeed all the mechanical phenomena of matter, so far as they have been analyzed, are probably referable to the action of such atoms, arranged in groups of different orders, namely, of ultimate atoms, chemical atoms, simple molecules, compound molecules, particles, etc.; the distance in all cases between any two atoms being much greater than the diameter of the atoms or molecules.

In order that we may bring all the phenomena of the "imponderable" agents of nature, (as they are called,) under the category of the laws of force and motion, we are obliged to assume the existence of an ætherial medium formed of atoms, which are endowed with precisely the same properties as those we have assigned to common matter; and this assumption leads us to the inference, that matter is diffused through all space.

That something exists between us and the sun, possessing the properties of matter, may be inferred from the simple fact that time is required for the transmission of light and heat through the intervening space. The phenomena of the transmitted motion, in these cases, are perfectly represented by undulations, in a medium composed of very minute atoms of ordinary matter, endowed with all the mechanical properties we have mentioned. Indeed, the motion is analogous—though not precisely similar—to the transmission of sound through air; the time however in the two cases being very different. Light passes the space between us and the sun in about eight minutes, while sound (through air) would require $13\frac{2}{3}$ years to perform the same journey. This difference in velocity is however readily explained by a difference in density and elasticity of air, and the ætherial medium. That the phenomena of light and heat from the sun are not the

effect of transmission of mere force, (without intervening matter,) such as that of attraction and repulsion, is evident from the fact that these actions require no perceptible time for their transmission to the most distant part of the solar system. If the sun were at once to be annihilated the planet Neptune would, at the same instant begin to move in a tangent to its present orbit. Also, the phenomena of electricity and magnetism involve the consideration of time; the discharge of the former through a copper wire is transmitted with about the velocity of light, and the development of the latter in an iron bar is attended with a change in the ponderable molecules of the metal which requires time for its completion.

According to the foregoing rules, we may assume with Newton, the existence of one kind of matter diffused throughout all space, and existing in four states, namely, the ætherial, the aëriform, the liquid, and the solid. This method of presenting the atomic hypothesis of the constitution of matter, may at first sight appear startling; but on a little reflection, it will be found a necessary consequence of the attempt to explain the mechanical phenomena of matter by an assemblage of separate atoms. It may be objected to the assumption of one kind of matter that the fact of the imponderable nature of light, heat, electricity and magnetism require at least two kinds of matter; but if we adopt the theory of undulation, the phenomena of the "imponderables" (as they are called) are merely the results of the motions of the atoms of the ætherial medium combined in some cases with the motion of the atoms of the body; and since the vibrations of the atoms of a mass of matter do not increase the attraction of the earth on the mass, an increase of temperature in a body cannot change its weight; and also because the ætherial medium fills all space, a portion of this medium can no more exhibit weight than a quantity of air when weighed in the midst of the atmosphere.

The points here noticed, relate merely to the fundamental conceptions of the corpuscular or atomic constitution of matter, and not to the arrangement of the atoms into sys-

tems of groups, which are necessary to represent the varied and complicated mechanical and chemical phenomena exhibited in the physical changes going on around us. Though he could not at this time attempt to give any details of application of this hypothesis, he drew attention to one class of facts of which it is important to furnish an expression in the arrangement of the atoms. He alluded to the facts of polarity, or those which exhibit the action of opposite forces at the extremities of molecules or of masses. The north and south poles of two magnets, brought together, neutralize each other; the attraction of one is balanced by the repulsion of the other, and the point of junction is without action on a third ferruginous body. In the same manner apparently, two chemical elements which enter into combination exhibit a neutralizing effect, which indicates the existence of polar forces in the phenomena of chemical action. Nothing however is perceptible of this kind in the effects of gravitation; the action of two particles on each other does not interfere with the action at the same time of these two on any number of other particles.

In conclusion it should be remembered that the legitimate use of speculations of this kind is not to furnish plausible explanations of known phenomena, or to present old knowledge in a new and more imposing dress, but to serve the higher purpose of suggesting new experiments and new phenomena, and thus to assist in enlarging the bounds of science and extending the power of mind over matter; and unless the hypothesis can be employed in this way, however much ingenuity may have been expended in its construction, it can only be considered as a scientific romance worse than useless, since it tends to satisfy the mind with the semblance of truth, and thus to render truth itself less an object of desire.

ON THE HEIGHT OF THE AURORA.

(Proceedings of the American Philosophical Society, vol. iv, p. 370.)

December 3d, 1846.

Professor Henry made a communication relative to some observations on the aurora borealis, with the object of determining the height of the meteor. The result of the observations tended to establish the fact that the arch of the aurora like the rainbow is a local phenomenon, each observer seeing a different object.

SCIENTIFIC WRITINGS OF JOSEPH HENRY.

PART II.

FROM 1847 TO 1878.

SCIENTIFIC PAPERS AND ABSTRACTS.

PROGRAMME OF ORGANIZATION OF THE SMITHSONIAN INSTITUTION.

(From the First Annual Report of the Secretary to the Board of Regents.)*

December 8, 1847.

GENTLEMEN: - - - In accordance with my instructions I consulted with men of eminence in the different branches of literature and science, relative to the details of the plan of organization, and arranged the various suggestions offered in the form of the accompanying programme. This, after having been submitted to a number of persons in whose knowledge and judgment I have confidence, is now presented to the Board, with the concurrence of the Committee on Organization, for consideration and provisional adoption. I regret that I could not give the names of those whose suggestions have been adopted in the programme; the impossibility of rendering justice to all has prevented my attempting this. Many of the suggestions have been offered by different persons independently of each other. - - -

The introduction to the programme contains a series of propositions suggested by a critical examination of the will of Smithson, to serve as a guide in judging of the fitness of any proposed plan for carrying out the design of the testator. - - -

That all the propositions will meet with general approval cannot be expected; and that this organization is the best that could be devised is neither asserted nor believed. To produce *à priori* a plan of organization which shall be found

*[The Plan adopted by the Board of Regents, December 13, 1847.]

to succeed perfectly in practice, and require no amendment, would be difficult under the most favorable circumstances, and becomes almost impossible where conflicting opinions are to be harmonized and the definite requirements of the Act of Congress establishing the Institution are to be observed. It is not intended that the details of organization as given in the programme, should be permanently adopted without careful trial; they are rather presented as suggestions to be adopted provisionally, and to be carried into operation gradually and cautiously, with such changes from time to time as experience may dictate.

INTRODUCTION.

General considerations which should serve as a guide in adopting a Plan of Organization.

1. Will of Smithson. The property is bequeathed to the United States of America, "to found at Washington, under the name of the Smithsonian Institution, an establishment for the increase and diffusion of knowledge among men."

2. The bequest is for the benefit of mankind. The Government of the United States is merely a trustee to carry out the design of the testator.

3. The Institution is not a national establishment, as is frequently supposed, but the establishment of an individual, and is to bear and perpetuate his name.

4. The objects of the Institution are—1st, to increase—and 2d, to diffuse—knowledge among men.

5. These two objects should not be confounded with one another. The first is to enlarge the existing stock of knowledge by the addition of new truths; and the second, to disseminate knowledge, thus increased, among men.

6. The will makes no restriction in favor of any particular kind of knowledge; hence all branches are entitled to a share of attention.

7. Knowledge can be increased by different methods of facilitating and promoting the discovery of new truths; and

can be most extensively diffused among men by means of the press.

8. To effect the greatest amount of good, the organization should be such as to enable the Institution to produce results in the way of increasing and diffusing knowledge, which cannot be produced either at all or so efficiently by the existing institutions in our country.

9. The organization should also be such as can be adopted provisionally, can be easily reduced to practice, receive modifications, or be abandoned in whole or in part, without a sacrifice of the funds.

10. In order to compensate, in some measure, for the loss of time occasioned by the delay of eight years in establishing the Institution, a considerable portion of the interest which has accrued should be added to the principal.

11. In proportion to the wide field of knowledge to be cultivated, the funds are small. Economy should therefore be consulted in the construction of the building; and not only the first cost of the edifice should be considered, but also the continual expense of keeping it in repair, and of the support of the establishment necessarily connected with it. There should also be but few individuals permanently employed by the Institution.

12. The plan and dimensions of the building should be determined by the plan of the organization, and not the converse.

13. It should be recollected that mankind in general are to be benefited by the bequest, and that therefore all unnecessary expenditure on local objects would be a perversion of the trust.

14. Besides the foregoing considerations, deduced immediately from the will of Smithson, regard must be had to certain requirements of the act of Congress establishing the Institution. These are, a library, a museum, and a gallery of art, with a building on a liberal scale to contain them.

SECTION I.

Plan of Organization of the Institution in accordance with the foregoing deductions from the will of Smithson.

To increase knowledge: It is proposed—

1. To stimulate men of talent to make original researches by offering suitable rewards for memoirs containing new truths; and,

2. To appropriate annually a portion of the income for particular researches, under the direction of suitable persons.

To diffuse knowledge: It is proposed—

1. To publish a series of periodical reports on the progress of the different branches of knowledge; and,

2. To publish occasionally separate treatises on subjects of general interest.

DETAILS OF THE PLAN TO INCREASE KNOWLEDGE.

I. *By stimulating researches.*

1. Rewards consisting of money, medals, &c., offered for original memoirs on all branches of knowledge.*

2. The memoirs thus obtained to be published in a series of volumes, in a quarto form, and entitled “Smithsonian Contributions to Knowledge.”

3. No memoir on subjects of physical science to be accepted for publication which does not furnish a positive addition to human knowledge, resting on original research; and all unverified speculations to be rejected.

4. Each memoir presented to the Institution to be submitted for examination to a commission of persons of reputation for learning in the branch to which the memoir pertains, and to be accepted for publication only in case the report of this commission is favorable.

5. The commission to be chosen by the officers of the Institution, and the name of the author (as far as practicable) concealed, unless a favorable decision be made.

*[In the annual report for 1855, this clause was changed to read—
“1. Facilities afforded for the production of original memoirs on all branches of knowledge.”]

6. The volumes of the memoirs to be exchanged for the transactions of literary and scientific societies, and copies to be given to all the colleges and principal libraries in this country. One part of the remaining copies may be offered for sale, and the other carefully preserved, to form complete sets of the work, to supply the demand from new institutions.

7. An abstract, or popular account of the contents of these memoirs to be given to the public through the annual report of the Regents to Congress.

II. *By appropriating a part of the income annually to special objects of research.*

1. The objects and the amount appropriated to be recommended by counsellors of the Institution.

2. Appropriations in different years to different objects; so that in course of time each branch of knowledge may receive a share.

3. The results obtained from these appropriations to be published, with the memoirs before mentioned, in the volumes of the "Smithsonian Contributions to Knowledge."

4. Examples of objects for which appropriations may be made: (a.) System of extended meteorological observations for solving the problem of American storms. (b.) Explorations in descriptive natural history, and geological, magnetical, and topographical surveys, to collect materials for the formation of a Physical Atlas of the United States. (c.) Solution of experimental problems, such as a new determination of the weight of the earth, of the velocity of electricity, and of light; chemical analyses of soils and plants; collection and publication of scientific facts accumulated in the offices of Government. (d.) Institution of statistical inquiries with reference to physical, moral, and political subjects. (e.) Historical researches and accurate surveys of places celebrated in American history. (f.) Ethnological researches, particularly with reference to the different races of men in North America; also explorations and accurate surveys of the mounds and other remains of the ancient people of our country.

DETAILS OF THE PLAN FOR DIFFUSING KNOWLEDGE.

I. *By the publication of a series of reports, giving an account of the new discoveries in science, and of the changes made from year to year in all branches of knowledge not strictly professional.*

1. These reports will diffuse a kind of knowledge generally interesting, but which at present is inaccessible to the public. Some of the reports may be published annually, others at longer intervals, as the income of the Institution or the changes in the branches of knowledge may indicate.

2. The reports are to be prepared by collaborators eminent in the different branches of knowledge.

3. Each collaborator to be furnished with the journals and publications, domestic and foreign, necessary to the compilation of his report; to be paid a certain sum for his labors, and to be named on the title-page of the report.

4. The reports to be published in separate parts so that persons interested in a particular branch can procure the parts relating to it without purchasing the whole.

5. These reports may be presented to Congress for partial distribution, the remaining copies to be given to literary and scientific institutions, and sold to individuals for a moderate price.*

II. *By the Publication of separate treatises on subjects of general interest.*

1. These treatises may occasionally consist of valuable memoirs, translated from foreign languages, or of articles

*The following are some of the subjects which may be embraced in the reports:

I. PHYSICAL CLASS.—1. Physics, including astronomy, natural philosophy, chemistry and meteorology. 2. Natural history, including botany, zoology, geology, &c. 3. Agriculture. 4. Application of science to arts.

II. MORAL AND POLITICAL CLASS.—5. Ethnology, including particular history, comparative philology, antiquities, &c. 6. Statistics and political economy. 7. Mental and moral philosophy. 8. A survey of the political events of the world; penal reform, &c.

III. LITERATURE AND THE FINE ARTS.—9. Modern literature. 10. The fine arts and their application to the useful arts. 11. Bibliography. 12. Obituary notices of distinguished individuals.

prepared under the direction of the Institution, or procured by offering premiums for the best exposition of a given subject.

2. The treatises should in all cases be submitted to a commission of competent judges previous to their publication.

3. As examples of these treatises, expositions may be obtained of the present state of the several branches of knowledge mentioned in the table of reports. Also of the following subjects, suggested by the Committee on Organization, viz: the statistics of labor, the productive arts of life, public instruction, &c.

SECTION II.

Plan of Organization, in accordance with the terms of the resolutions of the Board of Regents, providing for the two modes of increasing and diffusing knowledge.

1. The Act of Congress establishing the Institution contemplated the formation of a library and a museum; and the Board of Regents, including these objects in the plan of organization, resolved to divide the income into two equal parts.

2. One part to be appropriated to increase and diffuse knowledge by means of publications and researches, agreeably to the scheme before given. The other part to be appropriated to the formation of a library and a collection of objects of nature and of art.

3. These two plans are not incompatible with one another.

4. To carry out the plan before described, a library will be required, consisting, 1st, of a complete collection of the transactions and proceedings of all the learned societies in the world; 2d, of the more important current periodical publications, and other works necessary in preparing the periodical reports.

5. The Institution should make special collections, particularly of objects to verify its own publications.

6. Also a collection of instruments of research in all branches of experimental science.

7. With reference to the collection of books, other than those mentioned above, catalogues of all the different libraries in the United States should be procured, in order that the valuable books first purchased may be such as are not to be found in the United States.

8. Also catalogues of memoirs and of books in foreign libraries, and other materials, should be collected for rendering the Institution a centre of bibliographical knowledge, whence the student may be directed to any work which he may require.

9. It is believed that the collections in natural history will increase by donations as rapidly as the income of the Institution can make provision for their reception, and therefore it will seldom be necessary to purchase any articles of this kind.

10. Attempts should be made to procure for the gallery of art, casts of the most celebrated articles of ancient and modern sculpture.

11. The arts may be encouraged by providing a room free of expense for the exhibition of the objects of the Art-Union, and other similar societies.

12. A small appropriation should annually be made for models of antiquities, such as those of the remains of ancient temples, &c.

13. For the present, or until the building is fully completed, besides the Secretary, no permanent assistant will be required, except one to act as librarian.

14. The duty of the Secretary will be the general superintendence—with the advice of the Chancellor and other members of the establishment—of the literary and scientific operations of the Institution; to give to the Regents annually an account of all of the transactions; of the memoirs which have been received for publication; of the researches which have been made; and to edit, with the assistance of the librarian, the publications of the Institution.

15. The duty of the Assistant Secretary, acting as librarian, will be, for the present, to assist in taking charge of the collections, to select and purchase, under the direction of

the Secretary and a committee of the Board, books and catalogues, and to procure the information before mentioned; to give information on plans of libraries, and to assist the Secretary in editing the publications of the Institution and in the other duties of his office.

16. The Secretary and his assistants, during the session of Congress, will be required to illustrate new discoveries in science, and to exhibit new objects of art; also distinguished individuals should be invited to give lectures on subjects of general interest.

17. When the building is completed, and when in accordance with the act of Congress, the charge of the National Museum is given to the Smithsonian Institution, other assistants will be required.*

Explanation and Illustration of the Programme.

Though the leading propositions of the Programme have been fully discussed by the Board, yet it will be important to offer some remarks in explanation and illustration of them in their present connection.

That the Institution is not a national establishment, in the sense in which institutions dependent on the Government for support are so, must be evident when it is recollected that the money was not absolutely given to the United States, but intrusted to it for a special object, namely: the establishment of an institution for the benefit of men, to bear the name of the donor, and consequently to reflect upon his memory the honor of all the good which may be accomplished by means of the bequest. The operations of the Smithsonian Institution ought therefore to be mingled as little as possible with those of the Government, and its funds should be applied exclusively and faithfully to the increase and diffusion of knowledge among men.

That the bequest is intended for the benefit of men in general, and that its influence ought not to be restricted to a

* [Re-printed in Silliman's American Journal of Science, September, 1848, vol. VI (2d series), pp. 288-292.]

single district, or even nation, may be inferred not only from the words of the will, but also from the character of Smithson himself; and I beg leave to quote from a scrap of paper in his own hand the following sentiment bearing on this point: "The man of science has no country; the world is his country—all men his countrymen." The origin of the funds, the bequest of a foreigner, should also preclude the adoption of a plan which does not, in the words of Mr. Adams, "spread the benefits to be derived from the institution not only over the whole surface of this Union, but throughout the civilized world." "Mr. Smithson's reason for fixing the seat of this institution at Washington obviously was, that *there* is the seat of Government of the United States, and *there* the Congress by whose legislation, and the Executive through whose agency, the trust committed to the honor, intelligence, and good faith of the nation, is to be fulfilled." The centre of operations being permanently fixed at Washington, the character of this city for literature and science will be the more highly exalted in proportion as the influence of the Institution is more widely diffused.

That the terms *increase* and *diffusion* of knowledge are logically distinct, and should be literally interpreted with reference to the will, must be evident when we reflect that they are used in a definite sense, and not as mere synonyms, by all who are engaged in the pursuits to which Smithson devoted his life. In England there are two classes of institutions, founded on the two ideas conveyed by these terms. The Royal Society, the Astronomical, the Geological, the Statistical, the Antiquarian Societies, all have for their object the increase of knowledge; while the London Institution, the Mechanics' Institution, the Surrey Institution, the Society for the Diffusion of Religious Knowledge, the Society for the Diffusion of Useful Knowledge, are all intended to diffuse and disseminate knowledge among men. In our own country, also the same distinction is observed in the use of the terms by men of science. Our colleges, academies, and common schools, are recognized as institutions partially intended for the diffusion of knowledge, while the

express object of some of our scientific societies is the promotion of the discovery of new truths.

The will makes no restriction in favor of any particular kind of knowledge; though propositions have been frequently made for devoting the funds exclusively to the promotion of certain branches of science having more immediate application to the practical arts of life, and the adoption of these propositions has been urged on the ground of the conformity of such objects to the pursuits of Smithson; but an examination of his writings will show that he excluded from his own studies no branch of general knowledge, and that he was fully impressed with the important philosophical fact that all subjects of human thought relate to one great system of truth. To restrict therefore the operations of the Institution to a single science or art, would do injustice to the character of the donor, as well as to the cause of general knowledge. If preference is to be given to any branches of research, it should be to the higher and apparently more abstract; to the discovery of new principles rather than of isolated facts. And this is true even in a practical point of view. Agriculture would have forever remained an empirical art, had it not been for the light shed upon it by the atomic theory of chemistry; and incomparably more is to be expected as to its future advancement from the perfection of the microscope than from improvements in the ordinary instruments of husbandry.

The plan of increasing and diffusing knowledge, presented in the first section of the programme, will be found in strict accordance with the several propositions deduced from the will of Smithson, and given in the introduction. It embraces, as a leading feature, the design of interesting the greatest number of individuals in the operations of the Institution, and of spreading its influence as widely as possible. It forms an active organization, exciting all to make original researches who are gifted with the necessary power, and diffusing a kind of knowledge, now only accessible to the few, among all those who are willing to receive it. In this country, though many excel in the application of

science to the practical arts of life, few devote themselves to the continued labor and patient thought necessary to the discovery and development of new truths. The principal cause of this want of attention to original research, is the want, not of proper means, but of proper encouragement. The publication of original memoirs and periodical reports, as contemplated by the programme, will act as a powerful stimulus to the latent talent of our country, by placing in bold relief the real laborers in the field of original research, while it will afford the best materials for the use of those engaged in the diffusion of knowledge.

The advantages which will accrue from the plan of publishing the volumes of the Smithsonian Contributions to Knowledge, are various. In the first place, it will serve to render the name of the founder favorably known wherever literature and science are cultivated, and to keep it in continual remembrance with each succeeding volume, as long as knowledge is valued. A single new truth, first given to the world through these volumes, will forever stamp their character as a work of reference. The contributions will thus form the most befitting monument to perpetuate the name of one whose life was devoted to the increase of knowledge, and whose ruling passion, strong in death, prompted the noble bequest intended to facilitate the labors of others in the same pursuit.

Again, the publication of a series of volumes of original memoirs will afford to the Institution the most ready means of entering into friendly relations and correspondence with all the learned societies in the world, and of enriching its library with their current transactions and proceedings. But perhaps the most important effect of the plan will be that of giving to the world many valuable memoirs, which on account of the expense of the illustrations could not be otherwise published. Every one who adds new and important truths to the existing stock of knowledge must be of necessity to a certain degree in advance of his age. Hence the number of readers and purchasers of a work is generally in the inverse ratio of its intrinsic value; and

consequently authors of the highest rank of merit are frequently deterred from giving their productions to the world on account of the pecuniary loss to which the publication would subject them. When our lamented countryman, Bowditch, contemplated publishing his *Commentary on La Place* he assembled his family and informed them that the execution of this design would sacrifice one-third of his fortune, and that it was proper his heirs should be consulted on a subject which so nearly concerned them. The answer was worthy the children of such a father: "We value," said they, "your reputation more than your money." Fortunately in this instance the means of making such a sacrifice existed, otherwise one of the proudest monuments of American science could not have been given to the world. In the majority of cases however those who are most capable of extending human knowledge are least able to incur the expense of the publication. Wilson, the American ornithologist, states in a letter to Michaux that he has sacrificed everything to publish his work: "I have issued," he says, "six volumes and am engaged on the seventh, but as yet I have not received a single cent of the proceeds." In an address on the subject of natural history by one of our most active cultivators of this branch of knowledge we find the following remarks, which are directly in point: "Few are acquainted with the fact that from the small number of scientific works sold, and the great expense of plates, our naturalists not only are not paid for their labors but suffer pecuniary loss from their publications. Several works on different branches of zoology now in the course of publication will leave their authors losers by an aggregate of \$15,000. I do not include in this estimate works already finished—one, for instance, the best contribution to the natural history of man extant, the publication of which will occasion its accomplished author a loss of several thousand dollars. A naturalist is extremely fortunate if he can dispose of two hundred copies of an illustrated work, and the number of copies printed rarely exceeds two hundred and fifty." It may be said that these authors have their reward in the reputation which they thus purchase; but,

reputation should be the result of the talents and labor expended in the production of a work, and should not in the least depend upon the fact that the author is able to make a pecuniary sacrifice in giving the account of his discoveries to the public.

Besides the advantage to the author of having his memoir published in the *Smithsonian Contributions* free of expense, his labors will be given to the world with the stamp of approval of a commission of learned men, and his merits will be generally made known through the reports of the Institution. Though the premiums offered may be small, yet they will have considerable effect in producing original articles. Fifty or a hundred dollars awarded to the author of an original paper will in many instances suffice to supply the books, or to pay for the materials, or the manual labor required in prosecuting the research.

There is one proposition of the programme which has given rise to much discussion, and which therefore requires particular explanation. I allude to that which excludes from the contributions all papers consisting merely of unverified speculations on subjects of physical science. The object of this proposition is to obviate the endless difficulties which would occur in rejecting papers of an unphilosophical character; and though it may in some cases exclude an interesting communication, yet the strict observance of it will be found of so much practical importance that it cannot be dispensed with. It has been supposed from the adoption of this proposition that we are disposed to undervalue abstract speculations; on the contrary, we know that all the advances in true science—namely, a knowledge of the laws of phenomena—are made by provisionally adopting well-conditioned hypotheses, the product of the imagination, and subsequently verifying them by an appeal to experiment and observation. Every new hypothesis of scientific value must not only furnish an exact explanation of known facts, but must also enable us to predict in kind and quantity—the phenomena which will be exhibited under any given combination of circumstances. Thus, in the case of the undulatory hypothesis of light, it was inferred as a log-

ical consequence that if the supposition were true that light consisted of waves of an ætherial medium, then two rays of light like two waves of water under certain conditions should annihilate each other, and darkness be produced. The experiment was tried, and the anticipated result was obtained. It is this exact agreement of the deduction with the actual result of experience that constitutes the verification of an hypothesis, and which alone entitles it to the name of a theory, and to a place in the transactions of a scientific institution. It must be recollected that it is much easier to speculate than to investigate, and that very few of all the hypotheses imagined are capable of standing the test of scientific verification.

As it is not our intention to interfere with the proceedings of other institutions, but to co-operate with them so far as our respective operations are compatible, communications may be referred to learned societies for inspection, and abstracts of them given to the world through the bulletins of these societies; while the details of the memoirs and their expensive illustrations are published in the volumes of the Smithsonian Contributions. The officers of several learned societies in this country have expressed a willingness to co-operate in this way.

Since original research is the most direct way of increasing knowledge, it can scarcely be doubted that a part of the income of the bequest should be appropriated to this purpose, provided suitable persons can be found, and their labors be directed to proper objects. The number however of those who are capable of discovering scientific principles is comparatively small; like the poet, they are "born, not made," and, like him, must be left to choose their own subject, and wait the fitting time of inspiration. In case a person of this class has fallen on a vein of discovery, and is pursuing it with success, the better plan will be to grant him a small sum of money to carry on his investigations, provided they are considered worthy of assistance by competent judges. This will have the double effect of encouraging him in the pursuit, and of facilitating his progress. The Institution however need not depend upon cases of

this kind even if they were more numerous than they are, for the application of its funds in the line of original research. There are large fields of observation and experiment the cultivation of which, though it may afford no prospect of the discovery of a principle, can hardly fail to produce results of importance both in a practical and a theoretic view. As an illustration of this remark, I may mention the case of the investigations made a few years ago by a committee of the Franklin Institute of Philadelphia. The Secretary of the Treasury of the United States placed at the disposal of this society a sum of money for the purpose of making experiments with reference to the cause of the explosion of steam boilers. A committee of the society was chosen for this purpose which adopted the ingenious plan of writing to all persons in the United States engaged in the application of steam, and particularly to those who had observed the explosion of a steam-boiler. In this way opinions and suggestions in great variety as to the cause of explosions were obtained. The most plausible of these were submitted to the test of experiment; the results obtained were highly important, and are to be found favorably mentioned in every systematic work on the subject of steam which has appeared in any language within the last few years. New and important facts were established; and what was almost of as much consequence, errors which had usurped the place of truth were dethroned.

In the programme examples are given of a few subjects of original research to which the attention of the Institution may be turned. I will mention one in this place, which may deserve immediate attention. I allude to a small appropriation made annually for researches with reference to the remains of the ancient inhabitants of our country. This is a highly interesting field, and what is done in regard to it should be done quickly. Every year the progress of civilization is obliterating the ancient mounds; cities and villages are rising on the spots they have so long occupied undisturbed; and the distinctive marks of these remains are every year becoming less and less legible.

In carrying out the spirit of the plan adopted, namely

that of affecting men in general by the operations of the Institution, it is evident that the principal means of diffusing knowledge must be the *Press*. Though lectures should be given in the city in which Smithson has seen fit to direct the establishment of his Institution, yet as a plan of general diffusion of knowledge the system of lectures would be entirely inadequate; every village in our extended country would have a right to demand a share of the benefit, and the income of the Institution would be insufficient to supply a thousandth part of the demand. It is also evident that the knowledge diffused should if possible not only embrace all branches of general interest, so that each reader might find a subject suited to his taste, but also that it should differ in kind and quality from that which can be readily obtained through the cheap publications of the day. These requisites will be fully complied with in the publication of the series of reports proposed in the programme. A series of periodicals of this kind, posting up all the discoveries in science from time to time, and giving a well digested account of all the important changes in the different branches of knowledge is a desideratum in the English language. The idea is borrowed from a partial plan of this kind in operation in Sweden and Germany; and for an example of what the work should be I would refer to the annual report to the Swedish Academy of its perpetual secretary, Berzelius, on physical science. The reports can be so prepared as to be highly interesting to the general reader and at the same time of great importance to the exclusive cultivator of a particular branch of knowledge. Full references should be given in foot-notes to the page, number or volume of the work from which the information was obtained, and where a more detailed account can be found. It is scarcely necessary to remark that the preparation of these reports should be entrusted only to persons profoundly acquainted with the subjects to which they relate—namely, to those who are devoted to particular branches while they possess a knowledge of general principles. Sufficient explanations should be introduced to render the report intelligible to the general reader without

destroying its scientific character. Occasionally reports may be obtained from abroad—as for example accounts of the progress of certain branches of knowledge in foreign countries, and these may be translated if necessary and incorporated into other reports by some competent person in this country.

Besides the reports on the progress of knowledge, the programme proposes to publish occasionally brief treatises on particular subjects. There are always subjects of general interest of which brief expositions would be of much value. The preparation of these however should be intrusted to none but persons of character and reputation, and should be subjected to a revision by competent and responsible judges before they are given to the public. They may be presented in the form of reports on the existing state of knowledge relative to a given subject, and may sometimes consist of memoirs and expositions of particular branches of literature and science, translated from foreign languages. The reports and treatises of the Institution, sold at a price barely sufficient to pay the expenses of printing, will find their way into every school in our country, and will be used not as first lessons for the pupil, but as sources of reliable information for the teacher.

The second section of the programme gives, so far as they have been made out, the details of the part of the plan of organization directed by the act of Congress establishing the Institution. The two plans, namely, that of publication and original research, and that of collections of objects of nature and art, are not incompatible, and may be carried on harmoniously with each other. The only effect which they will have on one another is that of limiting the operation of each, on account of the funds given to the other. Still, with a judicious application and an economical expenditure of the income, and particularly by rigidly observing the plan of finance suggested by Dr. Bache, in the construction of the building, much good may be effected in each of the two branches of the Institution. To carry on the operations of the first a working library will be required, consisting of the past volumes and the transactions and proceedings of all the

learned societies in every language. These are the original sources from which the most important principles of the positive knowledge of our day have been drawn. We shall also require a collection of the most important current literature and science for the use of the collaborators of the reports; most of these however will be procured in the exchange for the publications of the Institution, and therefore will draw but little from the library fund.

The collections of the Institution, as far as possible, should consist of such articles as are not elsewhere to be found in this country, so that the visitors at Washington may see new objects, and the spirit of the plan be kept up, of interesting the greatest possible number of individuals. A perfect collection of all objects of nature and of art, if such could be obtained and deposited in one place, would form a museum of the highest interest; but the portion of the income of the bequest which can be devoted to the increase and maintenance of the museum will be too small to warrant any attempt toward an indiscriminate collection. It is hoped that in due time other means may be found of establishing and supporting a general collection of objects of nature and art at the seat of the General Government, with funds not derived from the Smithsonian bequest. For the present it should be the object of the Institution to confine the application of the funds, first, to such collections as will tend to facilitate the study of the memoirs which may be published in the Contributions, and to establish their correctness; secondly, to the purchase of such objects as are not generally known in this country, in the way of art and the illustration of antiquities, such as models of buildings, &c.; and thirdly, to the formation of a collection of instruments of physical research which will be required both in the illustration of new physical truths and in the scientific investigations undertaken by the Institution.

Much popular interest may be awakened in favor of the Institution at Washington by throwing the rooms of the building open on stated evenings during the session of Congress for literary and scientific assemblies, after the manner of the weekly meetings of the Royal Institution in London.

At these meetings, without the formality of a regular lecture, new truths in science may be illustrated, and new objects of art exhibited. Beside these, courses of lectures may be given on particular subjects by the officers of the Institution, or by distinguished individuals invited for the purpose. - - -

Preparations have been made for instituting various lines of physical research. Among the subjects mentioned in the programme as an example for the application of the funds of the Institution is terrestrial magnetism. I need scarcely say that this is a subject of high interest not only in a theoretical point of view, but also in its direct reference to navigation, and to the various geodetical operations of civil and military life. - - -

Another subject of research mentioned in the programme, and which has been urged upon the immediate attention of the Institution, is that of an extensive system of meteorological observations, particularly with reference to the phenomena of American storms. Of late years, in our country, more additions have been made to meteorology than to any other branch of physical science. Several important generalizations have been arrived at, and definite theories proposed, which now enable us to direct our attention with scientific precision to such points of observation as cannot fail to reward us with new and interesting results. It is proposed to organize a system of observations which shall extend as far as possible over the North American continent; and in order to effect this, it will be necessary to engage the co-operation of the British Government. - - -

The present time appears to be peculiarly auspicious for commencing an enterprise of the proposed kind. The citizens of the United States are now scattered over every part of the southern and western portion of North America, and the extended lines of telegraph will furnish a ready means of warning the more northern and eastern observers to be on the watch for the first appearance of an advancing storm.*

* [Reprinted in Silliman's American Journal of Science, November, 1848, vol. VI, pp. 305-317.]

ON HEAT, AND ON A THERMAL TELESCOPE.*

(Silliman's American Journal of Science, January, 1848, vol. v, pp. 113, 114.)

Professor Henry showed the analogy between light and heat, by stating that as two rays of *light* might be so opposed as to produce *darkness*, so two rays of *heat* might be so opposed as to produce *cold*. The facts with regard to heat as well as light therefore show that the theory of undulation is not an imagination, but the expression of a *law*. The minimum of heat, as proved by his experiments with the thermo-electric pile, does not correspond with the minimum of light. Among flames there are many which give but little light, but which give great heat; as for example the flame of hydrogen. The amount of *radiant* heat and *radiant* light were found to be about the same.

The spots on the sun are colder than the surrounding surface; and its surface is variously heated. This result he obtained by a very simple experiment of throwing the disc of the sun on a screen, and placing the very sensitive thermo-electric pile before its different parts. He had not yet concluded his experiments on the sun, and had not measured the comparative heating powers of the centre and circumference, from the results of which observations very important consequences would be drawn.

This apparatus he fitted to a common pasteboard tube, covered with gilt paper externally, and blackened internally, with which he measured the heat of distant objects. He could detect the heat of a man's face a mile off; that of a house five miles off. He thus discovered that the coldest spot of the sky is at the zenith. One day, on directing his

*[An abstract of a paper read before the Association of American Geologists and Naturalists, at its eighth annual meeting, held at Boston, September, 1847. The only published report however of the proceedings of this meeting of the Association appears to be that given in Silliman's American Journal of Science, above quoted. At this meeting, it was agreed by the body to resolve itself into the "American Association for the Advancement of Science;" and the new organization held its first meeting at Philadelphia on the following year, September 20, 1848.]

tube to a cloud, from which flashes of lightning proceeded, he was astonished to find it indicated a great degree of *cold*; he afterwards found out that a considerable quantity of hail had fallen from this cloud.

He was not satisfied with the appearances of heat supposed to have been derived from the moon. The heat that other observers have got, is probably the reflected heat of the sun, and not the moon's proper heat.

PRACTICAL OPERATIONS OF THE SMITHSONIAN INSTITUTION.

(From the Second Annual Report of the Secretary to the Board of Regents.)

December 13, 1848.

GENTLEMEN: By a resolution of the Board of Regents, at their last annual meeting, I was charged with the execution of the details of the programme which had been provisionally adopted, and was directed to report annually to the Board the progress made in the execution of the duty assigned to me. In accordance with this resolution, I present the following statement of the operations of the past year. - - -

It was recommended in my last report that the details of the plan should be adopted provisionally, and should be carried into operation gradually and cautiously, with such changes, from time to time, as experience might dictate. The Institution is not one of a day, but is designed to endure as long as our Government shall exist; and it is therefore peculiarly important that in the beginning we should proceed carefully, and not attempt to produce immediate effects at the expense of permanent usefulness. The process of increasing knowledge is an extremely slow one, and the value of the results of this part of the plan cannot be properly realized until some years have elapsed. - - -

In the publication of the first volume of the Contributions, the question occurred as to the propriety of securing the copyright to the Institution. I had not an opportunity of conferring with the Executive Committee on this point, and was therefore obliged to settle it on my own responsibility.

I concluded that it would be more in accordance with the spirit of the Institution to decide against the copyright. The knowledge which the Smithsonian Institution may be instrumental in presenting to the world should be free to all who are capable of using it. The re-publication of our papers ought to be considered as an evidence of their importance, and should be encouraged rather than prohibited. - - -

An appropriation of one thousand dollars was made at the last meeting of the Board, for the commencement of a series of meteorological observations, particularly with reference to the phenomena of American storms.

It is contemplated to establish three classes of observers among those who are disposed to join in this enterprise. One class, without instruments, to observe the face of the sky as to its clearness, the extent of cloud, the direction and force of wind, the beginning and ending of rain, snow, &c. A second class, furnished with thermometers, who, besides making the observations above mentioned, will record variations of temperature. The third class, furnished with full sets of instruments, to observe all the elements at present deemed important in the science of meteorology. It is believed that much valuable information may be obtained in this way with reference to the extent, duration, and passage of storms over the country, though the observer may be possessed of no other apparatus than a simple wind-vane.

As a part of the system of meteorology, it is proposed to employ, as far as our funds will permit, the magnetic telegraph in the investigation of atmospherical phenomena. By this means, not only notice of the approach of a storm may be given to distant observers, but also attention may be directed to particular phenomena, which can only be properly studied by the simultaneous observations of persons widely separated from each other. For example, the several phases presented by a thunder storm, or by the aurora borealis, may be telegraphed to a distance, and the synchronous appearances compared and recorded in stations far removed from each other. Also, by the same means, a single observatory, at which constant observations are made during

the whole twenty-four hours, may give notice to all persons along the telegraphic lines, of the occurrence of interesting meteorological phenomena, and thus simultaneous observations be secured. The advantage to agriculture and commerce to be derived from a knowledge of the approach of a storm, by means of the telegraph, has been frequently referred to of late in the public journals. And this, we think, is a subject deserving the attention of the General Government. - - -

Under the head of original researches, I may recall to the attention of the Regents the fact of my having been directed to continue my own investigations on physical science, and to report occasionally to the Board my progress therein. In the course of last year, I found an opportunity while at Princeton to commence a series of investigations on radiant heat, which apparently produced some results of interest, but which my subsequent engagements have prevented me from fully developing. I was also directed to cause to be made a series of experiments on the economical value of building material. - - -

The Smithsonian Contributions are intended to consist of entirely original additions to the sum of human knowledge, and are to be principally exchanged for the transactions of learned societies, and to be distributed among public institutions. The reports, on the other hand, are to be of a more popular kind, and are intended for as wide a distribution as the funds of the Institution, or the means of publishing them may permit. They will give an account of the progress of the different branches of knowledge in every part of the world, and will supply a desideratum in English literature.

The objects of the Smithsonian Institution are not educational. The press in our country already teems with elementary works on the different branches of knowledge, and to expend our funds in adding to these, would be to dissipate them without perceptible effect.

ON THE AURORA BOREALIS.*

(Proceedings American Association, Adv. of Science, vol. II, pp. 11, 12.)

August 14, 1849.

Professor Henry said: The paper of Professor Secchi seems to me to be one of considerable interest. It contains a number of ingenious suggestions, which may lead to new results. One fact alluded to in this paper is highly important, and though taken for granted since the days of Franklin, has only lately been fully established. I allude to the connection of the Aurora with electricity. Besides the observation mentioned in the preceding paper, I am informed by Mr. Herrick, of New Haven, that an electrical action had been observed at that place on the wires of the telegraph at the time of the appearance of the Aurora. The same fact has also been observed in England and on the continent, during the last year. It is highly desirable to ascertain whether this action is one of actual transfer of electricity from the space at one end of the wire to that at the other, or whether it is an inductive action of the Aurora at a distance, disturbing for an instant the electrical equilibrium of the wire. This could be readily determined by the character of the action on the needle of a galvanometer.

There was an Aurora last night visible at this place, which exhibited some peculiarities not frequently observed, (so far as I am informed) in this latitude. These were pointed out to me by Dr. A. D. Bache, and are similar in a degree to the appearances observed in Siberia. The Aurora, in these high latitudes, frequently presents the appearance of a number of concentric scrolls or curtains, the general axis of which is parallel to the dipping-needle. The Aurora of last night consisted, while we were observing it, of a number of parallel beams which together formed the skeleton of an arch with an irregular curtain border at the lower edge.

I may mention to the Association that the Smithsonian Institution, in connection with an extended system of meteor-

*[Remarks on a communication by Professor Angelo Secchi, of Georgetown College, D. C., to the Association, on "The Aurora Borealis."]

ology which it has undertaken to establish, has issued directions for the observation of the Aurora. These directions are similar to a set issued by the directors of the observatory at Toronto, for observers in Canada. The observations made in the two countries will thus form one extended system. The proprietors of the several telegraph lines have offered to grant us the use of their wires for meteorological purposes, and it is hoped when the lines are completed, and we have established a set of observers, extending, for example, from Toronto to Washington, or even farther south, we shall be able to study the phenomenon of the Aurora with more precision than it has ever been studied. On a long line extending north and south, the observer, for example, at Toronto, having noticed an Aurora, may call the attention to it of all the observers along the line, and thus the extent of the visibility, and the simultaneous appearance of any peculiar phase of the meteor, may be readily determined.

ON THE DIFFUSION OF VAPOR.*

(Proceedings American Association, Adv. of Science, vol. II, pp. 127, 128.)

August 16, 1849.

Professor Henry remarked that he was much interested in the experiment of Professor Horsford, in which vapor was shown to pass through a tube filled with air. It is well known that, according to the theory of Dalton, air and vapor are vacuums to each other. This theory is certainly in accordance with all the statical phenomena of the diffusion of vapor, but does not as well represent the dynamic effects. So great is the resistance to diffusion through a narrow tube, that Professor Espy has concluded that the theory is incorrect, and that diffusion of vapor cannot take place without the aid of a current of air. Professor Horsford's experiment proves that a diffusion does take place through a tube, but in this case the force of diffusion may be considered a maximum.

*[Remarks on a communication by Professor E. N. Horsford, to the Association, "On the Moisture, Ammonia, and Organic Matter of the Atmosphere."]

If the force is much less, the effect does not take place. Several years ago I placed a small quantity of water in a retort, and joined the beak of this to the open beak of another retort filled with air. The retort containing water was placed within a room kept constantly at a mean temperature of about 65° , while the body of the other retort was without a window, and constantly at a mean temperature of not more than 40° . Though the apparatus was suffered to remain thus, during a whole winter, not a single drop of water passed over. The force of diffusion due to the difference of tension in the two retorts was in this case too small to overcome the resistance of the atoms to a passage between each other.

ON THE RADIATION OF HEAT.

(Proceedings of the American Philosophical Society, vol. v, p. 108.)

October 19, 1849.

Professor Henry communicated some experiments which he had made upon the subject of the radiation of heat. It occurred to him, from the constitution of the atmosphere, that if the air were a good radiator of heat, the higher temperatures below and the lower above could not be permanent. By placing a thermo-multiplier before a flame, interposing a screen of wood with a hole through it, radiation from the flame was perceived becoming less as the flame was lowered, and still existing, though in small quantities, from the heated air above the flame. He also repeated the experiments upon the radiation of heat from flames. The radiation of heat from the flame of hydrogen is but small, as is its radiation of light. This radiation is much increased by placing a solid in the flame. This is in accordance with Count Rumford's assertion, that clay balls placed in the fire increased the amount of heat.

Professor Henry also mentioned some experiments which he had made some years ago upon the reflection of heat from ice with a concave mirror of that substance.

ON THE EXPANSIVE ENERGY OF LIGHTNING STROKES.*

(Proceedings American Association, Adv. of Science, vol. iv, pp. 7, 10.)

August 19, 1850.

Professor Henry mentioned an instance of an explosion during the passage of an electrical discharge through a house, from which fact he had been led to the same conclusions on this point with Professor Olmsted. He had himself made a series of experiments to ascertain whether the hypothesis is true or not. The results were attained by means of Kinnersley's air thermometer. His investigations convinced him that the effect is due to a sudden repulsive energy imparted to the air. He cited several instances—some of which were noticed by himself at Princeton, where the roof of one house was blown off, and the side of another blown out. He considered that the great mechanical effects of an electrical discharge are due in most cases to an expansive or repulsive power in the air. He had made some interesting experiments in galvanism, the effects of which he referred to the same cause. - - -

Professor Henry mentioned instances where ordinary electrical discharges had affected a circle of twenty miles in diameter. By means of an apparatus simply constructed for the occasion, he had succeeded in magnetizing a needle by a flash of lightning so far off that he could not hear the thunder. He explained the apparatus. He considered that every flash of such electricity produces effects to great distances, and may perhaps affect half the globe.

*[Remarks on a communication, by Professor Dennison Olmsted, of Yale College, to the Association, on "Notes of some points of Electrical Theory." Professor Olmsted illustrated by several observations the expansive energy of lightning strokes, and also the occurrence of the lateral discharge from good conductors well connected with the earth.]

ON THE FORMS OF LIGHTNING-RODS.*

(Proceedings American Association, Adv. of Science, vol. IV, pp. 39-42.)

August 20, 1850.

Professor Henry said the question of balls and points had not been fully settled. If electricity acts inversely as the square of the distance, then on the principle of central forces, the induction on a sphere at a distance from the cloud would be the same as if all the matter of the sphere were concentrated in its centre, and consequently the attraction of the ball or sphere on the electricity of the distant cloud would be the same as that of a point. When however the inducing body, or the discharge itself, came near the rod, it would be much more strongly deflected by the point than by the ball, because the former would be electrified by induction to a much greater degree of intensity, for the same amount of electricity which would be diffused over the surface of the ball would be condensed in the point, and hence it would tend to rupture the air, and thus give a more easy passage to the discharge.

His attention had been directed to the action on a ball, by the fixture on the dome of the Capitol at Washington, of a lantern, terminated by a ball. This apparatus had been erected at a great expense, for the purpose of lighting the public grounds. It consisted of a mast reaching to the height of ninety feet above the apex of the dome of the Capitol, terminated by a lantern about five feet in diameter and six or seven feet high. In this were jet gas burners, equal in illuminating power, according to the statement of the projector of the arrangement, to six thousand wax candles.

After the whole apparatus had been prepared, the speaker was requested to give an opinion as to the effect which the lightning might have upon it. His answer was, that

* [Remarks on a communication by Professor Elias Loomis, to the Association, "On the proper height of Lightning-rods;" in which a reference was made to the question of single or multiple points to the rod.]

it would attract the lightning from the heavens, and though the building might be protected by good conductors from the lantern to the earth, yet no protection which the present state of science could devise would be as safe as no exposure; the very idea of protection involving that of a less degree of danger. Though in the case of the ordinary lightning-rod the lightning is seldom or never attracted from the cloud by the conductor, yet in this case the great height of the mast, the height of the dome above the ground, and the elevated position of the building itself, gave a total elevation bearing a considerable ratio to the height of the cloud: add to this, the great amount of metallic surface, and, above all, the large gas burner, and we have an arrangement well calculated to elicit a discharge from the cloud, when under ordinary influences no effect of the kind would take place. It is well known to the Section that the best apparatus for collecting atmospheric electricity is a long pole, with a wire along it, and a lantern at the upper end. The fixture on the Capitol was indeed an exploring apparatus on a magnificent scale. The result was such as had been anticipated. The first thunder-storm which passed over the city after the erection of the lantern, discharged itself upon it, put out the light, and when the whole was taken down, several perforations were found melted in the copper ball which surmounted the lantern.

In this case the induction from the cloud took place over the whole surface of the lantern, and the attraction was in proportion to the number of particles in the surface of the metal. The principal action was however due to the stream of heated air from the burning gas. - - -

[Professor Olmstead mentioned the case of several inverted tin pans placed in a straight line on a bench in the path of the electric discharge, and that they were perforated on opposite sides as if by a bullet.]

Professor Henry thought the phenomenon was in accordance with known electrical action. If a number of conductors are placed in succession in the path of a discharge, the end of the first, to which the lightning is passing, will be-

come highly negative, while the other end of the same conductor must be highly positive; also, the first end of the second conductor will be negative, and the other end positive, and so on. The lightning therefore will enter the metal with much greater intensity than that with which it will pass along the conductor; and hence a hole may be melted at the point of entrance; for the same reason another hole might be expected at the point of exit, and in this way the perforations of the pans might be explained. The electricity did not pass through the space from side to side of the pan, as a bullet would have done, but took the circuit around the inverted bottom of the vessel.

He stated that in all cases when an electrical discharge passes through a conductor, the point at which the fluid enters, and that at which it passes out, are both marked with evidence of more intense action.

When a disruptive discharge takes place through the air between two conductors, in many cases a part of the matter of each conductor is transferred to the other. Professor Henry said that he had received accounts from different sources of a remarkable phenomenon connected with this action. In the case of a person killed many years ago by lightning, while standing near to the whitewashed wall of a room, the discharge took place between his body and the wall, and on the latter was depicted, in dark color, an image of his person. Other cases of the same kind had been observed.

ON THE PHENOMENA OF THE LEYDEN JAR.

(Proceedings American Association, Adv. of Science, vol. iv, pp. 377, 378.)

August 24, 1850.

Professor Henry gave an account of his investigation of the discharge of a Leyden jar. This was a part of a series of experiments he had made a few years ago on the general subject of the dynamic phenomena of ordinary or frictional electricity. On this subject he had made several thousand experiments. He had never published these in full, but had given brief notices of some of them in the Proceedings of

the American Philosophical Society. All the complex phenomena he had observed could be referred to a series of oscillations in the discharge of the jar. If we adopt the hypothesis of a single fluid, then we shall be obliged to admit that the equilibrium of the fluid after a discharge takes place by a series of oscillations, gradually diminishing in intensity and magnitude. He had been enabled to show effects from five of these waves in succession. The means used for determining the existence of these waves was that of the magnetization of steel needles, introduced into the axis of a spiral. A needle of this kind it is well known is susceptible of receiving a definite amount of magnetism, which is called its saturation. Now if the needle be of such a size as to be magnetized to saturation by the principal discharge, it will come out of the spiral magnetized to a less degree than that of saturation, by the amount of the adverse influence of the oscillations in the opposite direction to that of the principal discharge. If the quantity of electricity be increased, the power of the second wave may be so exalted that the needle will exhibit no magnetism; the whole effect of the first or principal wave will be neutralized by the action of the second. If the quantity of electricity be greater than this, then the needle will be magnetized in an opposite direction. If the electricity be still more increased, the needle will again exhibit a change in its polarity, and so on in succession, as the power of the successive waves is increased.

These experiments had been made several years ago, but he had not given them in detail to the public, because he had wished to render them more perfect. For the last three and a half years all his time and all his thought had been given to the details of the business of the Smithsonian Institution. He had been obliged to withdraw himself entirely from scientific research; but he hoped—now the Institution had got under way, and the Regents had allowed him some able assistants,—that he would be enabled, in part at least, to return to his first love—the investigation of the phenomena of nature.

ON THE LIMIT OF PERCEPTIBILITY OF A DIRECT AND REFLECTED SOUND.

(Proceedings American Association Adv. of Science, vol. v, pp. 42, 43.)

May, 6, 1851.

Professor Henry stated that at the meeting of the Association at Cambridge* he had made a communication relative to the application of the principles of acoustics to the construction of rooms intended for public speaking. In that communication he had stated, as an important proposition, that when two portions of the same sonorous wave reach the ear of an auditor,—one directly from the origin of the sound, and the other indirectly,—after one or more reflections, if the two do not differ in the paths they travel by a difference greater than a given quantity, the two sounds will re-enforce each other, and one louder sound will be perceived. If however the interval is greater than a certain limit, the two sounds will appear distinct, or an echo will be perceived.

As an illustration, suppose a speaker to stand before a wall at the distance of say ten feet: in this case the audience in front would hear but one sound. The direct and the reflected impulse meet the ear within the limit which he has called the limit of perceptibility. This limit—a knowledge of which is of considerable practical importance—may either be expressed in time or in space. The simplest method of obtaining its amount is that of clapping the hands, while standing before a perpendicular wall; if the distance of the observer be sufficient, an echo will be heard. If in this case the observer gradually approach the wall and continue to make the sound, at a definite point the echo will cease to be perceived, and the two sounds will appear as one. If the distance from the wall be now measured, twice the distance found will give the limit of perceptibility in space. If the same quantity be divided into the space through which the

*[This communication, made August 21, 1849, was reported by title only.—*Proceed. Am. Assoc.*, vol. II, p. 432.]

wave of sound is known to travel in a second, we shall have the limit of perceptibility in time.

The foregoing plan is the most simple—but not the most accurate—method of arriving at the quantity sought. The better plan is to employ another person to produce the sound, while the observer is stationary at the distance—at least 150 feet from the wall. The person who produces the sound being placed between the observer and the wall, at such a distance from the latter as to give a distinct echo, he is then directed gradually to approach the wall until the echo and the direct sound become one. The distance measured, as before mentioned, will give the limit required.

From a series of experiments on this plan, he found the limit of perceptibility to vary from about 60 to 80 feet, or in other words, the distance from the wall at which the echo ceased was from 30 to 40 feet. This will give from the $\frac{1}{20}$ to the $\frac{1}{15}$ part of a second, in time, for the ear to distinguish the difference of two successive sounds.

The experiments, when made under the same circumstances, gave the same result, almost within a single foot; but when a different source of sound was employed and different observers, there was observed a difference of results, giving the limits between $\frac{1}{20}$ and $\frac{1}{15}$ of a second. The limit was less with a sound produced by an instrument which gave a sudden crack, without perceivable prolongation, such as is produced by an ordinary watchman's rattle when made to emit a single crack. This difference may be explained by taking into consideration the actual length of the sonorous wave. If a sound occupies $\frac{1}{4}$ of a second, (which is about the time required for the utterance of a short, single syllable,) the length of this sonorous wave will be about 300 feet, and hence, when the distance travelled by the two sounds is not more than 80 feet to and from the wall, the two waves must overlap through a considerable portion of their whole length, and will be only separated at the two extremities. The portion of over-lapping may therefore determine the limit of perceptibility, and this again is combined with the fact of the continuance of a sonorous impression on the nerve of the ear.

ON THE THEORY OF THE SO-CALLED IMPONDERABLES.

(Proceedings American Association Adv. of Science, vol. VI, pp. 84-91.)

August 21, 1851.

Professor Henry said: In studying the phenomena of matter we commence with observing the action of masses upon each other, and from this we deduce laws. These, with regard to mechanical philosophy, are five in number, viz., the two laws of force, attraction and repulsion, varying with some function of the distance; and secondly, the three laws of motion, viz., the law of inertia, of the co-existence of motions, and of action and re-action. Of these laws we can give no explanation; they are at present considered as ultimate facts to which all mechanical phenomena are referred, or from which they are deduced by logical inference. The existence of these laws, as has been said, is deduced from the phenomena of the operations of matter in masses; but we apply them by analogy to the minute and invisible portions of matter which constitute the atoms or molecules of gases, and we find that the inferences from this assumption are borne out by the results of experience.

Indeed, the minutest portions of matter must be endowed with properties analogous to masses of the same kind of matter. An attempt has however been made by Boscovich to refer all the mechanical properties of matter to portions of space, filled with associated points, endowed with attracting and repelling forces, varying and alternating with changes of distances. In a communication to the American Philosophical Society,* I have shown that this hypothesis, which is at the present time adopted by many, is insufficient to explain all the facts. Matter thus constituted would indeed exhibit the phenomena of elasticity, compressibility, porosity, affinity, etc.; but it would not exhibit an obedience to the three laws of motion, namely, inertia, the co-existence or composition of motions, and action and re-action. We must

*[November 6, 1846. See *ante*, p. 255.]

therefore superadd to the hypothetical points of Boscovich these other conditions; but in so doing we arrive at a constitution of matter precisely similar to that adopted by Newton, namely, a system of indivisible and indestructible atoms endowed with the essential properties of matter in masses. Indeed, this is the only hypothesis which we can adopt in strict accordance with analogy, reasoning from the known to the unknown.

Besides the phenomena of the action of invisible atoms of gases on each other we have a large class known under the general name of the phenomena of the "imponderables." This name has been given because it is supposed that it is necessary to refer them to hypothetical fluids not subjected to the ordinary laws of force and motion. The term *imponderable* however expresses a quality with reference to the constitution of such fluids not warranted by the facts. A mass of air poised in air has no weight, and in this case may be considered imponderable. In the same way, if we suppose an elastic medium to pervade all space, any portion of this will be imponderable, even were our balances sufficiently delicate to detect its absolute weight. The existence of an elastic medium pervading all space is assumed in order that the phenomena of light, heat, electricity, and magnetism may be brought within the category of the laws of force and motion, and that we may be able to apply the principles of analytical mechanics in the way of deducing consequences to be afterwards tested by an actual appeal to experiment. Without assumptions of this kind it is impossible to arrive at the general expressions which constitute science in the proper sense of the term.

It is not necessary that an hypothesis be absolutely true in order that it may be adopted as an expression for a generalization for the purpose of explaining and predicting new phenomena; it is only necessary that it should be well conditioned in accordance with known mechanical principles. We have a remarkable instance of this in the Newtonian theory of emission of light. According to this, light is first considered as consisting of atoms of matter moving with

immense velocity, but subject to mechanical laws. The inference from this assumption is, that meeting obliquely a reflecting surface the atoms will rebound as would a perfectly elastic ball, making the angle of incidence equal to the angle of reflection. This fact being established by experiment, all the phenomena of reflected light are deduced mathematically as mechanical consequences from the primary assumption. Again, it is discovered that a ray of light, in entering obliquely a new medium, changes its direction; and this is readily explained by adding to the previous hypothesis the second condition, that the atoms of light, like all other matter, are subject to attraction, and that they are, in consequence of this, accelerated or retarded in velocity at the moment of entering the new medium. From this assumption readily flows the law of the permanency of the ratio of the sine of the angle of refraction to that of incidence.

In the progress of discovery it is further found that a ray of light is separated into different colors; and in order to explain this agreeably to the same analogies, we are obliged to admit that there are different kinds of atoms of light, with different properties, and moving with different velocities. Further, it is discovered that light, in passing by the edges of different bodies, produces fringes and other phenomena known by the name of diffraction. To explain these another supplementary hypothesis must be added, namely, that the atoms of light are alternately attracted and repelled by the variation in their distance from the solid body near which they pass. Another class of phenomena, denominated by Newton "fits of easy refraction and easy reflection," induce the assumption that the atoms of light are not homogeneous in property on all sides, but that each possesses an attracting and repelling pole; and that in their passage through space they are constantly revolving on axes perpendicular to the line joining their poles. Again, the discovery of Malus requires another supplementary hypothesis in order to a mechanical conception of the phenomena first observed by him. To explain these we must admit that the atoms of light possess different properties on different sides, in addition

to different properties at different ends. But now the original theory of emission, at first a simple mechanical conception, becomes so loaded with supplementary hypotheses that as a whole it is unwieldy, and we are induced to look for some other possible hypothesis which shall equally well connect the phenomena in accordance with known mechanical principles, and not be subject to the same charge of complexity. Such an assumption is found in the present received undulatory theory of light.

In reviewing the foregoing sketch of the rise, growth, and abandonment of the theory of emission we see that an hypothesis, though not absolutely true, may serve an important purpose in the way of the definite conception of old phenomena, and in the discovery and prediction of new; and indeed in some cases, paradoxical as it may appear, a false hypothesis, from its ease of application, may be of more use than one which is absolutely true. Man, with his finite faculties, cannot hope in this life to arrive at a knowledge of absolute truth; and were the true theory of the universe, or in other words, the precise mode in which Divine Wisdom operates in producing the phenomena of the material world, revealed to him, his mind would be unfitted for its reception; it would be too simple in its expression, and too general in its application, to be understood and applied by intellects like ours.

It may be asked why theories, so apparently different as those of emission and undulation, should both lead to the discovery of new truths? The answer is that the former is involved in the latter, and that all the supplementary hypotheses we have mentioned have their representation in the different phases of wave-motion. Thus an undulation is reflected in the same manner as an elastic ball; a change in velocity also takes place in the undulation on entering a new medium; and the fits of Newton are represented by lengths of waves, and the polarization of Malus by transverse vibrations reduced to the same or parallel planes. The undulatory theory is a more general expression, and contains truths which are not to be logically deduced from the theory of

emission. In order however that this theory may enable us to discover the greatest number of new phenomena, and assist us in ascertaining the more precise relations of known facts, it is necessary that all its parts should be definitely conditioned with reference to established mechanical principles. The phenomena of light and heat, and of chemical and phosphorogenic emanation from the sun, by strict analogy lead us to infer that something possessing inertia, and obedience generally to the laws of force and motion, must exist between us and this luminary. All the phenomena are best explained and predicted by supposing this something to consist of an elastic medium, the atoms of which, in a normal state, are distributed uniformly through space and retained in position by attracting and repelling forces. An ætherial medium, constituted in this manner, will admit of vibrations of different characters and of different forms; for example, if an impulse be given to an atom in a given direction, it will cause in succession a motion to be transmitted to the series of atoms which are found in the same line, and thus longitudinal undulations will be produced; also the motion of the atom to which the impulse is given will cause it to approach the atoms of the medium on the sides of the line just mentioned, and thus rows of atoms on all sides of the first row will be thrown into a state of transverse vibration. Similar systems of vibrations must also take place in air; but such is the constitution of the human ear that it takes cognizance only of longitudinal vibrations, and such the function of the human eye that it is only affected by transverse undulations. Besides these there may be other vibrations compound of the two; and in this way other emanations than those which have yet been observed may be conceived to exist.

The science of electricity, as left by Cavendish and Æpinus, and as expounded by Haüy and Robison, was (next to astronomy) one of the most perfect of the physical sciences. All the known phenomena of statical electricity were referred to the mechanical action of two species of matter; the atoms of each being self-repellant and attractive of the atoms of the

other ; one of these is called the electrical fluid, and the other ordinary matter. For the generalization of the same phenomena Dufay assumed three principles, two species of electrical, and one of ordinary matter. From either of these mechanical conceptions, could be deduced all the facts then known.

It would appear however that the tendency of the present day is to the accumulation of facts rather than to their critical examination, or the discovery of general expressions by which to represent them. Electricity and magnetism at the present time consist of almost a chaos of isolated phenomena which can scarcely be called scientific. Most of these however, I am convinced, are capable of being referred to the theory of Franklin or to that of Dufay, with the addition of a few supplementary hypotheses analogous to those which we have seen were added to the theory of emission. For example, we shall be obliged to admit that in some cases inductive effects are propagated wave-fashion ; and in others, that a change in the condition of the ponderable matter plays an important part. Thus, as I mentioned at the last meeting of the Association, I have found that in the discharge of a Leyden jar through a metallic wire a series of rebounds between the inside and the outside of the jar takes place precisely in the same way as the equilibrium would be restored by a series of waves were a quantity of air, condensed in one vessel, suffered to discharge itself into another in which a vacuum previously existed. I have also shown that during this discharge a series of inductions take place, extending to a surprising distance on all sides of the wire ; and as these are the results of currents in alternate directions, they must produce in surrounding space a series of plus and minus motions analogous to if not identical with undulations.

Next, that a change in the condition of the matter itself is required for the explanation of certain phenomena will be evident from the following experiment : If portions of the same current of galvanism be sent through two parallel wires, or if portions of the same discharge from a Leyden jar be transmitted simultaneously through two parallel strips of

platina foil, an attraction in both cases will be exhibited. If however the surface of a large circular metallic plate be covered at intervals with short needles placed parallel to each other, and a discharge of electricity be sent along the diameter of the circle at right angles to the needles, on examination, they will be found magnetized with different degrees of intensity. Those in the direct line of the discharge will exhibit a slight degree of polarity, while those at the circumference of the plate will show a much greater amount of magnetic force; proving that the electrical discharge, instead of passing in the shortest line between the two points, has divided itself into two portions, each passing at as wide a distance as possible from the other. This phenomenon is in strict accordance with the hypothesis that the plate has been traversed by an elastic fluid, the particles of which, being self-repellant, have separated as far as possible from each other; and it can therefore be referred to the action of a fluid co-existing with, but independent of, ordinary matter; while the phenomenon of the attraction of the two parallel conductors before mentioned can only be explained by a change in the condition of the gross matter itself combined perhaps with the action of an elastic fluid. I ought to state in this place that my friend Dr. Hare, from purely theoretical considerations, independent of experiment, has arrived at a similar conclusion.

There is another phenomenon which I may mention as producing a change in the properties of matter during the instantaneous passage of an electrical discharge. At the moment of the passage through the atmosphere of a discharge of electricity, the particles of the air are suddenly endowed with a surprisingly energetic repulsive tendency, to which is mainly to be attributed the mechanical effects produced by a discharge of lightning passing through a building. Also in the development of magnetism in a bar of iron or steel a change takes place in the ponderable molecules of the metal; this is evident from the fact that at the moment of magnetization a wave of undulation, capable of producing an audible sound, is transmitted along the bar; and again,

when the iron is de-magnetized, (if the expression may be allowed,) a similar change in the position of the molecules is indicated.

In the explanation of the statical phenomena of electricity we may either adopt the hypothesis of one or of two fluids, the mechanical results which are logically deduced from either being the same; in the case of the former we have one movable and one fixed principle; in that of the latter we have two movable fluids and a fixed medium. It is evident that the mechanical results will be the same in the two theories, provided we suppose the absolute motion of the one fluid to be equivalent to the sum of the motions of the two fluids. Though either theory may be adopted with reference to the statical phenomena, the theory of one fluid is more readily applicable to the facts connected with electricity in motion, and particularly that part of the theory which assumes the activity of ordinary matter may hereafter be fruitful in new deductions.

The discoveries of the last few years have tended more and more to show the intimate connection of all the phenomena of the "imponderables;" and indeed we cannot avoid the conclusion, forced upon us by legitimate analogy, that they all result from the different actions of one all-pervading principle. Take, for illustration, the following example of the development of the several classes of phenomena. An iron rod rapidly hammered becomes red hot, or in other words, emits heat and light. The same rod insulated by a non-conductor and struck with another non-conductor exhibits electrical attraction and repulsion. Again, if this rod be struck with a hammer while in a vertical position it becomes magnetic. We have here the evolution of the four classes of phenomena by a simple agitation of the atoms. We cannot, in accordance with the known simplicity of the operations of nature, for a moment imagine that these different results are to be referred to as many different and independent principles.

If we refer all these phenomena to one elastic medium it will be necessary, in order to explain the facts of electricity

and magnetism, that we suppose this medium to be capable of accumulation or condensation in certain portions of space, and of being lessened in quantity or rarefied in other portions; also, that in its return to its normal condition an actual transfer of the medium takes place. It follows from these assumptions that the fluid withdrawn from one portion of space must leave an equivalent deficiency in another, or in other words, that the amount of positive action must be equal in all cases to that of the negative. Further, since it appears from observation that the ætherial medium can only be condensed or accumulated in certain places by the insulating powers of ordinary matter, no electrical phenomena can be exhibited except in connection with such matter; hence electrical action cannot be expected in the regions of celestial space.

The most difficult phenomena for which to invent a plausible mechanical explanation, connected with this subject, are those of the attraction of the two wires transmitting a current of electricity, and the transverse action of a galvanic wire on a magnetic needle. The theory of Ampère, though an admirable expression of a generalization of the phenomena of electromagnetism, is wanting in that strict analogy with known mechanical actions which is desirable in a theory intended to explain phenomena of this kind.

In conclusion I would again revert to the importance, in the adoption of mechanical hypotheses, of conditioning them in strict accordance with the operations of matter under the known laws of force and motion as exhibited in time and space.

THE IMPROVEMENT OF THE MECHANICAL ARTS.

CLOSING ADDRESS AT THE EXHIBITION OF THE METROPOLITAN MECHANICS' INSTITUTE, OF WASHINGTON.

[From a pamphlet edition, published by the M. M. Institute, in 1853.]

Delivered March 19, 1853.

At the close of the Exhibition of the Metropolitan Mechanics' Institute it becomes my duty to offer some remarks relative to the objects and organization of the association; and in addition to these, I shall beg leave to call your attention to some points which present themselves more prominently to my mind, amidst the extended field of the history of mechanical inventions.

The object of this Institute is twofold: first, the improvement of mechanics and artists; second, the improvement of arts and inventions.

These two objects are inseparably connected, and the one necessarily follows as a consequence of the other. Whatever tends to develop the mind of the workman tends to advance the condition of his art. Every material operation and every invention is founded on some law of nature, and the more intimately the operator is acquainted with the principles of his art the better is he fitted to improve it. Without a knowledge of science the practice of art is mere empiricism, often involving operations which are not only unnecessary to the production of the desired result, but frequently detrimental.

The savage who recovers his health after drinking from a mineral spring considers his cure due not alone to the efficacy of the water, but also to the position of his body at the time of drinking, whether facing the east or the west, to the number of draughts, and perhaps in some cases he deems it necessary previously to propitiate the spirit of the fountain by a sacrifice of some object of value. We need not go to savage life for examples of this kind. In many parts of our own country—even among men otherwise intelligent—certain mechanical and agricultural operations are connected

with superstitious observances of the most ridiculous and inconvenient character. A knowledge of principles serves to eliminate these errors, to point out the necessary and essential conditions of practice, and to facilitate the introduction of improved methods. On the other hand, every improvement in the mechanic arts, as a general rule, tends to elevate the character of the artisan, and to render his employment more intellectual.

It is proposed in this Institute to improve the workman by lectures, collections of specimens of natural history, a library and a reading-room, and to advance the arts by exhibitions and by the examination of such new inventions as may be submitted to the judgment of the Institute. For conducting this part of the plan of operation a permanent Committee of Science and Arts will be formed, combining (among its members) theoretical knowledge with practical skill.

There is no place in the United States (taking in view the number of its mechanics) so well adapted to support an efficient institution of this kind as the city of Washington. It offers numerous examples of ingenious inventions and processes. The models of the Patent Office, the instruments of the Observatory, of the Coast Survey, of the Topographical Bureau; the processes of the navy-yard and of the arsenal are illustrations of the useful arts readily accessible and of the most instructive kind. Moreover, no city of the Union of the same size can command so large a number of scientific men, namely, those belonging to the army and the navy, and the institutions established here. Any association which tends to bring these into harmonious co-operation with the practical mechanics of the place may, and I doubt not will, be productive of important results.

The Institute has commenced its existence under very auspicious circumstances, and has found favor with the wise and the liberal. Notwithstanding this, the enterprise is not unfraught with danger, and those who were instrumental in establishing it assumed a responsibility of no small weight. They evoked a power which may be determined on good or

on evil; which, while it is capable of conferring blessings on this city and this country, may be the means of propagating error, and of administering to the selfish ends of designing men. There is no city in which a society of this kind requires to be more strictly guarded against baneful influences. The partisan politician may attempt to make it the stepping-stone of his political advancement. The pseudo-inventor, who seeks to enrich himself by pirating the labors of humble and unobtrusive genius, and the speculator, who wishes to impress Congress with the importance to the country and to the world, of a scheme intended to benefit himself alone, will be untiring in their endeavors to obtain the certificates and recommendations of the Institute. They will approach its judges and its committees with soft words and insinuating manners, and will not hesitate to offer bribes in such sophistical terms that, while cupidity is excited, the conscience is lulled to rest.

The location of this Institute at the seat of government of this vast Union will turn all eyes upon it, and will consequently tend to give it corresponding power and influence. But it must be recollected that in proportion to the conspicuousness of the position occupied by institutions or individuals is their responsibility to society increased. The higher they stand the more secure must be the principles on which they are supported. When men build upon a false foundation, the greater their elevation the more certain is their fall.

There is no place in this country where *motives* and *acts* are more critically examined than in the city of Washington. There is none in which *capacity*, *honesty of purpose*, and a *prudent, straightforward course* are more necessary to continued success, and none in which *deviations from right*, whether intentional or otherwise, are more readily detected and exposed.

The mere organization of the Institute, however well it may have been done, is not sufficient for its perpetuity and usefulness. It requires the constant application of individual effort to sustain it; the unwearied labor of a few master spirits to infuse the constant supply of vital energy; and

these must be men of high moral principle, not only strictly honest, but above suspicion of the contrary. They owe a strict accountability to the members of the Institute for the manner in which the income is expended, and to the world for the mode in which the high duty of acting as judges of the merit of inventions has been discharged. The task of the judges, and of the committee of science and the arts, to whom discoveries and inventions are referred, is one of delicacy and difficulty, and should not be entrusted to those unacquainted with the principles on which the proposition to be examined depends, or who do not possess the mental and moral qualifications necessary to the formation of a correct judgment. It is for the benefit of the community that the truth should prevail, and that the merits and defects of an invention should be rendered distinctly manifest. Where merit exists it should receive due credit, but not exaggerated praise. The simple statement of what has been accomplished is all that is needed, though it may not be all which a generous spirit is impatient to bestow. Nobleness of mind springs forward with ardor to meet every indication of a similar kind wherever it appears. The whole duty of the committee however in this case may be expressed in two words—*strict justice*. This is what every judge ought to give, and more than this no man ought to desire to receive.

It will often become the duty of the committee of examination of subjects of science and art to repress the premature zeal of visionary inventors. We need only examine the records of the Patent Office to be convinced of the immense expense of time and money continually lavished on futile attempts to innovate and improve. We may safely venture to affirm that out of every fifty propositions for improvements in arts or mechanics forty-nine at least are either useless or old. The object should be to distinguish and to adopt the good and reject the bad. But while pruning the luxuriant fruit of uncultivated invention care must be taken to perform the task with gentleness, and to show that the intention is to give additional vigor to the healthful branches and not to injure the parent plant.

There can be no reality in science if at this late day it cannot predict that certain proposed inventions are impossible, as well as declare that others are in accordance with established principles. An honest expression of opinion on such points, though it be met with the accusation of repressing the march of improvement, is necessary in order to save the public from having its attention perpetually distracted by the excitement of fallacious expectations, and the credulous from embarking their all in schemes which must end in disappointment and ruin.

One of the most fruitful sources of error and deception with regard to inventions arises from misconceptions of the nature and application of mechanical power, and this is one of the points on which I wish to arrest your attention for a few minutes. We understand by the term mechanical power that which moves machinery, transports heavy bodies, shapes the raw material into useful forms, and to use the short but expressive phrase of the mechanic, "*that which does work.*" Mechanical power, when properly understood, is a condition or state of matter. Thus a quantity of burning fuel, a moving mass of water or of air, are bodies in the condition of power, and by communicating a portion of their motion to other bodies they produce in them certain changes which are denominated *work*. The change thus produced is the measure of the amount of power in a given quantity of matter. For example, the number of bushels of grain which can be ground during the combustion of a bushel of coal is the measure of the amount of power in this quantity of fuel.

Power is always expended in doing work, and it is in the highest degree absurd to think of applying it to useful purposes without exhausting it. Every change of condition, every transformation of matter, every new motion, and every manifestation of life is at the expense of some motive power which, having performed its part, is forever neutralized.

Power is always the product of nature. God has not vouchsafed to man the means of its primary creation. It is found in the moving air and the rapid cataract—in the burning coal—the heaving tide; man transfers it from these to other

bodies and renders it the obedient slave of his will—the patient drudge, which in a thousand ways administers to his wants, his convenience, and his luxuries, and enables him to reserve his own energy for the higher purpose of the development of his mind and the expression of his thoughts.

The following is a list of all the primary powers which as yet have been used by man in accomplishing his varied purposes in the wide domain of practical life. These are :

1. Water power,
2. Wind power,
3. Tide power,
4. The power of combustion, and
5. The power of vital action.

To this list may hereafter be added the power of the volcano and the internal heat of the earth ; and besides these science at the present time gives no indications of any other. These are denominated primary powers, though in reality, when critically studied, they may all, except the two last mentioned, be referred to actions from without the earth, and principally to emanations from the sun.

Gravitation, electricity, galvanism, magnetism, and chemical affinity can never be employed as original sources of power. At the surface of the earth they are forces of quiescence, the normal condition of which must be disturbed before they can manifest power, and then the work which they are capable of performing is only the equivalent of the power which was communicated to them.

There is no more prevalent and mischievous error than the idea that there is in what are called the “imponderables” a principle of spontaneous activity. Heat is the product of chemical action, and electricity only manifests power when its equilibrium is disturbed by an extraneous force, and then the effect is only proportional to the disturbing cause. It was for this reason that the existence of electricity remained so long unknown to man. Though electricity is not in itself a source of power, yet from its extreme mobility and high elasticity it affords the means of transmitting power with

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scarcely any loss and almost inconceivable velocity to the greatest distance. A wave of disturbance starting from the impulse given at the battery will traverse the circumference of the earth in less time than I have been occupied in stating the fact.

Besides electricity and the principles before mentioned, there are other agents employed between the primary power and the *work*, namely, the elastic force of steam, of air, and of springs; also various instruments called machines. But these must not be confounded, as they frequently are, with the sources of power. It is not the engine which is the source of motion of the cars, nor yet the steam, but the repulsive energy imparted to the expanding water from the burning fuel.

A machine is an intermediate instrument to transmit, to modify, and to apply power; and with the exception of the power consumed in wearing away the rubbing parts—(that is, in producing friction,) and the small portion imparted to the air, the amount of power transmitted is just equal to that received.

The human body is itself an admirably contrived complex machine, furnished with levers, pulleys, cords, valves, and other appliances for the application and modification of the power derived from the food. It is, in fact, a locomotive engine, impelled by the same power which under another form gives activity and energy to the iron horse of the railway. In both, the power is derived from combustion of the carbon and hydrogen of the organic matter employed for food or fuel. In both the direction of power is under the influence of an immaterial, thinking, willing principle called the soul. But this must not be confounded, as it frequently is, with the motive-power. The soul of a man no more moves his body than the soul of the engineer moves the locomotive and its attendant train of cars. In both cases the soul is the directing, controlling principle, not the impelling power. Let, for example, a locomotive engine be placed upon the track, with water in the boiler and fire in the grate—in short, with all the potentials of motion, and it will still remain

quiescent. In this state let the engineer enter the tender and touch the valve; the machine instantly becomes instinct with life and volition; it has now a soul to govern its power and direct its operations; and indeed as a whole it may be considered as an enormous animal, of which the wheels and other parts are additions to the body of the engineer.

The facts I have given as to the source of power and its application rest upon the widest and best-established inductions of physical science, and a knowledge of them is absolutely essential to every one who desires to improve the art of applying the powers of the elements to useful purposes. And yet,—if we are to judge from the constant announcement in the papers of new motors, of machines moved by centrifugal force, of engines to do a large amount of work with the expenditure of an infinitesimal quantity of power, of contrivances by which electricity is to develop itself and do work by its own force,—we shall be convinced that on projects which are in opposition to the best-established truths of science hundreds of thousands of dollars are squandered and years of thought and labor wasted. One cause of error of this kind is the unfortunate name which was originally given to, and is still retained by, certain elementary machines, viz., the lever, the wheel and axle, the inclined plane, the pulley, and the screw. These are employed separately as instruments for the application of power, or in combination as the elementary parts of complex machines. Every tyro in science knows that they have no power in themselves, yet the name, *mechanical powers*, by which they are designated tends to perpetuate a pernicious error long after the fallacy is understood.

A machine, as I have before stated, is an instrument to apply and modify power, and to effect changes in the form and texture of matter denominated *work*. The combination of the elementary parts of machines so as to produce any desired motions has been studied with much success, and the whole reduced to rules. The diffusion of a general knowledge of these would much facilitate invention and prevent the necessity of the individual who devotes his mind to the

improvement of machines beginning anew instead of building on what has been done before him.

Every complex machine consists of parts which may be classified as follows :

1st. The receivers of the power—such as the buckets and other parts of the water-wheel, the vane of the windmill.

2d. The transmitters and modifiers of the motion, viz., wheels, pinions, levers, pistons, screws, &c.

3d. The supporters—such as the frames, the friction-rollers, &c.

4th. Regulators to render the motion uniform.

5th. Operators or parts applied immediately to the matter on which the work is to be done.

The preparation and publication of charts of the elementary parts of machines and their combinations would do important service to the practical mechanic, and is an object among many others worthy of the attention of this Institute.

The most important source of mechanical power among those we have mentioned, and which promises almost to supersede all others, is that of burning coal. This material—like a watch wound up, is matter in a state of power, or in a state of unstable equilibrium, ready to rush into combination with the oxygen of the atmosphere as soon as the initial action is given, and to evolve power in the form of heat until the whole is consumed. It has been proved that on an average *four ounces* of coal is sufficient to draw—on a railway, one ton a mile. It has also been found by experiment that a man working on a tread-mill continuously for eight hours will elevate one and a half million of pounds one foot high. Now, good Cornish engines will perform the same work by the expenditure of the power of a pound and a half of coal. It follows from these data that about five tons of coal would evolve as much power during its combustion as would be equal to the continued labor of an able-bodied man for twenty years, at the rate of eight hours per day; or in other words, to the average power of a man during the active period of his life. Providence has therefore stored away in the form of coal, for the use of man, an incalculable amount

of mechanical power. Beneath the soil of our own great coal basins there reposes power equivalent to the united force of myriads of giants, ready (like Aladdin's Genius) to be called into activity by the lamp of science, and as its obedient slave to build cities, to transport palaces, or to remove mountains. There is no other locomotive power over which man has any prospect of control in the least degree comparable with this.

I have made these remarks with reference to power, because mistakes on this subject are so frequent and so fatal. Allow me, in the next place, to call your attention to some other points having a direct bearing upon the progress of the mechanic arts.

In order that an important invention may be successful, two conditions must be favorable: First: It must be possible; that is, the scientific principle on which it is to be founded must be known. Second: The invention must be wanted; or in other words, it must be called for by the character and intelligence of the times, or rendered especially desirable in a particular place by some peculiarity of climate, topography, &c.

With reference to the first position, it may be said that in accordance with the well-known laws of permutation, an almost infinite number of new combinations or inventions may be formed from the present stock of scientific knowledge. This is true: but the inventions thus produced must be restricted as to kind, and though they be unlimited in number they are not so as to character. No combination of known principles, before the discovery of galvanism, was sufficient for the invention, by the most ingenious synthetical mind, of the electro-magnetic telegraph; but after the discoveries made by Galvani and Oersted, this invention became possible.

In the history of the progress and development of a branch of science a condition is reached when its principles become applicable to some practical purpose, and it is instructive to observe how at this period it suddenly assumes in the public mind a high degree of importance. The man who makes

the application, though he may not have spent a tithe of the labor and thought on the subject which was bestowed on it by those who brought it to its practical state, is crowned as the discoverer of the whole. After this however, competitors arise who claim a share of the reward, if not the honor of the invention. These labor to show that the first inventor derived his ideas from the discoverers. The public mind then takes another turn, and is disposed to do injustice on the other side, and it is only after a series of oscillations in public opinion that the true state of the case becomes generally known and acquiesced in.

With reference to the second proposition we may state that so important an element is the state of public intelligence in regard to the success of an invention that many of the most important processes of art have been more the result of the actual spirit and want of the age than the product of the ingenuity and knowledge of an individual; and in such cases the invention is frequently brought forth simultaneously by a number of different individuals. The art of printing may be placed in this category. At a certain period in the history of the world this invention was loudly called for by the pressing necessities and peculiarities of the times. It was then produced: but had the attempt been made at an earlier date to introduce it, the result would probably have been a failure. We have a similar example in the application of steam to navigation. The world had for years before this invention been in possession of the steam engine, and a boat had even been propelled by steam on the Clyde, in Great Britain, but the invention was not appreciated. Neither the time nor the place was favorable to its introduction, and it was reserved for our country, with its immense plexus of navigable rivers and its broad expanse of internal lakes, to call for this addition to the art of locomotion, and for the genius of Fulton to give a successful response. Even in this case the importance of the invention was so manifest, and its means of attainment so simple, that several competitors contended for the prize; and had any accident happened to retard for a few weeks the completion of Fulton's first boat

he would have been anticipated in the result of his enterprise by the fortunate experiment of the elder Stevens. In making this statement I would not wish to detract from the real merit of individuals; they have sufficient claims for remuneration and reputation in being among the first to appreciate properly the value of the improvement, and to avail themselves at the earliest point of time of the necessary means of accomplishing it. I may remark in passing that from the foregoing views and statements it is plain that the steamboat is emphatically an American invention. It was in this country that premiums were first offered for its production, and on the Hudson, in 1807, it was first reduced to practice. It was not adopted in England until 1812, and not until 1816 in France.

From a want of a knowledge of the state of science, and a due consideration of the proper time and place, many ingenious minds have wasted their energies in fruitless labor, waged with fortune an unequal war, and sunk into the grave the victims of disappointed hopes. Such men are frequently said to "live before their time;" but it remains to be proved whether in the aggregate of cases they have done more good or evil, and whether they most deserve our admiration or our pity. A premature, and consequently an unsuccessful, attempt often so prejudices the public mind against an invention that when the proper time actually arrives for its introduction public sentiment is found arrayed against it, and difficulties have to be overcome which would not have existed had the first essay never been made.

The man of true genius never lives before his time; he never undertakes impossibilities, and always embarks in his enterprise at the suitable place and period. Though he may catch a glimpse of the coming light as it gilds the mountain top, long before it has reached the eyes of his contemporaries, and though he may hazard a prediction as to the future, he *acts* with the present.

There are some partial exceptions to this rule, and among them I would mention with high respect that of Oliver Evans, than whom no man in this country has ever done

more to improve the art of locomotion. He indeed predicted that steam wagons would be used on common roads, and made attempts to reduce his idea to practice. The time however for the introduction of this invention has not even *yet* arrived, and at present we see no prospect of its coming. But he was more successful in the invention of the American high-pressure engine, which was so essential to the development of the vast resources of the interior regions of our continent. This engine was at the time of its introduction admirably adapted, in its cheapness, simplicity of arrangement, smallness of dimensions, and great power, to the abundance of fuel, the extent of transportation, and the primitive state of the arts in our country. The low-pressure engine used by Fulton was procured from England; and had steam navigation been confined to the employment of the complex and expensive machines of this class, the Mississippi and its tributaries would have remained for years unnavigated, except by the canoe of the native or the flat-boat of the pioneer.

The invention and introduction of the high-pressure engine required the application of genius, energy, and courage. The use of high steam had been proposed in England, but had been discarded on account of the supposed danger attendant on its use, and it was reserved for this country to demonstrate its practical importance. Without precursory labors equivalent to those of Evans the present railway locomotive would not have been in existence.

It gives me pleasure to pay this passing tribute to the memory of one to whom our country owes so deep a debt of gratitude, and whose name deserves a more conspicuous place than it now holds in the history of American inventions.

Every age of the world since the commencement of the historic period has been characterized by some leading or dominant idea, and each age has bequeathed something of value to—or made some abiding impression on—that which followed.

The great characteristic of the present time is the application of science to art; or in other words, the development of the inventive faculty of the human mind. The last cen-

ture was equally if not more fertile in the discovery of the great principles of nature from which we are now reaping so rich a harvest of practical results, but a knowledge of these was not then so interwoven with the thoughts of the common mind as to render them available for purposes of art. Indeed the facts and elementary principles of science, as well as the application of the rules which have been deduced from its higher generalizations, are now so familiar that art has become vain of her attainments, has set herself up as the architect of her own fortune, and disregards the counsel of her more learned and sagacious sister. Such a course however is usually accompanied with its own punishment. The new edifices, designed by empiricism, are generally unstable structures, and most frequently involve the ruin of the builder in their fall.

It is true many valuable inventions have been founded on the accidental discovery of simple facts; but such inventions can never be perfected unless the principles of science on which they are based are known. It is also true that many arts may be successfully practiced by persons entirely ignorant of the principles of these arts. We have a notable example of this in the art of navigation, and in many of the processes of engineering. The practical man in these cases employs rules and deductions furnished by abstract science, in the application of which he often becomes more expert than the original author; but sure progress in art cannot be obtained without anterior or contemporaneous progress in science. The inventor, to insure his success, must consult the discoverer, and the practical skill of the one be directed by the theoretical knowledge of the other.

After what has been said in different parts of this address it may be superfluous to give a formal definition of discovery and invention; but these terms are so frequently confounded with each other, and their misuse so much connected with error, that it is necessary they should be clearly defined, even at the risk of prolixity.

By a *discovery* in science is understood the development of a knowledge of the existence of some principle in nature not

before known or but partially understood; while the term *invention* indicates the application of this knowledge, either simply or in combination with other knowledge, to some useful purpose in the arts. For example, Franklin *discovered* the principle of electrical *induction*, or the action at a distance of a charged body on a conductor, and on this founded his *invention* of the lightning-rod.

It sometimes happens that the peculiar characteristic of mind and training necessary to the successful prosecution of these two branches of labor are found combined in the same individual. Of a happy combination of this kind James Watt affords a striking example, the like of which will become more common in proportion as the means of intellectual improvement afforded to workmen are extended. Generally however the two faculties exist in the greatest degree of development in separate individuals. The successful investigation of a new principle in science generally requires much previous study and preparation and a logical training, which few men—however vigorous may be their native intellect, can dispense with, and to acquire which the opportunities of the workmen are inadequate. On the other hand the successful introduction to common use of an invention requires a contest with the world from which the sensitive student of abstract science shrinks with repugnance. I consider these remarks of some importance, because in this country, where there is so great a demand for immediate practical results, the value of labor in the line of abstract science is not properly appreciated or encouraged.

We have said that every age of the world has bequeathed something of value to that which followed, and we may add that it is doubtful whether any great truth has ever been lost: though some may have apparently lain dormant for a time, yet they have continually produced results. Some arts have undoubtedly fallen into disuse, because they are no longer required, or because they have been superseded by more perfect processes. We however think it can be clearly established that modern science is capable of re-producing every invention of ancient art, and at an indefinite economy of human time and human labor.

I know we are frequently referred to the immense masses of stone transported and wrought by ancient art, which are found among the ruins of Baalbec and Thebes, and are frequently told that the management of these would far transcend the skill and power of modern engineers. Such assertions are however rather intended to convey an idea of the impression produced upon the beholder of these venerable ruins than a declaration of absolute truth. As a sufficient illustration of this we may mention the fact that in New York large buildings of brick and stone are moved from place to place while the inhabitants remain undisturbed within; or we may point to the Menai Strait tubular bridge, a structure of cast-iron several hundred tons in weight, suspended in mid-air over a chasm more than a hundred feet deep.

The pyramid of Cheops is said to have employed the power of 100,000 men for twenty years in its erection; but, vast as is this pile, were the steam-engines employed in one of our large cities directed to the task of rearing one of equal magnitude the whole would be accomplished in a few weeks.

I have said that no arts of importance have been lost, but perhaps this assertion is rather too general. There is one which may be considered an exception: I allude to the ancient art possessed by the few of enslaving and brutalizing the many, the art by which a single individual, invested with the magic of kingly power, was enabled to compel thousands of his subjects, through the course of a long reign, like beasts of burden, to haul materials and heap up huge piles of stone, which might transmit to posterity the fact that a worm like himself had lived and died. The pyramids of Egypt, venerable as they are with the age of accumulated centuries, are melancholy monuments of human degradation, of human vanity and cruelty.

There are certain processes of thought which require individual exertion rather than combined effort for their development. There are certain arts in which perfection depends on the genius and skill of the individual rather than on the condition of the race. Such are oratory, poetry, painting,

and sculpture. In these if an individual excel he excels for himself; his skill is not transferable, though his example may serve to awaken the same taste in many of his contemporaries and successors. For the development of these arts the individualism of the Greeks was well adapted, and they were accordingly advanced by this people almost, if not quite, to their maximum state of perfection.

These results of the labors of the ancients in the development of the beautiful have not been lost; on the contrary, they will ever remain impressed upon the human mind. The marble of the Parthenon may be reduced to atoms, and scattered to the winds of Heaven, but its form is imperishable. The moderns do not surpass the examples of the fine arts bequeathed to them by the ancients, because it would be idle to attempt to add to that which is perfect,—to paint the lily, or to gild refined gold. But they have invented tools and processes by which copies of these precious relics may be multiplied indefinitely, with unerring precision, by the application, not of manual skill, but of physical labor.

This union of the industrial with the fine arts vastly enlarges the influence of the latter, and enables them to be appreciated and genius to be admired by millions whom their single productions would never reach. There are at this time more minds enthusiastically alive to the beauty of ancient art than there were in the days of Phidias. Nothing then of importance with reference to art has been lost, but, on the contrary, much has been gained.

In these remarks we seek not to disparage the past, nor to unduly exalt the present. The character of the world, as it now exhibits itself in its mental and moral development, its knowledge of nature, and its skill in arts, is the result of all the impressions made on it from the earliest dawn of civilization to our own day. In the case of an individual every impression to which his mind is subjected, either from external nature or his own mental operations, or those of his fellow-men, produces an indelible effect, modifying all the previous impressions, and co-operating with them to form the peculiarities of his mental and moral character. An

analogous effect is produced on the whole human family during the successive ages of its existence.

By these remarks we do not wish to draw upon ourselves the imputation of advocating the inevitable progress of the human race. The world is subject to evil impressions as well as good, and whatever advance is made in the line of true progress will not be the result of a blind law of necessity, but of a providential design through human agency and properly-directed human labor. Without labor nothing of value can be accomplished. It is the essential pre-requisite of well-being, the original curse which proves a blessing in disguise. The remark has been properly made that could all the wants of man be supplied without labor there would be reason to fear that he would become a brute for the want of something to do, rather than a philosopher from an abundance of leisure. In all countries where nature does the most, man does the least. The sterile soil and the inclement sky seem to be the stimulants to mental and physical exertion, when once the necessary impulse has been given. True progress does not consist in obviating the necessity of labor, but in changing, by means of improvements in the arts, its character in rendering it more conducive to the supply of the wants and comforts of man, and to the development of his mental and moral nature.

We have received from the past a rich treasure of knowledge, the product of the body and mind, gathered under difficulties and danger, and improved by the thought and the experience of years. Our great object should be to purify this knowledge from error, to reduce it to its essential and simple elements, and to transmit it with the greatest amount of new truth to our successors. We should however recollect that accumulated knowledge, like accumulated capital, increases at compound interest, and that therefore each generation is bound to add much more largely to the common stock than that which immediately preceded it. It is the high privilege, as well as the sacred duty, of every one of us, to labor for the improvement of ourselves and our fellow-men, and to endeavor to the utmost of our ability to leave:

the world at least a little wiser and better than we found it. But in order to success in this effort, we must cultivate other provinces of thought than merely those which belong exclusively to the development of our knowledge of the external world. There are other regions of a higher and holier nature, without the cultivation of which no true progress can be made.

THOUGHTS ON EDUCATION.

INTRODUCTORY DISCOURSE BEFORE THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF EDUCATION.*

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No subject of human thought has perhaps received more attention than that of education. Every one has the material for speculating in regard to it in his own experience; but individual experience is too limited a basis on which to found a general theory of instruction, and besides this, (paradoxical as it may appear,) an individual is perhaps less able to judge correctly of the effects of the course of instruction to which he has been subjected than another person. No one can tell what he would have been under a different course of training, and the very process which he condemns may perhaps have been the one best suited to develop the peculiarities of mind which have led to his success in life; and indeed in some very rare instances the want of all training of a systematic kind may be the best condition under Providence for producing an entirely original character. Shakespeare's genius might have been shackled by the scholastic curriculum of Oxford or Cambridge: but these cases are extremely rare, for genius itself, like the blossoms of the aloe, is the solitary production of a century.

I bring forward my own views on education with diffidence. First, because I have read scarcely any thing on the subject, and what I shall say may be considered commonplace; secondly, because my views may in some respects be at variance with what are regarded as the established principles of the day. But important truths cannot be too often presented, and when re-produced by different minds under different circumstances they can scarcely fail to awaken new

*[Introductory Address delivered by the retiring President of the Association for the Advancement of Education, at its Fourth Annual Session, held at Washington, D. C., in December, 1854.]

trains of thought and renewed attention; and again, if the propositions which I maintain are erroneous, I desire that they may be discussed and disproved before they are given more widely to the public. What I shall advance may be viewed as suggestions for consideration, rather than propositions adequately proved.

In the establishment of a principle it is of the first importance that all probable suggestions relative to it may be subjected to critical examination, and tried by the test, as far as possible, of experience; it is in this way that science is advanced.

The first remark which may be made in regard to education is that it is a forced condition of mind or body. As a general rule it is produced by coercion,—at the expense of labor on the part of the educator, and of toil and effort on the part of the instructed. That there is no royal road to learning is an aphorism as true now as it was in the days when first uttered. God has placed a price on that which is valuable, and those who would possess a treasure must earn it at the expense of labor. Intellectual as well as material wealth can only be purchased at the price of toil. It is true the child may be induced to learn his task by the prospect of reward; by emulation; by an appeal to his affections; but all these, in some cases, are ineffectual, and recourse must be had to the stimulus of the rod. I do not by this remark intend to advocate a general recourse to corporeal coercion. It should be used sparingly, perhaps only in extreme cases, and for the purpose of eradicating a vicious habit. The philosophy of its use in this case is clear. We associate pain with the commission of an improper act, and thus prevent its recurrence.

I have said that education is a forced condition of mind or body. The child, if left to itself, would receive no proper development, though it might be surrounded with influences which would materially affect its condition. The savage never educates himself mentally; and were all the educational establishments of the present day abolished, how rapidly would our boasted civilization relapse into barbarism.

Another important fact is that every generation must educate and give character to the one which follows it, and that the true progress of the world in intelligence and morality consists in the gradual improvement of the several generations as they succeed each other. That great advance has been made in this way, no one can doubt who views the facts of history with an unprejudiced mind; but still the improvement has not been continuous. There have been various centers and periods of civilization. Egypt, Greece, and Rome, though they have left an impress upon the world which extends even to our time, and modifies all the present, have themselves "mouldered down." It appears therefore that civilization itself may be considered as a condition of unstable equilibrium, which requires constant effort to be sustained, and a still greater effort to be advanced. It is not, in my view, the manifest destiny of humanity to improve by the operation of an inevitable, necessary law of progress; but while I believe that it is the design of Providence that man should be improved, this improvement must be the result of individual effort, or of the combined effort of many individuals, animated by the same feeling, and co-operating for the attainment of the same end. The world is still in a degraded condition; ignorance, want, rapine, murder, superstition, fraud, uncleanness, inhumanity, and malignity abound. We thank God however that he has given us the promise, and in some cases the foretaste—of a happier and holier condition; that he has vouchsafed to us as individuals, each in his own sphere, the privilege, and has enjoined upon us the duty, of becoming his instruments, and thus co-workers in ameliorating the condition of ourselves and our fellow-men; and above all that he has enabled us through education to improve the generations which are to follow us. If we sow judiciously in the present, the world will assuredly reap a beneficent harvest in the future; and he has not lived in vain who leaves behind him as his successor—a child better educated morally, intellectually, and physically than himself. From this point of view the responsibilities of life are immense. Every individual by his

example and precept, whether intentionally or otherwise, does aid or oppose this important work, and leaves an impress of character upon the succeeding age which is to mould its destiny for weal or woe in all coming time.

Civilization itself, as I have before observed, is a state of unstable equilibrium which, if not supported by the exertions of individuals, resembles an edifice with a circumscribed base, which becomes the more tottering as we expand its lateral dimensions, and increase its height. Modern civilization is founded on a knowledge and application of the moral, intellectual, and physical laws by which Divine wisdom governs the universe. The laws of morality have been revealed to us, but they require constant enforcement and habitual observance. The laws of the intellectual and material universe have been discovered by profound study and years of incessant labor, and unless they are taught in purity and freed from error they fail to produce their legitimate result. But the illustration and enforcement of the laws of morality require the exertions of men of high talents and profound learning; and a true knowledge of the laws of nature can be imparted only by minds that have long been devoted to their study. Therefore a large number of highly educated men whose voice may be heard, and whose influence may be felt, is absolutely necessary to sustain the world in its present moral and intellectual development. The world however is not to be advanced by the mere application of truths already known; but we look forward, particularly in physical science, to the effect of the development of new principles. We have scarcely as yet read more than the title page and preface of the great volume of nature, and what we do know is nothing in comparison with that which may yet be unfolded and applied; but to *discover* new truths requires a still higher order of individual talent. In order that civilization should continue to advance, it therefore becomes necessary that special provision should be made for the *actual increase* of knowledge, as well as for its diffusion; and that support should be afforded, rewards given, and honors conferred, on those who *really add* to the sum of human knowledge.

This truth however is not generally appreciated, and the tendency is to look merely at the immediate results of the application of science to art, and to liberally reward and honor those who simply apply known facts rather than those who *discover* new principles.

From what we have said it would appear that, in order that civilization should remain stationary, it is absolutely necessary that the great truths which have been established should not become diluted, obscured, or forgotten; that their place should not be usurped by error; or in other words, that the great principles of science, which have been established through long years of toil and nights of vigilance, should not be superseded by petty conceits, by hasty and partial generalizations, and by vague speculations or empirical rules. Further, that civilization should not retrograde, it is indispensably necessary that the great truths of morality should not only be theoretically taught and intellectually apprehended, but actively, constantly, and habitually applied. But this state of things can only exist by means of the efforts of individuals actuated by a generous, liberal, and enlightened philanthropy. Unfortunately however the tendency of civilization, from the increase of wealth and security, is to relax individual effort. Man is naturally an indolent being, and unless actuated by strong inducements or educated by coercion to habits of industry, his tendency is to supineness and inaction. In a rude state of society an individual is dependent upon his own exertions for the protection of himself, his family, and his property; but as civilization advances, personal effort is less required, and he relies more and more on law and executive government. Moreover, as wealth and elementary education become more general without a corresponding increase of higher instruction, the voice of the profound teacher becomes less and less audible; his precepts and admonitions less and less regarded; he is himself obliged to comply with popular prejudices and conform to public opinion, however hastily formed or capricious such an opinion may be. Hence the tendency to court popular favor, to be influenced by it, rather than attempt to

direct it. Hence charlatanism and the various dishonest efforts to gain notoriety rather than a true reputation—so frequently observed. Knowledge has arrived at such a stage of advancement that a division of labor in regard to it is necessary. No one can be learned in all the branches of human thought; and the reputation of an individual therefore ought to rest on the appreciation of his character by the few—comparatively, who have cultivated the same field with himself. But these are not generally the dispensers of favor, and consequently he who aspires to wealth or influence seeks not their approbation, but the commendation and applause of the multitude. It is impossible that those who are actively engaged in the business of life should have time for profound thought. They must receive their knowledge, as it were, at second hand; but they are not content under our present system of education with the position of students; they naturally aspire to that of teachers; and every one who has learned the rudiments of literature or science becomes ambitious of authorship and impatient for popular applause. Knowledge in this way becomes less and less profound in proportion to its diffusion. In such a condition of things it is possible that the directing power of an age may become less and less intelligent as it becomes more authoritative, and that the world may be actually declining in what constitutes real moral, and intellectual greatness, while to the superficial observer it appears to be in a state of rapid advance. I do not affirm that this is the case at present. I am merely pointing out tendencies.

The present is emphatically a reading age; but who will venture to say that it is proportionately a *thinking* age? The sum of positive knowledge is embraced in but few books, and small would be the library necessary to contain the essence of all that is known. We read too much and too quickly to read understandingly. The world is gorged with intellectual food, and healthful digestion is comparatively unknown. Too many books are published; I do not mean to say that too many *standard works* are printed, but by far too many silly, superficial, and bad books are sent

forth from the teeming press of our day. The public mind is distracted amidst a multiplicity of teachers and asks in vain for TRUTH. But few persons can devote themselves so exclusively to abstract science as fully to master its higher generalizations, and it is only such persons who are properly qualified to prepare the necessary books for the instruction of the many. I cannot for a moment subscribe to the opinion which is sometimes advanced that superficial men are best calculated to prepare popular works on any branch of knowledge. It is true that some persons have apparently the art of simplifying scientific principles; but in the great majority of cases this simplification consists in omitting all that is difficult of comprehension. There is no task more responsible than that of the preparation of an elementary book for the instruction of the community, and no one should embark in such an undertaking who is not prompted by a higher motive than a mere love of notoriety, or the more general incentive, a hope of commercial success. He should love the subject upon which he intends to write, and by years of study and habitual thought have become familiar with its boundaries, and be enabled to separate the true and the good from that which is merely hypothetical and plausible.

In this connection I may mention the evils which result from literature and science becoming objects of merchandise, and yet not amenable to the laws of trade. I allude to the international copyright system. The tendency of the present condition of copyright law between England and America is greatly to debase literature, to supply cheap books, and not to impart profound wisdom or sound morality. English books are republished in this country and American books are reprinted in England because they are *cheap*, and not because they are *good*. Literary and scientific labor must be properly remunerated or the market will be supplied with an inferior article. The principles of free trade are frequently improperly applied to this question. The protection required and demanded by the literary man is not that of a premium on his work, but the simple price which it ought

to bear in the market of the world. He asks that the literary product of the foreigner may be paid for in order that justice may be done his brother, and also that he himself may receive a proper remuneration for his own labors. Would there be any manufactories of cloth, think you, in this country if the tailor had the means and inclination to procure free of cost all the material of the garments which he supplies to his customers? And can it be supposed that valuable literary works will be produced among us so long as our publishers are allowed to appropriate without remuneration the labors of the foreigner? The want of an international copyright law has, I know, produced a very unfavorable effect upon higher education in this country. It has prevented the preparation of text-books better suited to the state of education among us than those which are republished from abroad and adopted in many of our institutions of learning.

Another result of the wide diffusion of elementary knowledge without a proper cultivation of the higher intellectual faculties, and an inculcation of generous and unselfish principles, is the inordinate desire for wealth. To acquire power and notoriety in this way requires the least possible amount of talents and intelligence, and yet success in this line is applauded, even if obtained by a rigid application of the dishonest maxim that "*all is fair in trade.*" We have a notable example of this fact in the autobiography of an individual who glories in his shame and unblushingly describes the means by which he has defrauded the public. No one who has been called upon to disburse public money can have failed to be astonished at the loose morality on the part of those who present claims for liquidation. The old proverb here is very generally applied, namely, "the public is a goose, and he is a fool who does not pluck a feather!" A full treasury, instead of being considered a desirable or healthy state of the nation, should be regarded as the precursor of a diseased condition of the public morals. That the tendencies which I have mentioned do to a greater or less extent exist, and that they require the serious consider-

ation of the enlightened statesman and the liberal-minded and judicious friend of education, must be evident to every one who seriously and without prejudice observes the habits of the times.

The proper appreciation of profound learning and abstract science is not as a general rule what it ought to be. The most authoritative teacher is the editor of a newspaper. Whatever may have been his previous training, or however circumscribed his field of thought, he is the umpire to decide upon all questions even of the most abstract science or the most refined casuistry.

The question may be asked with solicitude—Are the tendencies we have mentioned inevitable? Are there no means of counteracting them? And is our civilization to share the fate of that of Egypt, Greece, and Rome? Is humanity destined to a perpetual series of periodical oscillations of which the decline is in proportion to the elevation? We answer, No! Though there have been oscillations, and will be again, they are like those which constitute the rising flood-tide of the ocean, although separated by depressions, each is higher than the one which preceded it. Something may have been *lost* at intervals; but on the whole more has been and will be *gained*. But how is this to be effected? The man of science and literature, the educator, and the Christian teacher, together with the enlightened editor, must combine their efforts in a common cause, and through the influence of the press, the school, the college, and the pulpit,—send forth a potential voice which shall be heard above the general clamor.

Common school or elementary education is the basis on which the superstructure of the plan of true progress should be established; but it must be viewed in its connection with a general system, and not occupy exclusively the attention and patronage of governments, societies, and individuals; liberal means must also be provided for imparting the most profound instruction in science, literature, and art.

In organizing new States and Territories the amplest provision ought to be made for all grades of education; and if

possible, every individual should have the opportunity offered him of as much mental culture as he is capable of receiving, or desirous of acquiring; notwithstanding comparatively few may have the industry and perseverance necessary to the highest attainment. It is also of the first importance, that modes of instruction be examined and thoroughly discussed, in order that what is valuable in the past should be retained, and what is really an improvement in the present, be judiciously and generously applied.

Having presented some general suggestions in regard to the bearing of education and the efforts of individuals on the progress of humanity, I now propose to offer for consideration a few observations on the theory of the process of instruction.

It may be surprising that the theory of an art so long practiced as that of education should not be definitely settled; but strange as it may appear, the fact is certain that few writers fully agree as to what is the true plan and process of education. No art can be perfect unless it rests upon a definite conception of fundamental principles; or in other words, unless its theory be well established upon a general law of nature. The laws which govern the growth and operations of the human mind are as definite and as general in their application as those which apply to the material universe; and it is evident that a true system of education must be based upon a knowledge and application of these laws. Unfortunately however psychologists have not classified and exhibited them in a form sufficiently definite to render their application easy, and the directors of education have too often considered merely the immediate practical result which might follow a particular course of training rather than that which would be conducive to the highest development of the individual. In this condition of the theory of education, I have myself ventured to speculate upon the subject, and though I may have nothing new of value to offer, it is my duty at this time to make such suggestions as may furnish topics of discussion or serve to illustrate established truths.

The theory which I would present for your consideration and critical examination, and which appears to me to be in accordance with the results of experience, may be briefly expressed as follows:

The several faculties of the human mind are not simultaneously developed, and in educating an individual we ought to follow the order of nature, and to adapt the instruction to the age and mental stature of the pupil. If we reverse this order, and attempt to cultivate faculties which are not sufficiently matured, while we neglect to cultivate those which are, we do the child an irreparable injury. Memory, imitation, imagination, and the faculty of forming mental habits exist in early life, while the judgment and the reasoning powers are of slower growth. It is a fact abundantly proved by observation that the mere child by the principle which has been denominated *sympathetic imitation* may acquire the power of expressing his desires and emotions in correct and even beautiful language without knowing or being able to comprehend the simplest principles of philology. He even seizes, as if by a kind of instinct, upon abstract terms, and applies them with ease and correctness; but as life advances the facility of verbal acquisition declines, and with some it entirely disappears. Hence the plan appears to me to be wise and in accordance with nature which makes the acquisition of language an essential part of early elemental education. The same child which acquires almost without effort his vernacular tongue may by a similar process be taught to speak the principal ancient and modern languages. He may also acquire the art of the accountant, and be taught by proper drilling to add long columns of figures with rapidity and correctness without being able to comprehend the simplest abstract principles of number and magnitude. Moreover, it is well known that the memory may be stored at a very early age with valuable rules and precepts, which in future life may become the materials of reflection and the guiding principles of action; that it may be furnished with heroic sentiments and poetic illustrations, with "thoughts which breathe and words that

burn," and which long after will spontaneously spring up from the depths of the mind, at the proper moment, to embellish and to enforce the truths of the future author, statesman, or divine.

But the period of life when acquisitions of this kind are most readily made is not that in which the judgment and reasoning powers can be most profitably cultivated. They require a more advanced age, when the mind has become more matured by natural growth and better furnished with the materials of thought.

Mental education consists in the cultivation of two classes of faculties, viz., the intellectual and the moral.

Intellectual instruction, of which we shall first speak, should have at least three objects:—

1. To impart facility in performing various mental operations.

2. To cultivate the imagination and store the memory with facts and precepts; and

3. To impart the art of thinking, of generalization, of induction and deduction.

The most important part of elementary mental instruction, and that which I have placed first in the foregoing classification, is that of imparting expertness in the performance of certain processes which may be denominated mental arts. Among these arts are spelling, reading, penmanship, drawing, composition, expertness in the first rules of arithmetic, and in the use of different languages. These can only be imparted by laborious drilling on the part of the teacher, and by acquired industry and attention on the part of the pupil. The practice in each case must be so long continued, and the process so often repeated, that it becomes a mental habit, and is at length performed with accuracy and rapidity almost without thought. It is only in early life, while the mind is in a pliable condition, that these mental facilities can most readily and most perfectly be acquired, whereas the higher principles of science, on which these arts depend, can only be thoroughly understood by a mind more fully matured. Expertness in the performance of an art does not de-

pend on a knowledge of its principles, and can be readily acquired without reference to them. The most expert accountants are frequently and perhaps generally those who have no knowledge of the philosophy of figures. On the other hand, a profound acquaintance with the principles of an art may exist without the ability to apply it in practice. I have known of mathematicians who were unable to perform with accuracy and dispatch the processes which constitute the application of the simple rules of multiplication and addition. The same is the case with the art of composition. A most learned rhetorician is not necessarily a fluent and pleasing writer.

The acquisition therefore of these arts should be the principal and prominent object of the primary or common school, and nothing ought to be suffered to usurp their place. Unfortunately the drilling which is at first required to induce the mental habit is so laborious and tedious to the teacher, and in most cases so irksome and distasteful to the pupil, that there is a tendency in our schools, and (I am sorry to say) a growing one, to neglect them, and to substitute other objects of more apparent—but of less intrinsic value. This is not only an irreparable injury to the individual, but also to the public. All the practical operations of life in which these processes are concerned (and they apply to all except those of mere handicraft skill) are badly performed. I may venture to say that the general substitution of instruction in the mere *rationale* of the rules of arithmetic without a proper drilling in the practice would produce more bankruptcies than all the changes of tariffs or fluctuations of trade.

It is an important principle, which should be kept in view by the teacher, that although the practice of an art is at first difficult and requires at each step an effort of mind, yet every repetition renders it easier, and at length we come to exercise it not only without effort, but as a pleasurable gratification of an habitual act. Perseverance therefore in this cause will ultimately receive a grateful reward. It should be impressed upon the minds of the directors of elementary education that the teacher who neglects to train his pupils

to expertness in these processes, or who merely does enough in this way to awaken a distaste, and who fails to overcome this condition of mind by subsequent judicious drilling, is unworthy of his high vocation, and should give place to a more industrious or more philosophical instructor.

All the processes we have enumerated, besides various manipulations and bodily exercises necessary to health, refinement, and convenience, may be taught previous to the age of ten or twelve years. At the same time the memory may be educated to habits of retention and precision; and for this purpose definite, and if possible elegantly expressed rules should be chosen, to be committed without the slightest deviation, and so impressed upon the memory that they will ever after remain a portion of the mental furniture of the man, always ready to be called up when needed, and always to be depended upon for accuracy. The mere understanding of the rule, and the power of being able to express it in a vague and indefinite way in original language, is in my judgment, not of itself sufficient. The memory is an important faculty of the mind, and is susceptible of almost indefinite cultivation. It should however in all cases be subservient to the judgment.

Habits of observation may also be early cultivated, and a boy at the age of twelve years may be taught to recognize and refer to its proper class almost every object which surrounds him in nature; and indeed the whole range of descriptive natural history may be imparted previous to this age.

Nothing, in my opinion, can be more preposterous or mischievous than the proposition so frequently advanced, that the child should be taught nothing but what it can fully comprehend, and the endeavor in accordance with this, to invert the order of nature, and attempt to impart those things which cannot be taught at an early age, and to neglect those which at this period of life the mind is well adapted to receive. By this mode we may indeed produce remarkably intelligent children who will become remarkably feeble men.

The order of nature is that of art before science, the entire concrete first, and the entire abstract last. These two extremes should run gradually into each other, the course of instruction becoming more and more logical as the pupil advances in years.

Thus far we have principally considered only the education of the habits and the memory, and it is particularly to these that the old system of drilling is peculiarly applicable. I know that this custom has, to a considerable degree, fallen into disuse, and the new and less laborious system of early precocious development been substituted in its stead. In this respect the art of instruction among us has retrograded rather than advanced, and "Young America," though a very sprightly boy may fail to become a very profound man!

I would not however by the foregoing remarks have it inferred that the reasoning faculties of the child should not receive due attention, and that clear conceptions of the principle of every process taught should not be elucidated and explained, as far as he is able to understand them; but that the *habits* and the *memory* should be the main objects of attention during the early years of the pupils' course. The error of the old system consisted in continuing the drilling period too long, and in not shading it off gradually into that of the logical, or what might be called the period of the acquisition and use of general principles.

The last part of mental education as given in our classification is that which relates to the cultivation of the judgment and the reasoning powers. These faculties of the mind, as we have repeatedly said, are latest in arriving at maturity, and indeed they may be strengthened continually and improved progressively through a long life, provided they have been properly directed and instructed in youth and early manhood.

They should be exercised in the study of mathematical analysis and synthesis; in deducing particular facts in a logical form from general principles; and instructed in the process of discovering new truths. The cultivation of the imagination should also be considered an essential part of

a liberal education, and this may be spread over the whole course of instruction, for like the reasoning faculties the imagination may continue to be improved until late in life.

From the foregoing remarks it will be evident that I consider the great object of intellectual education to be, not only to teach the pupil how to *think*, but how to *act* and to *do*, and I place great stress upon the early education of the *habits*. And this kind of training may be extended beyond the mental processes to the moral principles; the pupil may be taught on all occasions habitually and promptly, almost without thought, to act properly in any case that may occur, and this in the practical duties of life is of the highest importance. We are frequently required to act from the impulse of the moment, and have no time to deduce our course from the moral principles of the act. An individual can be educated to a strict regard for truth, to deeds of courage in rescuing others from danger, to acts of benevolence, of generosity, and justice; or on the other hand, though his mind may be well stored with moral precepts, he may be allowed to fall into opposite habits alike prejudicial to himself and to those with whom he is associated. He may "know the right, and yet the wrong pursue."

Man is the creature of habit; it is to him more than second nature; but unfortunately, while bad habits are acquired with readiness, on account of the natural desire to gratify our passions and appetites, good habits can only be acquired by unremitting watchfulness and labor. The combined habits of individuals form the *habits of a nation*, and these can only be moulded, as I have before said, by the coercive labor of the instructor judiciously applied.

The necessity of early and judicious moral training is often referred to, but its importance is scarcely sufficiently appreciated. The future character of a child, and that of the man also, is in most cases formed, probably, before the age of seven years. Previously to this time impressions have been made which shall survive amid all the vicissitudes of life—amid all the influences to which the individual may be subjected, and which will outcrop, as it were,

in the last stage of his earthly existence, when the additions to his character, made in later years, have been entirely swept away. In connection with this point I may mention one idea which has occurred to me, and which I have never seen advanced; but which, if true, invests the subject of early impressions with a fearful interest. The science of statistics shows that certain crimes which are common in the seasons of youth disappear, comparatively, with advancing age, and re-appear again toward the close of life; or in other words, that the tendencies to indulgences in disorders of imagination, and habits which were acquired in the early life of a vicious youth, or one exposed to evil associations, though they may be masked and kept in subjection by the judgment and the influences of position and reputation during early manhood, middle life, and first decline, resume their sway and close the career of the man who has perhaps for years sustained a spotless reputation—with ignominy and shame. How frequently do cases of this kind present themselves! I have now in my mind's eye an individual who for forty years was known and esteemed as a model of honor, purity, and integrity, but who at the age of seventy committed a crime which consigned his name to infamy. Depend upon it, this man was subjected to evil influences in early life, and the impressions then made, though neutralized by the conditions and circumstances which afterwards surrounded him, were never effaced, and when the latter ceased to produce their restraining effects, the former resumed their original sway. Pursuing this train of thought we would conclude that the child is not merely the father of the man, but more emphatically, the father of the *old* man; that the term second childhood has a more extended signification than that of the mere decline of the faculties. It also should convey the idea that the tendency of the dispositions and propensities of individuals is to return to the condition of earlier life. This principle is important also in an historical point of view. The aged, though they may forget the occurrences of middle and after life, recall with vivid distinctness the impressions of childhood, and thus the grand-

father with senile garrulity, transmits the history of his early times, as it were, across an intervening generation to his grandson. This again makes an indelible impression upon the plastic mind of his youthful auditor, to be alike transmitted to *his* children of the third generation. Abundant examples might be adduced to illustrate the proposition of the vivid recurrence of the effects of early impressions apparently effaced. Persons who have for long years been accustomed to speak a foreign language, and who have forgotten the use of any other, have frequently been observed to utter their dying prayers in their mother tongue.

In this country, so far as I have observed, the course of education is defective in two extremes; it is defective in not imparting the mental habits or facilities which can most easily be acquired in early life, and it is equally defective in the other extreme, in not instructing the student, at the proper period, in processes of logical thought, or deductions from general principles. While elementary schools profess to teach almost the whole circle of knowledge, and neglect to impart those essential processes of mental art of which we have before spoken, our higher institutions, with some exceptions, fail to impart knowledge, except that which is of a superficial character. The value of facts, rather than of general principles, is inculcated. The one however is almost a consequence of the other. If proper seeds are not sown, a valuable harvest cannot be reaped.

The organization of a system of public education in accordance with my views would be that of a series of graded schools, beginning with the one in which the mere rudiments of knowledge are taught, and ending with that in which the highest laws of mind and matter are unfolded and applied. Every pupil should have the opportunity of passing step by step through the whole series, and honors and rewards should be bestowed upon those who graduated in the highest school. Few however as I have said before, would be found to possess the requisite talent and perseverance necessary to finish a complete course. But at whatever period the pupil may abandon his studies, he should be

found fitted for some definite pursuit or position in life, and be possessed of the moral training necessary to render him a valuable citizen and a good man.

These are some of the subjects which I commend for discussion at the present meeting of the Association. The great aim should be to enforce the importance of thorough early training and subsequent high education. It should be our object to bring more into repute profound learning, and to counteract the tendency to the exclusive diffusion of popular and mere superficial knowledge. We should endeavor to enlarge the pyramid of knowledge by symmetrical increments, by elevating the apex, and expanding the base, always observing the conditions of stable equilibrium.

ON THE MODE OF TESTING BUILDING MATERIALS,
AND AN ACCOUNT OF THE MARBLE USED IN THE EXTENSION
OF THE UNITED STATES CAPITOL.

(Proceedings American Association Adv. of Science, vol. ix, pp. 102-112.)*

August 16, 1855.

A commission was appointed by the President of the United States, in November, 1851, to examine the marbles which were offered for the extension of the United States Capitol, which consisted of General Totten, A. J. Downing, the Commissioner of Patents, the Architect, and myself. Another commission was subsequently appointed, in the early part of the year 1854, to repeat and extend some of the experiments, the members of which were General Totten, Professor Bache, and myself.

A part of the results of the first commission was given in a report to the Secretary of the Interior, and a detailed account of the whole of the investigations of these committees will ultimately be presented in full in a report to Congress; and I propose here merely to present some of the facts of general interest, or which may be of importance to those engaged in similar researches.

Though the art of building has been practiced from the earliest times, and constant demands have been made in every age, for the means of determining the best materials, yet the process of ascertaining the strength and durability of stone appears to have received but little definite scientific attention; and the members of the commission, who had never before made this subject a special object of study, were surprised with unforeseen difficulties at every step of their progress, and came to the conclusion that the processes usually employed for solving these questions are still in a very unsatisfactory state.

It should be recollected that while the exterior materials

* [Re-printed in Silliman's American Journal of Science, July, 1856; vol. xxii, pp. 30-38. Also in the Smithsonian Report for 1856; pp. 303-310.]

of a building are to be exposed for centuries, the conclusions desired are to be drawn from results produced in the course of a few weeks. Besides this, in the present state of science we do not know all the actions to which the materials are subjected in nature, nor can we fully estimate the amount of those which are known.

The solvent power of water, which attacks even glass must in time produce an appreciable effect on the most solid material, particularly where it contains, as the water of the atmosphere always does, carbonic acid in solution. The attrition of siliceous dusts, when blown against a building, or washed down its sides by rain, is evidently operative in wearing away the surface, though the evanescent portion removed at each time may not be indicated by the nicest balance. An examination of the basin which formerly received the water from the fountain at the western entrance of the Capitol, now deposited in the Patent Office, will convince any one of the great amount of action produced principally by water charged with carbonic acid. Again, every flash of lightning not only generates nitric acid, (which in solution in the rain acts on the marble,) but also by its inductive effects at a distance produces chemical changes along the moist wall, which are at the present time beyond our means of estimating. Also the constant variations of temperature from day to day, and even from hour to hour, give rise to molecular motions which must affect the durability of the material of a building. Recent observations on the pendulum have shown that the Bunker Hill Monument is scarcely for a moment in a state of rest, but is constantly warping and bending under the influence of the ever varying temperature of its different sides.

Moreover, as soon as the polished surface of a building is made rough from any of the causes aforementioned, the seeds of minute lichens and mosses, which are constantly floating in the atmosphere, make it a place of repose, and from the growth and decay of the microscopic plants which spring from these, dis-coloration is produced, and disintegration assisted.

But perhaps the greatest source of dilapidation in a climate like ours, is that of the alternations of freezing and thawing which take place during the winter season; but though this effect must be comparatively large, yet, in good marble, it requires the accumulated results of a number of years, in order definitely to estimate its amount.

From a due consideration of all the facts, the commission is convinced that the only entirely reliable means of ascertaining the comparative capability of marble to resist the weather, is to study the actual effects of the atmosphere upon it as exhibited in buildings which for years have been exposed to these influences. Unfortunately however, in this country, but few opportunities for applying this test are to be found. It is true some analogous information may be derived from the examination of the exposed surfaces of marble in their out-crops at the quarry; but in this case the length of time they have been exposed, and the changes of actions to which they may have been subjected during perhaps long geological periods, are unknown; and since different quarries may not have been exposed to the same action they do not always afford definite data for accurate comparative estimates of durability, except where different specimens occur in the same quarry.

As we have said before, the art of testing the quality of stone for building purposes is at present in a very imperfect state; the object is to imitate the operations of nature, and at the same time to hasten the effect by increasing the energy of the action, and after all, the result may be deemed but as approximative, or to a considerable degree—merely probable.

About twenty years ago an ingenious process was devised by M. Brard, which consists in saturating the stone to be tested with a solution of the sulphate of soda. In drying this salt crystallizes and expands, thus producing an exfoliation of surface which is supposed to imitate the effect of frost. Though this process has been much relied on, and generally employed, recent investigations made by Dr. Owen lead us to doubt its perfect analogy with that of the operations of nature. He found that the results produced by the actual

exposure to freezing and thawing in the air during a portion of winter, in the case of the more porous stones, produced very different results from those obtained by the use of the salt. It appears from his experiments that the action of the latter is chemical as well as mechanical.

The commission, in consideration of this, has attempted to produce results on the stone by freezing and thawing by means of artificial cold and heat. This process is however laborious; each specimen must be inclosed in a separate box fitted with a cover, and the amount of exfoliation produced is so slight that in good marble the operation requires to be repeated many times before satisfactorily comparable results can be obtained. In prosecuting this part of the inquiries, unforeseen difficulties have occurred in ascertaining precisely the amount of the disintegration, and it has been found that the results are liable to be vitiated by circumstances which were not foreseen at the commencement.

It would seem at first sight, (and the commission when it undertook the investigation held the opinion,) that but little difficulty would be found in ascertaining the strength of the various specimens of marbles. In this however it was in error. The first difficulty which occurred was to procure the proper instrument for the purpose. On examining the account of that used by Rennie, and described in the Transactions of the Royal Society of London, the commission found that its construction involved too much friction to allow of definite comparative results. Friction itself has to be overcome as well as the resistance to compression, and since it increases in proportion to the pressure, the stronger stones would appear relatively to withstand too great a compressing force.

The commission first examined a hydraulic press which had previously been employed in experiments of this kind, for the use of the Government, but found that it was liable to the same objection as that of the machine of Rennie. The commission was however extremely fortunate in obtaining subsequently, through the politeness of Commodore Ballard, commandant of the Navy Yard, the use of an admirable in-

strument devised by Major Wade, late of the United States Army, and constructed, under his direction, for the purpose of testing the strength of gun metals. This instrument consists of a compound lever, the several fulcrum of which are knife-edges opposed to hardened steel surfaces. The commission verified the delicacy and accuracy of the indications of this instrument by actual weighing, and found, in accordance with the description of Major Wade, the equilibrium was produced by *one* pound in opposition to *two hundred*. In the use of this instrument the commission was much indebted to the experience and scientific knowledge of Lieutenant J. A. Dahlgren, of the Navy Yard, and to the liberality with which all the appliances of that important public establishment were put at its disposal.

Specimens of the different samples of marble were prepared in the form of cubes of one inch and a half in dimension, and consequently exhibiting a base of two and a quarter square inches. These were dressed by ordinary workmen with the use of a square, and the opposite sides made as nearly parallel as possible by being ground by hand on a flat surface. They were then placed between two thick steel plates, and in order to insure an equality of pressure, independent of any want of perfect parallelism and flatness on the two opposite surfaces, a thin plate of lead was interposed above and below between the stone and the plates of steel. This was in accordance with a plan adopted by Rennie, and the one which appears to have been used by most—if not all—of the subsequent experimenters, in researches of this kind. Some doubt however was expressed as to the action of interposed lead, which induced a series of experiments to settle this question, when the remarkable fact was discovered that the yielding and approximately equable pressure of the lead caused the stone to give way at about half the pressure it would sustain without such an interposition. For example, one of the cubes, precisely similar to another which withstood a pressure of upwards of 60,000 pounds when placed in immediate contact with the steel plates, gave way at about 30,000 with lead interposed. This interesting fact was verified in a series of

experiments embracing samples of nearly all the marbles under trial, and in no case did an exception occur to vary the result.

The explanation of this remarkable phenomenon—now that it is known, is not difficult. The stone tends to give way by bulging out in the centre of each of its four perpendicular faces, and to form two pyramidal figures with their apices opposed to each other at the centre of the cube, and their bases against the steel plates. In the case where rigid equable pressure is employed, as in that of the thick steel plate, all parts must give way together. But in that of a *yielding* equable pressure, as in the case of interposed lead, the stone first gives way along the outer lines or those of least resistance, and the remaining pressure must be sustained by the central portion around the vertical axis of the cube.

After this important fact was clearly determined, lead and all other interposed substances were discarded, and a method devised by which the upper and lower surfaces of the cube could be ground into perfect parallelism. This consists in the use of a rectangular iron frame into which a row of six of the specimens could be fastened by a screw at the end. The upper and lower surfaces of this iron frame were wrought into perfect parallelism by the operation of a planing machine. The stones being fastened into this, with a small portion of the upper and lower parts projecting, the whole were ground down to a flat surface until the iron and the face of the cubes were thus brought into a continuous plane. The frame was then turned over and the opposite surfaces ground in like manner. Care was of course taken that the surfaces thus reduced to perfect parallelism, in order to receive the action of the machine, were parallel to the natural beds of the stone.

All the specimens tested were subjected to this process, and in their exposure to pressure were found to give concordant results. The crushing force exhibited was therefore much greater than that heretofore given for the same material.

The commission also determined the specific gravities of the different samples submitted to its examination, and also the quantity of water which each absorbs.

The commission considers these determinations, and particularly that of the resistance to crushing,—tests of much importance, as indicating the cohesive force of the particles of the stone, and its capacity to resist most of the influences before mentioned.

The amount of water absorbed may be regarded as a measure of the antagonistic force to cohesion, which tends, in the expansion of freezing, to disintegrate the surface. In considering however the indication of this test, care must be taken to make the comparison between marbles of nearly the same texture, because a coarsely crystallized stone may apparently absorb a small quantity of water, while in reality the cement which unites the crystals of the same stone may absorb a much larger quantity. That this may be so was clearly established in the experiments with the coarsely crystallized marbles examined by the commission. When these were submitted to a liquid which slightly tinged the stone, the coloration was more intense around the margin of each crystal, indicating a greater amount of absorption in these portions of the surface.

The marble chosen for the Capitol is a dolomite, or in other words, is composed of carbonate of lime and magnesia in nearly atomic proportions. It was analyzed by Dr. John Torrey, of New York, and Dr. Frederick A. Genth, of Philadelphia. According to the analysis of the former it consists, in hundredth parts, of—

Carbonate of lime	54.621
Carbonate of magnesia	43.932
Carbonate of protoxide of iron365
Carbonate of protoxide of manganese	(a trace)
Mica472
Water and loss610

The marble is obtained from a quarry in the south-easterly part of the town of Lee, in the State of Massachusetts, and belongs to the great deposit of primitive limestone which abounds in that part of the district. It is generally white, with occasional blue veins. The structure is fine-grained. Under the microscope it exhibits fine crystals of colorless mica.

and occasionally also small particles of bisulphuret of iron. Its specific gravity is 2·8620; its weight 178·87 lbs. per cubic foot; it absorbs 103 parts of an ounce per cubic inch, and its porosity is great in proportion to its power of resistance to pressure. It sustains 23,917 lbs. to the square inch. It not only absorbs water by capillary attraction, but, in common with other marbles, suffers the diffusion of gases to take place through its substance. Dr. Torrey found that hydrogen and other gases, separated from each other by slices of the mineral, diffuse themselves with considerable rapidity through the partition.

This marble, soon after the workmen commenced placing it in the walls, exhibited a discoloration of a brownish hue, no trace of which appeared so long as the blocks remained exposed to the air in the stone-cutter's yard. Various suggestions and experiments were made in regard to the cause of this remarkable phenomenon, and it was finally concluded that it was due to the previous absorption by the marble—of water holding in solution a small portion of organic matter, together with the absorption of another portion of water from the mortar.

To illustrate the process, let us suppose a fine capillary tube, the lower end of it immersed in water, and of which the internal diameter is sufficiently small to allow the liquid to rise to the top, and be exposed to the atmosphere, evaporation will take place at the upper surface of the column, a new portion of water will be drawn up to supply the loss; and if this process be continued any material which may be dissolved in the water, or mechanically mixed with it, will be found deposited at the upper orifice of the tube, or at the point of evaporation.

If however the lower portion of the tube be not furnished with a supply of water, the evaporation at the top will not take place, and the deposition of foreign matter will not be exhibited, even though the tube itself may be filled with water impregnated with impurities. The pores of the stones, so long as the blocks remain in the yard, are in the condition of the tube not supplied at its lower end with water, and

consequently no current takes place through them, and the amount of evaporation is comparatively small; but when the same blocks are placed in the wall of the building the absorbed water from the mortar at the interior surface gives the supply of the liquid necessary to carry the coloring material to the exterior surface, and deposit it at the outer orifices of the pores.

The cause of the phenomenon being known, a remedy was readily suggested, which consisted in covering the surface of the stone to be embedded in mortar with a coating of asphaltum. This expedient has apparently proved successful. The discoloration is gradually disappearing and in time will probably be entirely imperceptible.

This marble, with many other specimens, was submitted to the freezing process fifty times in succession. It generally remained in the freezing mixture for twenty-four hours, but sometimes was frozen twice in the same day. The quantity of material lost was .00315 part of an ounce. On these data Captain M. C. Meigs has founded an interesting calculation, which consists in determining the depth to which the exfoliation extended below the surface as the effect of its having been frozen fifty times. He found this to be very nearly the ten thousandth part of an inch. Now, if we allow the alterations of freezing and thawing in a year on an average to be fifty times each, which in this latitude would be a liberal one, it would require ten thousand years for the surface of the marble to be exfoliated to the depth of one inch. This fact may be interesting to the geologist as well as to the builder.

Quite a number of different varieties of marble were experimented upon. A full statement of the result of each will be given in the reports of the committees.

On molecular Cohesion.

At the meeting of the Association at Cleveland I made a communication on the subject of *Cohesion*.* The paper how-

*[Proceedings of the American Association for the Advancement of Science, July, 1853; vol. VII, p. 270. Only the title published.]

ever was presented at the last hour; the facts were not fully stated and have never been published. I will therefore briefly occupy your time in presenting some of the facts I then intended to communicate, and which I have since verified by further experiments and observations.

In a series of experiments made some ten years ago I showed that the attraction of the particles for each other—of a substance in a liquid form—was as great as that of the same substance in a solid form.* Consequently, the distinction between liquidity and solidity does not consist in a difference in the attractive power occasioned directly by the repulsion of heat; but it depends upon the perfect mobility of the atoms,—the loss of adhesion, or *lateral* cohesion. We may explain this by assuming an incipient crystallization of atoms into molecules, and consider the first effect of heat as that of breaking down these crystals and permitting each atom to move freely around each other. When this crystalline arrangement is perfect, and no lateral motion is allowed in the atoms, the body may be denominated perfectly rigid. We have approximately an example of this in cast-steel, in which no slipping takes place of the parts on each other, or no material elongation of the mass; and when a rupture is produced by a tensile force a rod of this material is broken with a transverse fracture of the same size as that of the original section of the bar. In this case every atom is separated at once from the other, and the breaking weight may be considered as a measure of the aggregate attraction of cohesion among the whole transverse series of the atoms of the metal.

The effect however is quite different when we attempt to pull apart a rod of lead. The atoms or molecules slip upon each other. The rod is increased in length and diminished in thickness until a separation is produced. Instead of lead we may use still softer materials, such as wax, putty, &c., until at length we arrive at a substance in a liquid form. This will stand at the lower extremity of the scale, and between extreme rigidity on the one hand, and extreme liquid-

*[Proceedings Am. Philosophical Society. See *ante*, page 217.]

ity on the other, we may find a series of substances gradually shading from one extremity to the other.

According to the views I have presented the difference in the tenacity in steel and lead does not consist in the attractive *cohesion* of the atoms, but in their capability of slipping upon each other. From this view it follows that the form of the material ought to have some effect upon its tenacity, and also that the strength of the article should depend in some degree upon the process to which it has been subjected.

For example, I have found that softer substances in which the outer atoms have freedom of motion, while the inner ones by the pressure of those exterior are more confined, break unequally; the inner fibres (if I may so call the rows of atoms) give way first and entirely separate, while the exterior fibres show but little indications of a change of this kind.

If a cylindrical rod of lead three quarters of an inch in diameter be turned down on a lathe in one part to about half an inch, and then be gradually broken by a force exerted in the direction of its length, it will exhibit a cylindrical hollow along its axis of half an inch in length, and at least a tenth of an inch in diameter. With substances of greater rigidity this effect is less apparent, but it exists even in iron, and the interior fibres of a rod of this metal may be entirely separated, while the outer surface presents no appearance of change.

From this it would appear, that metals should never be elongated by mere stretching, but in all cases by the process of wire-drawing, or rolling. A wire or bar must always be weakened by a force which permanently increases its length—without at the same time compressing it.

Another effect of the lateral motion of the atoms of a soft heavy body, when acted upon by a percussive force with a hammer of small dimensions in comparison with the mass of metal, (for example, if a large shaft of iron be hammered with an ordinary sledge,) is a tendency to expand the surface so as to make it separate from the middle portions. The interior of the mass by its own inertia becomes as it were an anvil, between which and the hammer, the exterior portions

are stretched longitudinally and transversely. I here exhibit to the Association a piece of iron—originally from a square bar four feet long, which has been so hammered as to produce a perforation of the whole length entirely through the axis. The bar can be seen through as if it were the tube of a telescope.

This fact appears to me to be of great importance in a practical point of view, and may be connected with many of the lamentable accidents which have occurred in the breaking of the axles of locomotive engines. These, in all cases, ought to be formed by *rolling* and not with the hammer.

The whole subject of the molecular constitution of matter offers a rich field for investigation, and isolated facts which are familiar to almost every one, when attentively studied, will yield results alike interesting to abstract science and to practical art.

ON THE EFFECT OF MINGLING RADIATING SUBSTANCES WITH COMBUSTIBLE MATERIALS.

(Proceedings American Association Adv. of Science, vol. ix, pp. 112–116.)

August 17, 1855.

I beg leave to call the attention of the Association for a few moments to a paper published by our distinguished countryman, Count Rumford, in 1802, in the first volume of the Journal of the Royal Institution of Great Britain, page 28, entitled “Observations relative to the Means of Increasing the Quantities of Heat obtained in the Combustion of Fuel.”

“It is a fact,” says Count Rumford, “which has long been known, that clay and several other incombustible substances, when mixed with sea-coal in certain proportions, cause the latter to give out more heat in its combustion than it can be made to produce when it is burnt pure or unmixed.”

“It has been ascertained that when the sides and back of an open chimney fire-place, in which coals are burnt, are

composed of fire-brick and heated red hot, they throw off into the room more heat than the burning coals themselves."

"The fuel therefore," says Count Rumford, "should be disposed or placed so as to heat the back and sides of the grate, which must always be constructed of fire-brick and never of iron."

The vertical stratum of coal should be as thin as is consistent with perfect combustion, for a large mass of coal in the grate arrests the rays which proceed from the back and sides of the grate, and prevents their coming into the room. The grate or fire-place itself may be so contrived as to produce a proper degree of radiation, but when this is not the case, Rumford advises that the bottom of the grate be covered with a single layer of balls of fire-brick, each perfectly globular and two and a half or two and three quarters inches in diameter. "On this layer of balls fire is to be kindled, and in filling the grate more balls are to be added with the coals, care being taken to mix the coals and balls well together in due proportions. If this is done the fire will not only be very beautiful, but will send off a much greater quantity of radiant heat into the room than without them." Rumford also declares that these balls cause the cinders to be almost entirely consumed. "The same effect is said to be produced by the mixture of coals and clay when the fuel is burnt in a close fire-place, such as an iron stove; and it is the custom in the Netherlands to mix moistened clay with the coals before they are introduced into a stove of this form."

Count Rumford gives no account, in the paper I have cited, of experiments by which the fact of the greater radiation from the balls was tested.

In reading his paper some years since the idea occurred to me that this experiment would be worthy of repetition, with the more manageable and delicate appliances which science has of late years furnished for the use of the investigator. For this purpose I employed the thermo-electrical apparatus of Melloni, furnished with a tube like a telescope to circumscribe the field of radiation, and the result confirmed the statement of Rumford that more heat was radiated from

pieces of fire-brick mingled with the coal than from the combustible itself. The effect however would probably have been greater with bituminous coal. The arrangement for experimenting with coal in a fire-place was very imperfect, and I had recourse to the heat produced by the flame of a spirit lamp, and also of a jet of hydrogen. A flame of this kind was placed before the thermo-pile at such a distance that the needle of the galvanometer stood at 15° ; the end of the platinum wire coiled into a spiral form was then introduced into the flame, and an instant increase of the radiation of heat was observed, the galvanometer advancing to 27° .

It has long been well known that the introduction of a platinum wire into a pale flame of this character greatly increases the radiation of light, and from this experiment it is evident that the radiation of heat is increased in a like degree. After this a number of different substances were employed, such as glass, carbonate of lime, sulphate of lime, stone coal, fire clay, &c. The greatest effect appeared to be produced with pieces of carbonate of lime. The exact order however could not be determined without procuring a series of balls of the same diameter of these different substances. The most striking effect was produced at the very top of the flame, placing the platinum wire in the heated though almost non-luminous air, immediately above the highest point of combustion.

We cannot suppose in these experiments that the absolute amount of heat produced by the combustion of a given quantity of fuel is increased. The most probable conjecture is that the heat of combination is converted into radiant heat, and that the flame itself is cooled in proportion as the radiation is increased. In order to bring this idea to the test of experiment, a slip of mica, one-fifth of an inch in breadth, was introduced vertically into the lower part of the flame, while the platinum wire occupied the space just above the top. The slip of mica was placed with its flat side vertically so as not to affect by its radiation the heat of the wire. With this arrangement the radiation of heat from the platinum

was diminished. A corresponding diminution was also produced in the amount of radiant light given off, and this was readily perceptible to the sight. This effect was not due to the cooling of the flame by the conduction of the mica, since it is almost a non-conductor of heat, and this property was exhibited by the fact that the luminosity of the mica was confined to that part which was at the surface of the flame on either side.

It appears therefore from these experiments, that the introduction of a solid of great radiating power into a mass of materials in a state of combustion, increases the amount of heat thrown into the space around, without increasing the absolute quantity produced by combustion, the increase of radiant heat being at the expense of the heat of combination. To give a practical illustration of the condition of the matter, if a given quantity of fuel is employed in evaporating water, by combustion under a kettle, the useful effect would be diminished by inserting in the flame beneath or amid the combustible a better radiating substance than itself, while in the case of a fire to warm a room the effect would be directly opposite; a greater amount of heat would be thrown into room, and less of the heat of combustion would be carried up the chimney with the escaping gas. Or to give another example. If over a coal fire a boiling pot be suspended, and a roasting oven before it, the introduction of a radiating material would increase the effect on the latter at the expense of that on the former.

Count Rumford has elsewhere shown that flame is a bad conductor of heat, and in stoves and boilers heated by flame it is therefore necessary that the draft be made to impinge with considerable force upon projecting portions of the metal in order that the greatest amount of heat may be absorbed.

If a column of heated air moves rapidly through a perpendicular stove-pipe, but a comparatively small portion of the heat will be absorbed by the metal and radiated into space around. A cylindrical stratum of non-conducting air in contact with the metal will be comparatively at rest, and through this the moving column of heated air will rapidly

ascend, without communicating its heat to the metal. If however in this case the current of air be obstructed, and the cylindrical motion deranged by partially closing a damper, the heat immediately around the point of obstruction will be greatly increased. With a proper arrangement of parts I have known a dark stove-pipe immediately to become red opposite and above the damper by the partial closing of this valve. It is probable that heat might be economized in certain cases by introducing radiating materials in flues. It should however be recollected that the draft would be impeded by the introduction of foreign materials: 1st. On account of a direct obstruction; and, 2nd. Because of the diminished temperature.

It is frequently stated in works on chemistry that the heating power of the flame of the compound blowpipe is very great, while its illuminating power is quite small. The truth is however that the radiation of heat from its flame is only commensurate with its radiation of light, and that what tends to increase the one will also increase the other.

The radiation from heated—though non-luminous air, would, from these views, appear to be small, though from meteorological considerations they would seem to be considerable.

That a solid substance increases the radiation of the heat of a flame is an interesting fact in connection with the nature of heat itself. It would seem to show that the vibrations of gross matter are necessary to give sufficient intensity of impulse to produce the phenomena of ordinary radiant heat. Also, since the light is much increased by the same process we would infer that by means of the solid the vibrations constituting heat are actually converted into those which produce the phenomena of light. The whole subject is worthy of further investigation, both in a practical and abstract scientific point of view.

ACCOUNT OF EXPERIMENTS ON THE ALLEGED SPONTANEOUS
SEPARATION OF ALCOHOL AND WATER.

(Proceedings American Association Adv. of Science, vol. ix, pp. 140-144.)

August 20, 1855.

At the last meeting of the American Association a notice was given of a new process for procuring alcohol, for which a patent had been granted. The weak spirit, left to itself in a vessel of great height, was said to separate spontaneously into a strong alcohol, which rose to the top of the column, and into a weaker spirit which was found at the bottom.

For the following statement and remarks relative to granting the patent I am indebted to Dr. Leonard Gale, one of the principal examiners of the Patent Office:

“When the alleged invention was presented much doubt was expressed as to the working of the plan, and the author was requested to answer the following questions to satisfy the office on the subject:

“‘Have you employed this device for purifying alcohol or whiskey? If so, please state what kind, what size, and what proportioned apparatus you have used on a working scale, and what results you have obtained.’

“To this the applicant replies—

“‘I have used this device as a mode of separating alcohol from whiskey for several months. The column was of wrought iron about one hundred feet high, and twelve inches in diameter. It was elevated from the cellar through and above the building; the whiskey was forced in from the upper room of the building through an iron pipe leading over the top of the column, and down the inside about fifty feet. This sized column will, I find, separate about two hundred gallons of alcohol from the water in the space of twelve hours. The larger the diameter the more rapid the process of separation.’

“It had been stated by the party in correspondence that he had been led to the trial of the experiment by noticing

that the liquor in the upper part of a tall standing cask was thought to be stronger than that drawn out near the bottom."

This statement would seem to receive some countenance from the following remarks on the same subject in Gmelin's *Treatise on Chemistry*, vol. 1, p. 112, English edition:

"Similarly brandy kept in casks is said to contain a greater proportion of spirit in the upper, and of water in the lower part. Here again the question may be raised whether the cask may have been filled with successive portions of different strengths which may have disposed themselves in layers one above another."

"As to the propriety," says Dr. Gale, "of granting or refusing a patent, on the evidence before the office, in consideration of the oath of the inventor, the want of means in the office to satisfactorily verify or disprove the experiment, and lastly, the subsequent statement of the inventor that he had verified the experiment by several months' work on a practical scale, these facts were regarded as good ground for issuing the patent. If the party should be found to have made a false statement, and so committed a fraud on the Patent Office, these acts were his own, and for which he must be held responsible."

If the result said to be obtained were true, it would follow that the affinity of bodies for each other would be modified by pressure. Though from theoretical considerations, it might not be thought impossible that the attraction of two substances for each other might be increased by an increase of pressure, yet there is no antecedent probability that the attraction would be diminished under this influence. But as an account of this invention had been widely circulated in the newspapers, its author had received from the Patent Office the right to vend the privilege of its use, and the public were exposed to be defrauded in the purchase of that which was worthless, it seemed desirable to settle the question as to the truth of the principle by direct experiment, irrespective of theoretical considerations, and on a scale of sufficient magnitude to leave no doubt as to the result.

With this view, in behalf of the Smithsonian Institution,

I accepted the proffered co-operation of Professor George C. Schaeffer, of the Patent Office, and directed the putting up of the necessary apparatus in one of the towers of the Smithsonian building. The determination of the density of the liquid, and the details of the experiments, were intrusted to Professor Schaeffer, to whom I am also indebted for the following account of the process employed and the results obtained.

As the successful experiment was said to have been made with a column of liquid nearly one hundred feet high, and as the pressure of such a column was given as the cause of the separation of the water or alcohol from the mixture, the repetition of the experiment should be on a corresponding scale.

The great tower of the Institution building was already fitted for experiments requiring like conveniences. A well, or series of openings giving a height of over one hundred feet, passing through several stories was the place selected. A series of stout iron tubes of about an inch and a half in internal diameter formed the column, the total length of which was one hundred and six feet. Four stop-cocks were provided, one at bottom, one about four feet from the top, and the other two to divide the interval equally or nearly so.

The liquor used was common rye whiskey of 44 per cent. at 60° Fahr., and of 44 on the United States Revenue hydrometer, one of which was used in testing the liquor.

The experiment commenced on the 18th of November, 1854; a leak occurring caused the trial to be limited to the lower thirty feet, after the lapse of a few hours. On the 20th the tube was refilled, and after testing at intervals of a few days, the loss was supplied, the whole apparatus, with each cock and the top sealed up, was left to itself until December 14th, when it was again tried at each cock. With a slightly diminished quantity, about one hundred feet in height, the whole again stood until the 18th of April, 1855, when the tests were again made.

Fortunately for the result, the original liquor had been repeatedly tested at different temperatures; the contents of

every vessel used to contain it having been tried at each of the several fillings of the tube, which were made on the first days of the experiment, when a leak required its discharge for the purpose of tightening the joints. A portion of the original liquid which had been set aside was also tried at the end of the experiment, and at different temperatures.

The readings of the hydrometer were made with as much accuracy as possible under the circumstances, some of them being taken late at night and exposed in the open tower to a violent wind. No pains were spared to test the liquid under every variety of circumstances. At first the windows of the tower were open, but for the last two or three months they were closed. Fifty-four readings were made; nineteen of which were from the original liquid, and the remainder on that drawn from the different cocks. The result may be stated, as follows:

On plotting the readings of indication and temperature they all follow nearly in the same line, the deviations of those taken from the original fluid being quite as great as those taken from either the bottom or top, even after the lapse of months. Or in other words, within the limits of error (the extreme being but a portion of a degree of the hydrometer), there is not the slightest indication of any difference of density between the original liquor and that from the top or bottom of the column after the lapse of hours, days, weeks, or months. The fluid at the bottom of the tube, it must be remembered, was for five months exposed to the pressure of a column of fluid at least one hundred feet high. This pressure however is much within that at which inferior champagne bottles are burst, and if pressure alone could produce such an effect, wine of that kind should have long ere this given instances of it.

As the fact has been taken for granted, and chemists of repute have made use of it, there seems good ground for thus formally refuting an error which, at first sight, would not appear worthy of being dignified by so much notice.

REMARKS ON THE UNITED STATES LIGHT-HOUSE SERVICE.

(Report of the United States Light-House Board for 1873, pp. 3-7.)

October 14, 1873.

No part of the executive branch of the Government includes more diversified duties or involves greater responsibilities than the Light-House Establishment.

The character of the aids which any nation furnishes the mariner in approaching and leaving its shores, marks in a conspicuous degree, its advance in civilization. Whatever tends to facilitate navigation, or to lessen its dangers, serves to increase commerce, and hence is of importance not only to the dwellers on the seaboard, but to the inhabitants of every part of the country. Whoever has surplus products of industry to dispose of has a pecuniary interest in the improvement of commerce.

Every shipwreck which occurs enhances the cost of transportation, and therefore affects the interests of the producer. But it is not alone in view of its economical effects that the light-house system is to be regarded. It is a life-preserving establishment, founded on the principles of Christian benevolence. None can appreciate so well the value of a proper system of this kind as he who has been exposed for weeks and perhaps months to the perils of the ocean, and is approaching in the darkness of night a lee shore. He looks then, with anxious gaze, for the friendly light which is to point the way amid treacherous rocks and sunken shoals to a haven of safety. Or it may be in mid-day, when observations cannot be had, the sun and coast being hid by dense fogs, such as imperil navigation on our northern and western coasts. He then listens with breathless silence for the sound of the fog-trumpet which shall insure his position and give him the desired direction of his course.

With that entire confidence which is inspired by a perfect light-house system the alternatives of life and death, of riches and poverty, are daily hazarded; and therefore it is of the

first importance that the signals, whether of light or sound which indicate the direction of the course, and the beacons which mark the channel, shall be of the most improved character, and that they be under the charge of intelligent, efficient, and trustworthy attendants. But above all, one maxim should ever be observed, namely perfect regularity of exhibition of every signal from night to night and from year to year. A light for example which has been regularly visible from a tower, (it may be for years,) cannot be suffered to fail for a single night, or even for a single hour, without danger of casualties of the most serious character. A failure of such a light to send forth its expected ray is as it were a breach of a solemn promise, which may allure the confiding mariner to a disastrous ship-wreck, or to an untimely death.

In view of these facts our Government early established a light-house system, which though simple and inexpensive at first, has since been extended and improved to meet the wants of an increasing commerce and the unrivalled resources of the country. It has been maintained with an enlightened liberality which indicates a just appreciation of its importance.

The magnitude of the light-house system of the United States may be inferred from the following facts: First, the immense extent of the coast which from the St. Croix River on the boundary of Maine, to the mouth of the Rio Grande in the Gulf of Mexico, includes a distance of over 5000 miles; on the Pacific coast a length of about 1,500 miles; on the great northern lakes about 3,000 miles, and on inland rivers about 700 miles, making a total of more than 10,000 miles. Secondly, the magnitude of the system is exhibited by the fact that nearly every square foot of the margin of the sea throughout the whole extent of 5,000 miles along the Atlantic and Gulf coast is more or less illuminated by light-house rays, the mariner rarely losing sight of one light until he has gained another. Thirdly, the same fact is illustrated by the number of signals now in actual existence as exhibited in the following table:

Total Signals for the Entire Establishment.

Light-houses and lighted beacons.....	591
Light-houses and lighted beacons finished and lighted during the year ending July 1, 1873	29
Light-ships	21
Fog-signals, operated by steam or hot-air engines	35
Day or unlighted beacons.....	363
Buoys in position	2,838

To carry on so extended a system necessarily requires a carefully devised organization, based upon the history of all that has been recorded in regard to the subject, and a series of efficient officers and trained assistants.

The duties which belong to the light-house system involve the most varied knowledge and practical skill, a thorough acquaintance with the wants of commerce, engineering abilities of high order, with scientific acquirements, which shall appreciate the value of every new discovery that may find an application in the improvement of signals, and the ability to make or direct such investigations as may from time to time be found desirable. To insure these requisites the organization of the light-house system includes: First, a Light-House Board, consisting of two officers of the Navy, two engineer officers of the Army, and two scientific civilians, with the addition of an officer of the Navy and an engineer officer of the Army as secretaries, who are also members of the Board: Secondly, it also includes twelve inspectors from the Army or Navy, and as many engineer officers from the Army, who have united charge of the twelve districts into which the coast is divided.

The Light-House Board, having charge of the supervision of the whole system, is divided into five committees, to each of which special duties are assigned. These committees are on finance, engineering, floating aids, lighting, and experiments. It is the duty of each member of the Board to render himself intimately acquainted with the details of the business intrusted to his care, as well as to keep himself informed, as far as possible, of the condition of the general system. For this purpose, as well as that of insuring the proper working of the establishment in the several districts, it is advisable that

he should make, from time to time, inspections of light-houses at various points on the coast. The inspector of each district is required to visit, at stated intervals, each light-house within his jurisdiction after completion by the engineers, to correct any delinquencies on the part of the keepers, and to supply oil and other materials necessary to the efficient maintenance of the signals, and finally to inform the engineer as to any repairs which may be required. The district engineers, as well as the engineer officers of the board, find full employment for all the theoretical knowledge and practical skill they possess in the surveys of new sites, making studies for the construction of new permanent aids to navigation (many of them on submarine sites in exposed positions), in planning and rearing the towers, and in fitting up the lenticular apparatus. - - -

It has been thought that the light-house system is of a practical character, and therefore does not require the aid of high science. But in regard to this, it may be observed that the present system of light-house apparatus, now in use in every part of the civilized world, was invented and introduced into practice in its minutest details by a man of abstract science, the celebrated Fresnel, who shared with Young, of England, the invention of the undulatory theory of light, and its application to all the phenomena of optics.

The light apparatus introduced by the Board as a substitute for that previously in use is principally that of the French system. But the Board has from the first been alive to the introduction of improvements and has carefully considered every suggestion and tested every invention which gave promise of greater economy or efficiency. Instead of sperm-oil, which was first employed, it has introduced, at one-third of the cost, lard-oil, and with this, a required modification of the lamps, particularly those of the larger kind, in order that the oil may be burned at a higher temperature, especially in the northern portions of the United States.

But the greatest improvement which has been introduced is that relative to fog-signals,—indispensable aids to navigation, especially on the northeastern, and western portions of

our coast. At first these signals were principally confined to bells, weighing in some cases from 2,000 to 2,500 pounds. These were rung by winding up a weight which in its descent gave motion to a hammer striking the bell. In regard to this signal, an improvement has been introduced, by which an expenditure of about one-tenth of the power produces an equal effect. Bells are still used in cases where the signal is required to be heard only at a comparatively small distance, but in most cases much more powerful instruments are required, such as are founded on what is called resonance, in which the air itself is the resounding body as well as the conductor of sound. These instruments are of three kinds: first, the ordinary locomotive whistle, much enlarged in size and somewhat modified in form, and blown by steam from a high-pressure tubular boiler; second, the reed-trumpet actuated by air condensed in a reservoir by the power of a caloric engine; third, the syren-trumpet, operated by steam from a boiler sustaining a pressure of from 50 to 70 pounds per square inch. The sound from these instruments is many times more powerful than that from the largest bells. - - - -

The Light-House Board, during the past year, desirous of acquainting itself minutely with any improvements which of late years may have been introduced into the light-house service in Europe, obtained the sanction of the honorable the Secretary of the Treasury to commission Major George H. Elliot, of the Corps of Engineers of the Army and engineer-secretary of the Board, to visit Europe and report upon everything which he might observe relative to light-house apparatus and the management of light-house systems. He has lately returned, after having gathered information which will prove of importance in its application in our country, as is evident from his preliminary report.

Major Elliot was everywhere received with marked cordiality, and every facility was given him to inspect the various coasts and systems of administration, of which full information was furnished him, together with the drawings and models necessary for a perfect acquaintance with the latest improvements which have been adopted in Great Bri-

tain and on the continent. The special thanks of the Board are due to His Royal Highness the Duke of Edinburgh, the master; to Sir Frederick Arrow, the deputy master; and the elder brethren of Trinity House, for the warmth of their reception and the marked distinction they conferred upon him as the representative of the Board; and to M. Leonce Reynaud, inspector-general of bridges and roads, and director of the French light-house service, for his efforts to make the visit of Major Elliot profitable to his country and agreeable to himself.

RESEARCHES IN SOUND, IN RELATION TO FOG-SIGNALLING.

(Report of the United States Light-House Board for 1874, pp. 83-87.)

INTRODUCTION.

Fog.—Among the impediments to navigation none perhaps is more to be dreaded than that which arises from fogs; and consequently the nature of this impediment and the means which may be devised for obviating it are objects of great interest to the mariner. Fogs are in all cases produced when cold air is mingled with warm air saturated with moisture. In this case the invisible vapor of the warmer air is condensed by the cold into minute particles of liquid water, which by their immense number and multiplicity of reflecting surfaces obstruct the rays of light in the same way that a piece of transparent glass when pounded becomes almost entirely opaque and is seen by reflection as a white mass. So greatly does a dense fog obstruct light that the most intense artificial illumination, such as that produced by the combustion of magnesium, by the burning of oxygen and hydrogen in contact with lime, and that produced between the charcoal points of a powerful electrical apparatus are entirely obscured at comparatively short distances. Even the light of the sun, which is far more intense than that of any artificial illumination, is so diminished by a single mile of dense fog that the luminary itself becomes invisible. Recourse must therefore be had to some other means than that of light to enable the mariner to recognize his position on approaching the coast when the land is obscured by fog.

The only means at present known for obviating the difficulty is that of employing powerful sounding instruments which may be heard at a sufficient distance through the fog to give timely warning of impending danger. Investigations therefore as to the nature of sound and its applications to fog-signals become an important object to those in charge of aids to navigation. Such investigations are of special im-

portance in connection with the light-house service of the United States. The north-eastern coast of the United States on the Atlantic, and the entire western coast on the Pacific, included in our territory, are subject—especially during the summer months, to dense fogs which greatly impede navigation as well as endanger life and property.

The origin of the fogs on our coast is readily explained by reference to a few simple principles of physical geography. In the Atlantic ocean there exists a current of warm water proceeding from the Gulf of Mexico between Cuba and Florida which flows along our coast to the latitude of about 35° , and turning gradually to the eastward crosses the Atlantic and impinges against the coast of northern Europe. Throughout its entire course, on account of the immense capacity of water for heat, the temperature of the stream is greater than that of the ocean on either side. In addition to this stream the Atlantic ocean is traversed by another current of an entirely opposite character, one of cold water, which coming from the arctic regions down Davis's Strait, is thrown by the rotation of the earth, against our coast, passing between it and the Gulf-stream, and sinking under the latter as it approaches the southern extremity of the United States. These conditions are those most favorable to the production of fogs, since whenever the warm air, surcharged with moisture, is blown from the Gulf-stream in the Atlantic—over the arctic current along the coast, and mingles with the cold air of the latter, a precipitation of its vapor takes place in the form of fog. Hence, especially in summer, when the wind in the eastern part of the United States is from a south-easterly direction, fogs prevail. As we proceed southerly along the coast, the fog-producing winds take a more easterly direction.

A somewhat similar circulation in the Pacific ocean produces fogs on the western coast of the United States. In this ocean a current of warm water, starting from the equatorial regions, passes along the shores of China and Japan, and following the general trend of the coast, turns eastward and continues along our shore. The northern part of this cur-

rent being warmer than the ocean through which it passes, tends to produce dense fogs in the region of the Aleutian Islands and the coast of Alaska. As this current descends along the American coast into lower latitudes it gradually loses its warmth, and soon assumes the character—in regard to the water through which it passes, of a comparatively colder stream; and to this cause we would attribute the prevalence of fogs on the coast of Oregon and California, which are most prevalent during the spring and early summer, with wind from the north-west and west.

From what has been said, it is evident that the fogs in the Aleutian Islands occur chiefly in summer, when south-westerly winds prevail and mingle the moist air from the warm current with the colder air of the more northerly latitude. In winter, the wind being from the north chiefly, the moist air is driven in an opposite direction, and dense fogs therefore at this season do not prevail.

In regard to the fogs on the coast of Maine, the following interesting facts were furnished me by the late Dr. William Stimpson, formerly of the Smithsonian Institution, and of the Chicago Academy of Sciences, who had much experience as to the weather during his dredging for marine specimens of natural history in the region of Grand Manan Island, at the entrance of the Bay of Fundy.

“So sharply marked,” says Dr. Stimpson, “is the difference of temperature of the warm water from the Gulf-stream and that of the polar current, that in sailing in some cases only a few lengths of a ship the temperature of the water will change from 70° to 50° . The fog frequently comes rolling in with the speed of a race-horse; in some cases while dredging, happening to turn my eyes to the south, a bank of fog has been seen approaching with such rapidity that there was scarcely time in which to take compass-bearing of some object on shore by which to steer, before I would be entirely shut in, perhaps for days together.” He also mentions the fact that it frequently happened during a warm day, while a dense fog existed some distance from the shore, close in to the latter there would be a space entirely clear; this was

probably due to the reflection and radiation of the heat from the land, which converted the watery particles into invisible vapor.

Dr. Stimpson has also noticed another phenomenon of some interest. "When a dense fog, coming in regularly from the sea, reaches the land, it gradually rises in the atmosphere and forms a heavy, dark cloud, which is frequently precipitated in rain." This rising of fog is not due, according to the doctor, to a surface-wind from the west pressing under it and buoying it upward, since the wind at the time is from the ocean. It is probably due to the greater heat of the land causing an upward current, which when once started, by its inertia carries the cloud up to a region of lower temperature, and hence the precipitation. The height of the fog along the coast is not usually very great, and can be frequently overlooked from the mast-head. The deception as to size and distance of objects as seen in a fog is also a remarkable phenomenon when observed for the first time. A piece of floating wood at a little distance is magnified into a large object, and after much experience the doctor was not able to overcome the delusion. It is said that the sailors in the Bay of Fundy prefer of two evils a fog that remains constant in density to one that is variable, although the variation may be toward a greater degree of lightness, on account of the varying intensity producing a varied and erroneous impression of the size and distance of the objects seen through it. It is also his impression that sound can be heard as well during fog as in clear weather, although there is a delusion even in this, since the source of sound when seen, appears at a greater distance than in a clear atmosphere, and hence the sound itself would appear to be magnified.

Fogs also exist on the Mississippi, especially on the lower portion of the river. They are of two classes, those which result from the cooling of the earth, particularly during the summer in clear nights, with wind probably from a northerly direction, followed by a gentle, warm wind from the south surcharged with moisture, and the other induced by the water of the river, which coming from melting snow of

northern regions, is colder than the air in the vicinity. The air over the river being thus cooled below the temperature of a gentle wind from the south, the moisture of the latter is precipitated. This fog, which occurs in the last of winter, during the spring, and beginning of summer, is very dense, but is confined entirely to the atmosphere above the river, while the other class of fog exists over the land as well.

Fog-signals.—The importance of fog-signals as aids to navigation, especially on the north-eastern portion of our coast, the shore of which is exceedingly bold and to the approach of which the sounding-line gives no sure indication, has been from the first an object of special attention.

At the beginning of the operations of the Light-house Board, such instruments were employed for producing sound as had been used in other countries; these consisted of gongs, bells, guns, horns, &c. The bells were actuated by clock machinery, which was wound up from time to time and struck at intervals of regular sequence by which their position might be identified. The machinery however by which these bells was struck was of a rude character and exceedingly wasteful of power, the weight continuing to descend during the whole period of operation, including the successive intervals of silence. This defect was remedied by the invention of Mr. Stevens, who introduced an escapement arrangement, similar to that of a clock, which is kept in motion by a small weight, a larger one being brought into operation only during the instant of striking.

Bell-buoys were also introduced at various points. These consisted of a bell supported on a water-tight vessel and rung by the oscillation of the waves. But all contrivances of this kind have been found to be untrustworthy; the sound which they emit is of comparatively feeble character, can be heard at but a small distance, and is frequently inefficient during a fog which occurs in calm weather. Besides this, automatic fog-signals are liable to be interfered with by ice in northern positions, and in all sections—to derangement at times when no substitute can be put in their place, as can be in the cases of the bells rung by machinery under the immediate con-

trol of keepers. A signal which is liable to be interrupted in its warnings is worse than no signal, since its absence may give confidence of safety in the midst of danger, and thus prevent the necessary caution which would otherwise be employed.

Guns have been employed on the United States coast, first under the direction of General Bates, engineer of the twelfth district, at Point Bonita, San Francisco Bay, California. The gun at this station consisted of a 24-pounder, furnished by the War Department. The necessary arrangements being made, by the construction of a powder-house, and laying of a platform, and employment of a gunner,—notice to mariners was given that after the 8th of August, 1856, a signal-gun would be fired every hour and half-hour, night and day, during foggy or thick weather. The first year, with the exception of eighty-eight foggy days, omitted for want of powder, 1390 rounds were fired. These consumed 5560 pounds of powder, at a cost of \$1,487, pay of gunner and incidentals excluded. The following year the discharges were 1582, or about one-eleventh of the number of hours and half-hours of the whole time. The fog-gun was found to answer a useful purpose; vessels by the help of it alone having come into the harbor during a fog at night, as well as in the day, that otherwise could not possibly have entered. This signal was continued until it was superseded by a bell-boat. A gun was also used at West Quoddy Head, near the extreme eastern part of Maine. It consisted of a short piece or carronade, 5 feet long, with a bore of $5\frac{1}{4}$ inches, charged with four pounds of blasting powder. The powder was made up in cartridges and kept in chests in the work-house. The gun was only fired on foggy days, when the steamboat running between Boston and Saint John, New Brunswick, was approaching the light-house from the former place. In going in the other direction the signal was not so much required, because in the former case (of approach) the vessel had been for some time out of sight of land, and consequently its position could not be so well known. The firing was commenced with the hearing of the steamer's whistle as she

was approaching, and as the wind during the fog at this place is generally from the south, the steamer could be heard five or six miles. The firing was continued as frequently as the gun could be loaded until the steamer answered by a signal of three puffs of its whistle. The number of discharges was from one to six, the latter exhausting a keg of powder valued at \$8. The keeper of the light-house acted as gunner without compensation other than his salary. The cost of powder was paid by the steamboat company. The report of the gun was heard from two to six miles.

This signal has been abandoned,—because of the danger attending its use—the length of the intervals between the successive explosions—and the brief duration of the sound, which renders it difficult to determine with accuracy its direction.

The lamented General Hartman Bache, of the Light-House Board, adopted a very ingenious plan for an automatic fog-signal which consisted in taking advantage of a conical opening in the coast, generally designated a blow-hole. On the apex of this hole he erected a chimney which terminated in a tube surmounted by a locomotive whistle. By this arrangement a loud sound was produced as often as a wave entered the mouth of the indentation. The penetrating power of the sound from this arrangement would not be great if it depended merely on the hydrostatic pressure of the wave, since this, under favorable circumstances, would not be more than that of a column of water 20 feet high, giving a pressure, of about 10 pounds to the square inch. The effect however of the percussion might add considerably to this, though the latter would be confined in effect to a single instant. In regard to the practical result from this arrangement, which was continued in operation for several years, it was found not to obviate the necessity of producing sounds of greater power. It is however founded on an ingenious idea, and may be susceptible of application in other cases.

Experiments by Professor J. H. Alexander, in 1855.

The Light-House Board was not content with the employ-

ment alone of the fog-signals in ordinary use, but directed a series of experiments in order to improve this branch of its service. For this purpose the board employed Prof. J. H. Alexander, of Baltimore, who made a report on the subject, which was published among the documents. The investigations of Professor Alexander related especially to the use of the locomotive steam-whistle as a fog-signal, and in his report he details the results of a series of experiments in regard to the nature and adjustment of the whistle, the quantity of steam necessary to actuate it, with suggestions as to its general economy and management. He found, what has since been fully shown, that the power of the sound depends upon the pressure of the steam in the boiler, and the pitch upon the distance between the circular orifice through which the steam issues, and the edge of the bell. He appears however to be under an erroneous impression that the sound is produced by the vibrations of the metal of the goblet or bell, while in fact this latter portion of the apparatus is a resounding cavity, which as I have shown in subsequent experiments, may be constructed of wood as well as of brass, in order to produce the same effect. Prof. Alexander also mentions the effect of the wind in diminishing the penetrating power of sound when in an adverse direction, either directly or approximately. He also recommends the adoption of an automatic pump to supply the boilers with water, and also to open and shut the valves at the proper intervals for blowing the whistle. He states that the location of a sound can be determined more precisely in the case of loud, high sounds than in that of feebler or lower ones. I am not prepared to concur with him on this point, in view of experiments of my own. In all cases however loud sounds are more desirable than feebler ones, in order that they may be heard at a greater distance above the noise of the surf and that of the wind as it passes through the spars and rigging of vessels.

The board at this time however was not prepared to adopt these suggestions, and an unsuccessful attempt to use a steam-boiler, rendered abortive by the incapacity of the

keeper to give it proper attendance, discouraged for a time efforts in this line.

Previous to the investigations of Prof. Alexander at the expense of the Light-House Board, Mr. Daboll, of New London had for several years been experimenting on his own account with reference to a fog-signal. His plan consisted in employing a reed trumpet, constructed after the manner of a clarionet, and sounded by means of air condensed in a reservoir, the condensation being produced by horse-power operating through suitable machinery. Although the sound of this was more penetrating than that of bells, still the expense and inconvenience of the maintenance of a horse, together with the cost of machinery, prevented its adoption. Mr. Daboll however after this presented to the board a modification of his invention, in which a hot-air engine of Ericsson's patent was substituted as the motive power, instead of the horse; and the writer of this report, as chairman of the committee on experiments in behalf of the board, examined this invention and reported in favor of its adoption. The other members of the committee made an unfavorable report, on the ground that fog-signals were of little importance, since the mariner should know his place by the character of his soundings in all places where accurate surveys had been made, or should not venture near the coast until the fog was dissipated. The board however established Daboll trumpets at different stations which have been in constant use up to the present time.

PART I.—INVESTIGATIONS FROM 1865 TO 1872.

(Report of the United States Light-House Board for 1874, pp. 87-107.)

Experiments near New Haven, in 1865.

The subject of sound, in connection with fog-signals, still continued to occupy the attention of the board, and a series of investigations was made in October, 1865, at the light-house near New Haven, under the direction of the writer of this report, in connection with Commodore, now Admiral L.

M. Powell, inspector, and Mr. Lederle, acting engineer of the third district.

The principal object was to compare the sound of bells, of steam-whistles, and other instruments, and the effect of reflectors, and also the operation of different hot-air engines. For this purpose the committee was furnished with two small sailing vessels. As these were very imperfectly applicable, since they could not be moved without wind, the writer of the report devised an instrument denominated an "artificial ear," by which the relative penetrating power of different sounding bodies could be determined and expressed in numbers by the removal of the observer to a comparatively short distance from the point of origin of the sound. This instrument consisted of a conical horn made of ordinary tinned sheet iron, the axis of which was about 4 feet in length, the diameter of the larger end 9 inches, and tapering gradually to $1\frac{1}{4}$ of an inch at the smaller end. The axis of this horn was bent at the smaller end in a gentle curve, until the plane of the section of the smaller end was at right angles to the transverse section of the larger end, so that when the axis of the trumpet was held horizontally and the larger section vertically, then the section of the smaller end would be horizontal. Across the smaller end a thin membrane of gold-beater's skin was slightly stretched and secured by a thread. On this membrane fine sand was strewn. To protect the latter from disturbance by the wind it was surrounded by a cylinder of glass cut from a lamp-chimney, the upper end of which was covered with a plate of glass; and in the improved condition of the instrument, with a magnifying lens with which to observe more minutely the motions of the sand. To use this instrument in comparing the relative penetrating power of sound from different sources, as for example from two bells, the axis being held horizontal, the mouth was turned toward one of the bells, and the effect causing agitation of the sand was noted. The instrument was then removed to a station a little farther from the bell, and the effect again noted, the distance being increased step by step until no motion in the sand could be observed

through the lens. This distance being measured in feet or yards gave the number indicating the penetrating power of the instrument under trial. The same experiment was immediately repeated under the same conditions of temperature, air, wind, &c., with the other sounding apparatus, and the relative number of yards indicating the distance taken as the penetrating powers of the two instruments. It should be observed in the use of this instrument, that it is intended merely to concentrate the rays of sound and not to act as a resounding cavity; since in that case the sound—in unison with the resounding note, would produce an effect at a greater distance than one in discord.

The indications of this instrument were compared with the results obtained by the ear in the use of the two vessels, and in all cases were in exact accordance; and it was accordingly used in the following investigations, and has been found of great service in all subsequent experiments on the penetration of sound.

The only precaution in using it is that the membrane shall not be of such tension as to vibrate in unison with a single sound or its octaves; or in other words that the instrument must be so adjusted by varying the length of the axis or the tension of the membrane that it shall be in discordance with the sounds to be measured, and only act as a condenser of the sonorous waves.

The first experiments made were with regard to the influence of reflectors. For this purpose a concave wooden reflector had been prepared, consisting of the segment of a sphere of 16 feet radius and covered with plaster, exposing a surface of 64 square feet. In the focus of this, by means of a temporary railway, a bell or whistle could be readily introduced or withdrawn. The centre of the mouth of the bell was placed in the horizontal axis of the reflector. This arrangement being completed, the sound of the bell with and without the reflector behind it was alternately observed. Within the distance of about 500 yards the effect was evidently increased, as indicated by the motion of the sand on the membrane, but beyond this, the difference was less and

less perceptible, and at the limit of audibility, the addition of the reflector appeared to us entirely imperceptible. This result was corroborated by subsequent experiments in which a whistle was heard nearly as well in the rear of a reflector as before it. It would appear from these results that while feeble sounds at small distances are reflected as rays of light are, waves of powerful sound spread laterally, and even when projected from the mouth of a trumpet tend at a great distance to embrace the whole circle of the horizon.

Upon this and all the subsequent experiments, as it will appear, the principle of reflection as a means of re-inforcing sound is but slightly applicable to fog-signals. It is evident however that the effect will be somewhat increased by augmenting the size of the reflector and by more completely inclosing the source of sound in a conical or pyramidal reflector.

Another series of experiments was made to ascertain whether the penetration of the sound was greater in the direction of the axis of the bells, or at right angles to the axis; or in other words, whether the sound was louder in front of the mouth of a bell or of its rim. The result of this experiment was considered of importance, since in one of the light-houses a bell has been placed with the plane of its mouth at right angles to the horizon, instead of being placed as usual parallel to the same. The effect on the sound in these two positions was similar to that produced by the bell with a reflector, the noise at a short distance being greater with the mouth toward the observer than when the rim was in the plane of the ear. At a considerable distance however, the difference between the two sounds was imperceptible. In practice therefore it is of very little importance whether the axis of the bell is perpendicular or parallel to the horizon.

The first fog-signal examined in this series of experiments was a double whistle, improperly called a steam-gong, designed principally for a fire-alarm and for signals for the commencement of working-hours in large manufacturing establishments. It consisted of two bells of the ordinary steam-whistle on the same hollow axis, mouth to mouth,

with a flat, hollow cylinder between them, through the upper and lower surfaces of which the circular sheets of steam issue, the vibration of which produces the sound. In the instrument under examination, the upper bell was 20 inches in length of axis, and 12 inches in diameter, and the lower whistle was of the same diameter, with a length of axis of 14 inches. The note of the shorter bell was a fifth above that of the longer. This arrangement gave a melodious sound, unlike that of the ordinary locomotive-whistle, and on that account had a peculiar merit. The sound was also very loud, and according to testimony—had been heard under favorable circumstances more than twenty miles. It required a large quantity of steam however, to give it its full effect, and the only means to obtain an approximate idea as to this quantity was that afforded by observing its action on a boiler of a woolen manufactory near Newport. It was here blown with a pressure of at least 75 pounds. From theoretical considerations however, it might be inferred that its maximum penetrating power would not be greater than that of a single whistle using the same amount of steam, and this theoretical inference was borne out by the subsequent experiments of General James C. Duane. But from the strikingly distinctive character of its tone it has in our opinion an advantage over a single whistle expending an equal quantity of steam.

The fact that the vibration of the metal of the bell had no practical effect on the penetrating power of the sound was proved quite conclusively by winding tightly around each bell, over its whole length, a thick cord, which would effectually stop all vibration. The penetration of the sound produced under this condition was the same as that with the bells free. It is true, the latter produces a difference in the quality of the tone, such as that which is observed in a brass instrument and that of one of wood or ivory. The inventor was not aware that the sound produced was from the resonance of the air within the bell, and not from the metal of the bell itself, and had obtained a patent, not only for the invention of the double whistle, but also for the special compound of metal of which it was composed.

Another apparatus proposed to be used as a fog-signal was presented for examination by the Marine Signal Company, of Wallingford, Conn. It consisted of a curved tube of copper nearly in the form of the letter C, and was supported on an axis passing through the centre of the figure. An ordinary bell-whistle was attached to each extremity of the tube, the instrument being placed in a vertical position and partly filled with water, then made to oscillate on its centre of support. By this means the air was drawn in at one end and forced out through the whistle at the other. The motion being reversed the air was drawn in at the end through which it had just made its exit and forced out through the whistle at the other. By rocking the instrument, either by hand or by the motion of the vessel, a continued sound could be produced. The motive power in the former case was muscular energy, and the experiments which were made at this time, as well as all that have been made subsequently, conclusively prove that the penetrating power of the sound for practical use as a fog-signal depends upon the intensity of the motive energy employed. No instrument operated through levers and pumps by hand-power is sufficient for the purpose.

. One of these instruments with two 4-inch whistles gave a sound, (as indicated by the artificial ear,) the power of which was about one-tenth of that of a steam-trumpet. It was supposed however that this instrument would be applicable for light-ships; and that if extended entirely across the vessel and armed with whistles of large size, it would be operated by the rolling of the vessel, and thus serve to give warning in time of thick weather. But as it frequently happens that fog exists during a calm, this invention could not be relied upon to give warning in all cases of danger. Besides this, the ordinary roll of a ship is not sufficient to produce a hydrostatic pressure of more than five or six pounds to the square inch, which is insufficient to give an effective sound. It has however been proposed to increase the power by using quicksilver instead of water; but besides the first cost of this material, and the constant loss by leakage and oxidation, the tendency to affect the health of the crew is an objection

to the introduction of this modification of the apparatus into light-ships.

The other instruments which were subjected to trial were an ordinary steam-whistle and a Daboll trumpet. The bell of the whistle was 6 inches in diameter, 9 inches in height, and received the sheet of steam through an opening of one-thirtieth of an inch in width; was worked by a pressure of condensed air of from 20 to 35 pounds per square inch, and blown once in a minute for about five seconds. The air was condensed by a Roper engine of one-horse power. The penetrating power of the sound was increased by an increase in the pressure of the air, and also the pitch. The tone however of the instrument was lowered by increasing the distance between the orifice through which the circular sheet of air issued at the lower rim of the bell or resounding cavity. To prove conclusively that the bell performs the part of a mere resounding cavity, a wooden one—on a subsequent occasion, was substituted for that of metal without a change in the loudness or the pitch of the sound.

The penetrating power of the whistle was compared with a Daboll trumpet, actuated by an Ericsson engine of about the same power; the reservoir for the condensed air of each machine was furnished with a pressure-gauge, and by knowing the capacity of the condensing pumps and the number of strokes required to produce the pressure, the relative amount of power was determined. The result was that the penetrating power of the trumpet was nearly double that of the whistle, and that an equal effect was produced at the same distance by about one-fourth of the power expended in the case of the latter. It must be recollected though that the whistle sends sonorous waves of equal intensity in every direction, while the greatest power of the trumpet is in the direction of its axis. This difference however is lessened on account of the spreading of the sound to which we have before alluded.* The whistle was blown, as we have said, with

* It is worthy of note however that in the case of a sound having primarily an axial direction, the subsequent lateral diffusion must result in enfeebling the whole sphere of expanding sound-waves in a more rapid ratio than the square of the distance.

a pressure of from 20 to 35 pounds, while the trumpet was sounded with a pressure of from 12 to 15 pounds. In the case of the whistle, the pressure in the reservoir may be indefinitely increased with an increase of the penetrating power of the sound produced, while in the case of the trumpet a pressure greater than a given amount entirely stops the blast by preventing the recoil of the vibrating tongue; this being made of steel, in the larger instruments $2\frac{1}{2}$ inches wide and 8 inches long, would receive a pressure of steam, at only 10 pounds to the square inch, of 200 pounds, tending to press it into the opening and to prevent its recoil, this circumstance limits the power of a trumpet of given dimensions. It is well fitted however to operate with a hot-air engine, and is the least expensive in fuel of any of the instruments now employed. The whistle is the simpler and easier of management, although they both require arrangement of machinery in order that they may be operated automatically.

It is a matter of much importance to obtain a hot-air engine of sufficient power, and suitable for working fog-signals of all classes. This will be evident when we consider the difficulty in many cases of obtaining fresh water for producing steam, and the expense of the renewal of the boilers in the use of salt-water, as well as that of the loss of power in frequently blowing out the latter, in addition to the danger of the use of steam by unskillful attendants.

The merits of the two engines however under consideration could not be fully tested by the short trial to which they were subjected during these experiments. The principal objection to the Ericsson engine was the size of the fly-wheel and the weight of the several parts of the machine; the Roper engine was much more compact, and appeared to work with more facility, but from the greater heat imparted to the air the packing was liable to burn out and required to be frequently renewed. Although at first the impression of the committee was in favor of the Roper engine, yet in subsequent trials of actual practice it was found too difficult to be kept in order to be employed for light-house purposes,

and its use has consequently been abandoned ; another hot-air engine has been employed by the board, the invention of a Mr. Wilcox, which has also been discontinued for a similar reason. I was assured by the person last named, a very ingenious mechanician, that when the several patents for hot-air engines expired, a much more efficient instrument could be devised by combining the best features of each of those now in use.

For determining the relative penetrating power of these instruments, the use of two vessels had been obtained, with the idea of observing the sound simultaneously in opposite directions.

Unfortunately however, the location which had been chosen for these experiments was of a very unfavorable character in regard to the employment of sailing-vessels and the use of the artificial ear. It was fully open to the ocean only in a southerly direction, navigation up the bay to the north being limited to three and a half miles, while on shore a sufficient unobstructed space could not be obtained for the proper use of the artificial ear. With these obstructions and the necessity of beating against the wind, thereby constantly altering the direction of the vessel, exact comparisons were not possible, yet the observations made were sufficiently definite to warrant certain conclusions from them as to the relative power of the various instruments submitted to examination.

The following is a synopsis of the observations on four different days. Before giving these, it is necessary to observe that at each stroke of the piston of the hot-air engine a loud sound was produced by the blowing off of the hot air from the cylinder, after it has done its work. In the following statement of results the noise thus produced is called the exhaust. On the first day, but one set of observations was made, the vessel's course being nearly in the line of the axis of the trumpet. The order of penetrating power was as follows: 1st, trumpet ; 2nd, exhaust ; 3d, bell ; these instruments being heard respectively at $5\frac{1}{2}$, $3\frac{1}{2}$, and 2 miles. The whistle was not sounded.

On the second day, simultaneous observations were made

from two vessels sailing in nearly opposite directions. The results of the observations made on the vessel sailing in a southerly direction were very irregular. The trumpet was heard at $3\frac{5}{8}$ miles and lost at $4\frac{3}{8}$ miles with the wind slightly in favor of the sound, and heard at $6\frac{1}{4}$ miles with the wind somewhat against the sound; it was heard even at $7\frac{5}{8}$ miles from the masthead, though inaudible from the deck. In all these cases the position of the vessel was nearly in line with the axis of the trumpet.

The whistle and exhaust were heard at $7\frac{1}{4}$ miles with a feeble opposing wind, and lost at $6\frac{1}{4}$ miles when the force of the wind became greater.

The order of penetration in this series of observations was: 1st, trumpet and gong; 2nd, whistle; 3d, exhaust.

In the case of a vessel sailing northward, its course being almost directly against the wind and in the rear of the trumpet, all the sounds were lost at less distances than in the case of the other vessel. The observations showed very clearly the effect of the wind, the bell at a certain distance being heard indistinctly with a strong opposing wind and more and more plainly as the wind died away. The trumpet was heard only as far as the whistle, the vessel being in the rear of it.

On the third day, observations were made from the two vessels, both however sailing to the south. From the vessel sailing at right angles to the direction of the wind the order of penetration was: 1st, trumpet; 2nd, whistle; 3d, exhaust; 4th, bell.

In the case of the other vessel the opposing effect of the wind was greater, and the sounds were heard to a less distance; the order was: 1st, trumpet; 2nd, whistle; 3d, exhaust; 4th, bell; 5th, rocker.

On the fourth day, two trips were made by the same vessel in the course of the day, one being northward and the other southward. In the first case the trumpet was lost at $3\frac{1}{8}$ miles, the vessel being nearly in its rear; in the second case, the wind being almost directly opposed to the sound, the large bell was heard at $1\frac{1}{8}$ miles, and lost at $\frac{7}{8}$ of a mile,

which was probably due to increase of the force of the wind ; the trumpet was lost at $3\frac{1}{2}$ miles.

In all these observations, owing to the unfavorable conditions of the locality, and the direction of the wind, we were unable to obtain any satisfactory observations on sound moving with the wind. In all cases the results were obtained from sounds moving nearly against the wind, or at right angles to it. From the results of the whole it appears that the sound was heard farther with a light opposing wind than with a stronger one, and that it was heard farthest of all at right angles to the wind. From this latter fact however it should not be inferred that in this case sound could be heard farther at right angles to the wind than with the wind, but that in this direction the effect of the wind was neutralized. The results also exhibited, in a striking manner, the divergency of sound from the axis of the trumpet, the trumpet being heard in the line of its axis in front at 6 miles and behind at 3 miles, the wind being nearly the same in both cases.

All the observations were repeated on land with the artificial ear as far as the unfavorable condition of the surface would permit. Although the limit, as to distance, at which the sand might be moved was not in most cases observed, yet the relative degree of agitation at a given distance established clearly which was the most powerful instrument, the result giving precisely the same order of penetration of the different instruments as determined by direct audition.

During this series of investigations an interesting fact was discovered, namely, a sound moving against the wind, inaudible to the ear on the deck of the schooner, was heard by ascending to the mast-head. This remarkable fact at first suggested the idea that sound was more readily conveyed by the upper current of air than the lower, and this appeared to be in accordance with the following statement of Captain Keeney, who is commander of one of the light-house vessels, and has been for a long time engaged on the banks of Newfoundland in the occupation of fishing: "When the fishermen in the morning hear the sound of the surf to the leeward,

or from a point toward which the wind is blowing, they take this as an infallible indication that in the course of from one to five hours the wind will change to the opposite direction from which it is blowing at the time." The same statement was made to me by the intelligent keeper of the fog-signal at Block Island. In these cases it would appear that the wind had already changed direction above, and was thus transmitting the sound more in its own direction than that of the wind at the surface of the earth.

Another remarkable fact bearing on this same point is established by the observations of General James C. Duane. At Cape Elizabeth, 9 miles south-easterly from the general's house, at Portland, is a fog-signal consisting of a whistle 10 inches in diameter; at Portland Head, about 4 miles from the same city, in nearly the same direction, is a Daboll trumpet. There can be no doubt, says the general, that those signals can be heard much better during a heavy north-east snow-storm than at any other time. "As the wind increases in force, the sound of the nearer instrument, the trumpet, diminishes, but the whistle becomes more distinct; but I have never known the wind to blow hard enough to prevent the sound of the latter from reaching this city." In this case the sound comes to the city in nearly direct opposition to the course of the wind, and the explanation which suggested itself to me was that during the continuance of the storm, while the wind was blowing from the northeast at the surface, there was a current of equal or greater intensity blowing in an opposite direction above, by which the sound was carried in direct opposition to the direction of the surface current. The existence of such an upper current is in accordance with the hypothesis of the character of a north-east storm, which sometimes rages for several days at a given point on the coast without being felt more than a few miles in the interior, the air continuously flowing in below and going out above. Indeed, in such cases a break in the lower clouds reveals the fact of the existence above of a rapid current in the opposite direction.

The full significance however of this idea did not reveal

itself to me until in searching the bibliography of sound I found an account of the hypothesis of Professor George G. Stokes, in the proceedings of the British Association for 1856,* in which the effect of an upper current in deflecting the wave of sound so as to throw it down upon the ear of the auditor, or directing it upward far above his head, is fully explained. This subject will be referred to in the subsequent parts of the report, in the attempt to explain various abnormal phenomena of sound that have been observed during the series of investigations connected with the Light-House Board.

During these investigations an attempt was made to ascertain the velocity of the wind in an upper stratum as compared with that in the lower. The only important result however was the fact that the velocity of the shadow of a cloud passing over the ground was much greater than that of the air at the surface, the velocity of the latter being determined approximately by running a given distance with such speed that a small flag was at rest along the side of its pole. While this velocity was not perhaps greater than six miles per hour, that of the shadow of the cloud was apparently equal to that of a horse at full speed.

During this and subsequent investigations, inquiries were made in regard to the effect of fog upon sound, it being a subject of considerable importance to ascertain whether waves of sound like the rays of light are absorbed or stifled by fog. On this point however observers disagree. From the very striking analogy which exists in many respects between sound and light, the opinion largely prevails that sound is impeded by fog, although observers who have not been influenced by this analogy have in many instances adopted the opposite opinion that sound is better heard during a fog than in clear weather. For instance, the Rev. Peter Ferguson, of Massachusetts, informs me that from his own observations sound is conveyed farther in a fog than in a clear air. He founds this opinion on observations which he has made on the sound of locomotives of

* Report of British Association, 1856; Abstracts, p. 22.

several railways in passing over bridges at a distance. Unfortunately, the question is a difficult one to settle, since in order to arrive at a true result, the effect of the wind must be carefully eliminated. Captain Keeney, who has previously been mentioned, related the following occurrence, in the first part of which he was led to suppose that fog had a very marked influence in deadening sound, though in a subsequent part he came to an opposite conclusion: He was sailing during a dense fog with a slight wind bearing him toward a light-vessel, the locality of which he expected to find by means of the fog-signal. He kept on his course until he thought himself very near the ship, without hearing the stroke of the bell. He then anchored for the night, and found himself next morning within a short distance of the light-vessel, but still heard no sound, although he was assured when he got to it that the bell had been ringing all night. He then passed on in the same direction in which he had previously sailed, leaving the light-vessel behind, and constantly heard the bell for a distance of several miles, the density of the fog not perceptibly diminishing. In this case it is evident that the deadening of the sound was not due to the fog, but (as we shall hereafter see,) in all probability to the combined action of the upper and the lower currents of air.

On returning to Washington the writer took advantage of the occurrence of a fog to make an experiment as to the penetration of the sound of a small bell rung by clock-work, the apparatus being the part of a moderator-lamp intended to give warning to the keepers when the supply of oil ceased. The result of the experiment was contrary to the supposition of absorption of the sound by the fog, but the change in the condition of the atmosphere as to temperature and the motion of the air before the experiment could be repeated in clear weather rendered the result not entirely satisfactory.

Experiments at Sandy Hook in 1867.

The next series of experiments was made from October 10 to October 18, 1867, under the direction of the writer of this

report, in connection with General O. M. Poe, engineer-secretary of the Light-House Board, Commodore (now Admiral) Augustus L. Case, then inspector of the third light-house district, and Mr. Lederle, acting engineer of the same district.

The principal object of these investigations was to compare different instruments and to ascertain the improvements which had been made in them since the date of the last investigations, especially the examination of a new fog-signal called the siren, and the comparison of it with the Daboll trumpet, although other investigations were made relative to the general subject of sound in relation to fog-signals. The locality chosen was Sandy Hook, a narrow peninsula projecting northward about five miles into the middle of the Lower Bay of New York, (and almost at right angles to its coast,) having a width of about half a mile. Near the northern point on the east shore a temporary building was erected for the shelter of the engines and other instruments.

The comparisons in regard to penetrating power were made by the use of the artificial ear heretofore described, by carrying this off a measured distance until the sand ceased to move. This operation was much facilitated by previous surveys by members of the engineer corps, who had staked off a straight line parallel with the shore, and accurately divided it into equal distances of 100 feet.

On account of the character of the deep and loose sand, walking along this distance was exceedingly difficult, and to obviate this, a carriage with broad wheels drawn by two horses was employed. An awning over this vehicle protected the observer from the sun, and enabled him without fatigue and at his ease to note the agitations of sand on the drum of the artificial ear, the mouth of which was directed from the rear of the carriage toward the sounding instrument.

For these and other facilities we were indebted to General Andrew A. Humphreys, chief of the Engineer Bureau, who gave orders to the officer in charge of the military works at Sandy Hook to afford us every aid in his power in carrying on the investigation.

The instruments employed were—

1st. A first-class Daboll trumpet (the patent for which, since the death of Mr. Daboll, is owned by Mr. James A. Robinson,) operated by an Ericsson hot-air engine. It carried a steel reed 10 inches long, $2\frac{3}{4}$ inches wide, and $\frac{1}{2}$ inch in thickness at the vibrating end, but increasing gradually to an inch at the larger extremity. This was attached to a large vertical trumpet curved at the upper end into a horizontal direction and furnished with an automatic arrangement for producing an oscillation of the instrument of about 60° in the arc of the horizon. Its entire length, including the curvature, was 17 feet. It was $3\frac{1}{2}$ inches at the smaller end and had a flaring mouth 38 inches in diameter. The engine had a cylinder 32 inches in diameter, with an air-chamber of $4\frac{1}{2}$ feet in diameter and 6 feet long, and was able to furnish continually a five-second blast every minute at a pressure of from 15 to 30 pounds.

2d. A siren, originally invented by Cagniard de Latour, and well known to the physicist as a means of comparing sounds, and measuring the number of vibrations in different musical notes. Under the direction of the Light-House Board, Mr. Brown, of New York, had made a series of experiments on this instrument in reference to its adoption as a fog-signal, and these experiments have been eminently successful. The instrument as it now exists differs in two essential particulars from the original invention of Latour: 1st, it is connected with a trumpet in which it supplies the place of the reed in producing the agitation of the air necessary to the generation of the sound; and 2d, the revolving disk, which opens and shuts the orifices producing the blasts, is driven not by the blast itself impinging on oblique openings, as in the original instrument, but by a small engine connected with the feed-pump of the boiler.

The general character of the instrument may be understood from the following description: Suppose a drum of short axis, into one head of which is inserted a steam-pipe connected with a locomotive boiler, while the other end has in it a triangular orifice through which the steam is at

brief intervals allowed to escape. Immediately before this head, and in close contact with it, is a revolving disk in which are eight orifices. By this arrangement, at every complete revolution of the disk, the orifice in the head of the drum is opened and shut eight times in succession, thus producing a rapid series of impulses of steam against the air into the smaller orifice of the trumpet placed immediately in front of the revolving disk. These impulses are of such energy and rapidity as to produce a sound unrivalled in intensity and penetrating power by that of any other instrument yet devised.

The siren was operated by an upright cylindrical tubular boiler, with a pressure of from 50 to 100 pounds on the square inch. This form of boiler was subsequently replaced by an ordinary horizontal locomotive-boiler with a small engine attached for feeding it, and for rotating the disk, the latter being effected by means of a band passing over pulleys of suitable relative dimensions.

3d. A steam-whistle 8 inches in diameter. Through some mis-understanding a series of whistles of different diameters was not furnished as was intended.

The first experiments to be noted were those in regard to a comparison of the penetrating power of the siren and the whistle; the fitting up of the Daboll trumpet not having been completed. The principal object of this however, was to test again the truthfulness of the indications of the artificial ear in comparison with those of the natural ear.

An experiment was made both by means of the artificial ear on land, and by actually going off on the ocean in a steamer until the sounds became inaudible to the natural ear. By the latter method the two sounds ceased to be heard at the distances of six and twelve and a half miles, respectively. The indications of the artificial ear gave a similar result, the distance at which the sand ceased to move in one case being double that of the other. In both cases the conditions of wind and weather were apparently the same. In the case of the steamer the distance was estimated by noting the interval of time between the flash of steam and the perception of the sound.

Comparison of the Daboll trumpet and the siren.—The pressure of the hot air in the reservoir of the hot-air engine of the trumpet was about 20 pounds, and that of the steam in the boiler of the siren about 75 pounds. These pressures are however not considered of importance in these experiments, since the object was not so much to determine the relative amount of motive power employed, as the amount of penetrating energy produced by these two instruments, each being one of the first of its class.

1. At distance 50, the trumpet produced a decided motion of the sand, while the siren gave a similar result at distance 58. The two observations being made within ten minutes of each other, it may be assumed that the condition of the wind was the same in the two cases, and hence the numbers above given may be taken as the relative penetrating powers of the two instruments.

2. Another series of experiments was instituted to determine whether a high or a low note gave the greater penetration. For this purpose the siren was sounded with different velocities of rotation of the perforated disk, the pressure of steam remaining at 90 pounds per square inch. The effect upon the artificial ear in causing greater or less agitation of sand was taken as the indication of the penetrating power of the different tones. The number of revolutions of the disk in a given time was determined by a counting apparatus, consisting of a train of wheels and a series of dials showing tens, hundreds, and thousands of revolutions; this was temporarily attached to the projecting end of the spindle of the revolving disk by pushing the projecting axis of the instrument into a hole in the end of the spindle.

From the whole of this series of experiments it appeared that a revolution which gave 400 impulses in a second was the best with the siren when furnished with a trumpet. On reflection however it was concluded that this result might not be entirely due to the pitch, but in part to the perfect unison of that number of impulses of the siren with the natural tone of the trumpet. To obviate this complication a series of experiments was next day made on the penetra-

tion of different pitches with the siren alone, the trumpet being removed. The result was as follows:

The siren was sounded at five different pitches, the artificial ear being at such a distance as to be near the limit of disturbance by the sound. In this condition the lowest pitch gave no motion of sand. A little higher, slight motion of sand. Still higher, considerable motion of sand; and with a higher pitch again, no motion of sand. The best result obtained was with a revolution which gave 360 impulses in a second.

3. An attempt was made to determine the most effective pitch or tone of the steam whistle. It was started with what appeared to be the fundamental note of the bell, which gave slight motion of sand; a higher tone a better motion; still higher, sand briskly agitated; next, several tones lower, no motion; higher, no motion; still higher, no motion. The variation in the tone was made by altering the distance between the bell and the orifice through which the steam was ejected.

The result of this experiment indicated nothing of a definite character other than that with a given pressure there is a maximum effect produced when the vibrations of the sheet of air issuing from the circular orifice are in unison with the natural vibrations from the cavity of the bell, a condition which can be determined in any case only by actual experiment. In practice, Mr. Brown was enabled to produce the best effect by regulating the velocity until the trumpet gave the greatest penetrating power, as indicated by an artificial ear of little sensibility; which latter was adopted in order that it might be employed for determining the relative power while the observer was but a few yards from the machine. These experiments have been made in an apartment of less than 80 feet in length, in which the sounding apparatus was placed at one end, and the artificial ear at the other, substituting fine shot in the place of sand.

The experiments with the siren however, indicate the fact that neither the highest nor the lowest pitch of an instrument gives the greatest penetrating power, but one of a medium character.

Another element of importance in the construction of these instruments is the volume of sound. To illustrate this it may be mentioned that a harpsichord-wire stretched between two strings of India rubber when made to vibrate by means of a fiddle-bow, gives scarcely any appreciable sound. We attribute this to the want of quantity in the aerial wave; for if the same wire be stretched over a sounding-board having a wide area the effect will be a comparatively loud sound but of less duration with a given impulse. It was therefore suggested that the width of the reed in the Daboll trumpet, the form and size of the hole in the disk of the siren, and the circumference of the vibrating sheet of air issuing from the circular orifice of the whistle, would affect the power of the sound. The only means of testing this suggestion is by using reeds of different widths, sirens with disks of different-shaped openings, and whistles of different diameters. In conformity with this view Mr. Brown has made a series of empirical experiments with openings of different forms, which have greatly improved the operation of the siren, while Mr. Wilcox has experimented on several forms of reeds, of which the following is the result:

The best reed obtained was $2\frac{1}{4}$ inches wide, 8 inches long in the vibrating part, $\frac{5}{8}$ inch thick at the butt, and $\frac{1}{4}$ inch thick at the free end. This sounded at a pressure of from 20 to 30 pounds. The thinner reeds gave a sound at a less pressure, from 5 to 10 pounds, the thicker at from 20 to 30 pounds. A reed $8\frac{1}{2}$ inches long in the vibrating part, 1 inch thick at the butt, $\frac{3}{4}$ inch thick at the end, and 3 inches wide, did not begin to sound until a pressure of 80 pounds was reached, when it gave a sound of a dull character. Another reed of the same width, $\frac{5}{8}$ inch thick at the butt, and $\frac{7}{16}$ inch at the end, and same length, gave a sound at 75 pounds pressure, but still dull and of little penetrating power. These reeds were evidently too heavy in proportion to their elasticity. These were made without the addition of a trumpet, and therefore to produce the best result when used with a trumpet the latter must be increased or diminished in length until its natural vibrations are in harmony with those of the former, as will be

seen hereafter. General James C. Duane has also made experiments on whistles of different diameters, of which the result will be given.

Another consideration in regard to the same matter is that of the amplitude of the oscillations of the tongue or steel reed in its excursion in producing the sound; the time of oscillation (that is the pitch) remaining the same, the amplitude will depend upon the elasticity of the reed, the power to surmount which will again depend upon the pressure of steam in the boiler, and hence we might infer that an increase of pressure in the boiler with an increase of the elasticity of the reed, everything else being the same, would produce an increase in penetrating power. From the general analogy of mechanical effects produced by motive power we may denote the effect upon the ear by the expression mv^2 , in which m expresses the mass or quantity of air in motion, and v the velocity of the particles in vibration.

If this be the expression for the effect upon the ear, it is evident that in case of a very high note the amplitude of the vibration must be so small that the effect would approximate that of a continued pressure rather than that of distinct alternations of pressure, giving a vibrating motion to the drum of the ear.

4. Experiments were next made to determine the penetrating power in the case of the siren under different pressures of steam in the boiler. The experiments commenced with a pressure of 100 pounds. The pressure at each blast was noted by two observers, and to compare these pressures with the indications of the sand, the time of the blasts was also noted.

The following are the results :

Pressure.	Relative distances at which the sand ceased to move.
100.....	61
90.....	59
80.....	58
70.....	57
60.....	57
50.....	56
40.....	55
30.....	53
20.....	51

From this series of experiments it appears that a diminution of pressure is attended with a comparatively small diminution in the penetrating power of the siren.

In regard to this unexpected result,—of great practical importance, the following appears to be the explanation. It is a well-known principle in aerial mechanics that the velocity of the efflux of air from an orifice in a reservoir does not increase with an increase of condensation, when the spouting is into a vacuum. This is evident when we reflect that the weight or density of the air moving out is increased in proportion to the elasticity or pressure; that is, the increase in the propelling force is proportional to the increase in the weight to be moved, hence the velocity must remain the same.

In the foregoing experiments with high pressures—large in proportion to the resistance of the air, the velocity of efflux should therefore be but little increased with the increase of pressure, and inasmuch as the velocity is the most important factor in the expression mv^2 , which indicates the effect on the tympanum, the penetrating power of the sound should be in accordance with the above experimental results.

A similar result cannot be expected with the use of the whistle, or the trumpet, since in the former, the stiffness of the aerial reed depends upon its density, which will be in proportion to the pressure in the boiler; and in the case of the latter—on the one hand, no sound can be produced unless the pressure be sufficient to overcome the resistance of the reed, and on the other, the sound must cease when the pressure is so great as to prevent the recoil of the reed.

5. An experiment was made to determine the effect of a small whistle inserted into the side of a trumpet near the small end. The whistle being sounded before and after it was placed in the trumpet, the result was as follows: The penetrating powers were in the ratio of 40:51, while the tone was considerably modified. From this experiment it appears that a whistle may be used to actuate a trumpet or to exercise the functions of a reed. In order however to get the best results, it would be necessary that the trumpet

and whistle should be in unison, but it may be doubted whether an increase of effect, with a given amount of power, would result from using such an arrangement; it might nevertheless be of advantage in certain cases to direct the sound of a locomotive whistle in a definite direction, and to use a smaller whistle, especially in cities in which the locomotive passes through long streets; perhaps in this case the sound might be less disagreeable than that of the naked whistle, which sends its sound-waves laterally with as much force as in the direction of the motion of the engine.

6. General Poe called attention to the sound produced by the paddle-wheels of a steamer in the offing—at a distance estimated at four and a half miles. The sound was quite distinct when the ears were brought near the surface of the beach.

In this connection he stated that he had heard the approach of a small steamer on the northern lakes when its hull was still below the horizon, and was even enabled to designate the particular vessel from among others by the peculiarity of the sound.

The sound in the case of the steamer is made at the surface of the water, and it might be worth the trouble to try experiments as to the transmission of sound under this condition, and the collection of it by means of ear-trumpets, the mouths of which are near the water, the sound being conveyed through tubes to the ears of the pilot. In order however to determine in this case the direction of the source of sound, two trumpets would be necessary, one connected with each ear, since we judge of the direction of a sound by its simultaneous effects on the two auditory nerves. This suggestion, as well as many others which have occurred in the course of these researches, is worthy of special investigation.

7. A series of experiments was made to compare trumpets of different materials and forms, having the same length and transverse areas, all blown at a pressure of $9\frac{1}{2}$ pounds.

The following table gives the results:

No.	Material of trumpet.	Cross-section.	Relative distances at which the sand ceased to move.
1	Wood.	Square.	13
2	Brass.	Circular.	23
3	Cast iron.	Circular.	24
4	Wood.	Circular.	30

From these experiments it would appear that the material or elasticity of the trumpet had little or no effect on the penetrating power of the sound, although the shape appeared to have some effect, the pyramidal trumpet, or one with square cross-section (No. 1) giving a less result than the conical ones of the same sectional area. A comparison was made between a long straight trumpet and one of the same length curved at its upper end, which gave the same penetrating power with the same pressure. It is probable that a thin metallic trumpet would give greater lateral divergency to the sound, and also a slightly different tone.

8. The effect of a hopper-formed reflector was next tried with the whistle, the axis of which was about 5 feet in length, the mouth 6 feet square, and the small end about 18 inches. When the whistle was sounded at the small end of this reflector, the distance at which the sand ceased to move was 51; the sound of the same whistle without the reflector ceased to move the sand at 40. The ratio of these distances would have been less with a more sensitive instrument at a greater distance on account of the divergency of the rays.

9. In order to determine the diminution of sound by departing from the axis of the trumpet, a series of experiments was made with a rotating trumpet, the axis of which was at first directed along the graduated line of observation, and subsequently deflected from that line a given number of degrees. The following were the results:

Direction of the trumpet.	Relative distances at which the sand ceased to move.
Along the line -----	26
Deflected 30° -----	23
Deflected 60° -----	21
Deflected 90° -----	18
Deflected 120° -----	13

These results illustrate very strikingly the tendency of sound to spread on either side of the axis of the trumpet; had the experiments been made with a more sensitive instrument and at a greater distance the effect would have shown a much greater divergency of the sound. It should be observed however that the mouth of the trumpet in this case was 36 inches, which is unusually large.

From the experiments made near New Haven, and also from those at this station, it appears that the actual amount of power to produce sound of a given penetration is absolutely less with a reed trumpet than with a locomotive whistle. This fact probably finds its explanation in the circumstance that in each of these instruments the loudness of the sound is due to the vibration of the air in the interior of the trumpet and in the bell of the whistle, each of these being a resounding cavity; and furthermore that in these cavities the air is put in a state of sustained vibration by the undulations of a tongue, in the one case of metal, in the other of air; and furthermore it requires much more steam to set the air in motion by the tongue of air than by the solid tongue of steel, the former requiring a considerable portion of the motive power to give to the current of which it consists, the proper degree of stiffness (if I may use the word,) to produce the necessary rapidity of oscillation. But whatever may be said in regard to this supposition, it is evident in case reliable hot-air engines cannot be obtained, that the Daboll trumpet may be operated by a steam-engine, although at an increased cost of maintenance, but this increase we think will still not be in proportion to the sound obtained in comparison with the whistle.

Another question which naturally arises, but which has not yet been definitely settled by experiment, is whether both the siren and the whistle would not—equally with the trumpet, give more efficient results when worked by condensed air than by steam.

From hypothetical considerations this would appear to be the case, since the intensity of sound depends upon the density of the medium in which it is produced; and as the steam is considerably lighter than air, and as the cavities of all of these instruments are largely filled with steam, the intensity of sound would on this account seem to be less than if filled with air.

At the conclusion of the experiments at Sandy Hook, the siren was adopted as a fog-signal, in addition to the reed-trumpet and the locomotive-whistle, to be applied to the more important stations; while large bells were retained for points at which fog-signals were required to be heard at but comparatively short distances. These instruments of the first class being adopted, it became of importance to determine—in actual practice, the cost of maintenance, the best method of working them, and any other facts which might have a bearing on their use.

But as investigations of this kind would require much time and peculiar advantages as to location and mechanical appliances, this matter was referred to General J. C. Duane, the engineer in charge of the 1st and 2d light-house districts, who had peculiar facilities near his residence, at Portland, Me.,—in the way of workshops and other conveniences, and who from his established reputation for ingenuity and practical skill in mechanism, was well qualified for the work. The assignment of this duty to General Duane by the Light-House Board was made during my absence in Europe, in 1870, and as my vacation in 1871 was devoted to light-house duty in California, I had no opportunity of conferring with him on the subject until after his experiments were completed. His results are therefore entirely independent of those obtained under my direction, and I give them herewith in his own words, with such comments as they may suggest, and as are necessary to a proper elucidation of the subject.

Experiments at Portland, Me., 1871, by General James C. Duane.

The apparatus employed consisted of the first-class siren, a first-class Daboll trumpet, and steam-whistles of various sizes.

The points to be decided were:

- 1st. The relative power of these machines; *i. e.*, the distances at which they could be heard under various conditions of the atmosphere:
- 2d. The amount of fuel and water consumed by each:
- 3d. The attention and skill required in operating them:
- 4th. Their endurance:
- 5th. Whether they are sufficiently simple in construction to permit of their being managed and kept in running order by the class of men usually appointed light-house keepers.

In conducting these experiments the following method was pursued:

The signals were sounded at alternate minutes, and their sound compared at distances of two, three, and four miles, and from different directions. On every occasion the quantity of fuel and water consumed per hour by each—was carefully noted, and the condition of each machine examined, both before and after the trial, to ascertain whether any of its parts had sustained injury.

Before giving the results of these experiments some facts should be stated, which will explain the difficulty of determining the power of a fog-signal.

There are six steam fog-whistles on the coast of Maine; these have been frequently heard at a distance of twenty miles, and as frequently cannot be heard at the distance of two miles, and this with no perceptible difference in the state of the atmosphere.

The signal is often heard at a great distance in one direction; while in another it will be scarcely audible at the distance of a mile. This is not the effect of wind, as the signal is frequently heard much farther against the wind than with it. For example, the whistle on Cape Elizabeth can always be distinctly heard in Portland,—a distance of nine miles, during a heavy north-east snow-storm, the wind blowing a gale directly from Portland toward the whistle.

In this sentence, General Duane certainly does not intend to convey the idea that a signal is frequently heard “at a much greater distance against the wind than with it,” since this assertion would be at variance with the general experience of mankind; but the word “frequently” applies to the whistle on Cape Elizabeth, which has been already mentioned as a remarkably exceptional case, in which the sound is heard best against the wind during a north-east snow-storm.

The most perplexing difficulty however arises from the fact that the signal often appears to be surrounded by a belt, varying in radius from one to one and a half miles, from which the sound appears to be entirely absent. Thus in moving directly from a station, the sound is audible for the distance of a mile, is then lost for about the same distance, after which it is again distinctly heard for a long time. This action is common to all ear-signals, and has been at times observed at all the stations,—at one of which the signal is situated on a bare rock twenty miles from the main-land, with no surrounding objects to affect the sound.

All attempts to re-enforce the sound by means of reflectors have hitherto been unsuccessful. Upon a large scale, sound on striking a surface does not

appear to be reflected after the manner of light and heat, but to roll along it like a cloud of smoke.

This statement is in a measure in accordance with results which I have previously found in connection with investigations at the light-house near New Haven, in which the conclusion was arrived at, that although rays of feeble sound for a short distance observe the law that the angle of reflection is equal to the angle of incidence—after the manner of light, yet those of powerful sound tend to diverge laterally to such a degree as to render reflectors of comparatively little use.

In view of these circumstances, it will be obvious that it was extremely difficult to determine the extent of the power of the various signals under examination.

It should be remembered that while the sound from the whistle is equally distributed in all directions,* that from the two other signals, both of which are provided with trumpets, is not so distributed.

The difference is apparent near by, but (as we have seen before) on account of the tendency of sound to spread, it is imperceptible at a distance.

In the siren the sound is most distinct in the axis of the trumpet.

In the Daboll trumpet it is usually strongest in a plane perpendicular to this axis.

This is at variance directly with any observation I have myself made.

Relative power.—From the average of a great number of experiments the following result was obtained:

The power of the first-class siren, 12-inch whistle, and first-class Daboll trumpet, may be expressed by the numbers 9, 7, 4.

The extreme limit of sound of the siren was not ascertained. That of the 12-inch whistle is about twenty miles, and of the trumpet twelve.

Consumption of fuel and water.—The siren, when working with a pressure of 72 pounds of steam, consumes about 180 pounds of coal and 126 gallons of water per hour.

The 12-in. whistle, with 55 pounds pressure of steam, consumes 60 pounds of coal and 40 gallons of water per hour.

The Daboll trumpet, with 10 pounds pressure of air in the tank, consumes about 20 pounds of coal per hour.

The relative expenditure of fuel would be: Siren, 9; whistle, 3; trumpet, 1.

*The sound of the whistle is equally distributed horizontally. It is however much stronger in the plane containing the lower edge of the bell than on either side of this plane. Thus if the whistle is standing upright,—in the ordinary position, its sound is more distinct in a horizontal plane passing through the whistle, than above or below it.

The siren.—Of the three machines this is the most complicated. It uses steam at a high pressure, and some of its parts move with very great velocity, the siren spindle making from 1,800 to 2,400 revolutions per minute. The boiler must be driven to its full capacity in order to furnish sufficient steam. A large quantity of steam is at intervals suddenly drawn from the boiler, causing a tendency to foam and to eject a considerable amount of water through the trumpet.

The constant attention of the keeper is required to regulate the fire, the supply of water to the boiler, of oil to the journals, &c.

In general terms, it may be stated that the siren requires more skill and attention in its management than either of the other signals.

The Daboll trumpet.—As the caloric engine, which has been hitherto employed to operate this signal, requires little fuel, no water, and is perfectly safe as regards danger from explosion, it would—at the first glance, appear to be the most suitable power that could be applied to fog-signals, and was accordingly at first exclusively adopted for this purpose. It was however found to be so liable to accident, and so difficult to repair, that of late years it has been almost entirely rejected. In the steam-boiler, the furnace is surrounded by water, and it is impossible under ordinary circumstances to heat the metal much above the temperature of the water. The furnace of the caloric engine is surrounded by air, and is therefore liable to be burned out if the fire is not properly regulated.

The working-piston is packed with leather, and as it moves horizontally, with its whole weight resting on the lower side of the cylinder, the packing at its lower edge is soon worn out.

If the engine is allowed to stop with the piston at the furnace-end of the cylinder the leather is destroyed by the heat. The re-packing of a piston is a difficult and expensive operation, requiring more skill than can be expected among the class of men from which light-house keepers are appointed.

Another accident to which these engines are subject arises from a sudden check in the velocity of the piston, caused either by the jamming of the leather packing or the introduction of dirt into the open end of the cylinder, in which case the momentum of the heavy eccentrically-loaded fly-wheel is almost sure to break the main rocker-shaft.

The expense of repairs is considerably increased by the fact that these engines are not now in general use, and when important repairs are required it is usually necessary to send to the manufacturer.

This signal requires much attention. The fires must be carefully regulated to avoid burning out the furnace, the journals thoroughly oiled, and the cylinders well supplied with tallow.

The steam-whistle.—This machine requiring much less steam than the siren in proportion to the size of its boiler, there is not the same necessity for forcing the fire; the pressure of steam required is less, and the point from which it is drawn much higher above the water-level in the boiler, and there is consequently no tendency to foam.

The machinery is simple; the piston pressure very light, producing but little strain on the different parts of the engine, which is therefore not liable to get out of order and requires no more attention than a common stationary engine.

One marked advantage possessed by this signal is that should the engine become disabled, the whistle may still be sounded by working the valve by hand. This is not the case with the two others, where an accident to any part of the machinery renders the signal for the time useless.

It will thus be seen that the siren is the most expensive of the fog-signals as regards maintenance, and that it is adapted only to such stations as are abundantly supplied with water and situated in the vicinity of machine-shops where the necessary repairs can be promptly made.

On the other hand, as it is the most powerful signal, there are certain stations where it should have the preference; as for example Sandy Hook, which from its importance demands the best signal that can be procured, regardless of cost. Such stations should be provided with duplicate apparatus, well supplied with spare parts, to guard against any possibility of accident.

There should be a keeper whose sole business must be to attend the signal, and who should have sufficient mechanical skill to make the ordinary repairs. He should moreover be a licensed engineer.

There will also be required an assistant, who may be one of the light-keepers, to relieve him during the continuance of foggy weather.

The steam-whistle is the simplest in construction, most easily managed and kept in repair, and requires the least attention of all the fog-signals. It is sufficiently powerful for most localities, while its consumption of fuel and water is moderate.

It has been found on this coast that a sufficient quantity of rain-water can be collected to supply the 12-in. whistle at nearly every station. This has been the case for the last two years at Martinicus.

The Daboll trumpet, operated by a caloric engine, should only be employed in exceptional cases, such as at stations where no water can be procured, and where—from the proximity of other signals, it may be necessary to vary the nature of the sound.

The trumpet however may undoubtedly be very much improved by employing steam power for condensing the air. The amount of work required, which is that of compressing 70 cubic feet of air to an average pressure of 8 pounds per inch, would be less than two-horse power. For this purpose the expenditure of fuel and water would be moderate; indeed, the exhaust steam could be condensed and returned to the cistern, should the supply of water be limited.

The siren also is susceptible of improvement, especially as regards simplification.

In the foregoing remarks I think the General has expressed a somewhat undue partiality for the whistle, and somewhat over-estimated the defects of the other instruments. The trumpets with Ericsson engine have not been abandoned, except partially in the two districts under the direction of General Duane, to which he probably intended to confine his statement. They are still in use in the third district, where they are preferred by General Woodruff, who finds no difficulty in keeping them in repair, having employed a skilled machinist who has made these instruments his special study, and who—visiting them from time to time, makes repairs and supplies new parts.

The intermittent action of fog-signals makes it necessary to employ a peculiar form of boiler. The steam used is at a high pressure, and drawn off at intervals; consequently there is a tendency to foam and throw out water with the steam. To obviate this difficulty the form of boiler found by experience to be best adapted to this service is a horizontal tubular boiler (locomotive), with rather more than one-half of the interior space allowed for steam-room. The steam-dome is very large, and is surmounted by a steam pipe 12 ins. in diameter. Both the dome and pipe were formerly made much

smaller, but were gradually enlarged as long as any difficulty with regard to foaming was noticed. The steam is drawn off at a point 10 ins. above the water-level in the boiler. The main points to be observed are to have plenty of steam-room, and to draw the steam from a point high above the water-level. It will be readily perceived that a vertical tubular boiler is entirely unsuited to this work.

It is essential, both as regards economy of fuel and the efficient working of the signal, that the boiler (including the dome and stand-pipe) should be well covered with some good non-conductor of heat. A material (called salamander felting) manufactured in Troy, N. Y., was used on the fog-whistle boiler at House Island during the winter of 1870. There resulted a saving of more than 20 per cent. of fuel over that consumed in the same boiler when uncovered. Where this material cannot be procured, a thick layer of hair felting, covered with canvas, will be found to answer a good purpose.

Various expedients have been proposed with the view of keeping the water in the boilers hot when the signals are not in operation, that the signal may always be ready to sound at a very short notice, and that the water in the boiler and pipes may be prevented from freezing in extremely cold weather. One of these contrivances is "Sutton's circulating water heater." It consists essentially of a small, vertical, tubular boiler, entirely filled with water, and connected with the boiler or tank which contains the water to be heated, by two pipes on different levels. As soon as the water in the heater is warmed, a circulation commences, the hot water flowing through the upper pipe into the boiler, and the cold through the lower pipe from the boiler to the heater. As the furnace in the heater is very small but little fuel is consumed, and nearly the entire heat produced by the combustion is utilized.

The apparatus has been extensively employed in heating the water in tanks designed for filling the steam fire-engine boilers, when the alarm of fire is first given, and appears admirably adapted to this purpose. If used in connection with a steam boiler, it should be disconnected before steam is raised in the latter, as from its construction it is not calculated to withstand any considerable pressure.

An arrangement (similar in principle) has been used in the first light-house district, consisting of a small cylinder coal-stove, of the ordinary pattern, around the interior of which, and above the grate, is introduced a single coil of $\frac{3}{4}$ in. pipe. This coil is connected with the boiler by two pipes, one entering near the bottom, the other about 2 feet higher. It has been found that in consequence of the rapid circulation of the water through this coil, and the great capacity of water for heat, nearly all the heat from the fire in the stove is transferred to the water in the boiler. This arrangement possesses the advantage of the $\frac{3}{4}$ in. pipe, being strong enough to stand any pressure that can be used in the boiler, thus rendering it unnecessary to disconnect it at any time.

Experience has however proved that none of these contrivances are essential. It is seldom that an attentive keeper cannot foresee the approach of fog or snow in time to have the apparatus in operation as soon as required, even when obliged to start his fire with cold water in the boiler.

Keepers should be directed to watch the state of the weather carefully, and to light their fires at the first indication of fog or snow-storm. As soon as the water in the boiler is near the boiling point, should the necessity for sounding the signal have not yet arisen, the fire may be banked, and in this state the water may be kept hot for any length of time at a moderate expenditure of fuel. With proper care, no more fuel is required to keep the water at the requisite temperature by means of a banked fire than by any other method, and it is a matter of great importance to avoid complicating fog-signal apparatus by unnecessary appendages.

The same plan should be adopted in extremely cold weather to prevent the water in the boiler from freezing. There should be a small air-cock in the draught-pipe near its junction with the feed-pump, and in cold weather

this should be opened when the pump is not in use, in order to allow the pipe to empty itself.

When the draught-pipe cannot be protected from the cold, and the well is at a considerable distance from the engine, the following expedient has been employed with success: The pipe is enclosed in an India-rubber hose of about double its diameter, and from time to time steam is forced through the space between the hose and draught-pipe by means of a small pipe from the boiler.

Although the laws governing the reflection of light and heat are undoubtedly in a great measure applicable to sound, there are yet so many disturbing influences, such as inflection, refraction, (caused by the varying density of the atmosphere,) &c., interfering with the reflection of the latter, that but little use can be made of this property in directing and condensing the waves of sound issuing from a fog-signal. This fact may be illustrated by an account of some experiments made during the last year.

A whistle being sounded in the focus of a large parabolic reflector, it was very perceptible to an observer in the immediate vicinity that the sound was louder in the front than in the rear of the reflector. As the distance of the observer from the whistle was increased, this disparity rapidly diminished, and at the distance of a few hundred yards, entirely disappeared. The *beam* of sound had been dissipated and the *shadow* had vanished. The effect of a horizontal sounding-board 10 feet square, suspended over the whistle to prevent the escape of sound in a vertical direction, was inappreciable at the distance of a quarter of a mile.

The employment of a trumpet with the whistle was rather more successful. The trumpet was constructed of wood, in the form of the frustum of a square pyramid; the larger base being 10 ft. by 10 ft., the smaller base 2 ft. by 2 ft., and the length 20 ft. The axis was horizontal, and the whistle placed at the smaller end. By this arrangement the increased power of the sound could be perceived at the distance of a mile, the action being similar to that of a speaking-trumpet.

It is probable that some modification of this form of whistle may be advantageously employed in certain localities, but there is however a disadvantage attending the use of a trumpet with fog-signals.

The sound from a trumpet not being uniformly distributed, it is difficult to estimate the distance of the signal, or as the pilots term it "to locate the sound." This has been observed in the siren and Daboll trumpet. The sound from these signals being stronger on one course than any other, may be distinctly heard from a vessel when crossing the axis of the beam of sound, but as its distance from this line increases, the sound appears fainter and more remote, although the vessel may be approaching the signal.

From an attentive observation during three years of the fog-signals on this coast, and from the reports received from captains and pilots of coasting vessels, I am convinced that in some conditions of the atmosphere the most powerful signals will be at times unreliable.

Now it frequently occurs that a signal, which under ordinary circumstances would be audible at the distance of fifteen miles, cannot be heard from a vessel at the distance of a single mile. This is probably due to the reflection mentioned by Humboldt.

The temperature of the air over the land where the fog-signal is located, being very different from that over the sea, the sound—in passing from the former to the latter, undergoes reflection at their surface of contact. The correctness of this view is rendered more probable by the fact that when the sound is thus impeded in the direction of the sea it has been observed to be much stronger inland.

When a vessel approaches a signal in a fog, a difficulty is sometimes experienced in determining the position of the signal by the direction from which the sound appears to proceed, the apparent and true direction being

entirely different. This is undoubtedly due to the refraction of sound passing through media of different density.

Experiments and observation lead to the conclusion that these anomalies in the penetration and direction of sound from fog-signals are to be attributed mainly to the want of uniformity in the surrounding atmosphere, and that snow, rain, fog, and the force and direction of the wind, have much less influence than has generally been supposed.

In the foregoing I differ entirely in opinion from General Duane as to the cause of extinction of powerful sounds being due to the unequal density of the atmosphere. The velocity of sound is not at all affected by barometric pressure, but if the difference in pressure is caused by a difference in heat, or by the expansive power of vapor mingled with the air, a slight degree of obstruction of sounds may be observed. But this effect I think is entirely too minute to produce the results noted by General Duane, while we shall find in the action of the currents of wind above and below a true and sufficient cause.

The experimental whistles were of the following dimensions, viz., $2\frac{1}{2}$ inches, 3 ins., 4 ins., 5 ins., 6 ins., 10 ins., 12 ins., and 18 ins. in diameter. Those of $2\frac{1}{2}$ ins., 3 ins., 5 ins., and 10 ins. were fitted (instead of the ordinary bell) with long cylinders provided with movable pistons, so that the effective length of the bell could be altered at pleasure. The pitch of the blast was found to vary with the length of the bell, and the power of the whistle with its diameter. The ratio of the power to the diameter was not accurately obtained, but it is probable that the extreme range of sound of a whistle is proportional to the square root of its diameter.

This result (that the pitch varies with the length of the bell,) is in conformity with well-established principles of resounding cavities; and that the power should increase with the extent of the aerial reed (the vibrations of which give motion to the resounding air within the cavity,) is also as we have seen, in accordance with hypothetical considerations: but as the density of this stream of steam (and consequently the rapidity of its vibrations) depends upon the pressure of the steam in the boiler, a perfect whistle should have the capability of changing its dimensions, not only in relation to the width of its throat, but also in regard to the pressure of the steam in the reservoir.

The pitch giving the greatest range appears to be at the middle of the scale of sound. It is certain that a good result cannot be obtained from either a very shrill or a bass note. This remark is applicable to all varieties of signal.

The 10-in. and 12-in. whistles are recommended for ordinary use. The 18-in. whistle is more powerful, but the increase of power bears too small a proportion to that of the expenditure of fuel to render its employment generally advisable. The best results were obtained by giving the whistle the following proportions: The diameter of the bell equaling two-thirds of its length, and the set of the bell, *i. e.*, the vertical distance of the lower edge above the cup, the one-third to one-fourth of the diameter for a pressure of 50 to 60 pounds of steam.

A bell (whether operated by hand or by machinery,) cannot be considered an efficient fog-signal on the sea-coast. In calm weather it cannot be heard half the time at a greater distance than one mile, while in rough weather the noise of the surf will drown its sound to seaward altogether.

On approaching a station I have frequently seen the bell rung violently by the keeper, without being able to hear the sound until I had landed.

Nevertheless, all important stations should be provided with bells, as there are occasions when they may serve a useful purpose, but it should be well understood by mariners that they must not expect always to hear the bells as a matter of course.

Bells should not be omitted at stations furnished with steam fog-signals, especially when the latter are not in duplicate, and mariners should be warned that the bell will be sounded when the regular signal is disabled.

It has been observed that a bell rung by hand can be heard farther than when sounded by machinery, and many of the steamboat companies on this coast pay the keepers of bells rung by clock-work to ring them by hand when the boats of their line are expected to pass.

We think the difference in the effect of ringing of bells by hand or by machinery is so slight as to be inappreciable except at a short distance. It is true (as I have before observed,) that the sound is louder when the mouth of the bell is directed toward the hearer than when the edge is so directed, but on account of the spreading of this sound, the effect is lost in a small distance, and indeed in one light-house the bell is permanently placed with the axis of its mouth directed horizontally, and in this position, if the bell were struck interiorly with a hammer, which would give it a larger vibration than if struck exteriorly, I doubt whether any difference would be observed between the two methods of ringing; and if any existed it would probably be in favor of the fixed bell rung by machinery.

On rivers, narrow channels, and lakes, where the difficulty from the noise of the surf does not exist, this species of signal may be used to advantage, as its maintenance requires but a small expenditure of either money or labor, and by a proper arrangement of the machinery the intervals between the strokes of the bell may be so regulated as to avoid the danger of confounding the signals, however near together.

Although a bell may be heard better when sounded by hand than by clock-work, yet in thoroughfares where the signal must be kept in constant operation during the entire continuance of a fog, it would be impracticable to make use of the former method, and recourse must be had to machinery.

In arranging the signal the bell and machinery must be placed as low as possible, as the sound is heard much more plainly on the water when the bell is near its surface, and also as the machinery, when thus situated, is steadier and more readily accessible.

Particulars as to the siren.—The boiler of a second-class apparatus is 12 feet long, 42 inches in diameter, and has 300 feet of heating surface. The dome is 2 feet in diameter and 3 feet high.

The cylinder of the engine is 4 inches in diameter and 6 inches stroke. The prolongation of the piston-rod forms the plunger of the feed-pump. The main shaft carries three pulleys; the larger driving the siren-spindle, the second, the worm and screw-gear, and the third, the governor.

In the worm-gear the wheel makes two revolutions per minute, and is provided with a cam, which acting on a lever opens the valve, admitting steam through the siren-disks. The cam has such a length as to hold the valve open for about seven seconds. A counter-weight closes the valve as soon as the lever is released by the cam.

The siren itself consists of a cylindrical steam-chest, closed at one end by a perforated brass plate. The perforations are twelve in number, equi-distant from each other, and arranged on the circumference of a circle, whose center is in the axis of the cylinder. The other end is closed by a cast-iron head. The heads are connected by a brass pipe, through which the spindle passes.

The perforated head is covered on the exterior by a brass disk, attached to the spindle, having twelve rectangular notches corresponding to the apertures on the former, and so arranged that by its revolution these apertures are simultaneously opened and closed. The spindle is driven by a belt from the large pulley on the main shaft. This shaft makes 180 revolutions per minute; the spindle, 1,620; and as there are 12 apertures in the disks, from each there will issue jets of steam at the rate of 19,440 per minute. The sound produced by these impulses may be rendered more or less acute by increasing or diminishing the velocity of revolution.

The valve and valve-seat are disks similar to those already described, having however four openings instead of twelve. The valve revolves on the brass tube inclosing the siren-spindle, and is worked by a bevel gear. The trumpet is of cast-iron.

The Daboll trumpet.—The apparatus used in the foregoing experiments is a second-class trumpet, operated by an Ericsson caloric-engine. The air-pump is single-acting. Its cylinder is 12 ins. in diameter by 12 in. stroke. The engine makes 40 strokes per minute. There is a screw-thread raised on the main shaft, which acting on a wheel drives a bevel gear, giving motion to a cam-wheel. The latter makes one revolution in two minutes, and is furnished with three equidistant cams. These cams—pressing on the valve-lever, throw the valve open once in forty seconds, admitting the compressed air through the reed-chest into the trumpet.

The quantity of air forced into the tank should be in excess of that needed for the trumpet, the surplus being allowed to escape through a delicate safety-valve. This is necessary to provide against a deficiency in case of leakage, and also to allow the pressure of air to be regulated to accommodate the reed. Each reed requiring a different pressure, it is necessary to alter the pressure of the valve-spring whenever a reed is changed.

The first-class trumpet differs only in size from that described.

The caloric-engine for the first class has a 30 in. cylinder. The air-pump is 16½ ins. by 15 in. stroke.

The steam-whistle.—The boiler of this machine is that of the siren. On the forward part of the boiler, the bed-plate of a small engine is secured by two cast-iron brackets. The cylinder of this engine is 4 ins. by 9 ins. The fly-wheel shaft carries an eccentric, which, acting through a rod and pawl on a ratchet-wheel, gives the required motion to the cam-wheel shaft.

The cam-wheel (which makes one revolution per minute,) is provided with

one or more cams, depending on the number of blasts to be given in a minute; the length of the blast being regulated by that of the cams.

The valve for admitting the steam into the whistle is a balance-valve, the diameters of the two disks being respectively $3\frac{1}{4}$ ins. and $2\frac{3}{4}$ ins., which difference is sufficient to cause the pressure of steam to close the valve tight, without requiring too great a force to open it. The valve is worked by a stem attached to the rocker-shaft at the lower part of the steam-pipe. This shaft passes through a stuffing-box in the steam-pipe, and is provided with a collar, which the pressure of the steam forces against the interior boss on the pipe, thus making the joint steam-tight. The exterior arm on this rocker-shaft (as well as that on the engine,) is perforated in such a manner as to allow the throw of the valve to be adjusted.

In the comments I have made on the report of General Duane, the intention was not in the least to disparage the value of his results, which can scarcely be too highly appreciated; but inasmuch as the true explanation of the phenomena he has observed has an important bearing on the location of fog-signals and on their general application as aids to navigation, and is also of great interest to the physicist, who values every addition to theoretical as well as practical knowledge, I have thought not only that the remarks here offered are necessary, but also that special investigations should be made to ascertain more definitely the conditions under which the abnormal phenomena described by the General occur, and to assign if possible a more definite and efficient cause than those to which he has attributed them.

Much thought has therefore been given to the subject, and since the date of General Duane's report I have embraced every opportunity that occurred for making observations in regard to them. The first step we made toward obtaining a clew to the explanation of the phenomena in question resulted from observations made at New Haven, namely: 1st, the tendency of sound to spread laterally into its shadow; 2d, the fact that a sound is frequently borne by an upper current in an opposite direction to the wind at the surface; and 3d, that a sound moving against a wind is heard better at a higher elevation. The first point to consider is in what manner the wind affects sound. That it is in some way connected with the distance to which sound can be heard is incontestably settled by general observation. At first sight,

the explanation of this might seem to be very simple, namely, that the sound is borne on in the one direction and retarded in the other by the motion of the wind. But this explanation, satisfactory as it might appear, cannot be true. Sound moves at the rate of about 780 miles an hour, and therefore on the above supposition, a wind of 7·8 miles per hour could neither retard nor accelerate its velocity more than one per cent.—an amount inappreciable to ordinary observation; whereas we know that a wind of the velocity mentioned is frequently accompanied with a reduction of the penetrating power of sound—of more than 50 per cent.

The explanation of this phenomenon, as suggested by the hypothesis of Professor Stokes, is founded on the fact that in the case of a deep current of air, the lower stratum—or that next the earth, is more retarded by friction than the one immediately above, and this again than the one above it, and so on. The effect of this diminution of velocity as we descend toward the earth, is—in the case of sound moving with the current, to carry the upper part of the sound-waves more rapidly forward than the lower parts, thus causing their resultant impulse (the sound-beam) to incline toward the earth, or in other words to be thrown down upon the ear of the observer. When the sound is in a contrary direction to the current, an opposite effect is produced; the upper portion of the sound-waves is more retarded than the lower, which advancing more rapidly in consequence, inclines the rays of sound upward and directs them above the head of the observer. To render this more clear, let us recall the nature of a beam of sound, in still air, projected in a horizontal direction. It consists of a series of concentric waves perpendicular to the direction of the beam,—like the palings of a fence. Now if the upper part of the waves has a slightly greater velocity than the lower, the beam will be bent downward in a manner somewhat analogous to that of a ray of light in proceeding from a rarer to a denser medium. The effect of this deformation of the wave will be cumulative from the sound-centre onward, and hence—although the velocity of the wind may have no perceptible effect on the

velocity of the sound, yet this bending of the wave being continuous throughout its entire course, a marked effect must be produced.

A precisely similar effect will be the result, but perhaps in a considerably greater degree, in case an upper current is moving in an opposite direction to the lower, when the latter is adverse to the sound; and in this we have a logical explanation of the phenomenon observed by General Duane, in which a fog-signal is well heard during the occurrence of a north-east snow-storm. Certainly this phenomenon cannot be explained by any peculiarity of the atmosphere as to variability of density, or of the amount of vapor which it may contain.

The first phenomenon of the class mentioned by General Duane which I had the good fortune to witness was in company with Sir Frederick Arrow, and Captain Webb, of the Trinity House, London, in their visit to this country in 1872. At the distance of two or three miles from an island in the harbor of Portland, Maine, on which a fog-signal was placed, the sound which had been distinctly heard on approaching the island was lost for nearly a mile, and slightly regained at a less distance. On examining the position of the fog-signal, which was situated on the farther side of the island from the steamer, we found it placed immediately in front of a large house with rising ground in the rear, which caused a sound-shadow, into which the sound was projected at a distance, (on account of the lateral divergence of the rays,) but not in the immediate vicinity of the island. In the same year, I made an excursion in one of the light-house steamers, with Captain Selfridge, to an island on the coast of Maine, at which abnormal phenomena were said to have been observed; but on this occasion no variation of the sound was noted, except that which was directly attributable to the wind, the signal being heard much farther in one direction than in the opposite.

PART II.—ON SOME ABNORMAL PHENOMENA OF SOUND.

(Bulletin Philosophical Society Washington; vol. II, Appendix, pp. 45–52.)

Read December 11, 1872.

The communication which I propose to make this evening is brought forward at this time especially on account of the presence of Dr. Tyndall, he being connected with the light-house system of Great Britain, while the facts I have to state are connected with the light-house service of the United States, and must therefore be of interest to our distinguished visitor. The facts I have to present form part of a general report to be published by the United States Light-House Board.

The Light-House Board of the United States has from its first establishment aimed not only to furnish our sea-coast with all the aids to navigation that have been suggested by the experience of other countries and to adopt the latest improvements, but also to enrich the light-house service with the results of new investigations, and new devices for the improvement of its efficiency, or in other words to add its share to the advancement of a system which pertains to the wants of the highest civilization.

Among the obstructions to navigation none are more serious, especially on the American coast, than those caused by fogs. Fog (as it is well known) is due to the mingling of warmer air surcharged with moisture—with colder air, and nowhere on the surface of the earth do more favorable conditions exist for producing fogs than on both our Atlantic and Pacific coasts. On the Atlantic the cold stream of water from the polar regions in its passage southward, on account of the rotation of the earth—passes close along our eastern coast from one extremity to the other, and parallel to this but opposite in direction, for a considerable distance is the great current of warm water known as the Gulf-stream. Above the latter the air is constantly surcharged with moisture, and consequently whenever light winds blow from the latter across the former, the vapor is condensed into fog, and since

in summer along our eastern coast the southerly wind prevails, we have during July, August, and September, especially on the coast of Maine, an almost continuous prevalence of fogs so dense that distant vision is entirely obstructed.

On the western coast the great current of the Pacific, after having been cooled in the northern regions, in its passage southward gives rise to cold and warm water in juxtaposition, or in other words a current of the former through the latter, and hence whenever a wind blows across the current of cold water a fog is produced.

From the foregoing statement it is evident that among the aids to navigation, fog-signals are almost as important as light-houses. The application of the science of acoustics to the former, is however far less advanced than is that of optics to the latter. Indeed, attempts have been made to apply lights of superior penetrating power (as the electric and calcium lights) to supersede the imperfect fog-signals in use. When however we consider the fact that the absorptive power of a stratum of cloud, which is but a lighter fog, of not more than a mile or two in thickness, is sufficient to obscure the image of the sun, the intensity of the light of which is far greater than that of any artificial light, it must be evident that optical means are insufficient for obviating the difficulty in question.

The great extent of the portions of the coast of the United States which are subject to fogs—renders the investigation of the subject of fog-signals one of the most important duties of the Light-House Board.

In studying this subject it becomes a question of importance to ascertain whether waves of sound (like those of light) are absorbed or stifled by fog; on this point however observers disagree. While from the striking analogy in many respects between light and sound, the opinion has largely prevailed that sound is impeded by fog, the opposite opinion has been adopted by some observers—that sound is in some instances better heard during a fog than in clear weather. To settle this question definitely the Light-House Board has directed that at two light-houses on the route from Boston

to Saint John, the fog-signal shall be sounded every day on which the steamboats from these ports pass the station, both in clear and foggy weather, the pilots on board these vessels having, for a small gratuity, engaged to note the actual distance of the boat when the sound is first heard on approaching the signal and is last heard on receding from it. The boats above mentioned estimate their distance with considerable precision by the number of revolutions of the paddle-wheel as recorded by the indicator of the engine, and it is hoped by this means to definitely decide the point in question. We think it probable that fog may very slightly diminish the penetrating power of sound, or in other words produce an effect analogous to that on the propagation of light. But when we consider the extreme minuteness of the particles of water constituting the fog as compared with the magnitude of the waves of sound, the analogy does not hold except in so small a degree as to be of no practical importance, or in other words the existence of a fog if a true—is we think a wholly insufficient cause of diminution of sound; a view borne out by the great distance at which our signals are heard during a dense fog.

Another cause of the diminution of the penetrating power of sound (also probably a true one) is the varying density of the atmosphere—from heat and moisture, in long distances. The effect of this however would apparently be to slightly distort the wave of sound rather than to obliterate it. However this may be, we think from all the observations we have made, the effect is small in comparison with another cause, viz., that of the influence of wind. During a residence of several weeks at the sea-shore, the variation in intensity of the sound of the breakers at a distance of about a mile, in no case appeared to be co-incident with the variations of an aneroid barometer or a thermometer, but to be in every instance affected by the direction of the wind.

The variation in the distinctness of the sound of a distant instrument as depending on the direction of the wind is so marked, that we are warranted in considering it the principal cause of the inefficiency in certain cases of the most

powerful fog-signals. The effect of the wind is usually attributed (without due consideration) to the motion of the body of air between the hearer and the sounding instrument; in the case of the wind coming toward him, it is supposed that the velocity of the sound is re-enforced by the motion of the air, and when in the opposite direction that it is retarded in an equal degree. A little reflection however will show that this cannot be the cause of the phenomena in question, since the velocity of sound is so vastly greater than that of any ordinary wind that the latter can only impede the progress of the former by a very small percentage of the whole.

Professor Stokes, of Cambridge University, England, has offered a very ingenious hypothetical explanation of the effect of wind on sound, which we think has an important practical bearing—especially in directing the line of research, and subsequent application of principles.

His explanation rests upon the fact that during the passage of a wind between the observer and the sounding instrument, its velocity will be impeded at the surface of the earth on account of friction and other obstacles; and the velocity of the stratum immediately above will be somewhat reduced by that below, and so on, the retardation being gradually lessened as we ascend through the strata. From this it follows that the sound wave will be deformed, and the direction of its *normal* changed. Suppose for example that the wind is blowing directly from the observer. In this case the retardation of the sound wave will be greater above than below, and the upper part of the wave-front will be thrown backward, so that the axis of the phonic ray will be deflected upward, and over the head of the observer. If on the other hand a deep river of wind (so to speak) is blowing directly toward the observer, the upper part of the front of the wave will be inclined down and toward him, concentrating the sound along the surface of the earth.

The science of acoustics in regard to the phenomena of sound as exhibited in limited spaces has been developed with signal success. The laws of its production, propagation, reflection, and refraction have been determined with

much precision, so that we are enabled in most cases to explain, predict, and control the phenomena exhibited under given conditions. But in the case of loud sounds and those which are propagated to a great distance, (such as are to be employed as fog-signals,) considerable obscurity still exists. As an illustration of this I may mention the frequent occurrence of apparently abnormal phenomena. General G. K. Warren informs me that at the battle of Seven Pines, in June, 1862, near Richmond, General Johnston, of the Confederate army, was within three miles of the scene of action with a force intended to attack the flank of the Northern forces, and although he listened attentively for the sound of the commencement of the engagement, the battle—which was a severe one lasting about three hours, ended without his having heard a single gun. (See Johnston's report.) Another case of a similar kind occurred to General McClellan at the battle of Gaines' Mills, June 27, 1862, also near Richmond. Although a sharp engagement was progressing within three or four miles for four or five hours, the General and his staff were unaware of its occurrence, and when their attention was called to some feeble sound, they had no idea that it was from anything more than a skirmish of little importance. (See Report of the Committee on the Conduct of the War.) A third and perhaps still more remarkable instance is given in a skirmish between a part of the Second Corps under General Warren and a force of the enemy. In this case the sound of the firing was heard more distinctly at General Meade's headquarters than it was at the headquarters of the Second Corps itself, although the latter was about midway between the former and the point of conflict. The sound appeared so near General Meade's camp that the impression was made that the enemy had advanced between it and General Warren's command. In fact, so many instances occurred of wrong impressions as to direction and distance derived from the sound of guns, that little reliance came to be placed on these indications.

In the report of a series of experiments made under the direction of the Light-House Board by General J. C. Duane, of

the Engineer Corps, is the following remark: "The most perplexing difficulty arises from the fact that the fog-signal often appears to be surrounded by a belt varying in radius from one to one and a half miles, from which the sound appears to be entirely absent. Thus in moving directly from a station the sound is audible for the distance of a mile, is then lost for about the same distance, after which it is again distinctly heard for a long time."

Again, in a series of experiments at which Sir Frederick Arrow and Captain Webb, of the Trinity Board, assisted, it was found that in passing in the rear of the opposite side of an island in front of which a fog-signal was placed the sound entirely disappeared, but by going farther off to the distance of two or three miles, it re-appeared in full force, even with a large island intervening. Again, from the experiments made under the immediate direction of the present chairman of the Light-House Board, with the assistance of Admiral Powell and Mr. Lederle, the light-house engineer, and also from separate experiments made by General Duane, it appears that while a reflector in the focus of which a steam whistle or ordinary bell is placed—re-enforces the sound for a short distance, it produces little or no effect at the distance of two or three miles, and indeed the instrument can be as well heard in still air at the distance of four or five miles in the line of the axis of the reflector whether the ear be placed before or behind it. From these results we would infer that the divergency of sound, or its tendency to spread laterally as it passes from its source, is much greater than has been supposed from experiments on a small scale. The idea we wish to convey by this is that a beam of sound issuing through an orifice, although at first proceeding like a beam of light in parallel rays, soon begins to diverge and spread out into a cone, and at a sufficient distance may include even the entire horizon.

We may mention also in this connection, that from the general fact of the divergence of the rays of sound, the application of reflection as a means of re-enforcing sound must of necessity be in a considerable degree a failure.

By the application of the principle we have stated and the effect of the wind in connection with the peculiarities of the topography of a region and the position of the sounding body, we think that not only may most of the phenomena we have just mentioned be accounted for, but also that other abnormal effects may be anticipated.

In critically examining the position of the sounding body in the experiment we have mentioned, in which Sir Frederick Arrow and Captain Webb assisted, it was found that the signal was placed on the side of a bank with a large house directly in the rear, the roof of which tended to deflect the sound upwards so as to produce in the rear a shadow, but on account of the divergency of the beam this shadow vanished at the distance of a mile and a half or two miles, and at the distance of say three miles the sound of the instrument was distinctly heard. I doubt not that on examination all the cases mentioned by General Duane, with one exception, might be referred to the same principle, the exception being expressed in the following remarkable statement in his report to the Light-House Board: "The fog-signals have frequently been heard at a distance of *twenty* miles, and as frequently cannot be heard at the distance of *two* miles, and with no perceptible difference in the state of the atmosphere. The signal is often heard at a great distance in one direction, while in another it will be scarcely audible at the distance of a mile. For example, the whistle at Cape Elizabeth can always be distinctly heard in Portland,—a distance of nine miles, during a heavy north-east snow-storm, the wind blowing a gale directly from Portland toward the whistle."

This is so abnormal a case, and so contrary to generally received opinion, that I hesitated to have it published under the authority of the board until it could be verified and more thoroughly examined. In the observations that have been made under my immediate supervision the sound has always been heard farther *with* the wind than against it. It would appear therefore from all the observations that the normal effect of the wind when blowing directly against the sound, is to greatly diminish it.

There is however a meteorological condition of the atmosphere during a north-east storm on our coast which appears to me to have a direct bearing on the phenomenon in question. It is this, that while a violent wind is blowing from the north-east into the interior of the country, a wind of equal intensity is blowing in an opposite direction at an elevation of a mile or two. This is shown by the rapid eastwardly motion of the upper clouds as occasionally seen through breaks in the lower.

As a further illustration of this principle I may mention that on one occasion (in 1855) I started on my way to Boston from Albany, in the morning of a clear day, with a westerly wind. The weather continued clear and pleasant until after passing the Connecticut river, and until within fifty miles of Boston. We then encountered a storm of wind and rain which continued until we reached the city. On inquiry I learned that the storm had commenced in Boston the evening before, and although the wind had been blowing violently toward Albany for *twenty* hours it had not reached inwardly more than fifty miles. At this point it met the *west* wind and was turned back above in almost a parallel current. This is the general character of north-east storms along our coast, as shown by Mr. Espy, and is directly applicable to the phenomenon mentioned by General Duane, which must be accepted as a fact, though by no means a general one applicable to all stations. While a violent wind was blowing toward his place of observation from Cape Elizabeth at the surface of the earth, a parallel current of air was doubtless flowing above with equal or greater velocity in the opposite direction. The effect of the latter would be to increase the velocity of the upper part of the wave of sound, and of the former to diminish it; the result of the two being to incline the front of the wave of sound toward the observer, or to throw it down toward the earth, thus rendering the distant signal audible under these conditions when otherwise it could not be heard. I think it is probable that the same principle applies to other cases of the abnormal propagation of sound.

For the production of a sound of sufficient power to serve as a fog-signal, bells, gongs, &c., are too feeble except in special cases where the warning required is to be heard only at a small distance. After much experience the Light-House Board has adopted for first-class signals,—instruments actuated by steam or hot-air engines, and such only as depend upon the principle of resonance, or the enforcement of sound by a series of recurring echoes in resounding cavities.

Of these there are three varieties. First, the steam-whistle, of which the part called the bell is a resounding cavity, the sound it emits having no relation to the material of which it is composed; one of the same form and of equal size of wood producing an effect identical with that from one of metal. Another variety is the fog-trumpet, which consists of a trumpet of wood or metal actuated by a reed like that of a clarinet. The third variety is called the siren trumpet, which consists of a hollow drum, into one head of which is inserted a pipe from a steam-boiler, while in the other head a number of holes are pierced, which are alternately opened and shut by a revolving plate having an equal number of holes through it. This drum is placed at the mouth of a large trumpet. The sound is produced by the series of impulses given to the air by the opening and shutting of the orifices and consequent rushing out at intervals with explosive violence of the steam or condensed air. The instrument, as originally invented by Cagniard de Latour, of France, was used simply in experiments in physics to determine the pitch of sound; but Mr. T. Brown, of New York, after adding a trumpet to it and modifying the openings in the head of the drum and the revolving plate, under the direction of the Light-House Board, perfected it as a fog-signal, and as such it has been found the most powerful ever employed.

In ascertaining the penetrating power of different fog-signals I have used with entire success an instrument of which the following is a description: A trumpet of ordinary tinned iron of about 3 feet in length, and 9 inches in diameter at the larger end, and about 1 inch at the smaller, is gradually bent so that the axis of the smaller part is at

right angles to the axis of the larger end; on the smaller end is soldered a cone of which the larger end is about 2 inches in diameter. Across the mouth of this cone is stretched a piece of gold-beater's skin. When the instrument is used, the opening on the larger end is held before the apparatus to be tested, the plane of the mouth of the trumpet being vertical and the membrane being horizontal; over the membrane is strewed a small quantity of fine sand, which is defended from the agitation of the air by a cylinder of glass, the upper end of which is closed by a lens. When the apparatus under examination is sounded, (being sufficiently near,) the sand is agitated; the testing instrument is then moved farther off step by step until the agitation just ceases; this distance being measured is taken as the relative penetrating power of the sounding apparatus. The same process is repeated with another sounding instrument, and the distance at which the sound ceases to produce an effect on the sand is taken as the measure of the penetrating power of this apparatus, and so on. On comparing the results given by this instrument with those obtained by the ear on going out a sufficient distance the two are found to agree precisely in their indications. The great advantage in using this contrivance is that the relative penetrating power of two fog-signals may be obtained within a distance of a few hundred yards, while to compare them by the ear requires the aid of a steamer and a departure from the origin of sound in some cases of fifteen or twenty miles.

PART III.—INVESTIGATIONS DURING 1873 AND 1874.

(Report of the United States Light-House Board for 1874, pp. 107-117.)

Observations on Sound and Fog-Signals in August, 1873.

Professor Henry, chairman, and Commander John G. Walker, naval secretary, of the Light-House Board, left Portland August 12th, 1873, at 3 o'clock P. M. in the steam-tender *Myrtle*, Captain Foster, for Whitehead light-station, at which place abnormal phenomena of sound had been observed.

Whitehead light-station is on a small island about a mile and a half from the coast of Maine, on the western side of the entrance to Penobscot Bay, and in the direct line of the coasting-steamers and other vessels from the westward,—bound into the Penobscot Bay and river. The light-house and fog-signal are situated on the south-east slope of the island, the surface of which consists almost entirely of rock, the middle being at an elevation of 75 feet above the mean tide-level.

The phenomena which had been observed at this and other stations along the coast consisted of great variation of intensity of sound while approaching and receding from the station. As an example of this we may state the experience of the observers on board the steamer *City of Richmond* on one occasion, during a thick fog at night in 1872. The vessel was approaching Whitehead from the south-westward, when at a distance of about six miles from the station, the fog-signal—which is a 10-inch steam-whistle, was distinctly perceived and continued to be heard with increasing intensity of sound until within about three miles, when the sound suddenly ceased to be heard, and was not perceived again until the vessel approached within a quarter of a mile of the station, although from conclusive evidence furnished by the keeper, it was shown that the signal had been sounding during the whole time. The wind during this time was from the south, or approximately in an opposite direction to the sound. Another fact connected with this occurrence was that the keeper on the island distinctly heard the sound of the

whistle of the steamer, which was blown as soon as the whistle at the station ceased to be heard, in order to call the attention of the keeper to what was supposed to be a neglect of his duty in intermitting the operations of his signal. It should be observed that the sound from the steamer in this case was produced by a 6-inch whistle, while that of the station was from an instrument of the same kind, of 10 inches in diameter; or in other words a lesser sound was heard from the steamer, while a sound of greater volume from the station was unheard in an opposite direction. It is evident that this result could not be due to any mottled condition or want of acoustic transparency of the atmosphere, since this would absorb the sound equally in both directions. The only plausible explanation of this phenomenon is that which refers it to the action of the wind. In the case of the sound from the steamer, the wind was favorable for its transmission, and hence it is not strange that its sound should be heard on the island when the sound from the other instrument could not be heard on the steamer. To explain on the same principle the fact of the hearing of the sound at the distance of six miles, and afterward of losing it at the distance of three miles, we have only to suppose that in the first instance the retarding effect of the wind was small, and that in the second it became much greater on account of a sudden increase in the relative velocity of the current in the upper and lower portions.

After making a critical examination of the island and the position of the machinery, and also in regard to any obstacle which might interfere with the propagation of the sound, the keeper was directed to put the instrument in operation and to continue to sound it for at least two hours, or until the steamer was lost sight of; which direction was complied with. In passing from the island, almost directly against a light wind, the intensity of the sound gradually diminished as a whole—with the increase of distance, but varied in loudness from blast to blast, now louder, then again more feeble, until it finally ceased at a distance of about fifteen miles, as estimated by the intervals between the blasts and the sight of the steam as seen through a spy-glass, and also from points on the Coast-Survey charts.

The result of this investigation clearly showed the power of the apparatus in propagating sound under conditions not entirely favorable, since the wind—though light, was in opposition to the sound.

Cape Elizabeth Light-Station, Maine, August 29, 1873.—The fog-signal at this place is on a prominent headland to which the course of all vessels is directed when bound from the southward into Portland Harbor. It is furnished with two light-houses 919 feet apart and 143 feet above sea-level. The easterly tower is connected with the keeper's dwelling by a wooden-covered way 200 feet long and about 12 feet high; the station is furnished with a 10-inch steam fog-whistle, placed to the southward of the easterly tower at a distance of about 625 feet and about at right angles with the covered way; it therefore has a background, including the covered way, of about 65 feet above the height of the whistle, which was found to reflect a perceptible echo. The whistle was actuated by steam at 55 pounds pressure, consuming from 60 to 65 pounds of anthracite coal per hour. The whistle itself differs from the ordinary locomotive-whistle by having a projecting ledge or rim around the lower part through which the sheet of steam issues to strike against the lower edge of the bell. What effect this projecting ledge or rim may have is not known to the observers. This whistle is provided (for the purpose of concentrating the sound in a given direction) with a hollow truncated pyramid 20 feet long, 10 feet square at the large end, and $2\frac{1}{2}$ feet square at the small end, the axis of the pyramid being placed parallel to the horizon, with the whistle at the smaller end. In order to ascertain the effect of this appendage to the whistle, the simplest plan would have been to note the intensity of sound at various points on a circle of which the whistle should be the centre. This being impracticable on account of the intervention of the land, the observations were confined to points on the three arcs of a circle of about 120° , of which the axis divided the space into 80° and 40° , and a radius of one, two, and three miles. The result of these observations was that starting from the

axis of the trumpet on the east side, the sound grew slightly less loud until the prolongation of the side of the trumpet was reached, when it became comparatively faint and continued so until the line between the whistle and observer was entirely unobstructed by the side of the trumpet, when the sound was apparently as loud as in the prolongation of the axis itself. On the west side of the axis of the trumpet, the sound in a like manner diminished from the axis until the prolongation of the side of the trumpet was reached, when it became feeble, again slightly increased, and then gradually diminished until the line of direction made an angle of about 80° with the axis of the trumpet, when it ceased to be heard at a distance of about one and a half miles. It should be observed however that at this point the line of sight of the observers was obstructed by the side of the trumpet and the smoke-stack of the boiler. The wind was light, at south-southwest, approximately in opposition to the direction of the sound when it ceased to be heard.

We are informed that complaints had previously been made by officers of steamers passing near this point that the sound was here inaudible previous to the introduction of this trumpet; it would therefore follow that it is of no use to increase the effect on the western side of the axis, and is of injury to the sound on the lines of prolongation of its sides. If the sound should cease to be heard at the point mentioned when the trumpet is removed, the only apparent cause of the phenomenon will be the prevailing direction of the wind, which coming from the south-west will be in opposition to the sound of the whistle; but in the case of the present investigation the force of the wind was so small that it scarcely appeared adequate to produce the effect, and this question therefore must be left for further investigation. It may be important to state that in the case where the sound ceased to be heard, it was regained by sailing directly toward the station about one mile, or at half a mile from the station.

After making the foregoing observations as to the intensity of sound in different directions from the station,

the observations were closed by sailing directly along the axis of the trumpet until the sound, which gradually grew fainter as the distance increased, finally ceased to be heard at a distance of about nine miles. In comparing this last result with an instrument of about the same power at Whitehead, which gave a perceptible sound at a distance of fifteen miles, the only apparently variable circumstance was the velocity of the wind,—in both cases adverse to the direction of the sound; but in that of Cape Elizabeth it was of considerably greater force.

During the foregoing experiments, when the vessel was about a mile from the station, steaming directly outward,—in the prolongation of the axis of the instrument, there was heard after each sound of the whistle a distinct echo from the broad, unobstructed ocean, which was attributed at the time, as in other cases, to reflections from the crests and hollows of the waves. A similar phenomenon has since been elsewhere observed, and referred to a reflection from air of a different density. This observation becomes important in regard to the solution of the question as to the abnormal phenomena of sound.

Cape Ann Light-Station, Massachusetts, August 31, 1873.—This is one of the most important stations on the New England coast. It is furnished with two first-order lights and a 12-inch steam whistle, actuated by 60 pounds pressure of steam. The present is the fourth engine which has been erected at this station, in consequence of the complaints either as to the inefficiency of the sound or its failure to be heard in certain directions. It was at first proposed to sail entirely around the island in order to test the intensity of the sound in different directions, but this was found impracticable on account of the shallowness of water on the inland side; the observations were therefore confined to the direction in which complaints had been made as to the deficiency of the signal, namely, in a southerly direction. The result of these observations (the points of which included an arc of 120°) was that the sound was heard with equal intensity except when the direction of the station was to the northward and

eastward of the observers; then in one instance the sound became very indistinct, and in another was entirely lost, both at a distance of about two miles. In these cases the line of sight between the observers and the signal was interrupted—in the first by a small building the gable-end of which was within 10 feet of the whistle, and in the second by the south light-tower, which is within 30 feet of the whistle. In this series of experiments, as in the last, the wind was against the sound; the effect was noted by passing over the arc several times at different distances. The wind was from the southward and westward and very light, and the sound was finally lost at about six miles, and in the direction of the obstructions.

Boston Light-Station, August 31, 1873.—The light-house is situated on a low rocky island, on the north side of the main outer entrance to Boston Harbor, nine miles from the city. It is furnished with three caloric engines, two of the second class and one of the first. The two second-class engines are so arranged as to act separately or together, and in the latter arrangement serve to duplicate the larger engine. At the time the observations were made, the larger engine was about being repaired, and one of the smaller engines with the double air-reservoir was used. The larger engine is used with 12 pounds pressure of air, which falls to 8 pounds in producing the sound. The smaller engine, with the double reservoir, is started with 9 pounds pressure, which falls to 8 pounds. This difference in the pressure of air in the two engines is caused by the larger ratio of the reservoir to the size of the reed. With a greater pressure than 12 pounds to the square inch in the larger engine, and 9 pounds in the smaller, no sound is produced; the reed is unable to act against the pressure, and consequently the orifice remains closed. The trumpet of the larger of the engines is reported to have been heard eighteen miles at sea, which—in consideration of the results obtained at Whitehead, we thought very probable. The time required (from starting fires) to get a good working-pressure, is about half an hour. The amount of coal consumed per hour is 17 pounds.

There is moreover at this station a bell, operated by a Stevens clock, not at present used. It is placed on a high, wooden frame structure, on which one of the ancient bell striking machines was originally erected. The most proper position for the fog-signal is on the ground occupied by this bell-tower, but as this was not removed at the time of the erection of the trumpets, they were placed in such positions as to have the line of sound interrupted to the north-eastward by the bell and light towers. It was therefore thought probable that this was the cause of the deficiency of sound in this direction. To test this, the vessel was caused to traverse the arcs of several concentric circles, in the portion of the horizon where the sound was most required as a signal. The first arc traversed was about one and one-half miles from the signal. The vessel on this crossed the axis where the sound was quite loud, and proceeded northward until the sight of the trumpet was obscured by the before-mentioned towers, when the sound became almost inaudible. The vessel next returned across the axis, on a circle of about three miles radius, with similar results; but after crossing the axis the sound on the southern side continued to be but little diminished in intensity along an arc of two and a half miles, or as far as the land would allow the vessel to go. The vessel was next put upon an arc of which the radius was one and a half miles, and on the south side of the axis, and sailed to the northward until the axis was reached; it was then turned and run for the entrance of the harbor, hugging the southern shore, keeping as far from the signal as possible. Throughout this passage the sound was clear and loud, showing very little—if any diminution of power as the several positions deviated more and more from the direction of the axis, until the vessel was at right angles with the axis, the land not permitting any greater distance. The vessel approached to within three-quarters of a mile of the signal and then continued still farther around, until nearly in the rear of it, the sound still continuing clear and loud. The vessel next proceeded up the harbor, nearly in the line of the axis of the trumpet prolonged in the rear

still continuing to hear the signal distinctly until the keeper, losing sight of the vessel, stopped sounding the instrument. These observations were made under very favorable circumstances, it being nearly calm. What wind did exist was about equally favorable to points on either side of the axis. The inference from these observations is—first, that small objects placed near the source of sound tend to diminish its intensity in the direction of its interruption, and should therefore if possible be removed, or the instrument so placed as to obviate such obstructions; and second, that even with the trumpet, the sound so diverges from the axis as to be efficient even in the rear of the instrument.

Observations on Fog-Signals, in August and September, 1874.

The first of these investigations was made August 25, on board the steamer *Putnam*, at Little Gull Island, with Commodore Stephen D. Trenchard, inspector of lights of the third district, accompanied by Governor Charles R. Ingersoll, of Connecticut, and Captain John H. Upshur, U. S. N.

At this place are two sirens, the one to replace the other in case of an accident. One of the sirens was sounded with the pressure of 50 pounds per square inch. The wind was across the axis of the trumpet, and almost precisely at right angles to it.

The steamer was headed against the wind, on a line at right angles to the axis of the trumpet. The sound in this case also travelled against the wind, which was at an estimated velocity of from 4 to 5 miles per hour. The distance travelled before the sound became inaudible, was estimated by the speed of the steamer, at $3\frac{1}{2}$ miles.

The steamer was next headed in an opposite direction and returned along its previous path, across the mouth of the trumpet of the siren, the sound gradually increasing in strength without any marked irregularity, until the siren was reached, and on leaving this, (the course remaining the same,) the sound gradually diminished in intensity, but with less rapidity than before, until it was finally lost at a distance of $7\frac{1}{2}$ miles. In the latter instance, the movement of

the sound was with the wind. The result of these observations was conformable to that generally obtained from previous ones, namely that the sound is seldom or never heard at the same distance in different directions, and moreover that it is generally heard farther with the wind than against it.

The observations of this day also illustrate the spread of the sound-wave on either side of the axis of the trumpet, a fact which has frequently been noticed in other investigations. It may be well to mention that the siren trumpet at this locality is directed horizontally, with its prolonged axis passing over a space of very rough ground, (immediately in front of the mouth of the trumpet,) the surface of which is principally composed of bowlders: one of these (of very large size) is directly in front of the trumpet, and the idea occurred to me that this rough surface might produce some effect on the transmission of sound to a distance. I observed by strewing sand upon a paper that the former was violently agitated when held near the surface of the large boulder just mentioned, during the blast of the siren trumpet.

At this station, during the visit of Sir Frederick Arrow, the sound was lost in the direction of the axis of the trumpet at a distance of two miles, and then again regained with distinctness at the light-vessel, a distance of four and one-half miles; this was what I have denominated an abnormal phenomenon, which was due as I think—to a slight variation in the velocity of the lower and upper parts of the current of air, but unfortunately, the demand for the use of the vessel as a light-house tender prevented the attempt to ascertain whether the same phenomenon would be observed a second time, and to further investigate its cause.

Observations September 1, 1874.—The second series of investigations this season was made with General J. G. Barnard, of the Light-House Board, and General I. C. Woodruff, engineer of the third district. We proceeded on this occasion in the steamer *Mistletoe* to Block Island,—one of the outer stations of the Light-House Board, fully exposed (without intervention of land) to the waves and storms of the ocean.

On the southerly side of this island a light-house is about being erected, and a siren station had been established at this locality, and was in full operation.

There are here two sirens attached to one boiler, one to be used in case of an accident to the other. For the sake of experiment they are of slightly different qualities, one with a larger trumpet with a revolving disk of the old pattern, giving a lower tone; the other a smaller trumpet having a revolving disk with openings allowing a much more sudden and full blast of steam, and revolving with greater velocity so as to give a higher pitch. The latter is far the superior instrument, as was evident to us by the sound which it produced, and as had been established by the use of the artificial ear in the manufactory of Mr. Brown. The effect on the unguarded ear was scarcely endurable, and the very earth around appeared to tremble during the blast. The keeper (an intelligent man who has been promoted from the position of assistant keeper at Beaver Tail light to this station) informed us that a fleet of fishing-vessels coming in, distinctly heard it at a distance estimated by their rate of sailing at scarcely less than thirty miles; this was on two separate occasions. The keeper had been directed to note and record the date at which he heard the sound from other signals; he reported that he had frequently heard the fog-signal at Point Judith, a distance of seventeen miles, and that the observer at the latter place frequently heard his signal; but on comparing records, the two sounds had not been heard simultaneously by the two keepers; when a sound was heard from one station, the opposite sound was not heard from the other, illustrating again the general rule that sound is not transmitted simultaneously with equal intensity in opposite directions.

This occasion also furnished very favorable conditions for observing the remarkable phenomenon of the ocean-echo. At the cessation of each blast of the trumpet, (after a slight interval,) a distinct and prolonged echo was returned from the un-obstructed ocean. It is important to observe—in regard to this phenomenon, that the siren is placed near the edge

of a perpendicular cliff, at an elevation of from 75 to 100 feet above the ocean, and furthermore that the direction of the wind formed an angle of about 35° with the axis of the trumpet. Now the loudness of this echo was not the greatest at the siren-house, but increased in intensity until a point was reached several hundred yards from the trumpet, approximately more in accordance with a reflection from the waves. The wind was blowing from the shore with the direction of the sound as it went off from the trumpet, and nearly against it on the return of the echo. I have attributed this phenomenon (which was first observed in 1866 at East Quoddy Head, on the coast of Maine, and since at various stations, at which the trumpet or siren has been used,) to the reflection of the sound from the crests and slopes of the waves, and the observation we have mentioned would appear to favor this hypothesis. In connection with this explanation, I may mention that my attention has been called by General M. C. Meigs, of the United States Army, to an echo from the palings of a fence, and also from a series of indentations across the under side of the arch of one of the aqueduct bridges of the Washington water-works. The fact that the sound was much louder at a point considerably distant from the trumpet was noted by one of the party entirely unacquainted with the hypothesis.

The keeper of this station confirmed (without a leading question) the statement of Captain Keeney, that a feeble sound of a distant object—as the roar of the surf, can frequently be heard against the direction of the wind, and that in this case it always betokens a change in the weather, and is in fact used generally by the fishermen as a prognostic of a change in the direction of the wind, which will in the course of a few hours invariably spring up from an opposite quarter. In such case it is highly probable (as has been stated,) that a change has already taken place in the direction of the upper strata of the air, although from theoretical considerations we might infer that the same result would be produced if the wind were stationary above and moving with a considerable velocity in a direction opposite

to the sound at the surface of the earth, the velocity gradually diminishing as we ascend, for in this case also the inclination of the sound waves would be downward.

Observations September 23, 1874.—The third series of investigations was made in company with Captain John L. Davis and Major Peter C. Hains, both of the Light-House Board, and General I. C. Woodruff, engineer of the third district, and Mr. Brown, patentee of the siren. For the purpose, three light-house tenders were employed, viz: the *Mistletoe*, Captain Keeney; the *Putnam*, Captain Field; and the *Cactus*, Captain Latham.

The place of operation chosen for the first day's series was about $1\frac{1}{2}$ miles from the northern point of Sandy Hook.

From the experience gained by the accumulated observations which had been made, it was concluded that the phenomena of sound in regard to perturbing influences could not be properly studied without simultaneously observing the transmission of sound in opposite directions. It was therefore concluded to employ at least two steamers in making the investigations.

In regard to this point the commission was fortunate in being able to command the use, for a limited period, of the three tenders mentioned above, which happened to be at the time assembled at the light-house depot, Staten Island, and could be spared from their ordinary operations for a few days without detriment to the service. It was also fortunate in selecting for the scene of the investigations an unobstructed position in the lower bay of New York, and perhaps still more fortunate in the season of the year when on account of the heat of the sun—a land and sea breeze which changed their directions at a particular hour of the day, enabled results to be obtained bearing especially on the phenomena to be investigated.

Attention was first given to the character of the several steam-whistles which were intended to be used as the sources of the sound during the series of investigations.

These whistles, which were sounded during the whole of the observations with twenty pounds of steam on each boiler,

gave at first discordant sounds, and were found by their effect upon an artificial ear to be considerably different in penetrating power; they were then adjusted by increasing or diminishing the space between the bell and the lower cylinder, (by turning a screw intended for that purpose on the axis of the bell,) until they produced the same effect upon the sand in the membrane of the artificial ear; but in order to be further insured of the equality of the penetrating power of the several whistles, the three steamers abreast—forming as it were a platoon, were directed to proceed against the wind, sounding all the time in regular succession,—the *Cactus* first, then after an interval of a few seconds, the *Mistletoe*, and then the *Putnam*,—until the stationary observers lost the sound of each. They became inaudible all very nearly at the same moment. The sound of the *Putnam* was thought to be slightly less distinct; it was therefore chosen as a stationary vessel, from which the observations of the sound of the other two were to be made.

The *Putnam* being anchored at the point before mentioned, arrangements were made for sending off the other two vessels in opposite directions, one with and the other against the wind, with instructions to return when the sound became inaudible to those on the stationary vessel, this to be indicated by a flag-signal. It should be mentioned that the velocity of the wind was measured from time to time during the subsequent experiments with one of Robinson's hemispherical cup anemometers, made by Casella, of London. The velocity of the wind as observed by this instrument just before the starting of the vessels, was 6 miles per hour, the instrument being freely exposed on the paddle-boxes of the steamer. A sensitive aneroid barometer marked 30·395 ins. and continued to rise gradually during the day to 30·43 ins.; the temperature was 71° F.

1st trial.—The vessels left at 11:18 A. M., the wind being from the west, Captain Davis taking charge of the sounding of the whistle on the *Cactus*, which proceeded east with the wind, the sound coming to the ear of the observer against the wind; while the sounding on the *Mistletoe* was in charge of Gen-

eral Woodruff, and as the vessel steamed against the wind, the sound came to the observers on the stationary vessel with the wind; the other members of the party remained on the *Putnam*, at anchor at the point before mentioned, off the Hook, Major Hains having charge of the signals. The sound of the first of the vessels was heard faintly at 14 minutes after leaving, but not heard at 16 minutes; we may therefore assume that it became inaudible at 15 minutes. And within a minute of the same time,—by a mistake of the signal, the other ceased to advance, and commenced to come back; the sound from it however was very distinct, while at the same moment the sound from the other was inaudible. On account of the mistake mentioned, the relative distance at which the sounds from the two vessels might have become inaudible cannot be accurately given; but the fact observed, that the sound which came with the wind was much more audible than the other, is in conformity with the generally observed fact that sound is heard farther with the wind than against it. In the meantime the velocity of the wind had sunk to $1\frac{1}{2}$ miles per hour.

2d trial.—Next the vessels leaving at 11:55 A. M. changed positions; the *Cactus*, under Captain Davis, steamed west, directly in the direction from which the wind came, while the *Mistletoe*, under General Woodruff, steamed east, directly before the wind. The result of this trial was well marked in all respects; the sound of the *Mistletoe* was lost in 9 minutes, which, from the speed of the steamer, was estimated at about $1\frac{1}{2}$ miles, while the sound of the *Cactus* was heard distinctly for 30 minutes, or at an estimated distance of 5 miles. The wind at the middle of this trial had sunk to 0.42 mile per hour, or nearly to a calm. The result of this trial was somewhat abnormal, for though the wind had sunk nearly to a calm, the sound was still heard three times as far in the direction of the slight wind as against it.

3d trial.—After a lapse of an hour and a half a third trial was made; the wind had changed within two points of an exactly opposite direction, blowing (from the indications of the anemometer) at the rate of $10\frac{1}{2}$ miles per hour.

The *Cactus* again steamed in the eye of the wind, which was now however from nearly an opposite point, while the other vessel steamed in an opposite direction. The sound of the *Cactus* was lost (with the wind) at the end of twenty-seven minutes, or at a distance of four and a half miles.

The sound of the *Mistletoe* (moving against a brisk wind then blowing) was lost at the end of thirty minutes, or at a distance of five miles.

This result was entirely unexpected and much surprised every member of the party, since it was confidently expected that an increase in the intensity of the wind of more than ten miles per hour, and a change to the opposite direction, would materially affect the audibility of the sound, and give a large result in favor of the sound that moved in the same direction with the wind; but this was not the case. In the course of all the observations in several years in which investigations have been carried on under the direction of the chairman of the board, this is the only instance in which he had heard a sound at a greater distance against the wind than with it; although (as before stated) a number of cases have been reported by other observers in which (under peculiar conditions of the weather) this phenomenon has been observed.

To briefly recapitulate the results, we have in this case three instances in succession in which a sound was heard farther from the west than from the east, although in the meantime the wind had changed to nearly an opposite direction. Had these results been deduced from the first observations made on the influence of wind on sound, or in other words without previous experience, the conclusion would have been definitely reached that something else than wind affected the conveyance of sound, and this conclusion would have been correct if the suggestion had been confined to the wind at the surface; but from previous observations and theoretical conclusions, the observed phenomena are readily accounted for by supposing that during the whole time of observation the wind was blowing from the west in the higher part of the aerial current, and that the calm and

opposing wind observed were confined to the region near the surface. An unsuccessful attempt to test this hypothesis, was made by means of a balloon of tissue-paper, constructed by Major Hains, but which was unfortunately burned in the attempt to inflate it with heated air.

The remainder of this day was devoted to observations on the sound of the siren at the light-house at Sandy Hook. For this purpose the *Cactus*, under Captain Davis, was directed to steam in the eye of the wind, while the *Mistletoe*, under General Woodruff, steamed before the wind, and the *Putnam* steamed at right angles to the wind. Unfortunately, on account of the diminution of light at the closing in of the day, nothing could be observed. The only result obtained was that one of the duplicate sirens was heard more distinctly than the other, namely the one with the higher note.

Experiments September 24, 1874.—The place chosen for the observations of this day was still farther out on the ocean, at the Sandy Hook light-vessel, 6 miles from the nearest point of land. The pressure of the atmosphere was a little greater than the day before, being 30.52; the temperature about the same, 72° Fahr., wind light, from a westerly direction, as on the previous day, with a force (as indicated by the anemometer,) of 1.2 miles per hour. Having been provided with a number of India-rubber toy balloons, the two vessels were sent off in opposite directions,—leaving at 10:40 A. M., the *Mistletoe* toward the west, against the wind, the *Cactus* toward the east, with the wind. A change was also made in observing the sound. In these observations the sound was noted at each vessel from the other, the speed of the steamers being the same; the distance between them when the *Mistletoe* lost the sound of the *Cactus* was two miles, while the *Cactus* continued to hear the *Mistletoe's* sound (coming with the wind) until they were four miles apart. Simultaneously with this observation a balloon was let off from the *Putnam* at the light-vessel, which in its ascent moved continuously obliquely upward in a line slightly curving toward the horizon, in the direction of the

wind at the surface, as far as it could be followed with the eye, indicating a wind in the same direction in the several strata through which it passed, but of a greater velocity in the upper strata.

Second trial.—The vessels now changed places, the *Cactus* steaming west, the *Mistletoe* east, the wind having entirely ceased at the surface of the earth. In this case the *Cactus* lost the sound of the *Mistletoe* when the vessels were two miles apart, while the *Mistletoe* continued to hear the sound of the *Cactus* until they were three miles apart. A balloon let off ascended vertically until it attained an elevation of about one thousand feet, when turning east it followed the direction of the previous one. The sound in this case from the east was heard three miles, and that from the west was heard two miles, while in the preceding observations the distances were as 2 to 1; the only changing element (as far as could be observed) was that of the wind at the surface, which had somewhat diminished.

Third trial 12:45 P. M.—The wind previous to this trial had changed its direction 10 points or about $112\frac{1}{2}^{\circ}$ round through the south, and (as indicated by the anemometer) had a velocity of 4.8 miles per hour. In this case the *Cactus*, going against the wind, lost the *Mistletoe's* sound coming to her against the wind, when the vessels were 1 mile apart, while the *Mistletoe* heard the *Cactus*, the sound coming to her with the wind, when the vessels were $1\frac{7}{8}$ miles apart. The several balloons set off at this time were carried by the surface wind westwardly until nearly lost to sight, when they were observed to turn east, following the direction of the wind that prevailed below in the earlier observations. The results of the whole series of observations are extremely interesting. In all the experiments the difference in the audibility of the sound in different directions was very marked, and indeed it rarely happens that the sound is equal in two directions, although from the hypothesis adopted, this may be possible, since according to it both the upper and lower currents have an influence upon the audibility of sound in certain directions.

In the first trial, (of September 23,) the motion of the air being in the same direction both below and above, but probably more rapid above than below on account of resistance, the upper part of the sound-wave would move more rapidly than the lower, and the wave would be deflected downward, and therefore the sound (as usual) heard farther with the wind than against it. In the third experiment of the same day, in which the wind changed to an almost opposite direction, if the wind remained the same above,—as we have reason to suppose it did from the observations on the balloons on the second day, the sound should be heard still farther in the same direction—or against the wind at the surface, since in this case the sound-wave being more retarded near the surface would be tipped over more above and the sound thus be thrown down.

The observations of the next day (Sept. 24) are also in conformity with the same hypothesis, the change in the wind being probably due to the heating of the land—as the day advanced, beyond the temperature of the water, and thus producing a current from the latter to the former, while the wind observed in the morning from the west was the land-wind due to the cooling of the latter.

In the morning the wind was blowing from the west both in the higher strata and at the surface of the earth, and in this condition the sound was heard farther with the wind than against it.

The wind at the surface about mid-day gradually ceased, and shortly afterward sprang up from an easterly direction; in this condition the sound, (with the wind at the surface) was heard at a greater distance. This is also in strict conformity with the theory of a change in the form of the sound-wave, as in the latter case, the lower portion would be retarded, while the upper portion of the wave would be carried forward with the same velocity, and hence the sound would be thrown down on the ear of the observer. To explain the result of the third trial of the second day, we have only to suppose that the influence of the upper current was less than that of the lower. The conditions for these observations

were unusually favorable, the weather continuing the same during the two days, and the change of the wind also taking place at nearly the same hour.

The fact thus established seems entirely incompatible with the supposition that the diminution in the sound is principally caused by a want of homogeneity in the constitution of the atmosphere, since this would operate to absorb sound equally in both directions.

In May, 1873, Professor Tyndall commenced a series of investigations on the subject of the transmission of sound, under the auspices of the Trinity House, of England, in which whistles, trumpets, guns, and a siren were used; the last-named instrument having been lent by the Light-House Board of the United States to the Trinity House for the purpose of the experiments in question. The results of these investigations were in most respects similar to those which we had previously obtained. In regard to the efficiency of the instruments, the same order was determined which has been given in this report, namely the siren, the trumpet, and the whistle. Professor Tyndall's opinion as to the efficiency of the siren may be gathered from the following remarks. Speaking of the obstruction of sound in its application as a fog-signal, he says, "There is but one solution of this difficulty, which is to make the source of sound so powerful as to be able to endure loss and still retain sufficient residue for transmission. Of all the instruments hitherto examined by us the siren comes nearest to the fulfillment of this condition, and its establishment on our coasts will in my opinion prove an incalculable boon to the mariner." Professor Tyndall arrived at the conclusions which the information we had collected tended to establish, that the existence of fog however dense does not materially interfere with the propagation of sound, and also that sound is generally heard farther with the wind than against it, although the variation of the intensity of the sound is not in all cases in proportion to the velocity of the wind. The result of his investigations in regard to the pitch of sound was also similar to those we have given; and indeed all the facts which

he has stated are (with a single exception as to the direction of the echo) in strict accordance with what we have repeatedly observed. We regret to say however, that we cannot subscribe to the conclusions which he draws from his experiments as to the cause of the retardation of sound, that it is due to a flocculent condition of the atmosphere, caused by the intermingling with it of invisible aqueous vapor.

That a flocculent condition of the atmosphere, due to the varying density produced by the mingling of aqueous vapor, is a true cause of obstruction in the transmission of sound is a fact borne out by deduction from the principles of wave-motion, as well as by the experiments of the distinguished physicist of the Royal Institution of Great Britain; but from all the observations we have made on this subject, we are far from thinking that this is the efficient cause of the phenomena under consideration. A fatal objection we think to the truth of the hypothesis Professor Tyndall has advanced, is that the obstruction to the sound—whatever may be its nature, is not the same in different directions. We think we are warranted in asserting that in the cases of acoustic opacity which he has described, if he had simultaneously made observations in an opposite direction, he would have come to a different conclusion. That a flocculent condition of the atmosphere should slightly obstruct the sound is not difficult to conceive; but that it should obstruct the ray in one direction and not in an opposite, or in a greater degree in one direction than in another, the stratum of air being the same in both cases, is at variance with any fact in nature with which we are acquainted. We would hesitate to speak so decidedly against the conclusions of Professor Tyndall,—for whose clearness of conception of physical principles, skill in manipulation, and power of logical deduction, we entertain the highest appreciation, were the facts which have been obtained in our investigations of a less explicit character.

While the phenomena in question are incompatible with the assumption of a flocculent atmosphere as a cause, they are in strict accordance with the hypothesis of the refraction

of the waves of sound—due to a difference in velocity in the upper and lower portions of the currents of air. We do not say however that the transmission of sound in the atmosphere is fully investigated, or that the abnormal phenomena which are said to have been observed in connection with fog-signal stations have been fully explained. So far from this, we freely admit that we are as yet in ignorance as to how the hypothesis we have adopted is applicable to the critical explanation of the obstruction to sound in the abnormal cases mentioned by General Duane. We feel however considerable confidence in its power to afford a rational explanation of these phenomena when the conditions under which they exist shall have been accurately determined.

We are further confirmed in our conclusion by the publication of an interesting paper in the Proceedings of the Royal Society, by Professor Osborne Reynolds, of Owens College, Manchester, intended to show that sound is not absorbed by the condition of the atmosphere, but refracted in a manner analogous to the hypothesis which has been adopted in the preceding report.

Much further investigation is required to enable us to fully understand the effects of winds on the obstruction of sound, and to determine the measure of the effect of variations of density in the air due to inequality of heat and moisture. But such investigations can be made only under peculiar conditions of weather and in favorable localities, with the aid of a number of steamers, and a series of observers, by whom the transmissibility of sound may be simultaneously observed in different directions. The position which we were so fortunate to obtain in our experiments in the lower bay of New York at the season of the prevalence of land and sea breezes was exceptionally favorable for the study of the action of wind upon sound. It is the intention of the Light-House Board to continue observations in regard to this matter, and to embrace every favorable opportunity for their prosecution under new and varied conditions.

PART IV.—INVESTIGATIONS IN 1875.

(Report of the United States Light-House Board for 1875, pp. 104–126.)

Preliminary Remarks.—In the appendix to the Light-House Report of 1874 I gave an account of a series of investigations relative to fog-signals which had been made at different times under the direction of the chairman of the committee on experiments.

These investigations were not confined to the instruments for producing sound, but included a series of observations on sound itself in its application to the uses of the mariner. In the course of these investigations the following conclusions were early arrived at:

1st. That the rays of a beam of loud sound do not (like those of light) move parallel to each other from the surface of a concave reflector, but constantly diverge laterally on all sides; and although at first they are more intense in the axis of the reflector they finally spread out so as to encompass the whole horizon, thus rendering the use of reflectors to enforce sound—of little value in fog-signals.

2d. That the effect of wind in increasing or diminishing sound is not confined to currents of air at the surface of the earth, but that those of higher strata are also efficient in varying its transmission.

3d. That although sound is generally heard farther with the wind than against it, yet in some instances the reverse is remarkably the case, especially in one locality, in which the sound is frequently heard against a north-east snow-storm more distinctly than when the wind is in an opposite direction. This anomaly was referred to the action of an upper current in an opposite direction to that at the earth, such a current being known to exist in the case of north-east storms on our coast. But in what manner the action of the wind increased or diminished the audibility of sound was a problem not solved. It could not be due, as might be thought at first sight, to the acceleration of the sonorous impulse by the addition of the velocity of the wind to that of sound, on the

one hand, nor to the retardation of the latter by the motion of the wind, on the other. The inadequacy of this explanation must be evident, when we reflect that sound moves at the rate of 750 miles an hour, and therefore a wind of $7\frac{1}{2}$ miles an hour would only increase its velocity one per cent.; whereas the actual increase in audibility produced by a wind of this intensity is in some instances several hundred per cent.

In this state of our knowledge, a suggestion of Professor Stokes, of Cambridge, England, which offered a plausible explanation of the action of the wind, became known to me, and was immediately adopted as a working hypothesis to direct investigations.

This suggestion—the importance of which appears to have escaped general recognition, is founded on the fact that the several strata into which a current of air may be divided do not move with the same velocity. The lower stratum is retarded by friction against the earth and by the various obstacles it meets with, the one immediately above by friction against the lower, and so on; hence the velocity increases from the ground upward; a conclusion established by abundant observation. Now in perfectly still air, a sounding instrument—such as a bell, produces a series of concentric waves perfectly spherical; but in air in motion the difference of velocity above and below disturbs the spherical form of the sound-wave, giving it somewhat the character of an oblique ellipsoid, by tending to flatten it above to the windward, and to increase its convexity above to the leeward; and since the direction of the sound is perpendicular to the sound-wave, when moving against the wind it will be thrown upward above the head of the observer, and in the opposite direction downward toward the earth. A similar effect will be produced, but with some variations and perhaps greater intensity, by a wind above opposite to that at the surface of the earth.

These propositions will be rendered plain by the following illustrations (Figures 1, 2, and 3), for which I am indebted to an article just published in the American Journal of Sci-

ence, by Mr. William B. Taylor, on "Recent Researches in Sound," as the present report is passing through the press.

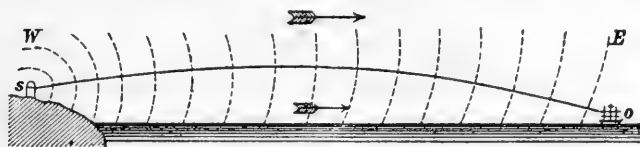


FIG. 1.—Favoring wind.

In these cuts, Figure 1 represents the effect of a favorable wind in depressing the waves of sound; *s* being the signal-station and *o* the point of observation. The wind blowing from *W* to *E*, as the spheroidal faces of the sonorous waves become more pressed forward by the greater velocity of the wind above, (assuming it to be retarded at the surface by friction,) and the direction of the acoustic beam being constantly normal to the wave-surfaces, the lines of direction of the sound will gradually be bent downward and reach the ear of the observer with an accumulated effect at the point *o*; being re-enforced by the lower sound-rays which are reflected from the surface of the water.

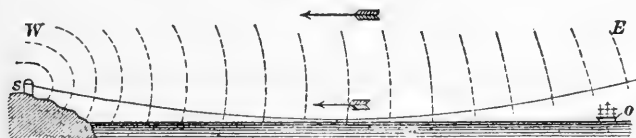


FIG. 2.—Adverse wind.

Figure 2 represents the ordinary effect of an opposing wind here blowing from *E* to *W* against the sound; the wave-faces being more resisted above than below by the swifter wind, (assuming as before a retardation at the surface,) the sound-beams are curved upward, and the lowest ray that in still air would reach the distant observer at *o*, is gradually so tilted up, that it passes above the ear of the listener, leaving him practically in an acoustic shadow; even after due allowance made for the divergence of the sound by dissipation downward.

Figure 3 represents the disturbing effect of two winds, the lower in opposition to the sound, and the upper with it.



FIG. 3.—Compound wind.

In this case the principal effect will be a depression of the sound-beam, similar to that shown in Figure 1, but more strongly marked, as the difference of motion will be greater as we ascend. Attending this action, says Mr. Taylor, there will probably be some lagging of the lower stratum by reason of the surface-friction, the tendency of which will be to distort the lower part of the sound-waves, giving the lowest sound-beam a reverse or serpentine curvature. Such an effect is represented by the lower line *s t o*, (Figure 3,) the lower ray being at first turned up (by the adverse wind, somewhat as shown in Figure 2) and afterward thrown down by the dominant influence of the higher current of air, rendering the sound less audible at an intermediate point—*t*, than at the more distant station *o*. This hypothetical case of compound refraction offers a plausible explanation of the paradox of a nearer sound being diminished in power by the wind which increases the effect of a more distant one.

In these figures and all the succeeding ones, the direction of the wind is indicated by arrows.

The hypothesis we have adopted (in connection with the fact of the lateral spread of sound) gives a simple explanation of various abnormal phenomena of sound such as have been observed in the previous investigations, and of which the following are examples: First, the audibility of a sound at a distance, and its inaudibility nearer the source of sound; second, the inaudibility of a sound at a given distance in one direction, while a lesser sound is heard at the same distance in an opposite direction; third, the audibility of the sound of an instrument at one time at the distance of several

miles, while at another time the sound of the same instrument cannot be heard at more than a fifth of the same distance; fourth, the circumstance that while the sound is heard generally farther with the wind than against it, in some instances the reverse is the case; fifth, the sudden loss of sound in passing from one locality to another in the same vicinity, the distance from the source of the sound being the same.

The first four of these phenomena find a ready explanation in the hypothesis adopted, by supposing an increase or diminution in the relative velocity of the currents of wind in the upper and lower strata of air. The fifth is explained—either by an irregular twisting of the sound-beam, (as above suggested,) or by the inter-position of an obstacle which casts a sound-shadow—disappearing at a given distance by the convergence of the rays on each side of the obstacle into what would be an optical shadow.

Accounts of these investigations were presented from time to time to the Light-House Board and to the Philosophical Society of Washington in 1872. Subsequently a series of investigations on the same subject was instituted in England by the Elder Brethren of the Trinity House under the direction of their scientific adviser, the eminent physicist, Dr. Tyndall. While in the latter investigations various abnormal phenomena similar in most instances to those we have mentioned were observed, they were referred by Dr. Tyndall to an entirely different cause, viz, to the existence of acoustic clouds, consisting of portions of the atmosphere in a flocculent or mottled condition, due to the unequal distribution of heat and moisture, which absorbing and reflecting the sound, produce an atmosphere of acoustic opacity. While we do not deny the possible existence of such a condition of the atmosphere, we think it insufficient to account for all the phenomena in question, and believe that a more general and efficient cause is that of the *wind*, in accordance with the hypothesis of Professor Stokes.

We regret to differ in opinion from Dr. Tyndall, and have published our dissent from his views in no spirit of captious

criticism or desire to under-value the results he has obtained, some of which are highly important. Our only object in our remarks and in our investigations is the establishment of truth.

The determination of the question as to the cause of the abnormal phenomena of sound we have mentioned, and the discovery of new phenomena—are matters not merely of abstract scientific interest, but of great practical importance, involving the security of life and property, since they include the knowledge necessary to the proper placing of fog-signals and the instruction of mariners in the manner of using them.

The hypothesis we have adopted,—that of the change of direction of sound by the unequal action of the wind upon the sound-waves, is founded on well-established mechanical principles, and offers a ready explanation of facts otherwise inexplicable. It is also a fruitful source from which to deduce new consequences to be verified or dis-proved by direct experiment. It would however ill become the spirit of true science to assert that this hypothesis is sufficient to explain all the facts which may be discovered in regard to sound in its application to fog-signals, or to rest satisfied with the idea that no other expression of a general principle is necessary. An investigation however to be fruitful in results must as a general rule be guided by *a priori* conceptions. Hap-hazard experiments and observations may lead to the discovery of isolated facts, but rarely to the establishment of scientific principles. There is danger however in the use of hypotheses, particularly by those inexperienced in scientific investigations, that the value of certain results may be over-estimated, while to others is assigned less weight than really belongs to them. This tendency must be guarded against. The condition of the experiment must be faithfully narrated, and a scrupulously truthful account of the results given. While we have used the above-mentioned hypothesis in the following investigations as something more than an antecedent probability, we have not excluded observations which may militate against it, and we hold

ourselves ready to admit the application of other principles, or to modify our conception of those we have adopted when new facts are discovered which warrant such changes. But we require positive evidence, and cannot adopt any conclusions which we think are not based upon a logical correlation of facts.

The investigations described in the following account—though simple in their conception, have been difficult and laborious in their execution. To be of the greatest practical value they were required to be made on the ocean, under the conditions in which the results are to be applied to the use of the mariner, and therefore they could only be conducted by means of steam-vessels of sufficient power to withstand the force of rough seas, and at times when these vessels could be spared from other duty. They also required a number of intelligent assistants skilled in observation and faithful in recording results.

Observations in August, 1875, at Block Island.

The party engaged in these investigations consisted of the chairman of the Light-House Board; General I. C. Woodruff, U. S. A., engineer third light-house district; Dr. James C. Welling, president of the Columbian University, Washington, D. C.; Mr. T. Brown, of New York, patentee of the siren; Mr. Edward Woodruff, assistant superintendent of construction; and Captain Keeney, commander of the light-house steamer *Mistletoe*.

We arrived at Block Island on the afternoon of the 4th of August, 1875. This place was chosen as the site of the experiments,—first, on account of its insular position, being as it were in the prolongation of the axis of Long Island, distant fifteen miles from the most easterly part of the latter, and entirely exposed to the winds and waves of the Atlantic Ocean; and secondly, because there are on Block Island two light-houses, one of which is of the first order, and connected with it are two fog-signals, one of them with the latest improvements.

Observations in regard to the Aerial Echo.—This phenomenon has been frequently observed in the researches of the Light-House Board, in case of powerful sounds from the siren and from the fog-trumpet.* It consists of a distinct reflection of sound as if from a point near the horizon in the prolongation of the axis of the trumpet. The question of the origin of this echo has an important bearing—according to Dr. Tyndall, on the explanation of the abnormal phenomena of sound we have mentioned. He refers it to the non-homogeneous condition of portions of the air which reflect back the waves of sound in accordance with the analogy of the reflection of light at the common surface of two media of different densities. We have adopted the provisional hypothesis that it is due to the reflection from the waves and the larger undulations of the surface of the ocean in connection with the divergency of beams of powerful sounds. To bring these hypotheses to the test of a crucial experiment, arrangements were made under the direction of Mr. Brown to change the direction of the axis of one of the sirens from the horizontal to the vertical position.

August 5, 1875.—The first observations were made August 5, with the siren in its usual horizontal position, while the air was so charged with fog as to render the sound of the instrument necessary for the guidance of the mariner, the image of the sun being obscured and the land invisible from the sea. Under these conditions an echo was heard when the pressure of the steam reached 50 pounds per square inch. The reflection in this case (as usual) was from a point in the sea-horizon in the prolongation of the axis of the trumpet. It was not however heard more distinctly when standing near the origin of the sound than

* The same phenomenon is mentioned by Froissart in his account of the embarkation of the expedition of the French and English to the coast of Africa to assist the Genoese against the pirates in 1390. "It was a beautiful sight," says the chronicler, "to view this fleet, with the emblazoned banners of the different lords fluttering in the wind, and to hear the minstrels and other musicians sounding their pipes, clarions, and trumpets, whose sounds were re-echoed back by the sea." (See Illustrations of Froissart by H. N. Humphrey, Plate IV.)

at several hundred feet on either side of it. The interval between the cessation of the original sound and the commencement of the echo was not as marked as in some previous observations, not being more than four or five seconds. The duration of the echo was on the average about eight seconds,—beginning with the time of its first perception and not with the cessation of the sound of the trumpet. General Woodruff and Dr. Welling both noted the peculiar character of the echo, which was that of a series of reflections varying in intensity from a maximum, near the beginning, and gradually dying away. The wind was nearly at right angles to the axis of the trumpet and also to that of the crests of the swell of the ocean, which was rolling in from the effects of a distant commotion. The barometer at 12 m. indicated 30·2 inches; the dry-bulb thermometer 73° F., the wet-bulb 70° F., indicating a remarkable degree of aqueous saturation. During the whole day the air in all the region around Block Island was undoubtedly in a homogeneous condition.

August 6, 1875.—On this day the weather was nearly the same. The fog-signal on the 5th instant was kept in operation for the use of the mariner nineteen hours, and on this day it was blown twenty hours continuously. The barometer marked 30·2 inches; the thermometer 70° F.; the fog not as equally distributed as on the preceding day; the north end of the island (distant four miles) being distinctly visible. The wind was S. W. to S., making an angle of about 60° with the axis of the fog-trumpet. The echo continued to be heard distinctly with a sound varying in intensity, but was not so loud as we have heard it on certain occasions in previous years.

During this and the preceding day workmen were employed under Mr. Brown in inserting a flexible India-rubber tube two inches in diameter between the revolving plate of the siren and the smaller end of the trumpet, so that it might be brought into a vertical position. This work—though apparently simple, was difficult in execution, since it involved the necessity of strong supports for the cast-iron

trumpet, which itself weighed eight hundred pounds, and also a union of the parts of sufficient strength to resist the pressure of the steam at fifty pounds to the square inch.

August 7, 1875.—Wind from the S. S. W.: fog continued. The workmen had not as yet completed the attachment.

August 9, 1875.—Barometer 30·30 inches at 12 M. Dry-bulb thermometer 74° F.; wet bulb 71°·5. Wind S. S. W. Fog dense along the south coast, but light over all the northern portion of the island. The echo was heard all day, not very loud, but distinct. Siren still horizontal, the arrangement for elevating it not having been—at 10 A. M., completed. Experiments were made on the reciprocal sounds of the whistles from two steamers, the results to be given hereafter. At 5 P. M. the adjustment of the flexible tube to the smaller end of the trumpet was finished, which giving an additional length to the instrument of about 5 feet, threw it out of unison with the siren proper. To restore this unison the speed of revolution of the perforated plate was diminished, and after this the trumpet, still being horizontal, was sounded. An echo—similar in character to those which had been observed on the preceding day, and the earlier part of the same day, was produced.

August 10, 1875.—Barometer 30·1 inches. Dry bulb 74°; wet bulb 69° F. Wind W. S. W.; atmosphere hazy. Observations first made with the trumpet horizontal. Echo as that of preceding days, distinct but not very loud, and coming principally from the portion of the horizon in the direction of the axis of the trumpet. The position of the trumpet was then changed, its axis being turned to the zenith in order to make what was thought might be a crucial experiment. When the trumpet was now sounded a much louder echo was produced than that which was heard with the axis of the trumpet horizontal, and it appeared to encircle the whole horizon; but though special attention was directed to the point by all the party present, no reverberation was heard from the zenith. The echo appeared however to be more regular and prolonged from the ocean portion of the horizon than from that of the land.

In this experiment, while there was no reflection from the

zenith in which the sonorous impulse was strongest, there must have been reverberations from the surface of the land and the ocean. This will be evident when we consider the great divergency of sound by which sonorous waves from a vertical trumpet are thrown down to the plane of the horizon on every side, some of which meeting oblique surfaces must be reflected back to the ear of the observer near the source of the sound. This inference will be more evident when it is recollected that the reflected rays of sound diverge as well as those of the original impulse. Hence reflection from the surface of the sea is a true cause of the echo, but whether it be a sufficient one may require further investigation. For this explanation it is not necessary that the sea should be covered with crested waves; a similar effect would take place were the surface perfectly smooth but in the form of long swells, which in places exposed to an open sea are scarcely ever absent. Moreover the increased loudness of the echo is a fact in accordance with the same view.

The observations were repeated with the same effect on succeeding days, until this class of experiments was ended by the bursting of the India-rubber tube. Had a distinct echo been heard from the zenith the result would have been decidedly in favor of the hypothesis of a reflection from the air; but as this was not the case the question still remained undetermined, especially since the atmosphere during these experiments was evidently in a homogeneous condition. We do not agree however in the position taken in the report of the Trinity Board, that on the origin of this echo depends the whole solution of the problem as to the efficient cause of the abnormal phenomena of sound. The ingenious experimental illustrations of the reflection of sound from a flame or heated air establish clearly the possibility of such reflection; but it must be remembered that they were made under exaggerated conditions, the atmosphere being in a state of extreme rarefaction in a limited space, and the sound of a feeble character, while the phenomena in nature are produced with a comparatively small difference of temperature and with powerful sounds.

Experiments as to the Effect of Elevation on Audibility.—For this investigation the first-order light-house at Block Island offered peculiar facilities. It is situated near the edge of a perpendicular bluff 152 feet above the sea. The tower being 52 feet above the base—gives a total height (to the focal plane of the lens) of 204 feet, on the level of which the ear of the observer could be placed.

The first and second experiments of this class were made on the 10th of August, with two light-house steamers—the *Putnam* and the *Mistletoe*, moving simultaneously in opposite directions. The barometer indicated 30.1 inches of atmospheric pressure; the dry-bulb thermometer indicating 74° F., and the wet-bulb 69°. The wind at the time of the experiments was from the west, and of a velocity of seven miles per hour. The vessels started from the point C, Fig. 4, opposite the light-house, A, about one mile distant, a position as near the shore as it was considered safe to venture. The *Putnam* steamed with the wind, the *Mistletoe* steamed against the wind, each blowing its whistle every half minute. The duration of the sound was noted at the top of the tower and at the level of the sea, Mr. Brown being the observer at the latter station, while the chairman of the Board, with an assistant, observed at the former. On comparing notes, the watches having been previously set to the same time, the following results were found.

First experiment.—The duration of the sound on the tower when coming against the wind was nine minutes, while at the base of the cliff it was heard only one minute. It was afterward found from the records on board of the *Putnam*, the sound of which came against the wind, that this vessel was moving during the experiment at half speed, and hence the duration of the sound on the tower should be considered as $4\frac{1}{2}$ minutes, and the difference in favor of audition on the tower 4 minutes instead of 8, as given by the first record.

Second experiment.—The sound of the *Mistletoe*, coming to the observers with the wind, was heard on the tower during 15 minutes, while it was heard at the base of the cliff during 34 minutes, the difference being 19 minutes in favor of hear-

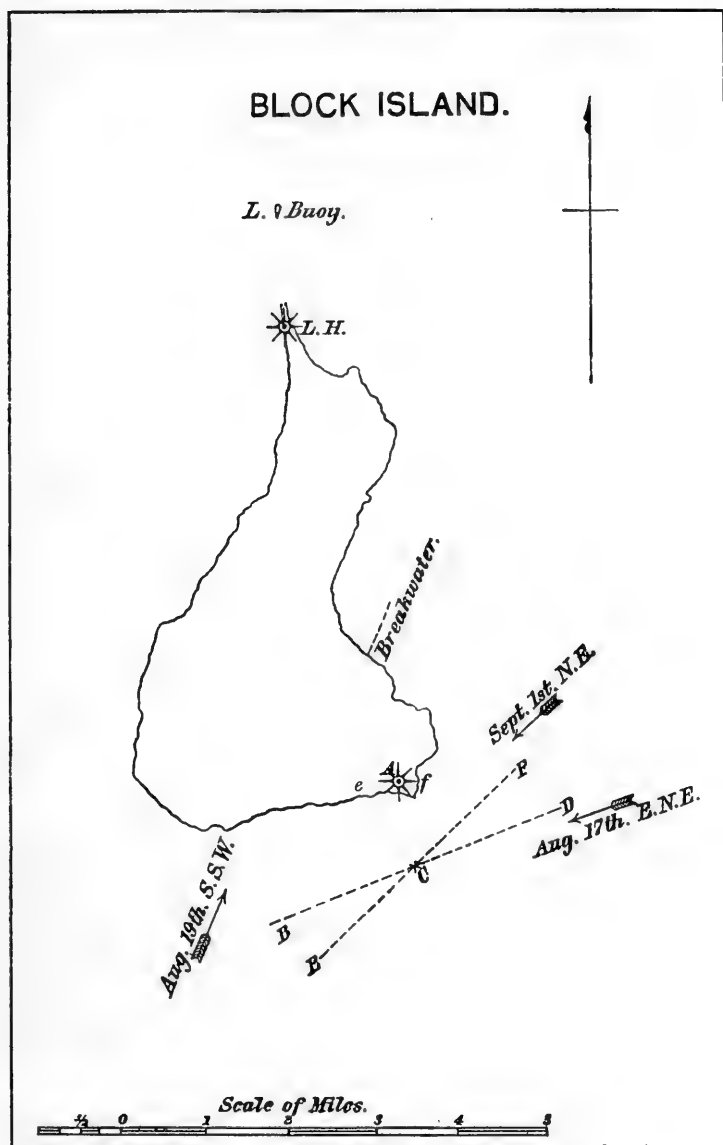


FIG. 4.

ing at the level of the sea. This result—which differs from that of all the other experiments of the same class, deserves special attention.

After making the foregoing experiments of this class, and others on the effect of wind on sound, to be described in the next section, the vessels were called off for other duty, and the investigations were not resumed until August 17, when the following experiments were made:

Third experiment.—The wind was from the E. N. E., at the rate of about five miles per hour at the surface, and a greater velocity at the height of the tower. Barometer, 30.25 ins.; thermometer, 72°.

In this and the subsequent experiments of the same day, but one steamer—the *Mistletoe*—was employed. It started at 10:30 A. M. from the point *C*, Fig. 4, at the foot of the cliff, and steamed W. S. W. along *CB* for about 12 minutes, or a distance of two miles, blowing the whistle every half-minute. To note the duration of the sound, Dr. Welling was stationed at the foot of the cliff, at the level of the sea, while the chairman of the Light-House Board, with an assistant who acted as clerk, was on the upper gallery of the tower, the ears of the latter being almost precisely 200 feet above those of the observer at the foot of the cliff.

The watches having been previously set to the same time, on comparing results it was found that the whistle was heard at the top of the tower for twelve minutes and at the bottom of the cliff for five and one-half minutes, making the difference in favor of audition on the tower six and one-half minutes. In this experiment the sound came to the observers nearly against the wind.

Fourth experiment.—This consisted in directing the *Mistletoe* to proceed in the opposite direction from the same point, along the line *CD*. It started at 11:5 A. M. the breeze being light at the time, and proceeded about two and one-half miles before the sound was lost to the observers. On comparing notes it was found that the sound was heard at the top of the tower during fifteen minutes, and at the level of the sea for eleven minutes, giving a difference in favor of the hearing on the top of the tower of four minutes.

Fifth experiment.—In this, the *Mistletoe* steamed again in the direction with the wind, the sound from its whistle coming

to the ears of the observers against the wind. Starting about 11:45 A. M. and steaming about two miles, the sound was heard on the tower during twelve minutes and at the foot of the cliff during five and one-half minutes, making a difference of six and a half minutes in favor of audition on the tower. Previous to this experiment the wind had veered one point to the west, bringing the direction of the sound to the observers in less direct opposition to the wind than in the last experiment.

Sixth experiment.—In this case the steamer was directed to proceed in the opposite direction, or against the wind, so that the sound of the whistle would reach the ear of the observers in the same direction as that of the wind. It started at 12:19 P. M. and proceeded two and one-sixth miles; the whistle was heard during thirteen minutes on the top of the tower, and at the bottom of the cliff during precisely the same time, the difference between the top of the tower and the bottom of the cliff in this case being nothing.

Seventh experiment.—The vessel having again been called off on other duty the next experiment was made the 1st of September. On this day the wind was north-east; the velocity at the top of the tower was thirteen and a half miles per hour, and at the bottom of the tower eleven miles per hour. The barometer indicated 30.2 inches pressure, the dry bulb 72°, and the wet bulb 67.5°.

The theoretical conditions for exhibiting the effect of height on audition in this experiment were much more favorable than any of the preceding. First, the velocity of the wind was greater; second, the difference between the velocities at top and bottom of the tower was well marked, and the direction of the wind was more favorable for direct opposition to the sound as it came to the ear of the observer. In this case, General Woodruff was the observer at the bottom of the cliff, while the chairman of the Light-House Board and his assistant, with several visitors, were at the top of the tower.

The steamer started at 10:58 A. M. and proceeded during eight minutes, or a mile and one-third, when the sound was

lost at the top of the tower. In this case, though the sound was heard for eight minutes at the top of the tower, and the first five blasts marked on the notes as quite loud, it was not heard at all at the bottom of the cliff, at least a hundred yards nearer the source of the sound.

This result, which interested and surprised a number of intelligent visitors, who were in the tower at the time, strikingly illustrates the effect of elevation on the audibility of sound moving against the wind. The result was so important that it was thought advisable to immediately repeat the experiment under the same conditions.

Eighth experiment.—The *Mistletoe* was again directed to proceed, in the direction of the wind, along the line it had previously traversed. It started at 11:25 A. M., and proceeded during six minutes, or one mile, when the sound was lost at the top of the tower. In this case, the first blast of the whistle was feebly heard at the base of the cliff, but no other, while thirteen blasts were heard at the top of the tower, of which the first six were marked as loud.

That this remarkable effect was not produced by an acoustic cloud or a flocculent atmosphere is evident from the experiment which immediately succeeded.

Ninth experiment.—In this trial, the *Mistletoe* was directed to proceed against the wind, so that the sound of its whistle should come to the ears of the observers with the wind. It started at 11:48 A. M., and proceeded during sixteen minutes, or two and two-thirds miles, when the sound of its whistle was lost to the observers on the top of the tower. In this case the sound of the whistle became audible at the bottom of the cliff as soon as the position of the vessel became such as to bring the sound to the observers approximately with the wind, and continued to be audible during fifteen minutes, or within one minute as long as the sound was heard at the top of the tower.

It may be mentioned as an interesting fact that an assistant who was observing the sound with General Woodruff at the foot of the cliff, when the sound could not be heard at the level of the sea (in the sixth experiment), perceived it

distinctly by ascending the side of the cliff to a height of twenty-five or thirty feet.

All the conditions and results of these experiments are strikingly in conformity with the theory of the refraction of sound which we have previously explained.

The following recapitulation of the results of the foregoing experiments will exhibit their correspondence with the general theory :

Sound heard coming against the wind.

Experiments.	Duration at the top of the tower.	Duration at the base of the cliff.	Difference in favor of audition on the tower.
First -----	4½ minutes--	½ minute-----	4 minutes.
Third -----	12 minutes--	5½ minutes-----	6½ minutes.
Fifth -----	12 minutes--	5½ minutes-----	6½ minutes.
Seventh ----	8 minutes--	Not heard	8 minutes.
Eighth ----	6 minutes--	First blast heard, but no other, ½ minute after starting.	5½ minutes.
	<u>42½ minutes--</u>	<u>12 minutes.</u>	<u>30½ minutes.</u>
Average--	8·5 minutes--	2·4 minutes-----	6·1 minutes.

Sound heard coming with the wind.

Experiments.	Duration at the top of the tower.	Duration at the base of the cliff.	Difference in favor of audition at base of cliff.
Second ---	15 minutes--	34 minutes-----	19 minutes.
Fourth ---	15 minutes--	11 minutes-----	— 4 minutes.
Sixth ---	13 minutes--	13 minutes-----	0 minutes.
Ninth ---	16 minutes--	15 minutes-----	— 1 minute.
	<u>59 minutes--</u>	<u>73 minutes-----</u>	<u>14 minutes.</u>
Average--	14¾ minutes--	18¼ minutes-----	3½ minutes.

From the first of the foregoing tables it appears that the elevation of the observer has a marked effect on the audition of sound moving against the wind ; while from the second, (with one important exception,) it has very little if any effect

on sound moving with the wind. Another experiment relative to the same class of phenomena was made on the 19th of August (see Fig. 4), the wind being S. S. W. Two observers—General Woodruff, and Dr. Welling, starting from the bottom of the cliff immediately below the light-house, went along the beach, the one in the direction *A f*, and the other in direction *A e*. General Woodruff found that the sound of the siren was distinctly heard all the way to the breakwater, and was so loud that it probably could have been heard for several miles in that direction. Dr. Welling—on the contrary, entirely lost the sound within a quarter of a mile of the light-house. This result is readily explained as a case of lateral refraction; the wind was in the direction traversed by General Woodruff, and contrary to that pursued by Dr. Welling. In the one case the wind—retarded by the surface of the cliff, moved with less velocity than it did farther out, and consequently the sound was thrown against the face of the cliff, and on the ear of the observer, and in the other thrown from it, thus leaving as it were a vacuum of sound. The effect in this case was very striking, since the siren was pointed toward the zenith, and the sound in still air could have been heard for miles in every direction.

Investigations as to the Effect of Wind on Audibility.—These observations were made by the aid of two steamers. Captain Walker, naval secretary of the board, having completed a series of inspections in the third district, sent the steamer *Putnam*, under Captain Fields, to aid the *Mistletoe* in the investigations. They were commenced on the 9th of August, at 12 o'clock. The wind was S. S. W. with a velocity of $7\frac{1}{4}$ miles per hour. Barometer, 30.3 inches; thermometer, dry bulb, 74° F.; wet bulb, 71.5° F.

The two steamers started from a buoy near the north end of the island, the one steaming against the wind, and the other with it, each blowing its whistle every minute. The distance travelled by each steamer was estimated by the running time, which from previous observations was found to be ten miles per hour. Each vessel was furnished with a whistle of the same size, of 6 inches diameter, actuated by

the same pressure of 20 pounds of steam, and which by previous comparison—had been found to give sound at this pressure of the same penetrating power. The observations on the *Mistletoe* were made by General Woodruff, and on the *Putnam* by Dr. Welling, each assisted by the officers of the respective vessels. The two steamers proceeded to the buoy,—off the north end of the island, in which position the wind was unobstructed by the land—a low beach. Indeed, the island being entirely destitute of trees, and consisting of a rolling surface, the wind had full sweep over it in every direction.

First experiment.—The *Putnam* went against the wind and the *Mistletoe* in the opposite direction. The *Putnam* lost the sound of the whistle of the *Mistletoe* in two minutes and stopped, but continued to blow the whistle. The *Mistletoe* continued on her course and heard the *Putnam's* whistle for twenty minutes in all. During the first two minutes both vessels were in motion, and therefore the space through which the sound was heard moving against the wind—would be represented by 4, while the space through which the sound was heard moving with the wind—would be represented by $20 + 2 = (22)$, the ratio being $1:5\frac{1}{2}$.

Second experiment.—In this, the *Putnam* went with the wind and the *Mistletoe* in the opposite direction. The *Mistletoe* lost the sound of the *Putnam's* whistle in two minutes. The *Putnam* then stopped and remained at rest, while the *Mistletoe* continued on her course until the *Putnam* lost sound of her whistle, twenty-six minutes later. As both steamers were separating during the first two minutes with equal speed, the distance travelled by the sound heard moving against the wind is represented by 4, while the distance of the sound heard with the wind is represented by $26 + 2 = 28$, the ratio being $1:7$. It should be mentioned however that the notes in this experiment are defective and somewhat discrepant.

Third experiment.—The *Putnam* went against the wind, the *Mistletoe* in the opposite direction. The *Putnam* lost the sound of the whistle of the *Mistletoe* in two minutes,

while the *Mistletoe* continued to hear the whistle of the *Putnam* ten minutes longer. Owing to a mis-understanding, one of the steamers stopped for two minutes and then resumed its course. As both steamers were separating during the first two minutes with equal speed, the distance of the sound heard moving against the wind is represented by 4, while the sound was heard with the wind through a space denoted by $2 \times 10 + 4 - 2 = 22$, the ratio being $1 : 5\frac{1}{2}$.

Fourth experiment.—The vessels again changed directions, the *Putnam* going with the wind and the *Mistletoe* in the opposite direction. The *Mistletoe* lost the sound in two minutes, and the *Putnam* nine minutes later. As each steamer was moving from the other at the same rate, the distance of the sound heard moving against the wind would be represented by 4, while the distance of the sound moving with the wind would be represented by $9 \times 2 + 4 = 22$, the ratio being again $1 : 5\frac{1}{2}$.

Fifth experiment.—This experiment was made August 10, by the same vessels and same observers: wind W. S. W., of about the same intensity as on previous days; barometer 30.1 ins.; dry bulb 74° F., wet bulb 69°. The *Putnam* steamed against the wind, and the *Mistletoe* in the opposite direction. The *Putnam* lost the sound in two minutes, and the *Mistletoe* nine minutes later. The two vessels moving apart with equal velocity, the space traversed by the sound moving against the wind was represented by 4, while that in the opposite direction was represented by 22 viz., $9 \times 2 + 4 = 22$.

Sixth experiment.—The vessels were next separated in a direction at right angles to the wind, when each lost the sound of the other on an average of six minutes, giving a distance travelled by the sound (while audible) of 12 spaces.

Seventh experiment.—The vessels were next directed along an intermediate course between the direction of the wind and a line at right angles to it with the following results: The *Mistletoe*, against the wind, lost the sound in about two minutes, while the *Putnam* heard the sound seven minutes longer. As in the previous case, the two vessels moving

apart with equal velocity would in two minutes be separated by a space represented by 4, which would indicate the audibility of the sound moving against the wind, and for the same reason the other vessel, hearing the sound seven minutes longer, would have the additional space represented by 14, and adding to this four spaces, we have 18 to represent the audibility of the sound in the direction approximating that of the wind.

The following table exhibits at one view the results of the foregoing experiments, which relate to sound moving against the wind and with the wind, reduced to miles:

Experiment.	Sound with the wind.	Sound against the wind.
	<i>Miles.</i>	<i>Miles.</i>
1-----	3.66	0.66
2-----	4.66	0.66
3-----	3.66	0.66
4-----	3.66	0.66
5-----	3.66	0.66

These results are in accordance with those of all the direct observations on the effect of wind on sound, which had previously been made by the Light-House Board, with the exception of those at Sandy Hook in September, 1874, as given in the last report, in which the sound was heard from a steamer farther against the wind than in the direction of the wind. This anomaly was explained by the existence of an upper current of air moving in an opposite direction to that at the surface, in accordance with the hypothesis of the refraction of sound.

It will be observed that four of the experiments give exactly the same distances to represent the audibility of sound with and against the wind. This co-incidence was not observed until after the notes were collated for discussion, and (if not accidental) was due to the equal velocity of the wind, and the general conditions of the atmosphere on the two days.

To give a definite idea of these relations, we have plotted

the results obtained on August 10, in Fig. 5, converting the distances into miles, referring them to a common centre, and tracing through the several extremities of the lines representing the distances—a continuous line, which may be designated as the curve of audibility. C being the centre to which the sounds are referred, CA represents the distance at which the sound was heard against the wind, and CB , in the direction of the wind, while CE and CD represent the distance at right angles to the wind, and CF and CG the distances respectively with and against the wind on an intermediate course.

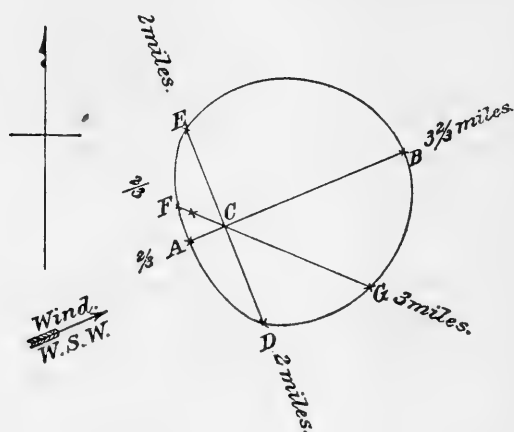


FIG. 5.

The curve which is presented in the foregoing figure may be considered as that which represents the normal limit of audibility during the two days in which the experiments were made. The line DE divides the plane of the curve into two unequal portions, $DAFE$ and $DGBE$, the former representing the audibility of sound moving against the wind, and the other the audibility of sound moving with the wind.

We can scarcely think that any other condition of the air than that of its motion could produce a result of this kind. It exhibits clearly the fact that sound is not heard as a general rule at right angles to the wind farther than with the

wind, as has been asserted. In this case the ratio of the latter to the former is as 11 to 6, or nearly double.

The investigation of the relation of wind to the penetration of sound was renewed in a series of subsequent experiments, the results of which are to be given in a succeeding part of this report.

It should be observed, in comparing Fig. 5 with the subsequent figures representing the curve of audibility, that the arrow representing the direction of the wind points in the longest direction to the figure, whereas in other figures the pointing is in the opposite direction. The difference arises from the fact that in Fig. 5 the sound is supposed to radiate from the centre, *C*, while in the others the sound converges to the centre as a point of observation. The foregoing diagram and all that follow in this report were plotted by Mr. Edward Woodruff, assistant superintendent of construction of the third light-house district.

Experiments at Little Gull Island, September, 1875.

The next series of experiments made during this season was at Little Gull Island, at the east end of Long Island Sound. This location was chosen on account of its convenience of approach from the harbor of New London, seven miles distant, at which the light-house steamers of the third district usually remain when not engaged in active service, and also because there is a light-house on the island furnished with two sirens of the second order, and an extent of water on every side which would allow the vessels used in the experiments to proceed from the island as a centre to a considerable distance in every direction. The island itself is a small protuberance above the water, merely sufficient in area to support a raised circular platform of about 100 feet in diameter, on which the light-house and other buildings are erected. The following sketch (Fig. 6) will give an idea of the position of Little Gull Island relative to the mainland and the islands in the vicinity.

From this it will be seen that the position was not the most favorable for a stable condition of the atmosphere. As

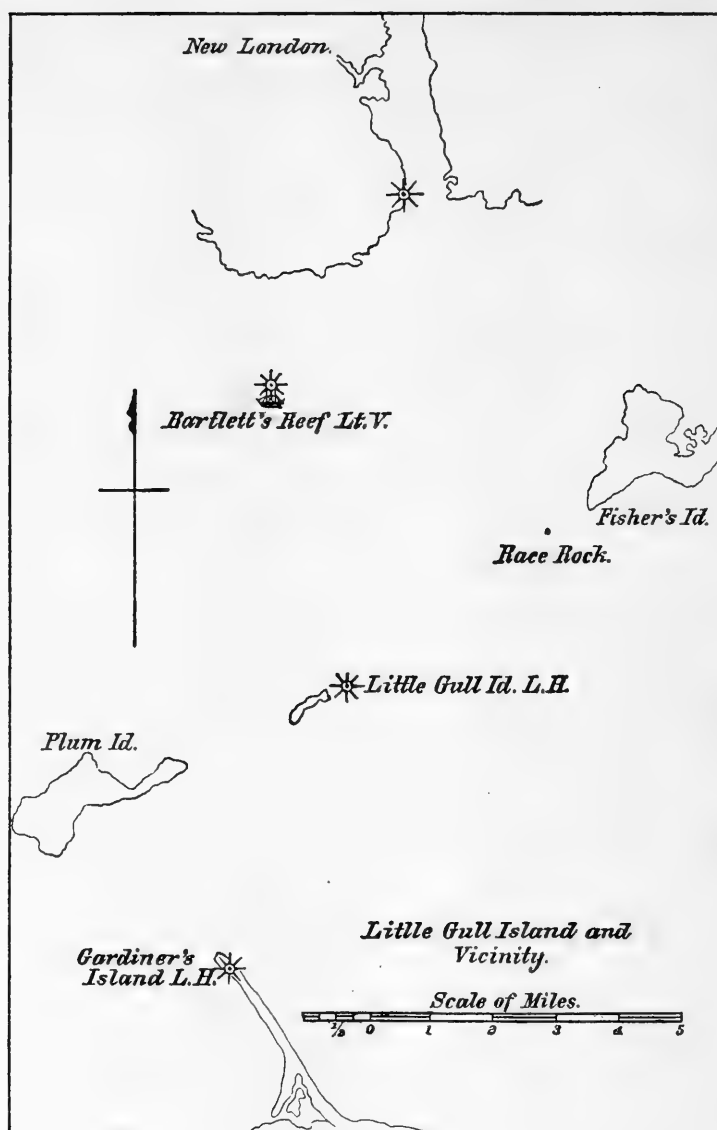


FIG. 6.

the heat of the sun increases during the first part of the day, the temperature of the land rises above that of the sea; and this excess of temperature produces upward currents of air,

disturbing the general flow of wind both at the surface of the sea and at an elevation above. But although the locality was unfavorable for obtaining results tending to exhibit the effects of broad currents of wind flowing in one direction it had the advantage of offering more varied phenomena than could otherwise have been exhibited. Before commencing the experiments, instructions were given to attach a rotating iron neck to the trumpet of one of the sirens, in order that it might be directed to the zenith, while the other siren remained with its axis in a horizontal direction. The observers in these investigations consisted of the chairman of the Board; General Woodruff, engineer of the third district; Mr. Porter Barnard, assistant superintendent of construction; Captain Keeney, and other officers of the *Mistletoe*; with an assistant who acted as one of the observers and recording clerk. The *Mistletoe* was daily employed, though on two occasions the *Cactus*—another of the light-house steamers, rendered assistance.

Observations on the Echo.—The first observations to be mentioned are those relating to the echo; the results however in regard to this are not very satisfactory. The sirens were of the second order, and therefore the echoes produced were not so distinct as those from the larger instrument at Block Island. The echo from the horizontal trumpet was distinct, and in the prolongation of its axis; the interval however between the blast of the siren trumpet and the commencement of the echo was very brief; so short indeed that the ending of the one and the beginning of the other were generally difficult to distinguish. A slight break in the apparatus of the siren produced a continuous hum, which interfered somewhat with the distinct appreciation of the sound of the echo. The keeper thought the weather was not favorable for the production of echoes. He thinks they are heard most distinctly during a perfect calm, which did not occur during the course of these investigations.

The axis of the siren with the movable trumpet being directed to the zenith, strict attention was given by all the observers to any echo which might be produced from it; but in this case, as in that at Block Island, the slight echo

which was heard came from all points of the horizon. On one occasion General Woodruff called attention to a small cloud passing directly over the zenith, from which a few drops of rain fell upon the platform on which the light-house is erected. Advantage was taken of this occurrence to direct strong blasts of the siren toward the cloud, but no perceptible echo was returned. We have failed therefore in this series of investigations to obtain any positive facts in addition to those already known as to the character of the echo. In regard to the hypothesis offered for its explanation, if we found little in its support, we have met with nothing to invalidate it. But whatever may be the cause of the phenomenon, we do not consider it an important factor in explanation of the results we have obtained, since it was too feeble to produce any effect in the way of absorbing any notable part of the original sound. Its importance from Dr. Tyndall's point of view is its apparent support of the hypothesis of a flocculent condition of the atmosphere.

Observations on Effect of Elevation on Audibility.—The next class of experiments at Little Gull Island had relation to the effect of elevation on sound. The conditions here however for arriving at definite results on this point were by no means so favorable as those at Block Island. The height which could be commanded was only that of the tower of the light-house, the gallery of which is 74 feet above the platform upon which the buildings are erected, and 92 feet above the level of the sea,—much less than that at Block Island. Besides this, the variableness of the wind at the surface of the ocean and at heights above was not favorable for the illustration of the point in question.

The theoretical conditions in order that the sound may be heard with greater distinctness at an elevation than below, are (as we have said before,) that the wind be moving with a greater velocity in a given direction at an elevation than at the surface of the earth, and that the difference in the velocities may be against the sound-wave, so that its upper part may be more retarded than the lower. In this case the direction of a beam of sound will be curved upward, leaving as it were a vacuum of sound beneath. The distance of the

origin of sound however must not be too great relatively to the elevation of the observer; otherwise it will pass over his head, as well as over that of the observer at the surface of the earth. In most instances the sound was not continuous, but was interrupted,—heard for a time, then lost; again becoming audible, it was heard until finally lost. Besides this, it was difficult to determine when the sound ceased to be heard, since this depended on the sensibility of the ear and the greater or less attention of the observer at the time of the observation. To obviate these difficulties, as well as the unfavorable condition of too great a distance of the origin of sound from the observer, it was concluded to adopt as the duration of the sound the elapsed time between its beginning and the period when it was first lost.

The observer on the tower was Mr. P. Barnard, while the one below was General Woodruff. From the records of the observations of these gentlemen the following tables are compiled, the first of which indicates the relative duration of sound on the top of the tower and at the bottom,—the sound moving against the wind; the second the same duration, the sound moving with the wind; and the third, the same with the sound at right angles to it.

TABLE 1.—*Sound against the wind.*

Date.		Heard at top of tower.	Heard at foot of tower.
1875.		<i>min. sec.</i>	<i>min. sec.</i>
September 2	-----	5 30	4 00
4	-----	4 30	3 30
4	-----	5 30	3 00
4	-----	5 00	4 00
6	-----	7 00	2 15
6	-----	4 00	3 00
7	-----	5 00	2 15
8	-----	6 00	4 00
8	-----	5 30	3 45
8	-----	3 30	2 15
8	-----	3 00	1 15
Mean	-----	4 57	3 01

It appears from Table 1 that without a single exception the duration of the sound was greater at the top of the tower than at the bottom, although the difference in favor of the top of the tower in the several experiments is very variable. These results are in accordance with what was anticipated.

TABLE 2.—*Sound with the wind.*

Date.	Heard at top of tower.	Heard at foot of tower.
1875.	<i>min. sec.</i>	<i>min. sec.</i>
September 2-----	30 00	30 00
3-----	16 30	18 00
4-----	21 00	20 30
4-----	18 00	23 30
6-----	12 30	12 30
7-----	6 30	5 30
Mean -----	17 25	18 20

TABLE 3.—*Sound nearly at right angles to the wind.*

Date.	Heard at top of tower.	Heard at foot of tower.
1875.	<i>min. sec.</i>	<i>min. sec.</i>
September 2-----	6 00	4 00
2-----	6 45	10 00
2-----	25 00	23 00
2-----	16 30	4 00
3-----	21 00	19 15
3-----	16 00	14 30
3-----	23 30	16 45
4-----	19 30	17 30
6-----	6 30	5 30
7-----	5 00	6 45
7-----	12 00	12 30
8-----	4 15	3 15
8-----	9 30	5 00
Mean -----	13 12	11 00

In the observations recorded in Table 2 the durations of the sound at the bottom and top of the tower are nearly the same, from which we might infer that the elevation of the observer has little effect on the hearing of sound moving with

the wind. Were it not for the result of the first experiment of this class at Block Island, we should not hesitate to adopt this as a general conclusion.

From the general mean of observations given in Table 3, it would appear that the sound moving at right angles to the wind can be heard better at an elevation than at the surface,—a result not anticipated.

Observations on Effect of Wind on Sound.—This series was commenced on the 2d of September. Barometer, 30·3 inches; thermometer, dry bulb, 70·5° F., wet bulb, 67·5°; wind at the surface of sea six miles per hour, and variable: at 3 P. M. the velocity was eight miles at the surface. (See Fig. 7.)

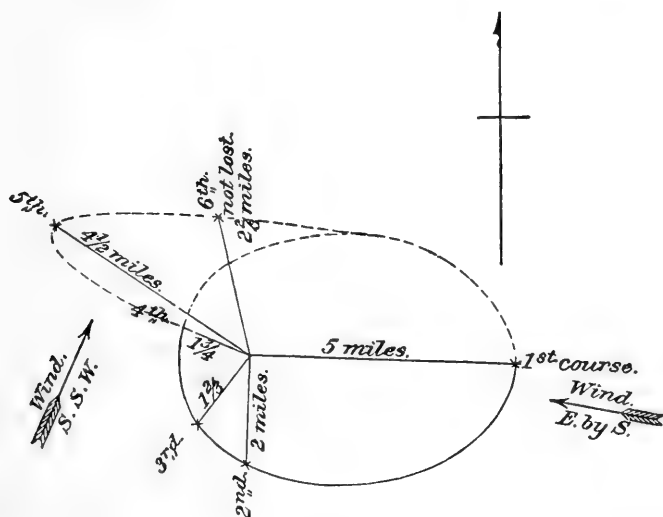


FIG. 7.

The experiments were made by means of the steamer *Mistletoe*, which proceeded from the light-house, as a centre, in different directions, blowing the whistle every half-minute, and returning at a signal, when the sound was lost; the time being noted by different observers, and the distance estimated by the position of the steamer in reference to known objects on the Coast Survey chart, as well as by

angles of azimuth and time of sailing. The steamer was directed to proceed, as indicated in Fig. 7, 1st, against the wind, so that the sound would come to the observers with the wind; 2d, at right angles to the wind; 3d, in an intermediate direction between the last course and the direction of the wind; 4th, approximately with the wind, so that the sound would come to the ears of the observers against the wind; 5th, in an intermediate direction; and, 6th, again at right angles to the wind. It was supposed that by this arrangement a symmetrical curve of sound would be obtained; and we think this would have been the case had the wind remained constant in direction. It did remain nearly the same during the time of describing the first, second, and third courses, and only slightly varied during the fourth; but previous to running the fifth and sixth courses the wind had changed to a direction nearly at right angles to its first course.

As is shown in Fig. 7, the first, second, third, and fourth courses form a normal curve of audition; the fifth and sixth courses however give discordant results, being much longer than a symmetrical curve would indicate, showing a change in the condition of the medium from that which existed during the running of the other courses; this change was evidently that of the wind, which veering (as above stated) through an arc of a little more than 90° , brought it nearly at right angles to the fifth course, and approximately in the direction of the sixth course; the wind also increased its velocity. These changes are sufficient, without other considerations, to give a rational account of the phenomena observed. They both tend to increase the distance at which the sound would be heard.

In these experiments, as in subsequent ones, it is to be regretted that for want of balloons the motion of the air above could not be ascertained, as was done at Sandy Hook in September, 1874. Previous to sailing from the depot at Staten Island attempts had been made to secure a supply of toy balloons, but none could be found at that time in the city of New York. Arrangements were therefore made for procuring a reservoir of condensed hydrogen, by which India-rub-

ber balloons could be inflated at the time they were wanted. Unfortunately this apparatus did not arrive in time to be of much avail in this series of experiments. Besides this, on account of the smallness of the balloons, the ascent was too slow compared to the horizontal motion to indicate the direction of the wind at a considerable elevation above the points of observation. They were however of use in pointing out definitely the direction of the wind and the changes it was undergoing. Moreover, at the time of leaving New York we were able to procure only one anemometer, whereas we ought to have had a number, one for the top of the tower, one for the bottom, and one for each vessel.

Experiments of September 3.—Barometer, 30.02 inches; thermometer, dry bulb, 72.5° F.; wet bulb, 70°; wind from the east, but too slight to move the cups of the anemometer; it soon however sprang up from the opposite direction, in which it continued during the remainder of the day, attaining a velocity of five and a quarter miles per hour.

In these experiments two light-house steamers were employed, the *Mistletoe* and *Cactus*, which enabled us to obtain the results in half the time, and thus to obviate in some degree the effect of any change in the direction of the wind. On this occasion the sound was noted at the light-house as it converged to a centre from the whistle of each vessel, and also simultaneously by each vessel as it diverged from the vertical siren.

We were enabled in this way to produce two curves by a reverse process. These are plotted in Fig. 8, and exhibit a remarkable degree of similarity. The corresponding parts of the two curves, being in each case reversed, exhibit the fact that through the same space in opposite directions the audibility of the sound was similarly increased with the wind and diminished against it. The effect however of the wind in the experiments of this day was less marked than on any in the whole series, and consequently the two curves of audition more nearly approximate circles.

We can see in this result no other effect than that which would be produced from a wind flowing with a uniform but

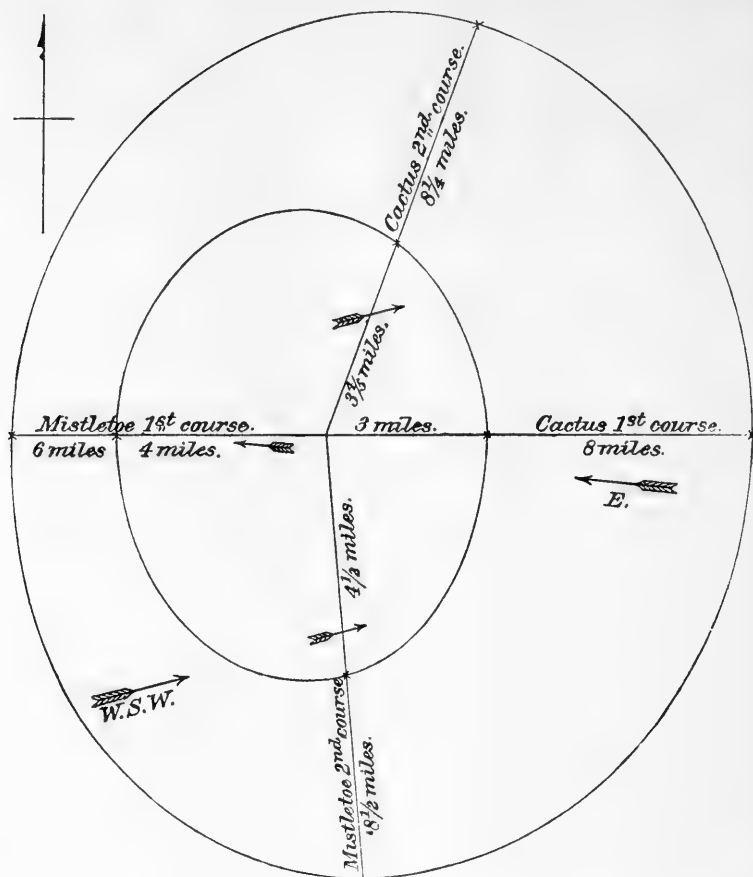


FIG. 8.

slow velocity at the surface, while having a slightly increased velocity above. Had there been no wind, according to this view the two curves would have exhibited two concentric circles.

Experiments of September 4.—Barometer, 29.85 inches, falling; thermometer, dry bulb, 77° F.; wet bulb, 73.25°. Wind south by west, twelve and one-fourth miles per hour at the top of the tower, and nine and one-fourth miles at the bottom; variable.

These experiments were also made with two vessels. The distances and directions are given in Fig. 9. With the exception of the fourth course of the *Cactus* the other courses would form nearly a symmetrical curve, but in this case the sound of the whistle of the *Cactus* was lost at the point *a* at a distance of one mile, and was afterward regained at the point *b*, and continued audible until the steamer reached the point *c*.

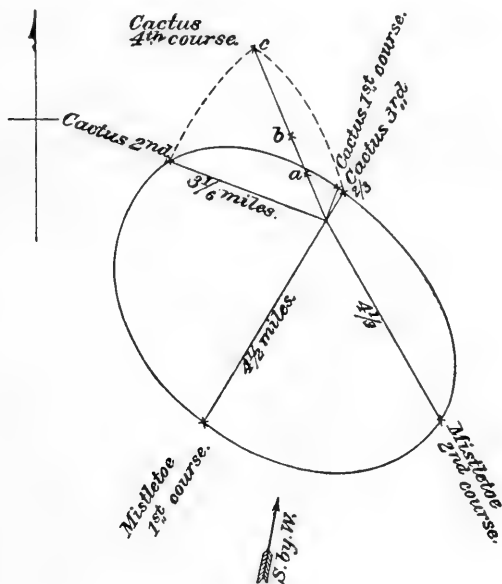


FIG. 9.

This presents one of the abnormal phenomena of sound which might in part be accounted for by the existence of a flocculent cloud between a and b , but why the sound could be heard so much farther in this direction than in the others is not easy to explain on that hypothesis.

The line *b c* was described after all the lines of Fig. 9 had been completed, and therefore the curve given in the figure correctly represents the boundary of the area of audition while these courses were being run, the point *a* being the termination under that condition of the fourth course of

though of higher velocity than on any other occasion, was variable. On this day the experiments were principally made with the *Mistletoe*. The *Cactus* (being obliged to leave on other duty) ran one course a distance of two-thirds of a mile before the sound of her whistle was lost at the light-house. She afterwards steamed off in the direction *cb* (Fig. 10), noting the sound of the siren, which was lost at the point *b*, afterward regained, and heard distinctly ten and one-half miles distant.

During the passage of the first course of the *Mistletoe*, the wind at the surface and above was from south-west, the latter being indicated by a cloud passing the zenith. During the second course the wind was variable, changing its direction about 90° , principally from the north-west; while during the third course the wind was again from the south-west. The long course of the *Cactus* marked on the figure indicates the sound of the siren from the centre outward, as it was heard seven and one-fourth miles, then lost for an interval, and afterward heard again at a distance of three and one-fourth miles farther, making in all ten and one-half miles.

Experiments of September 7.—Barometer, 30.1 inches; ther-

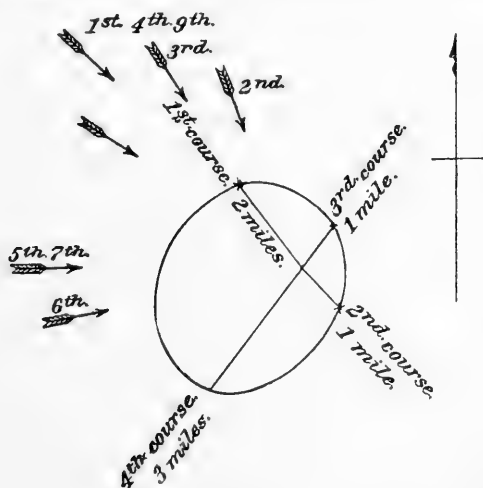


FIG. 11.

mometer, dry bulb, 73° F.; wet bulb, 62° ; wind eight miles

per hour at top of tower, and five miles per hour below. The wind was variable, as indicated by the letting-off of balloons, which however did not rise to any great height. The direction of the wind is shown in Fig. 11 by arrows. There is nothing remarkable in the curve of audition of this day. It indicates as usual a greater distance toward the side on which the sound was moving with the wind.

Experiments of September 8.—Barometer, 30.3 inches; thermometer, dry bulb, 70° F., wet bulb, 64.5°: wind, west-south-west, fifteen miles per hour at top of tower, nine miles per

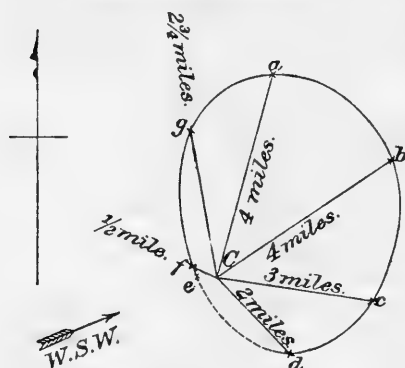


FIG. 12.

hour below. Fig. 12 indicates the curve of audition of the vertical siren as compared with that of the horizontal siren. The steamer first proceeded along the line *Ca* nearly in the direction of the axis of the horizontal trumpet. For the distance of the first three miles the horizontal trumpet was the louder. At the point *a*, four miles

distant, the two were distinct and very nearly equal. At *b* they were distinct, also very nearly equal, the vertical perhaps a little more distinct. At *c* very nearly equally distinct. At *d* the vertical siren was decidedly more distinct just before entering the optical shadow of the light-house tower and the keeper's dwelling. This shadow continued to the point *e*, which was nearly the extent of the acoustic as well as of the optical shadow, since from *d* to *e* the sound was heard from neither instrument, and the origin of sound was too near to cause much difference between these two shadows. From *f* to *a*, through the point *g*, the two instruments continued to be fully heard—the vertical the more distinct. The effect of the wind in this figure is also very distinctly marked, the longer lines indicating the distance the sound was heard with the wind, and the shorter against

it. The curve of this figure is not traced through points at which the sound was absolutely lost, but at which it was heard feebly and with nearly equal distinctness.

Thus far all the facts we have observed, if not in strict conformity with our conception of the hypothesis of Professor Stokes, are at least not incompatible with it. We are now however to direct attention to a fact of much interest, which may not have escaped the attention of the reader; namely the remarkable difference in the area of audition as exhibited in the several figures, all drawn to the same scale. If we compare for example the curve of Fig. 10 with the inner curve of Fig. 8, it might at first sight be inferred that the smallness of the curve in the former case was due to a mottled condition of the atmosphere, which by absorbing the sound—diminished the sphere of audition; but unfortunately for this explanation, it would appear from the observations made by the *Cactus* within the hour of obtaining the data for describing the curve, that the air was then in a remarkably favorable condition for the transmission of sound, since on the south-east course of the steamer (as shown in Fig. 10), the siren was heard ten and a half miles, the ordinary limit of the maximum penetrating power of this instrument—a siren of the second order; while on the 3d of September, the day on which the large curve, Fig. 8, was described, the greatest distance at which the sound of the same instrument could be heard was eight and a half miles.

The only difference in the condition of the air observed during the time of describing the curve of audition given in the figure, and the hearing of the sound by the *Cactus* for ten and a half miles, was a change in the direction (and perhaps in the intensity) of the wind, in the latter case the direction being the same as that of the course of the *Cactus*.

Before therefore admitting any other solution of the question as to the cause of the difference in the area of audition, we must inquire whether it is not possible to refer it to the action of the wind itself.

The most marked difference in the conditions which apparently affected the phenomenon on the days in question

was that of the greater velocity of the wind, both at the surface of the sea and at the top of the tower, and by comparing the several figures in regard to the wind it will be seen that where the condition of the air was nearest that of a calm the larger was the curve of audition, and the nearer the figures approach to a circle, of which the point of origin of sound (or the point of perception) is the centre. From these facts we are inclined to think that sound is not heard as far during a time of high wind in any direction as it is during a perfect calm, and that it is heard farthest with a gentle wind. This conclusion, which was not anticipated at the beginning of these investigations, is we think in strict conformity with the hypothesis adopted. In the case of sound moving against a strong wind, the sonorous waves being thrown up above the ears of the observer, the sphere of audition in that direction is without question greatly diminished; and that it should also be diminished when sound is moving with a strong wind having a greater velocity above than below is not difficult to conceive. In this case the sound-wave will be so thrown down against the earth, and so much of it absorbed, as to weaken the intensity of that part which reaches the ear, while in the case of a feeble wind moving faster above than below, the portion of the wave thrown down from above will only be sufficient to compensate for the smaller loss by friction, and thus the sound may be heard at a greater distance than in still air. But on this point, as well on as others, further experiments are required.

While we consider the wind as the principal agent in producing the abnormal phenomena of sound, we do not by any means regard it as the sole agent. Prof. Osborne Reynolds, of Owens College, Manchester, England, without any knowledge of investigations in America on this subject, instituted a series of experiments on the effect of wind upon sound, and finally adopted precisely the same hypothesis which we have used for generalizing the observed phenomena. He has however, in a very ingenious and important paper presented to the Royal Society in 1874, extended the same principle to the effect of heat in changing the form of the sound-wave, and has shown—both by reasoning and experiment, that the

normal direction of the sound-wave in still air, instead of proceeding horizontally, should be turned upward on account of the greater velocity of sound near the earth, due to the greater heat of the strata in that position than of those above. This principle, which indicates the existence of a true refraction of sound independent of the motion of the medium, is undoubtedly applicable as a modifying influence to the phenomena we have recorded. It produces however only a slight effect in the case we have last mentioned, since the observation on board the *Cactus* shows the condition of the air was that of little acoustic absorption. It would nevertheless favor the hypothesis that sound in perfectly still air of homogeneous density could be heard farther than sound in a moving medium, or in one of unequal temperature. This is also in accordance with the fact repeatedly observed in arctic regions, in which the sound of the human voice is heard at great distances during times of extreme cold. In this case the air is of a uniform temperature above and below, but of diminished elasticity, and should on this account transmit sound with less intensity; and yet the audibility is increased, which is explained by the assumption that its stillness and uniformity of temperature more than compensate for the diminished elasticity. The same may be said with regard to the audibility of sound during a fog, which usually exists during extreme stillness of the air.

Whatever be the cause of the variation in the limit of audition as exhibited in the diagrams, it is less efficient than the ordinary action of the wind in producing the same phenomena. This is evident from the fact that while the ratio of the extreme variation in the limits of audition in the first case is not more than 1 : 3, in the second it is that of 1 : 5.

Moreover, when the effect of the wind on the audition of sound in relation to elevation is considered, we think we are fully warranted in asserting, as we did in our last report, that the wind is a more efficient cause of the variable penetration of sound than the invisible acoustic clouds postulated by Professor Tyndall for the explanation of the phenomena.

The object of these investigations, as stated at the beginning of this report, was to obtain facts which might serve to

establish the true theory of the abnormal phenomena of sound; an object (independent of its scientific interest) of much practical importance in its application to fog-signals. Although the observations were not as perfect as we could wish in many respects, (from want of certain appliances,) they are yet sufficient we think to establish principles of much practical value. For example, if the mariner in approaching a fog-signal—while the wind is blowing against the sound, fails to perceive it on deck, he will probably hear it by ascending to the mast-head; or in case a sound from a given station is constantly obscured in a certain direction, while it is audible in adjacent directions, we may attribute it to a sound-shadow produced by some interposed object. If again the obscuration of sound in a given direction is only observed during a wind moving against the sound, the cause will probably be found in a lateral refraction, due to the retardation of the current of wind against a perpendicular wall or cliff, as in the case observed at Block Island August 19. The subject is indeed one of great complexity, and requires further investigation; but the results thus far obtained may be considered as furnishing the preliminary data on which to found more precise observations. These should be made with the aid of a number of steamers simultaneously employed, each furnished with anemometers and balloons for determining with more accuracy the direction and velocity of the wind.

We hope to renew the investigations during next summer, and in view of this, have directed that in the meantime the light-keepers at Block Island and at Point Judith shall continue to sound their sirens a certain length of time every Monday, noting the direction and velocity of the wind, the temperature and pressure of the air, and the audibility of the sound as it comes reciprocally from each instrument.

It is shown from the results thus far obtained from these reciprocal observations that sound is occasionally heard more distinctly against the wind than in a contrary direction. We think however that these instances are generally followed by a change in the direction of the wind at the surface of the earth

PART V.—INVESTIGATIONS IN 1877.

(Report of the United States Light-House Board for 1877, pp. 61-72.)

On account of the occurrence of the Centennial Exhibition at Philadelphia, which absorbed most of the time of the officers of the Light-House Board not devoted to ordinary light-house service, but few observations were made relative to sound in 1876, and an account of what were made is incorporated in the following report.

Agreeably to previous engagement I visited Portland, Me., to make some further investigation in regard to the abnormal phenomenon of sound noticed in a former report. We left Portland on the afternoon of September 3, 1877, in the steamer *Iris*, which had been fitted up during the year under the direction of the inspector, Commander H. F. Picking, and was in excellent condition, and well adapted to the duty of a light-house tender. The party consisted of General J. C. Duane, engineer of the first district; Commander H. F. Picking, inspector of the first district; Mr. Edward L. Woodruff, assistant engineer of the third district; Mr. Charles Edwards, assistant engineer of the first district, and myself.

Old Anthony Station.—We first examined one of the automatic whistling-buoys invented by Mr. Courtenay, of New York. This was in place and emitting sounds at a station called Old Anthony,—off Cape Elizabeth, about nine miles from Portland. On approaching it at right angles to the direction of the wind, we heard it at the distance of a mile. But the sound did not appear loud even within a few rods. It was however of considerable quantity, being from a locomotive whistle of ten inches in diameter. The instrument is operated by the oscillation of the waves, which at this time were not of sufficient height to move it vertically through a space of more than one foot. It emitted a sound at each oscillation. This invention consists of a large pear-shaped buoy about twelve feet in diameter at the water-surface, floating about twelve feet above the same plane. In the interior of this

buoy is a large tube or hollow cylinder three feet in diameter, extending from the top through the bottom to a depth of about thirty feet below the latter. This tube is open at the bottom, but projects air-tight through the upper part of the buoy, and is closed with a plate having three orifices in it, two for letting in the air into the tube, and one between the others for letting it out to operate the whistle. These orifices are connected with three tubes which extend downward to near the level of the water, where they pass through a diaphragm which divides the cylinder into two parts.

When the buoy rises, the water in the cylinder by its inertia retains its position, and a partial vacuum is formed between the head of the column and the diaphragm, into which the air is drawn through two of the tubes, and when the buoy descends, the escape through the injection tube being prevented by valves, the air is forced out of the inner tube and actuates the whistle.

The mooring-chain, which is sixty fathoms in length, is attached to the cylinder at a point just below the buoy, and is secured to a large stone weighing about six tons. The apparatus rides perpendicularly.

The sound in this instrument is not produced merely by the difference in hydrostatic pressure of the water in the two positions of the buoy, but by the accumulated effect of impulse generated by the motion of the apparatus.

Plans have been devised—but have not yet been perfected, to condense the air in the buoy by the effect of repeated oscillations, until a valve loaded to a definite pressure would open automatically and allow the air to escape. In this way the sound from the accumulated pressure would be produced at intervals to a greater or less extent, and would serve to diversify the character of the sound so as to enable the mariner to distinguish different locations. The invention—even in its present form, is considered a valuable addition to the aids to navigation, has received the unqualified approbation of all navigators on this coast who are acquainted with its operation, and will probably be introduced in all countries where its merits are known. Experience has shown

that it can be permanently moored in deep water, and that vessels can safely approach it within the nearest distance and take perfect departure from it.

The Light-House Board has adopted this buoy as one of its permanent aids to navigation, and will in time introduce it at all points where its presence will be of importance to the navigator. In order to obtain reliable data as to the operations of the automatic buoy, Commander Picking has established a series of observations at all the stations in the neighborhood of the buoys, giving the time of hearing it, the direction of the wind, and the state of the sea; from which it appears that in the month of January, 1877, one of these buoys was heard every day at a station one and one-eighth miles distant; every day but two, at a station two and one-quarter miles distant; fourteen times at a station seven and one-half miles distant, and four times at a station eight and one-half miles distant. It is heard by the pilots of the New York and Boston steamers at distances of from one-fifth to five miles, and has frequently been heard by the inspector of the first light-house district at a distance of nine miles, and even (under the most favorable circumstances) fifteen miles.

We sailed around the buoy and observed the difference in the intensity of sound relatively to the direction of the wind, which was at the time a fresh breeze of from twelve to fifteen miles per hour from the westward;—the greatest intensity being apparently at points forty-five degrees on either side of the axis of the wind. The effect however was not very definitely marked, though the sound on the whole appeared to be greater on the semi-circumference of the circle to the leeward; but the velocity of the wind was so great that the noise produced by it on the rigging of the vessel prevented the effects from being definitely observed.

Experiments have been made with this buoy carrying whistles of different sizes, the result being that a whistle of less than ten inches diameter does not give a sound which can be heard as far as one of the latter size, although when near by, it appears to the ear equally loud.

There is a difference between the quantity of sound and

the loudness. Two sounds may be equally loud when heard near by, yet differ very much in regard to their being heard at a distance, the loudness depending upon the intensity of sound or on the amplitude of vibration of the sounding body, while the quantity of sound depends on the extent of the vibrating surface.

The size of the whistle must be limited by the quantity of air ejected at each oscillation of the buoy. The fact that the ten-inch whistle gives a sound which can be heard farther than one of eight inches, appears to have a bearing on the question of the united effect (the actuating force being the same) of two sounds of the same quantity and pitch, since the sound from several parts of the circumference of the larger whistle may be considered as a union of several sounds of less quantity.

Whitehead Station.—After these observations on the automatic buoy we proceeded along the coast to Whitehead, at the entrance of Penobscot Bay, a distance of sixty miles, which we reached at about twelve o'clock at night, and cast anchor in Seal Harbor, near the Whitehead light-house.

Our first operation next morning was the examination of an automatic fog-bell, invented by Mr. Close, and which has been erected by a special appropriation of Congress. It is very simple in conception, and would do good service in southern latitudes, where it would not be affected by the ice. It consists of an upright shaft thirty-two feet long, fastened to the rock beneath the water and kept in a vertical position by a series of iron rods serving as braces. Around this shaft is a hollow metallic float, having sufficient buoyancy to elevate a vertical rod by the motion of the waves, having at the upper end a rack gearing into a ratchet-wheel. By means of projecting pins on the surface of the wheel, the hammer of the bell is elevated and the bell sounded at each descent of the float. This arrangement is the most simple and efficient of the kind of which we have any knowledge.

The objection to it is its liability to be deranged by the action of ice and the rusting of the parts from exposure to the weather.

Our next operation at this place (the principal object of our excursion) was the examination of the remarkable abnormal phenomenon, which has been frequently observed by the captains of the steamers plying between Boston and New Brunswick, and has also been noticed on two different occasions by officers of the light-house establishment. The phenomenon, as reported by these authorities, consists in hearing the sound distinctly (on approaching the station) at the distance of from six to four miles, then losing it through a space of about three miles, and not hearing it again until within about a quarter of a mile of the instrument, when it becomes suddenly audible in almost full power. This phenomenon is always noticed when the vessel is approaching the signal from the south-west, and the wind is in the same or in a southerly direction, and therefore opposed to the direction of the sound from the station, as is usually the case during a fog. Commander Picking, having frequently received complaints from masters of vessels as to losing the sound at this place, concluded to verify the facts by his own observation. For this purpose, he embraced the opportunity of an inspection-tour in July, 1877, to approach the station from the southwest during a fog. In his own words, he heard the sound distinctly through a space of from six to four miles, then lost it, and could hear nothing until within a quarter of a mile of the station, when the blast of the whistle burst forth in full sound. The wind at this time was from the southward, or against the sound. This cessation in the hearing of the sound could not have been due to the failure of the instrument to emit sound, since its operation is automatic when once started, and in this case the fog so lifted on nearing the station as to admit the observation of the puffs of steam emitted at each blast of the whistle.

On a previous occasion General Duane and Mr. Edwards on approaching the same signal from the south-west heard the sound at about six miles distance, then lost it, and did not again hear it until within about a quarter of a mile. The wind in this instance was also the same as that in the observation of Commander Picking, namely from the south-west.

So well established was the phenomenon, that General Duane attempted to remedy the evil by elevating the duplicate whistle (with which every station is provided) to a height twenty-two feet above the level of the other whistle, by placing it on the upper end of a tube. But this arrangement produced no beneficial effect.

September 4, 1877.—In the morning of September 4, on which we commenced our experiments, the weather was clear, the wind west-southwest, the velocity from ten to twelve miles, remaining nearly constant during the day. Our first object was to verify by direct observation the several features of the phenomenon, and for this purpose we steamed to the southward, or directly to the windward, from the station through the region in which the abnormal phenomena had been noticed. The pressure of the atmosphere, as indicated by an aneroid barometer, was 28.9 inches. The temperature of the air was 67° Fahrenheit; that of the water at various points along our course was 58°, except at two points where the thermometer indicated 57°. This difference was too small to have any perceptible effect on the density of the rapidly moving air which was passing over the surface of the water. As we increased our distance from the signal the sound slightly diminished in loudness until the distance was between a quarter and half a mile, when it suddenly ceased to be heard, and continued inaudible through a distance of about a mile, when it was faintly heard and continued to increase in loudness until we reached the distance of four miles; at this point it was heard with such clearness that the position of the station could be located with facility; but on proceeding farther in the same direction it appeared to diminish gradually except at one point, when a blast, as indicated by the steam issuing from the whistle, was inaudible; but on turning the vessel around the next blast was distinctly heard.

As a second experiment, we retraced the same line back to the station and observed the same phenomena in a reverse order. The sound was heard the loudest at a point four

miles from the station; afterward it diminished and then became inaudible through a space of two miles, and then suddenly burst forth in nearly full intensity at the distance of a quarter of a mile, and continued loud until the station was reached.

As a third experiment, the same line was traversed again, the only difference in the condition of the experiment being that the whistle on the steamer was sounded every minute between the blasts of the signal at the station; and while the observers on the vessel noted the sounds from the latter, those at the station observed the sounds from the former. The same phenomena as described in the previous experiments were witnessed by those on board the vessel, but on receiving the report of the observers at the station it was found that no cessation of the sound from the steamer was observed through the whole distance traversed by the vessel. It should be noted that the whistle at the station is ten inches in diameter, actuated by a pressure of sixty pounds of steam, and that on board the vessel six inches in diameter with twenty-five pounds of steam. It appears from this remarkable result that a feeble sound passes freely through what has been called the region of silence when sent in the direction of the motion of the wind, when a louder sound does not pass in the opposite direction.

As a fourth experiment, the vessel proceeded northward on the side of the station opposite to that before traversed, but in the prolongation of its previous course. The sound from the signal to the observers on the vessel was in this case with the wind, while that from the vessel to the observers at the station, was against the wind. In this experiment no cessation was observed on the vessel in the hearing of the sound from the station; it was heard with varying intensity to the distance of four and a half miles, and could probably have been heard much farther had our progress not been interrupted by land. On returning to the station the observers there reported that after the vessel had left the station, and was scarcely more than a hundred yards distant, not a single blast of its whistle was heard. In this case

the phenomena which had been observed on the southerly side of the station were exhibited in a reverse order on the northerly side.

In what may be considered the fifth experiment, the vessel being at a distance of four miles from the station—on the line traversed in the first two experiments, the sound was slightly heard. The vessel then altered its course so as to steam around the signal, keeping at the same distance until the direction of the station, from the vessel, was nearly at right angles to the direction of the wind; at this point no sound was heard from the station, although it had been slightly heard at points along the curved line traversed in reaching the point mentioned. The vessel then proceeded toward the station in a straight line, but no sound was heard until it approached the latter within a quarter of a mile. The observers at the station however heard the sound from the vessel through the whole distance.

This experiment was made to ascertain the truth of the general impression that at this place the sound is heard better coming at right angles—or across the wind, than in the direction in which it was blowing. The experiment however was found in conformity with the general rule previously established, that the sound was usually heard farthest with the wind, least against the wind, and at an intermediate distance across the wind.

The primary object of these investigations has been to determine the mechanical causes to which the phenomena may be referred, from which new conclusions may be deduced—to be further tested by experiment, and such definite views obtained, as may be of value in the employment of fog-signals for the uses of the mariner.

For this purpose a number of different hypotheses may be provisionally adopted, and each compared with the actual facts observed.

The first hypothesis which had been suggested for the explanation of the phenomena in question was that they are due to some configuration of the land; but on inspecting the Coast-Survey chart of this region it will be seen that the

nearest land consists of a series of broken surfaces not rising above the ocean enough to reflect sound or in any way to produce sound-shadows in the region through which the phenomena are observed. This hypothesis therefore is inadmissible.

Another hypothesis is that of invisible acoustic clouds (as they have been called), or portions of atmosphere existing over the water in the region of silence, which might absorb or variously reflect the sound. That such a condition of a portion of the atmosphere really exists in some cases is a fact which may be inferred from well-established principles of acoustics, as well as from experimental data. They would occur especially in the case of dissolving clouds, which would be accompanied by local diminutions of temperature, and also from portions of air which have been abnormally heated by contact with warm earth. But if the phenomena in question were produced by a cloud of this kind, its presence ought to be indicated by carrying through it the usual set of meteorological instruments. This was done in the foregoing experiments, but no change was observed in the indications either of the thermometer or barometer. Unfortunately we had not a hygrometer in our possession, but this observation was less necessary, since from abundant testimony it is established that the same phenomena are exhibited during a dense fog, in which all parts of the atmosphere for miles in extent must be in a homogeneous condition. Furthermore, a local cloud could not continue to exist in a given space for more than an instant while a wind was blowing with a velocity of from ten to twelve miles an hour. Again, this hypothesis fails entirely to explain the fact that this phenomenon is always observed at nearly the same place, especially during a fog, when the wind is in a southerly direction. Finally, it is impossible to conceive of a cloud so arranged as a screen producing a sound-shadow of greater intensity on one side than on the other.

Another hypothesis is that of the refraction of sound due to the action of the wind. It is an inference from well-

established theory, as well as from direct observation, that the sound is refracted by the wind, that it tends to be thrown upward when moving against the wind, and downward with the wind. This result is attributed very properly to the different velocities of the strata, that next the surface being most retarded, those above being less retarded.

The upper part of the front of the wave is thus thrown backward, and the direction of the wave turned upward. In the case of the experiment south of the station, the wind passing over a long line of rough sea was moving less rapidly in its lower stratum than in the higher, and consequently the sound-wave was thrown backward above, and as it issued from the instrument, tended to rise above the head of the observer, and at a certain distance from the origin of the sound—depending upon the difference of velocity above and below, was lost entirely to the observer, and a sound-shadow was thus produced by refraction, which is either surmounted by an undulating course of the sound beam, or is closed in again by the lateral spread of the sound at a given distance.

In the experiment on the other side of the signal, (the vessel proceeding to the north,) the wind coming to the observer on the vessel—had to pass over a rougher surface than that of water, and consequently the difference of velocities above and below, would cause the refraction to be greater; hence the sound from the vessel was almost entirely lost to the observer at the station, while the sound from the station was heard uninterruptedly on the vessel, since it was moving with the wind.

On examining the records of experiments of previous years, I find a number of cases recorded where sounds were heard at a greater distance, while inaudible at a less distance, especially one in connection with the fog-signal at Gull Island, in 1874. In this case the sound, in passing from the signal, was heard distinctly at the distance of about two miles against the wind, then lost for a space of about four and a half miles, and heard again distinctly for a distance of perhaps one mile. At the same station, during the

experiments of 1875, the sound of the whistles of the steamers was heard for a certain distance, then ceased to be heard for a considerable interval, after which it was heard again. Furthermore the pilots of the steamboats from New York to Boston report that the sound of the automatic buoy is found to be intermittent, being heard at a distance, then becoming inaudible, and heard again as the steamer approaches the source of sound.

From all the facts gathered on this subject, I think it highly probable that in all cases in which sound moving against the wind is thrown up above the head of the observer, it tends to descend by the lateral spread of the sound-wave and to reach the earth again at a distance; the conditions however for the actual production of this effect are somewhat special, and will depend upon the amount of the initial refraction and the quantity of the sound-waves. Besides the lateral spread of the sound-wave there are two other causes sufficient (in certain cases) to bring a portion of the sound-waves which have been elevated in the air—back again to the earth: the first is when an upper current of wind is moving in an opposite or approximately opposite direction to that at the surface of the earth, in which case an opposite or downward refraction would take place; and the second is the case in which the surface-wind is terminated above by a stratum of comparatively still air; in this case also, a reverse refraction (but of less amount) would take place, which would tend to bring the sound-wave downward.

We can readily imagine that a solitary island, cooled by the radiation of heat at night, would every morning send a current of cold air in all directions from its centre. In this case, the sound from a whistle placed in the centre of the island would be inaudible in a space entirely surrounding it, and thus give rise to a condition mentioned by General Duane, in which a fog-signal appeared to be surrounded by a belt of silence.

September 5, 1877.—The next experiment was made on the morning of the 5th, on leaving the station. In this case we proceeded along the direction of the same line in which the

first, second, and third experiments were made on the day before. The wind had changed about four points to the southward. As in the preceding experiments, the sound was lost again at the distance of about one-fourth of a mile, but was not distinctly regained, though some of the observers thought they heard it at a distance of two and one-half miles.

The only perceptible difference in the wind (on the 5th) was that it was a little less rapid, and four points more to the southward.

From a subsequent report of the keepers, the whistle of the vessel was heard continuously as far as the puffs of steam could be observed—a distance of six or seven miles. In this case the sound was moving with the wind. These results therefore are in accordance with those previously obtained.

Monhegan island.—The next experiments were made at Monhegan, an island sixteen miles south-west of Whitehead. On this island there is a Daboll trumpet actuated by a hot-air engine.

We departed from this station in a westerly direction at an angle of 45° to the right of the direction of the wind, and after proceeding about one mile, as estimated by time, we lost the sound of the signal. We then turned at right angles to our former course and proceeded toward the leeward, keeping about the same distance from the signal, when the sound was regained at a point which probably depended upon the direction of the wind and the axis of the trumpet combined. From this point it was heard to a point at the leeward, and thence we retraced our course at about the same distance and proceeded across the axis of the trumpet toward the windward, where the sound was again lost. The only definite result from this experiment was another case of the sound being heard farther to leeward than to the windward.

After this experiment we returned to Portland.

An interesting fact may be mentioned in connection with this station, having a bearing upon the protection of light-houses from lightning. The fog-signal is placed on a small island separated from the large island by a water-space of about one-eighth of a mile. General Duane, desiring to

connect the light-house and fog-signal by an electrical communication, suspended a wire between the two points and attempted to form a ground connection by depositing a plate of metal in the ground on each island, but to his surprise,—though the arrangements were made by a skilled telegrapher, no signal would pass. The two islands being composed of rock and the soil limited in thickness, the conduction was imperfect, and it was only by plunging the plate of metal into the water on each side of the space between the two islands that a signal could be transmitted.

No further experiments on sound were made during this excursion, because the vessel could no longer be spared from more pressing light-house duty in the way of inspection, and transportation of the stated supply of materials to the stations.

On my return to New York, accompanied by Mr. Woodruff, I took the route by the Western railway to the Hudson River at Troy. This line was chosen in order to make some investigations relative to any peculiarities of sound which might be observed in the Hoosac tunnel, through which the railroad passes. For this purpose we spent a day at East Windsor, a village situated near the west end of the tunnel, and were very cordially received by the engineers in charge.

The tunnel is four and three-quarters miles in length, twenty-four feet wide, and twenty feet high to the crown of the arch. It ascends slightly from either end to a point near the centre, where there is a ventilating shaft 1,028 feet high extending to the outer air above. In winter, when the external temperature is less than that within the tunnel, there is a constant current from each end toward the centre, and in the summer, when the temperatures are reversed, there is a current out of the tunnel at either end, except when the external wind is sufficiently strong (especially from the west) to reverse the direction of the current from one half, and direct the stream entirely out of the other entrance. At the time of our visit, there was a gentle current flowing out of both ends.

The only peculiarity of sound which had been observed

(as stated by the engineers,) was that it was greatly stifled immediately after the passage of the locomotive, by the smoke with which the air was filled at the time. So great was this in some cases, that accidents were imminent to the workmen, (who are constantly occupied in the tunnel in lining the crown of the arch with brick,) by the sudden appearance of a locomotive, the approach of which had not been heard.

That the audibility of sound should be diminished by smoke was so contrary to previous conceptions on the subject, (since sound is not practically interrupted by fog, snow, rain, or hail,) I was induced to attribute the effects which had been observed to another cause, and to regard the phenomenon as due to an exaggerated flocculent condition of the air in the tunnel, adopting in this instance the hypothesis advanced by Dr. Tyndall, and so well illustrated by his ingenious experiments. The effect which would be produced in the condition of the air in the tunnel by the passage of a locomotive is indicated by the appearance of the emitted steam extending behind the smoke-stack of a locomotive in rapid progress before the observer at a distance. This consists of a long stream composed of a series of globular masses produced by the successive puffs of steam which are emitted at equal intervals. Allowing the diameter of the driving-wheels to be five feet, then since four puffs are made at each revolution of the wheels, a puff of hot steam would be given out at every four feet travelled by the engine, and these puffs mingling with the air at the ordinary temperature would produce an exaggerated flocculent condition. On our expressing a desire to witness the effect upon sound of the passage of a locomotive through the tunnel, Mr. A. W. Locke, one of the engineers who had charge of the western section, politely offered us the means of experimenting on this point, and also of passing leisurely through the tunnel on a hand-car.

To observe the effect of a locomotive on the sound, we took advantage of the entrance of a freight train impelled by two engines; the extra one being necessary to drive the load up the inclined plane to the middle of the tunnel, where it was de-

tached and returned along the same line, while the train was drawn the remaining distance along the eastern decline by a single engine. In order to make the experiment with regard to sound, the time was accurately noted during which the noise of the entering engines could be distinctly heard, which would give approximately the distance the sound travelled through the flocculent atmosphere produced by the locomotive before becoming inaudible, and again the time was noted from the first hearing of the returning engine until it reached the end of the tunnel. In the meantime the current of air blowing through the tunnel had removed a considerable portion at least—of the flocculent atmosphere, so that the sound in this case came through an atmosphere of comparatively uniform temperature, or one much less flocculent than the other; the result was that the duration of sound in the first case was about a minute, while in the second it was upward of two minutes. The darkness in the tunnel—on account of the smoke, was so profound—immediately after the passage of a locomotive, that with two large torches, charged with mineral oil, the sides of the tunnel at a distance of six feet could scarcely be observed; while in the other half of the tunnel, where no smoke existed, the eastern opening could be observed like a star at the distance of upward of two miles. It was therefore not surprising that the stifling of the sound which was observed should be referred to the smoke as a palpable cause, and that the more efficient one of the varying density or flocculent condition should be disregarded.

The method of determining by experiment the question as to which of these causes was the efficient one did not occur to me until we had left the tunnel, and then the simple expedient suggested itself to me for the purpose of repeating the experiment, that instead of locomotives charged with wood, two locomotives charged with charcoal or coke—which emits no smoke, but only transparent gases, principally carbonic acid,—should be used in an experiment similar to the one just described. This experiment Mr. Locke has kindly promised to perform as soon as it can conveniently be arranged.

The opportunity was embraced while at the mouth of the tunnel to make some observations which might have a bearing upon the phenomena of the aerial echo. For this purpose advantage was taken of a large tool-chest which happened to be placed about twenty or thirty feet within the western mouth of the tunnel. By slamming down violently the cover of this chest, a loud sound of an explosive character was produced, from which a prolonged echo was returned from the interior of the tunnel. This echo was slightly intermittent, suddenly increasing in loudness at intervals for a moment, and again resuming its uniform intensity. This effect was attributed to projecting pieces of rock in that part of the tunnel which had not been lined with brick. An echo was however evidently returned from that portion of which the sides were not projecting, which I would consider an effect of the same cause which produces the aerial echo.

Aerial Echoes.

During the year 1877 (as also in 1876) series of experiments were made on the aerial echo, in which I was assisted in the first series by General Woodruff, engineer of the third light-house district, and in the second series by Edward Woodruff, assistant engineer of the same district. These experiments were made principally at Block Island, though some were also made at Little Gull Island. Especial attention has been given to this phenomenon, (which consists in a distinct echo from the verge of the horizon in the direction of the prolongation of the axis of the trumpet of the siren,) because the study of it has been considered to offer the easiest access to the solution of the question as to the cause of all the abnormal phenomena of sound, and also because it is in itself an object of much scientific interest.

In my previous notice of this phenomenon, in the report of the Light-House Board for 1874, I suggested that it might be due to the reflection from the crests of the waves of the ocean; but as the phenomenon has been observed during all conditions of the surface of the water this explanation is not tenable.

Another hypothesis has been suggested, that it is due to a flocculent condition of the atmosphere, or to an invisible acoustic cloud of a density differing from that of the general atmosphere at the time. To test this hypothesis experimentally, the large trumpet of the siren was gradually elevated from its usual horizontal position to a vertical one. In conception this experiment appears very simple, but on account of the great weight of the trumpet it required the labor of several men for two days to complete the arrangements necessary to the desired end. The trumpet in its vertical position was sounded at intervals for two days, but in no instance was an echo heard from the zenith, but one was in every case produced from the entire horizon. The echo appeared to be somewhat louder from the land portion of the circle of the horizon than from that of the water. On slowly restoring the trumpet to its former direction, the echo gradually increased on the side of the water until the horizontal position was reached, when the echo as usual appeared to proceed from an azimuth of about twenty degrees of the horizon, the middle of which was in the prolongation of the axis of the trumpet. A similar experiment was made with one of the trumpets of the two sirens at Little Gull Island. In this case the trumpet was sounded in a vertical position every day for a week with the same result.

From these experiments it is evident that the phenomenon is in some way connected with the plane of the horizon, and that during the continuance of the experiment of sounding the trumpets while directed towards the zenith, no acoustic cloud capable of producing reflection of sound existed in the atmosphere above them.

Another method of investigating this phenomenon occurred to me, which consisted in observing the effects produced on the ear of the observer by approaching the origin of the echo. For this purpose, during the sounding of the usual interval of twenty seconds of the large trumpet at Block Island, observations were made from a steamer which proceeded from the station into the region of the echo and in the line of the prolongation of the axis of the trumpet, with the following results:

1. As the steamer advanced, and the distance from the trumpet was increased, the loudness of the echo diminished, contrary to the effect of an echo from a plane surface, since in the latter case the echo would have increased in loudness as the reflecting surface was approached, because the whole distance travelled by the sound to and from the reflector would have been lessened. The effect however is in accordance with the supposition that the echo is a multiple sound, the several parts of which proceed from different points at different distances of the space in front of the trumpet, and that as the steamer advances towards the verge of the horizon it leaves behind it a number of the points from which the louder ones proceed, and thus the effect upon the ear is diminished as the distance from the trumpet is increased.

2. The duration of the echo was manifestly increased, in one instance, from five seconds, as heard at the mouth of the trumpet, to twenty seconds. This would also indicate that the echo is a multiple re-action of varying intensities from different points, and that at the place of the steamer, the fainter ones from a greater distance would be heard, which would be inaudible near the trumpet.

3. The arc of the horizon, from which the echo appeared to come, was also increased in some cases to more than three times that subtended by the echo at the place of the trumpet. This fact again indicates that the echo consists of multiple sounds from various points at or near the surface of the sea, the angle which the aggregate of these points subtend necessarily becoming greater as the steamer advances.

But perhaps the most important facts in regard to the echo are those derived from the series of observations on the subject made by Mr. Henry W. Clark, the intelligent keeper of the principal light-house station on Block Island, and by Mr. Joseph Whaley, keeper of the Point Judith light-house. Mr. Clark was furnished with a time-marker to observe the duration of the echo, and both were directed to sound the trumpets every Monday morning for half an hour, noting the temperature, the height of the barometer, the state of

the weather as to clearness or fog, the direction and intensity of the wind, and the surface of the ocean.

From the observations made at these two points, for more than two years at one station and over a year at the other, the echo may be considered as produced constantly under all conditions of weather, even during dense fogs, since at Block Island it was heard 106 times out of 113, and at Point Judith 50 times out of 57, and on the occasions when it was not heard the wind was blowing a gale, making a noise sufficiently loud to drown the sound of the echo. These results would appear to quite effectually disprove the hypothesis that the phenomenon is produced by an acoustic cloud accidentally situated in the prolongation of the axis of the trumpet. An occurrence of such regularity must be due to something more permanent in its effects than a difference in temperature or density of a portion of air—from that of the general atmosphere; since such a condition could not exist in a dense fog embracing all the region in which the phenomenon occurs. Indeed, it is difficult to conceive how the results can be produced, even in a single instance, from a flocculent portion of atmosphere in the prolongation of the axis of the trumpet; since a series of patches of clouds of different temperature and density (if sufficient) would tend to absorb or stifle by repeated reflections—a sound projected into their interior, rather than to transmit it to the ear of the observer.

The question therefore remains to be answered: What is the cause of the aerial echo? As I have stated, it must in some way be connected with the plane of the horizon. The only explanation which suggests itself to me at present, is that the spread of the sound—which fills the whole atmosphere from the zenith to the horizon with sound-waves, may continue its curvilinear direction until it strikes the surface of the water at such an angle and direction as to be reflected back to the ear of the observer. In this case, the echo would be heard from a perfectly flat surface of water; and as different sound-rays would reach the water at different distances and from different azimuths, they would produce the pro-

longed character of the echo, and its angular extent along the horizon.

While we do not advance this hypothesis as a final solution of the question, we shall provisionally adopt it as a means of suggesting further experiments in regard to this perplexing question at another season.

General Conclusions.

From all the experiments which have been made by the Light-House Board in regard to the transmission of sound in free air, and those derived from other observations which can be fully relied upon, the following conclusions may be considered established, subject however to such further modification and extension as subsequent investigation may seem to indicate:

1. The audibility of sound at a distance (the state of the atmosphere being constant) depends upon the character of the sound. The distance through which a sound may be heard is governed by the pitch, the loudness, and the quantity of sound. The pitch or frequency of the impulses in a given time must not be too high, otherwise the amplitude of vibration will be too small to allow a sufficient quantity of air to be put into motion; neither must the pitch be too low, for in this case the motion of the atoms of air in the sound-wave will not be sufficiently rapid to convey the impulse to a great distance. Again the greater the loudness of the sound, (which depends upon the amplitude of the vibrations of the sounding-body,) the greater will be the distance at which it will be heard. And finally, the greater the quantity of sound, (which depends upon the magnitude of the vibrating surface,) the greater will be the distance to which it is audibly transmitted. These results are derived from observations on the siren, the reed-trumpet, and the automatic buoy. The effect of quantity of sound is shown by the fact that in sounding different instruments at the same time, it was found that two sounds apparently of the same loudness were heard at very different distances.

2. The audibility of sound depends upon the state of the

atmosphere. A condition most favorable to the transmission of sound is that of perfect stillness and uniform density and temperature throughout. This is shown by the observations of Parry and other Arctic explorers; although in this case an efficient and co-operating cause is doubtless the downward refraction of sound, due to the greater coldness of the lower strata of air, as first pointed out by Professor Reynolds. Air however is seldom in a state of uniform density, but is pervaded by local currents, due to contact with portions of the earth unequally heated, and from the refractions and reflections to which the sound-wave is subjected in its passage through such a medium it is broken up and lost to the ear at a less distance.

3. But the most efficient cause of difference in audibility is the direct effect produced by the wind. As a general rule, a sound is heard farther when moving with the wind than when moving against it. This effect, which is in conformity with ordinary observation, is not due to an increase of velocity of the sound-wave in one direction and a diminution in the other by the motion of the wind except in an imperceptible degree; for since sound moves at the rate of about seven hundred and fifty miles an hour, a wind of seven miles and a half an hour could increase or diminish the velocity of the sound-wave only one per cent., while the difference of effect observed is in some cases several hundred per cent. The true cause of the remarkable variation in the audibility of sound-beams at a distance—is to be found in the change of their direction. Sound moving with the wind is ordinarily refracted or thrown down toward the earth; while moving against the wind it is ordinarily refracted upward and passes over the head of the observer, so as to be heard at a distance at an elevation of several hundred feet, when inaudible at the surface of the earth.

4. Although as a general rule the sound is heard farther when moving with the wind than when moving against it, yet in some instances sound is heard farthest against the wind; but this phenomenon is shown to be due to a dominant upper wind, blowing at the time in an opposite direc-

tion to that at the surface of the earth. Such winds are not imaginary productions invented to explain the phenomena, but actual existences—established by observation, as in the case of the experiments made at Sandy Hook, in 1874, by means of balloons, and from the actual motion of the the air in the case of north-east storms, as observed at stations on the coast of Maine.

5. Although sound issuing from the mouth of a trumpet is at first concentrated in a given direction, yet it tends to spread so rapidly that at the distance of three or four miles it fills the whole space of air within the circuit of the horizon, and is heard behind the trumpet nearly as well as at an equal distance in front of its mouth. This fact precludes the use of concave reflectors as a means of increasing the intensity of sound in a given direction; for although they do give an increase of sound in the direction of the axis, it is for only a comparatively short distance.

6. It has been established (contrary to what was formerly thought to be the case,) that neither fog, snow, hail, nor rain, materially interferes with the transmission of loud sounds. The siren has been heard at a greater distance during the prevalence of a dense and widely-extended fog than during any other condition of the atmosphere. This may be attributed to the uniform density and stillness of the air at the time.

7. In some cases sound-shadows are produced by projecting portions of land or by buildings situated near the origin of the sound; but these shadows are limited in extent, and are closed in at some distance by the spread of the sound-waves, thus exhibiting the phenomenon of sound being heard at a distance when lost on a nearer approach to the station.

8. It frequently happens on a vessel leaving a station that the sound is suddenly lost at a point in its course, and after remaining inaudible some time is heard again at a greater distance, and is then gradually lost as the distance is increased. This phenomenon is observed only when the sound is moving against the wind, and is therefore attrib-

uted to the upward refraction of the sound-wave, which passes over the head of the observer and continues an upward course until it nearly reaches the upper surface of the current of wind, when the refraction will be reversed and the sound sent downward to the earth; or the effect may be considered as due to a sound-shadow produced by refraction, which is gradually closed in at a distance by the lateral spread of the sound-wave near the earth, in a direction which is not affected by the upward refraction. Another explanation may be found in the probable circumstance of the lower sheet of sound-beams being actually refracted into a serpentine or undulating course, as suggested in the Appendix to the Report of the Light-House Board for 1875.* Such a serpentine course would result from successive layers of unequal velocity in an opposing wind; as being retarded at and near the surface of the earth, attaining its maximum velocity at a height of a few hundred feet, and then being again retarded at greater elevations, by the friction of upper counter currents or of more stationary air. In some cases the phenomenon is due to one or the other of these causes, and in other cases to all combined. That it is not due to the obstructing or screening effects of an abnormal condition of the atmosphere is shown by the fact that a sound transmitted in an opposite direction, through what is called the region of silence, passes without obstruction. It is probable from all the observations, that in all cases of the upward refraction of a sound moving against the wind it tends again to descend to the earth by the natural spread of the sound; though it may generally be so enfeebled by diffusion as well as by distance, as to be inaudible.

9. The existence of a remarkable phenomenon has been established, exhibited in all states of the atmosphere,—during rain, snow, and dense fog, to which has been given the name of aerial echo. It consists of a distinct echo, apparently from a space near the horizon of fifteen or twenty degrees in azimuth, directly in the prolongation of the axis

*[See PART IV; *ante*, p. 450.]

of the trumpet. The loudness of this echo depends upon the loudness and quantity of the original sound, and therefore it is produced with the greatest distinctness by the siren. It cannot be due to the accidental position of a flocculent portion of atmosphere, nor to the direct reflection from the crests of the waves, as was at first supposed, since it is always heard except when the wind is blowing a hurricane.

As a provisional explanation, the hypothesis has been adopted that in the natural spread of the waves of sound some of the rays must take such a curvilinear course as to strike the surface of the water in a vertical direction, and thus be reflected back—by a similar deviation, to the station or location of the origin of the sound.

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SCIENTIFIC WRITINGS OF JOSEPH HENRY.

PART II.—CONTINUED.

(1847 to 1878.)

SCIENTIFIC PAPERS AND ABSTRACTS.

PART II.—CONTINUED.

METEOROLOGY: NOTES ON RAIN-GAGES.*

(From the Smithsonian Annual Report for 1855, pp. 229-231.)

- - - Observations have been made at the Smithsonian Institution with rain-gages of different sizes and various forms, the result of which has been to induce a preference for the smaller gages. The one which was first distributed to the observers by the Institution and the Patent Office consists of a funnel terminated above by a cylindrical brass ring, bevelled into a sharp edge at the top, turned perfectly round in a lathe, and of precisely five inches diameter. The rain which falls within this ring is conducted into a two-quart bottle placed below to receive it. To prevent any water which may run down on the outside of the funnel from entering the bottle, a short tube for enclosing its neck is soldered on the lower part of the funnel. The funnel and bottle are placed in a box or small cask sunk in the ground to the level of its surface and provided with a covering board having a circular hole in its centre to receive and support

*[Extract from a Circular of Directions to the meteorological observers of the Smithsonian Institution, prepared by Professors Guyot and Henry (the larger portion by the former), and published in 1850. The circular comprised detailed instructions for the placing, management, and observation of thermometers (free and registering), psychrometers (or wet-bulb thermometers), barometers, rain and snow-gages, wind-vanes, and anemometers, besides suggestions as to personal accounts of the sky, clouds, fogs, &c., as well as of thunder-storms, tornadoes, auroras, and other occasional phenomena.]

the funnel. To prevent the rain-drops which may fall on this board from spattering into the mouth of the funnel some pieces of old cloth or carpet may be tacked upon it.

The object of placing the receiving ring so near the surface of the earth is to avoid eddies caused by the wind, which might disturb the uniformity of the fall of rain.

In the morning, or after a shower of rain, the bottle is taken up and its contents measured in the graduated tube belonging to the apparatus, and the quantity in inches and parts recorded in the register. The tube or gage which was first provided for this purpose will contain when full only one-tenth of an inch of rain, the divisions indicating hundredths and thousandths of an inch. As this however is found to be too small for convenience, another gage—which will contain an inch of rain, and indicating tenths and hundredths—will be sent to observers.

Another and simpler form of the gage has since been adopted by the Institution and Patent Office, to send by mail to distant observers. It is one of those which have been experimented on at the Institution, and is a modification of a gage received from Scotland, which was recommended by Mr. Robert Russell.

It consists of—1. A large brass cylinder, two inches in diameter, to catch the rain: 2. A smaller but longer brass cylinder for receiving the water and reducing the diameter of the column, to allow of greater accuracy in measuring the height: 3. A whalebone scale divided by experiment, so as to indicate tenths and hundredths of an inch of rain: 4. A wooden cylinder to be inserted permanently in the ground for the protection and ready adjustment of the instrument. To facilitate the transportation, the larger and smaller cylinders are connected together by a screw-joint.

To put up this rain-gage for use—1. Let the wooden cylinder be sunk into the ground, in a level unsheltered place, until its upper end is even with the surface of the earth: 2. Screw the larger brass cylinder on the top of the brass tube and place the latter into the hole in the axis of the wooden cylinder, and the arrangement is completed.

The depth of rain is measured by means of the whalebone scale, the superficial grease of which should be removed by rubbing it with a moist cloth before its use. Should the fall of rain be more than sufficient to fill the smaller tube, then the excess must be poured out into another vessel, and the whole measured in the small tube in portions.

Care should be taken to place the rain-gage in a level field or open space sufficiently removed from all objects which would prevent the free access of rain, even when it is falling at the most oblique angle during a strong wind. A considerable space also around the mouth of the funnel should be kept free from plants—as weeds or long grass, and the ground should be so level as to prevent the formation of eddies or variations in the velocity of the wind.

Measuring snow.—To ascertain the amount of water produced from snow, a column of the depth of the fall of snow and of the same diameter as the mouth of the funnel should be melted and measured as so much rain. The simplest method of obtaining a column of snow for this purpose is to procure a tin tube about two feet long (having one end closed) and precisely of the diameter of the mouth of the gage. With the open end downward, press this tube perpendicularly into the snow until it reaches the ground, or the top of the ice, or last preceding snow; then take a plate of tin sufficiently large to cover it, pass it between the mouth of the tube and the ground, and invert the tube. The snow contained in the tube, when melted, may be measured as so much rain. When the snow is adhesive, the use of the tin plate will not be necessary.

From measurements of this kind, repeated in several places when the depth of the snow is unequal, an average quantity may be obtained. As a general average, it will be found that about ten inches of snow will make one inch of water.

ON THE RAIN-FALL AT DIFFERENT HEIGHTS.

(From the Smithsonian Annual Report for 1855, pp. 213, 214.)*

December, 1855.

The subject of the difference of rain at different elevations has received much attention in this country and in Europe; though more investigations are required to settle definitely all the principles on which it depends. It would appear that the greater part of the observed difference is due to eddies of wind, which carry the air containing the falling drops more rapidly over the mouth of the upper gauge than over an equal portion of the unobstructed surface of the ground. Professor Bache found, from a series of observations on the top and at the bottom of a shot-tower in Philadelphia, that not only was there a difference due to elevation, but also to the position of the upper gauge, whether it was placed on the windward or leeward side of the tower. It would also appear, that when the air is saturated with moisture down to the surface of the earth, the descending drop would collect at least a portion of the water it meets with in its passage to the ground, but the amount thus collected would not be sufficient to account for the difference observed. Besides this, the condition does not always exist; the air near the earth is frequently under-saturated during rain, and in this case a portion of the drop would be evaporated, and its size on reaching the earth less than it was above. If the drop is increased by the deposition of new vapor in its descent, then the rain at the bottom ought to be warmer than at the top, on account of the latent heat evolved in the condensation; on the other hand, if the drop be diminished by evaporation during its fall, then the temperature of the rain caught at the greater elevation ought to be in excess. That evaporation does sometimes take place during the fall of rain, would appear from the fact that

* [Remarks appended to an article on the subject, by Prof. O. W. Morris, of New York.]

clouds are seen to exhibit the appearance of giving out rain though none falls to the earth, the whole being entirely evaporated. That the air should ever be under-saturated during rain is at first sight a very surprising fact; it may however be accounted for on the principle of capillarity. The attraction of the surface of a spherical portion of water for itself is in proportion to the curvature or the smallness of the quantity, and hence the tendency to evaporate in a rain-drop ought to be much less than in an equal portion of a flat surface of water.

If the diminution of quantity of rain at the upper station depends principally on eddies of wind, then the effect will be diminished by an increase in the size of the drops, which will give them a greater power of resistance; and the size of the drop will probably be influenced by the intensity of the electricity of the air, as well as by its dryness. The former, as well as the latter, will tend to increase the evaporation from the surface of the drop.

It is a well-established fact, which at first sight would appear to be at variance with the results of observations on towers, that a greater amount of rain falls in some cases on high mountains than on the adjacent plains. For example, the amount of water which annually falls at the convent of St. Bernard is nearly double that which falls at Geneva. This effect however is due to the south wind, loaded with moisture, ascending the slope of the mountain into a colder region, which causes a precipitation of its vapor. From what is here said, it will be evident that the subject of rain is one which involves many considerations, and which still presents a wide field for investigation.

A series of observations has been commenced at the Smithsonian Institution on the quantities of rain at different elevations, as well as on gauges of different sizes and forms, the results of which will be given in one of the meteorological reports.

METEOROLOGY IN ITS CONNECTION WITH AGRICULTURE.

PART I.—GENERAL CONSIDERATIONS.

(Agricultural Report of Commissioner of Patents for 1855, pp. 357-374.)

All the changes on the surface of the earth, and all the movements of the heavenly bodies, are the immediate results of natural forces acting in accordance with established and invariable laws; and it is only by that precise knowledge of these laws, which is properly denominated science, that man is enabled to defend himself against the adverse operations of nature, or to direct her innate powers in accordance with his will. At first sight, meteorology might appear to be an exception to this general proposition, and the changes of the weather and the peculiarities of climate in different portions of the earth's surface to be of all things the most uncertain and furthest removed from the dominion of law; but scientific investigation establishes the fact that no phenomenon is the result of accident, nor even of fitful volition.

The modern science of statistics has revealed a permanency and an order in the occurrence of events depending on conditions in which nothing of this kind could have been supposed. Even those occurrences which seem to be left to the free will, the passion, or the greater or less intelligence of men are under the control of laws—fixed, immutable, and eternal. No one knows the day nor hour of his own death, and nothing is more entirely uncertain in a given case of expected birth, than whether a boy or girl shall be born; but the number out of a million of men living together in one country who shall die in ten, twenty, forty, or sixty years, and the number of boys and girls who shall be born in a million of births, may be predicted from statistical data with almost unerring precision. The statistics of courts of justice have disclosed the astonishing fact,—incomprehensible to our understanding because we do not know the connecting influences which concur to produce the result,—that in every

large country the number of crimes, as well as each kind of crime, can be foretold for every coming year with the same certainty as the number of births and deaths. Of every hundred persons accused before the supreme tribunal in France, sixty-one are condemned; in England, seventy-one; the variation on an average from these numbers hardly amounting to a hundredth part of the whole. Not only the number of suicides in general for several years to come can be foretold with confidence, but also the relative proportion by firearms and by hanging.

The astonishing facts of this class lead us inevitably to the conclusion that all events are governed by a Supreme Intelligence, who knows no change, and that under the same conditions, the same results are invariably produced. If the conditions however are permanently varied, a corresponding change in the results will be observed; for example, the effect of the introduction of an extended system of moral education, in diminishing crime, would be revealed by the statistics.

It is this regularity observable in phenomena when studied in groups of large numbers, which enables us to arrive at permanent laws in regard to meteorology, and hence to predict with certainty the average temperature of a given place for a series of decades of years, and which furnishes the basis (in accordance with the principles of insurance) of a knowledge of what species of plant or animal may be profitably raised in a given locality. We need not however in this branch of knowledge, as in that of the statistics of crime, be confined to the mere discovery of the existence and the measure of the constants of nature; but uniting the results of observations with those of experiments in the laboratory and mathematical deductions from astronomical and other data, we are enabled not only to refer the periodic changes to established laws, but also to trace to their source various perturbing influences which produce the variations from the mean, and thus arrive at an approximate explanation at least, of the meteorological phenomena which are constantly presented to us.

No truth is more important in regard to the material well-being of man, and none requires to be more frequently enforced upon the public mind, than that the improvement and perfection of art depends upon the advance of science. Although many processes have been discovered by accident, and practiced from age to age without a knowledge of the principles on which they depend, yet as a general rule such processes are imperfect, and remain, like Chinese art, for centuries unchanged or unimproved. They are generally wasteful in labor and material, and involve operations which are not merely unessential, but actually detrimental. The dependence of the improvement of agriculture upon the advance of general science, and its intimate connection with meteorology in particular, must be evident when we reflect that it is the art of applying the forces of nature to increase and improve those portions of her productions which are essential to the necessity and comfort of the human race.

Modern science has established, by a wide and careful induction, the fact that plants and animals consist principally of solidified air, the only portions of an earthy character which enter into their composition being the ashes that remain after combustion. All the other parts were originally in the atmosphere, were absorbed from the mass of air during the growth of the plant or animal, and are given back again to the same fountain from which they were drawn, in the decay of the vegetable, and in the breathing and death of the animal.

The air consists of oxygen, nitrogen, carbonic acid, the vapor of water, traces of ammonia, and of nitric acid. A young plant placed in the free atmosphere and exposed to the light of the sun, gradually increases in size and weight by constantly receiving carbon from the carbonic acid of the air, which being thus decomposed, evolves the liberated oxygen. The power by which this decomposition is produced is now known to be due to the solar ray, which consists of a peculiar impulse or vibration, propagated from the distant sun, through a medium filling all space.

It is a principle of nature that power is always absorbed

in producing a change in matter. This change may be permanent, or it may be of such a character as to re-produce the power which was expended in effecting it. For example, the moving power of a cannon ball is permanently expended in passing into the side of a ship; but if the same ball were shot into the mouth of another cannon, and made to compress a spring, the recoiling of the latter would give to the ball, in an opposite direction, precisely the same velocity which it had expended in compressing the spring, supposing nothing lost by friction, &c. This example serves to illustrate the effect of the impulse from the sun. It decomposes the carbonic acid which surrounds the leaf of the plant, or in other words, overcomes the natural attraction between the carbon and the oxygen of which the acid is composed, and in this effort the motions of the atoms of the ætherial medium are themselves stopped. The power however in this case is not permanently neutralized, for when the plant is consumed, either by rapid combustion or by slow decay,—that is, when the carbon and the oxygen are again suffered to rush into union to form carbonic acid,—the same amount of power is evolved in the form of light, heat, or nervous force which was absorbed in the original composition. If the plant moreover be consumed in the animal, the same power is expended in building up the organization, in producing locomotion, and the incessant action of the heart, and the other involuntary movements necessary to the vital process.

Plants are therefore the recipients of the power of the sun-beam. They transfer this power to the animal, and the animal again returns it to celestial space, whence it emanated. To properly so direct this power of the sun-beam that no part of it may run to waste, or be unproductive of economical results, it is essential that we know something of its nature; and the lifetime of labor of many individuals, supported at public expense, would be well applied in exclusive devotion to this one subject. The researches which have been made in regard to it have developed the fact that the impulses from the sun are of at least four different char-

acters, namely, the lighting impulse, the heating impulse, the chemical impulse, and the phosphorogenic impulse; and it has further been ascertained that though each of these impulses may produce an effect on the plant, the decomposition of the carbonic acid is mainly due to the chemical action. A series of experiments is required to determine the various conditions under which these impulses from the sun may be turned to the greatest amount of economical use, and what modifications they may demand, in order to the growth of peculiar plants. It has not yet been clearly ascertained whether some of these emanations cannot be excluded with beneficial result, or in other words, whether they do not produce an antagonistic effect; nor is it known what relative proportions of them are absorbed by the atmosphere, or reflected from our planet by the floating clouds of the air, without reaching the earth. To determine these facts requires a series of elaborate experiments and accurate observations.

We have said that the chemical vibration is that which principally decomposes the carbonic acid in the growth of the plant; but we know that the heating impulse is an auxiliary to this, and that heat and moisture are essential elements in the growth of vegetation. The small amount of knowledge we already possess of the character of the emanations from the sun has been turned to admirable account in horticulture. In this branch of husbandry we seek—even more than in agriculture—to modify the processes of nature; to cultivate the plants of the torrid zone amid the chilling winds of the northern temperate zone, and to render the climate of sterile portions of the earth congenial to the luxurious productions of more favored regions. We seek to produce artificial atmospheres, and to so temper the impulses from the sun that the effects of variations in latitude and the rigor of the climate may be obviated.

From all that has been said therefore it will be evident that the hopes of the future, in regard to agriculture, rest principally upon the advance of abstract science, not upon the mere accumulation of facts, of which the connection and dependence are unknown, but upon a definite conception of

the general principles of which these facts are the result. All the phenomena of the atmosphere should be studied and traced to the laws on which they depend. The labor bestowed upon investigations of this kind is not (as the narrow-sighted advocate of immediate utilitarian results would affirm) without practical importance; on the contrary, it is the basis of the highest improvement of which the art of agriculture is susceptible. On every acre of ground a definite amount of solar force is projected, which may under proper conditions be employed in developing organization; and the great object of the husbandman is to so arrange the conditions, that the least possible amount of this may be lost in un-economical results. Independently however of the practical value of a knowledge of the principles on which the art of agriculture depends, the mind of the farmer should be cultivated as well as his fields, and after the study of God's moral revelation, what is better fitted to improve the intellect than the investigation of the mode by which He produces the changes in the material universe?

The climate and productiveness of a country are determined, first, by its latitude, or its distance on either side of the equator; second, by the configuration of the surface as to elevation and depression; third, by its position, whether in the interior of a continent or in proximity to the ocean; fourth, by the direction and velocity of the prevailing winds; fifth, by the nature of the soil; and lastly, by the cultivation to which it has been subjected.

First, in regard to latitude: The productive power of a soil (other things being the same) depends on two circumstances,—solar radiation and moisture; and these increase as we approach the equator.

If the kind of food were a matter of indifference, the same extent of ground which supports one person at the latitude of 60° would support twenty-five at the equator; but the food necessary to the support of persons in different latitudes varies with respect to quality as well as to quantity; and the other conditions mentioned, with regard to climate, should enter largely into the estimate we form in relation to the actual productiveness of different parallels of latitude.

Though some of the heat of the sun is absorbed in its passage through the atmosphere, yet by far the greater portion (particularly at the equator) arrives at the surface of the earth, is absorbed by the soil, and is imparted to the stratum of air in contact with it. From various determinations it is a well-established fact that the temperature of celestial space beyond our atmosphere is at least 50° below the zero of Fahrenheit's scale. The upper surface of the atmosphere and the Arctic regions must therefore partake of this low temperature, while that of the lower stratum at the surface of the earth is at the equator about 80° . The air therefore diminishes in temperature as we ascend, but the rate of this diminution varies within certain limits in different parts of the earth; and to settle the law of diminution definitely, a series of observations by means of ascents in balloons will be required. For practical purposes however we may assume in the temperate zone that the diminution due to altitudes or mountains is about 1° of Fahrenheit for 300 feet. Furthermore, as we ascend and the pressure of the superincumbent strata is thus reduced, the air becomes lighter; and though the temperature of the several portions diminishes very rapidly, yet the whole amount of heat in each pound of air is very nearly the same. For example, if a certain weight of air were carried from the surface of the earth to such a height that it would expand into double its volume, the heat which it contained would then be distributed throughout twice the space, and the temperature would consequently be much diminished, though the absolute amount of heat would be unchanged. If the same air were returned to the earth whence it was taken, condensation would ensue, and the temperature would be the same as at first.

2. On this principle a wind passing over a high mountain is not necessarily cooled; for the diminution of temperature which is produced by the rarefaction of the ascent would be just equivalent to the increase which is due to the condensation in an equal descent. This would be the case if the air were perfectly dry; but if it contained moisture, para-

doxical as it may seem, it would be warmer when it returned to the lower level than when it left it. In ascending to the top of the mountain it would deposit its moisture in the form of water or snow, and the "latent heat" given out from this, would increase the heat of the air; and when it descended on the opposite side to the same level from which it ascended, it would be warmer on account of this additional heat. The configuration of the surface of our continent has on this account therefore a marked influence on the temperature of its different parts.

3. The effect on its climate, of the position of a country, as regards its proximity to the ocean, will be evident from the facts relative to the radiation and absorption of heat by different substances. All bodies on the surface of the earth are constantly receiving and giving out heat. A piece of ice exposed to the sun sends rays to this luminary, and receives in return a much greater amount. The power however of radiating and receiving heat is very variable in different bodies. Water exposed to the same source of heat receives and radiates in a given time far less than earth; consequently the land (especially in the higher latitudes) during the long summer days or during the growing season, receives much more heat than the corresponding waters of the same latitude; and though the radiation at night is less from the water than the land, yet the accumulating increase of temperature of the latter will be much greater than that of the former. The reverse takes place in the winter. While therefore the mean temperature of the ocean and of the land in the same latitude may remain the same, the tendency of the land is to receive the greater portion of the heat of the whole year during the months of summer, and thus, by a harmonious arrangement with respect to the production of organic life, to increase the effect of the solar radiation, and to widen the limits within which plants of a peculiar character may be cultivated.

Proximity to the sea however has another effect on the climate, which depends upon the currents of the former, by which the temperature of the earth due to the latitude is

materially altered. Heated water is constantly carried from the equatorial regions towards the poles, and streams of cold water returned, by means of which the temperature of the earth is modified and the extremes reduced in intensity. The great currents of the ocean are seven in number, and may be best and most clearly described in connection with an hypothesis as to their origin. For this purpose let us suppose the earth at rest and the equatorial regions continually heated by the sun. In this condition a continuous current of air from the north and another from the south would blow towards the equator, there ascend and flow backward in the upper regions towards the poles. If we next suppose the earth to be in motion on its axis from west to east, and compound the effects of this motion with that of the winds towards the equator on either side, they will not meet directly opposite each other, as in the previous supposition, but at an acute angle, and produce a belt of wind from east to west entirely around the earth in the region of the equator. The continued action of this wind on the surface of the water would evidently give rise to a current of the ocean in the belt over which the wind passed. If now instead of considering the earth entirely covered with water, we suppose the existence of two continents extending from north to south, forming barriers across the current we have described, and establishing two separate oceans, similar to the Atlantic and Pacific, then the continuous current to the west would be deflected right and left or north and south at the western shore of each ocean, and would form four immense whirlpools, namely, two in the Atlantic, one north and the other south of the equator, and two in the Pacific, similar in situation and direction of motion. The regularity of the outline of these whirls will be disturbed by the configuration of the deflecting coasts, and the form of the bottom of the sea, as well as by islands and irregular winds. For a like reason a similar whirlpool will tend to be produced in the Indian Ocean, the current from the east being deflected down the coast of Africa, and returning again into itself along a southern latitude on the western side of Australia. A fifth

whirl exists in this ocean, and in some seasons is at times divided into two, giving rise to the peculiar currents of this part of the earth's surface. Besides these great circular streams, the water supplied by all the rivers emptying into the Arctic basin, as well as that from all the precipitation in this region, returns to the south in a current between Europe and America, which as we shall hereafter see has a very marked influence on the temperature of our coast. A similar current, but more diffuse and less in amount, must constantly flow from the Antarctic regions. In this view we have adopted the hypothesis which ascribes the principal effect to the trade winds. A portion however will be due to the currents produced by the heating of the water itself. To illustrate the effect of these currents on the climate of the United States, let us consider those of the North Atlantic and North Pacific oceans, between which our continent is situated.

The great whirl in the North Atlantic, the western and northern portions of which are known as the Gulf Stream, passes southward down the coast of Africa, crosses the ocean in the region of the equator, is deflected from the northern portion of South America and the coast of Mexico along the United States, and re-crosses the Atlantic at about the latitude of 40° , to return into itself at the place where it started. A portion however of this current (probably owing to the configuration of the bottom) passes off in a tangent to the circumference of the great whirl and flows northward along the coasts of Ireland and Norway. By this current the heated waters of the equator are carried northward along the eastern coast of the United States and precipitated upon the shores of Northern Europe, giving the temperature of a southern latitude even to North Cape, the extremity of Europe, which would otherwise be as cold as Greenland. This stream has less effect upon the climate of the United States than upon that of the western coast of Europe; first, because the prevailing wind is from the west; and secondly, because between our shores and the Gulf Stream the cold polar current intervenes.

In the North Pacific Ocean, on the western side of our continent, the great circle of water passes up along the coast of Japan, re-crosses the ocean in the region of the Aleutian Islands, mingles with the fitful current outward through Behring's Strait, and thence down along the northwest coast of North America. In this long circuit the northeastern portion of it is much more cooled than the similar portion of the whirl of the Atlantic. It therefore modifies the temperature of the northwestern coast and produces a remarkable uniformity along its whole extent, from Sitka to the southern extremity of California. It is an interesting fact, which we have just derived from Captain John Rodgers, that an offshoot from the great whirl in the Pacific, analogous to that which impinges on the coast of Norway, enters along the eastern side of Behring's Strait, while a cold current passes out on the western side, thus producing almost as marked a difference in the character of the vegetation on the two shores of the strait as between that of Ireland and Labrador.

4. The effect of prevailing currents of air on the climate of different portions of the earth is no less marked than that of proximity to the sea. We have seen that on one side of a line over which the sun passes, a current of air flows from the northeast, and on the other from the southeast, giving rise to the trade winds. These winds ascend obliquely, and according to the views of Dove and others, rise to the upper regions of the atmosphere, flow backward towards the poles, and partaking of the rotary motion of the earth, gradually turn to the eastward and approach its surface, producing a series of whirls overlapping each other entirely around the globe. Whatever may be the cause however of the phenomena, Professor Coffin, in his admirable paper on the winds of the northern hemisphere, has shown that from the equator to the pole the whole space is occupied by three great belts, or zones, of prevailing wind: the first extends from the equator to an average latitude of 35° north, in which the current is from the northeast, constantly growing less intense as we approach the northern limit; the second

is that from 35° to about 60° , the current from the west being more intense in the middle of the belt, and gradually diminishing on either side almost into a calm; third, from 60° to the pole, or rather to a point of greatest cold in the Arctic regions, the wind is in a northeasterly direction.

The first of these belts would constitute what is called the trade winds, produced as we have said, by the combined effects of the heat of the sun and the rotation of the earth; the second is the return trade, and the third the current which would be produced by an opposite effect to that of the rarefaction of the air by the sun at the equator, namely, the condensation of the air by the cold portion of the earth. The air should flow out in every direction from the coldest point, and combining its motion towards the south with the rotation of the earth, it should take a direction from the east to the west or become a northeasterly wind.

The effects which these currents must have upon the climate of the United States will be made clear by a little reflection. The trade winds within the tropics, charged with vapor, in their course towards the west impinging upon the mountainous parts of South America, will deposit their moisture on the eastern slope and produce a rainless district on the western side. Again, a lower portion of the Atlantic and Gulf trade wind will be deflected from these mountains along the eastern coast of the United States and through the valley of the Mississippi as a surface wind, and thus give rise to our moist and warm summer breezes from the south, while the principal or upper portion of the trade wind (or the return westerly current) sweeping over the Pacific Ocean, and consequently charged with moisture, will impinge on the Coast Range of mountains of Oregon and California, and in ascending its slopes deposit moisture on the western declivity, giving fertility and a healthful climate to a narrow strip of country bordering on the ocean and sterility to the eastern slope. All the moisture however will not be deposited in the passage over the first range, but a portion will be precipitated on the western side of the next, until it reaches the eastern elevated ridge of the Rocky

Mountain system, where we think it will be nearly, if not quite, exhausted. East of this ridge, and as it were, in its shadow, there will exist a sterile belt, extending in a northerly and southerly direction many hundred miles. The whole country also included between the eastern ridge of the Rocky Mountains and the Pacific Ocean, with the exception of the narrow strip before mentioned, will be deficient in moisture, and on account of the heat evolved (as before shown) by the condensation of moisture on the ridges, will be at a much higher temperature than that due to latitude. This mountain region and the sterile belt east of it occupy an area about equal to one-third of the whole surface of the United States, which with our present knowledge of the laws of nature and their application to economical purposes must ever remain of little value to the husbandman.

According to this view, the whole valley of the Mississippi owes its fertility principally to the moisture which proceeds from the Gulf of Mexico, and the inter-tropical part of the Atlantic Ocean. The Atlantic Gulf Stream therefore (as already remarked) produces very little effect in modifying the climate of the northern portion of the United States; both on account of the cold polar current which intervenes between it and the shore, and because of the prevalent westerly wind, which carries the heat and moisture from us, and precipitates them on the coast of Europe.

5. The influence of the nature of the soil on the climate of a country, may be inferred from its greater or less power to absorb and radiate heat, and from its capacity to absorb, or transmit over its surface, the water which may fall upon it in rain, or be deposited in dew. In the investigation of this part of the subject, the observations of the geologist, and the experiments of the chemist and the physicist must be called into requisition.

6. In regard to the influence of *cultivation* on the climate of a country much also may be said, though at first sight it might appear that man, with his feeble powers, could hope to have no influence in modifying the action of the great physical agents which determine the heat and moisture of

any extended portions of the globe. But though man cannot direct the winds, nor change the order of the seasons, he is enabled, by altering the conditions under which the forces of nature operate, materially to modify the results produced; for example, removing the forests from an extended portion of country exposes the ground to the immediate radiation of the sun, and increases in many cases the amount of evaporation; in other places it bakes the earth and allows the water to be carried off to the ocean in freshets, and in some instances, in destructive inundations.

Drying extensive marshes, or the introduction of a general system of drainage, has a remarkable influence in modifying the temperature. The water which would evaporate, and by the latent heat thus absorbed, would cool the ground, is suffered to pass through it to the drain beneath, and is thus carried off without depriving the earth of a large amount of heat, which would otherwise be lost. Besides this, the removal of forests gives greater scope to the winds, which are hence subjected to less friction in their passage over the earth.

The whole subject of the removal of forests is one which deserves more attention than it has usually received. In the progress of settlement, it is evident that a great portion of the wooded land of a new country must give place to the cleared field, in order that man may reap the rich harvest of the cereals, which in his civilized condition are necessities as well as luxuries of life; yet the indiscriminate destruction of the forests is of doubtful propriety. By the judicious reservation of trees along the boundaries of certain portions of land, in accordance with the known direction of the prevailing wind, the climate may be ameliorated within a restricted portion of the earth, both for the production of plants and animals. While in some parts of the country the clearing of nearly all the ground is absolutely necessary for agricultural purposes, in others it may be profitable to allow forests of considerable extent to remain in their pristine condition. Cases of this kind however can be determined only by the particular climate of each district of the country.

It is now an established truth that certain localities are screened from miasmatic influence by the intervention of trees. A more general recognition of this fact might add much to the healthfulness of localities in other respects highly desirable.

The solar rays, in passing through the atmosphere, do not heat it in any considerable degree, but they heat the earth against which they impinge; therefore the temperature of the lower stratum of air is derived, directly or indirectly, from the soil on which it rests; and this temperature, as has been remarked, will depend upon whether the surface be marshy or dry, clothed with herbage, or covered with sand, clay, or an exposed rock. From this fact it is evident that man has, in this particular also, considerable power in modifying the climate of portions of the earth; and history furnishes us with many examples in which great changes, within human control, have been produced in the course of ages. Nineveh and Babylon, once so celebrated for their advance in civilization and opulence, and Palmyra and Baalbec, for their magnificence, offer at this day to the traveller the site of ruins which attest their past greatness, in the midst of desolation. Canaan, described in the Bible as a fertile country, "flowing with milk and honey," is now nearly deprived of vegetation, and presents a scene of almost uninterrupted barrenness. The climate of these countries is undoubtedly modified by the present state of the surface, and might again be ameliorated by cultivation, were the encroachments of the sands of the desert stayed by borders of vegetation of a proper character. Many parts, even of our own country, which now exhibit a surface of uninterrupted sand, may be rendered productive, or covered with trees and herbage.

A series of observations on the progress of temperature below the surface, in different parts of the country, and even in different fields of the same plantation, would be of value in ascertaining the proper time to introduce the seed, in order that it might not be subjected to decay by premature planting, or lose too much of the necessary influence of

summer by tardy exposure in the ground. This may perhaps be most simply effected by burying a number of bottles filled with water at different depths in the ground, say one at the depth of 6 inches, another at 12, and a third at 18 inches. These in the course of time would take the temperature of the earth in which they were embedded, and would retain it sufficiently long unchanged, to admit of its measurement, by inserting a thermometer into the mouth of the bottle.

No improvement is more necessary, for rendering the art of agriculture precise, than the introduction into its processes of the two essential principles of science, namely, those of weight and of measure. All the processes in our manufactories, on a great scale, which were formerly conducted by mere guesses, as to heat and quantities, are now subjected to rules, in which the measure of temperature and the weight of materials are definitely ascertained by reliable instruments.

The foregoing are general views as to the great principles which govern the peculiarities of climate, and especially that of the United States, the truth of which, in reference to our continent, and the modifications to which they are to be subjected, are to be settled by observations in the future.

In order however that the science of meteorology may be founded on reliable data, and attain that rank which its importance demands, it is necessary that extended systems of co-operation should be established. In regard to climate, no part of the world is isolated; that of the smallest island in the Pacific is governed by the general currents of the air and of the waters of the ocean. To fully understand therefore the causes which influence the climate of any one country, or any one place, it will be necessary to study the conditions, as to heat, moisture, and the movements of the air of all others. It is evident also that, as far as possible, one method should be adopted, and that instruments affording the same indications, under the same conditions, should be employed.

It is true that, for determining the general changes of

temperature, and the great movements of the atmosphere of the globe, comparatively few stations of observation, of the first class, are required; but these should be properly distributed, well furnished with instruments, and supplied with a sufficient corps of observers, to record at all periods of the day the prominent fluctuations. Such stations however can only be established and supported by the co-operation of a number of governments.

A general plan of this kind for observing the meteorological and magnetical changes more extensively than had ever before been undertaken, was digested by the British Association in 1838, in which the principal governments of Europe were induced to take an active part; and had that of the United States and those of South America joined in the enterprise, a series of watch-towers of nature would have been distributed over every part of the earth. The following were the stations of the several observatories established: Those of the English Government were at Greenwich, Dublin, Toronto, St. Helena, Cape of Good Hope, Van Dieman's Land, Madras, Simla, Singapore, and Aden. The Russian observatories were at Boulowa, Helsingfors, Petersburg, Sitka, Catherineburg, Kasan, Barnaoul, Nicolaieff, Nertschinsk, Tiflis, and Pekin. Those of Austria were at Prague and Milan. In the United States, an observatory was established at Girard College, under the direction of Professor Bache. The French Government had one at Algiers; the Prussian Government, one at Breslau; the Bavarian Government, one at Munich; and the Belgian, one at Brussels. There was one at Cairo, supported by the Pasha of Egypt, and one in India, at Travandrum.

These observatories were established to carry out a series of observations at the same moment of absolute time, every two hours, day and night, during three years, together with observations once every month, continuing 24 hours, at intervals of five minutes each. They were all furnished with standard instruments, and followed instructions adopted by the directors of the general system. Operations were commenced in 1839, and in a number of cases, were continued

through nine years. The number of separate observations amounted to nearly six millions, which required at least as much labor for their reduction as that expended in the observations themselves. The comparisons of these observations are still in progress, and will occupy the attention of the student of magnetism and meteorology for many years to come. The system was established more particularly to study the changes of the magnetic needle, and on this subject alone it has afforded information of sufficient importance to repay all the labor and time expended on it. It has shown that the magnetic force is scarcely constant from one moment to another, that the needle is almost incessantly in motion, that it is affected by the position of the sun and moon, and by perturbations, connected with meteorological phenomena, of a most extraordinary character.

In regard to meteorology, this system furnished reliable data for the great movements of the atmosphere, and the changes in its thermal and hygrometric condition. But to obtain a more minute knowledge of the special climatology of different countries, it is necessary that a series of observations, at a great many places, should be continued through a number of years, and at stated periods of the day—not as frequent as those of the observations we have mentioned, but embracing as many elements, and even adding to these, as new facts may be developed or new views entertained. In many countries accordingly, provision has been made by their respective governments, for continued though local systems of this kind. The Government of Prussia appears to have taken the lead in this important labor, and its example has been followed by those of Great Britain, Russia, Austria, Bavaria, Belgium, Holland, and France. In these countries, regular and continuous observations are made with reliable instruments, on well-digested plans.

Though the Government of the United States took no part with the other nations of the earth in the great system before described, yet it has established and supported for a number of years a partial system of observation at the different military posts of the army. Among other duties

assigned to the surgeons, at the suggestion of Surgeon General Lovell, was that of keeping a diary of the weather, and of the diseases prevalent in their vicinity. The earliest register received, under this regulation, was in January, 1819. The only instruments at first used were a thermometer and wind-vane, to which in 1836, a rain-gauge was added. The observations were made at 7 A. M. and 9 P. M., and the winds and weather were observed morning, noon, and evening. It is to be regretted that in 1841, the variable hour of sunrise was substituted for that of 7 A. M., since the latter admits of an hourly correction which cannot be applied to the former, except at the expense of too great an amount of labor.

The results of the observations for 1820 and 1821 were published at the end of each year; those from 1822 to 1825, inclusive, were issued in the form of a volume by Surgeon General Lovell; those from 1826 to 1830, and from 1830 to 1842, inclusive, were prepared and published in two volumes, under the direction of the present Surgeon General, Dr. Thomas Lawson. At the commencement of 1843 an extension of the system was made by the introduction of new instruments, and an additional observation to the number which had previously been recorded each day, and hourly observations for twenty-four hours were directed to be taken at the equinoxes and solstices.

During the past year a quarto volume has been published, which contains the results of the observations of the thermometer, direction and force of winds, clearness of sky, and fall of rain and snow, during a period of twelve years, from the first of January, 1843, to January, 1855, arranged in monthly tables and annual summaries. To these are added consolidated tables of temperature and rain for each separate station, comprising the results of all the thermometric observations made by medical officers since 1822, and of all measurements of rain and snow since the introduction of the rain-gauge in 1836.

The tabular part of this volume contains the most important results of the observations of the army system of registration, and will be considered the most valuable contribu-

tion yet made toward a knowledge of the climatology of the United States. Truth however will not permit us to express the same opinion in reference to the isothermal charts which accompany this volume. These we consider as premature publications, constructed from insufficient data, and on a principle of projection by which it is not possible to represent correctly the relative temperatures in mountainous regions.

With the learning and zeal for science possessed by the officers of the United States army, and the importance which they attach to meteorology, in its connection with engineering and topography, it is hoped that this system may be further extended and improved, that each station may be supplied with a compared thermometer and psychrometer, and that at a few stations a series of hourly observations may be established, for at least a single year. The present Secretary of War, we are assured, would willingly sanction any proposition for the improvement of this system, and we doubt not the Surgeon General is desirous of rendering it as perfect as the means at his disposal will permit.

A local system of meteorological observations was established in the State of New York in 1825, and has been uninterruptedly conducted from that time until the present. Each of the academies which participated in the literature fund of the State was furnished with a thermometer and rain-gauge, and directed to make three daily observations relative to the temperature, the direction of the wind, cloudiness, &c. The system was re-modeled, in 1850, so as to conform to the directions of the Smithsonian Institution, and a considerable number of the academies were furnished with full sets of compared instruments, consisting of a barometer, thermometer, psychrometer, rain-gauge, and wind-vane.

A summary of the results of the observations from 1826 to 1850, inclusive, has just been published by the State of New York, under the direction of the regents of the University. They are presented in the form of a quarto volume, to which is prefixed a map of the State, showing the

direction of the wind and the position of each station. This volume, the computations for which were made by Dr. Franklin B. Hough, is also a valuable contribution to meteorology, and does much credit to the intelligence and perseverance of those who introduced and have advocated the continuance of this system, and to the liberality of the State which has so long and so generously supported it.

A system of State observations in Pennsylvania was established in 1837. For this purpose the Legislature appropriated \$4,000, which sum was placed at the joint disposal of a committee of the American Philosophical Society and the Franklin Institute. The results of this system have not yet been presented to the world in a digested form.

Another State system was established in Massachusetts in 1849, the records of which have been presented to the Smithsonian Institution, and will be published, in considerable detail, either at the expense of the State or of the Smithsonian fund.

A system of meteorological observations was established by the Smithsonian Institution in 1849, the principal object of which was to study the storms that visit the United States, particularly during the winter months. This system, which has been continued up to the present time, was afterward extended, with a view to collect the statistics necessary to ascertain the character of the climate of North America, to determine the average temperature of various portions of the country, and the variations from this at different periods of the year. It was intended to reduce, as far as possible, the several systems of observations to one general plan which had previously been established, and to induce others to engage in the same enterprise. But in order that the results might be comparable with those obtained in other countries, it was regarded as of primary importance that the instruments should be more accurate than those which might be requisite for the mere determination of the phenomena of storms. The Institution therefore procured standard barometers and thermometers from London and Paris, and with the aid of Professor Guyot, a distinguished meteorologist, copies of

these were made, with improvements, by Mr. James Green, a scientific artisan, of New York. A large number of these instruments have been constructed and sold to observers. Full sets have been furnished by the Institution to parties in important positions, and in some cases, half the cost has been paid from the Smithsonian fund.

A growing taste having been manifestly created for the study of practical meteorology, directions for observations and a volume of tables for their reduction, have been prepared and widely circulated at the expense of the Institution. It has also distributed blanks to all the observers of the different systems alluded to, except those of the army, and has received in return, copies of all the observations which have been made. It has in this way accumulated a large amount of valuable material relative to the climate of this country and to the character of the storms to which it is subjected. The completeness and accuracy of the observations have also increased from year to year; and by an arrangement which the Institution has now made with the Patent Office, it is hoped that the system will be extended, and its character improved.

It being manifest from the foregoing statements, and from other evidences, that much interest is awakened in this country on the subject of meteorology, it is hoped that the means may be afforded for reducing and publishing the materials which have been and may hereafter be accumulated, and that important results to agriculture, as well as to other arts, may be hence deduced.

*Description of the Tables.**

The numbers given in the accompanying meteorological tables are mostly those indicating average or mean results. The principle of deducing general laws from a multiplicity of facts or observations—though liable in themselves to error, is of the greatest value in modern science. If we observe

*[Twenty pages of Meteorological Tables following this part are omitted in the present re-print.]

the temperature of a given place every hour in the day, add all the observations into one sum for a year, and divide by the number of hours in a year, we shall get the mean annual temperature. By this method of observation we shall ascertain the warmest and the coolest hours of each day, and by repeating the same process for a number of years, we shall learn the temperature of each hour, eliminated from all perturbations, and in this way arrive at truths which could not be obtained by any other means. If we examine the individual records we shall find the warmest time to recur, on different days, at different hours. We know however that if there were no perturbing influences the warmest period of the day would be that at which the heat received from the sun is just equal to the cooling of the earth by radiation into space. At every instant from the rising of the sun previous to this the earth would be receiving more heat than it gave off, and hence the temperature would constantly increase until the heating and cooling were equal. After this the earth would give off more heat than it would receive, and the temperature would begin to descend. On individual days however, clouds may intervene, or winds of varying temperatures and velocities may prevail, so as to change the hour of maximum heat; but as these are not periodical or governed by recurring laws, the probability is that they will act in opposite directions; that is, on some days hasten the maximum period, and on other days retard it, and thus in the course of a year, or several years, neutralize each other. The method of averages therefore enables us to separate the effects produced by irregular variations from those which are due to permanent causes. The latter are called periodic variations, while to the former has been given the name of non-periodic. By continuing the observations for a number of years, in ascertaining the temperature at a given place, we find by the method we have explained a result from which that of the individual years will oscillate on either side within certain limits, while for two separate decades of years it will scarcely differ at all; and this is the mean temperature of the place. The same

statement may be made in regard to the other elements of meteorology, and the result of all the observations may be divided into two great classes, periodical and non-periodical, though by a very long series of observations, it may happen that a phenomenon which at first may appear entirely fitful, will afterwards prove to be recurring; and at all events the non-periodic variations are found to be restricted within definite limits, the maximum amount of which it is highly necessary to obtain.

The first element given in the tables is that of the mean height of the barometer from month to month. This is perhaps less immediately essential to the agriculturist than any other meteorological element. It is however of much importance in determining the progress of storms and the area over which the commotions of the atmosphere connected with them are perceptible, though no violent disturbances may be observed. For example, if the barometer on a given day is higher or lower than the average for the month, we are then convinced that it is subjected to some unusual perturbation; and by drawing a line on a map through all the places at which a given amount of disturbance is felt at a particular time, we are enabled to trace the boundary of a storm, and to indicate its progress, development, and end. For this purpose it is not necessary even that the barometers should be strictly comparable with each other; it is only necessary that the results should be comparable among themselves. When the barometers have been accurately compared with each other, (as in the case of those of Green, of New York, constructed under the direction of the Smithsonian Institution,) they afford the data for determining the relative elevation of different places of observation above the level of the sea.

The indications of the barometer, compared with those of the hygrometer, thermometer, and wind-vane, furnish us with a method of predicting changes in the weather. These however in many cases will be found to depend upon rules applicable to particular places, and which can only be determined by a long series of local observations.

The next element given in the tables is the mean monthly temperature. By comparing this with the average deduced from a number of years' observations we are enabled to ascertain the variations of each month from the normal temperature of the same month as deduced from a series of years, and to compare the temperature of the "growing" portions of different years with each other. When experiments shall have been made upon the amount and distribution of heat necessary to give the best development to particular plants, by a table of this kind we are enabled to select the months best suited to their cultivation. Moreover, each plant requires a certain amount of heat for its proper growth, though this amount may vary considerably in intensity; for example, a comparatively low degree of heat may be compensated by its longer continuance. This rule however is confined within certain limits; for if the temperature rises above a given degree, or falls below a particular point, the vitality of the plant may be destroyed. By a well-conducted series of experiments and observations the agriculturist may be enabled to determine, without a ruinous series of actual trials, what plant may be safely cultivated in a given place.

Besides the mean temperature, the extremes are also given, and these are of essential importance in determining the variations of temperature to which the plant is to be subjected. The length of the growing summer in a given year, and in a particular place, may for instance be measured by the interval which occurs between two killing frosts.

The next element in order, presented in the accompanying tables, is that of the moisture; and this is of much importance in judging of the productiveness of different years and different places. Unfortunately however, comparatively few observations are regularly made on the variations of moisture in the atmosphere, in the United States. It is to be hoped that our returns for another year will indicate an increased number of the stations where valuable observations of this kind are taken. The figures in the tables do not indicate the actual amount of water, for example, in a

cubic foot of air, but the fractional part of the whole amount necessary to produce entire saturation ; thus if saturation is represented by 100, 57 indicates that this number of parts of water is contained in the air, or that it is a little more than half saturated. We are obliged to adopt this method of representation, because the relative moisture and dryness of the air depend upon the temperature, and not on the absolute quantity of vapor present. Thus air at 32° F., which contains as much water as it can hold, or in other words is saturated would by heating, become exceedingly dry, though containing absolutely the same amount of water. The relative dryness is indicated by the complement of the numbers in the table, and consequently may be found by subtracting these numbers from 100. The state of our feelings is much more affected by the moisture of the atmosphere than by the temperature, and the sensation called " closeness " is principally due to the great amount of humidity, or in other words, to the diminution of the dryness of the air, which prevents evaporation from the surface of the body, and its attendant cooling effects. A series of observations on the relative humidity in the regions west of the Mississippi, and the northern portions of the middle part of our continent, in connection with the different winds, would be highly interesting in determining the source of the vapor in these regions, as well as settling definitely the fact in regard to their average productiveness.

Another element intimately connected with the moisture in the air, is the amount of rain and snow, particularly the former. Besides the whole amount which falls during a year, it is necessary to know the relative quantity which falls in different months. A large amount of rain may fall at once, and a greater relative proportion of it will be carried off, before the earth can have time to be fully saturated through the streams of creeks and rivers, and thus do much less in the way of fertilizing the earth, than if the same amount were distributed over a longer period.

The indications of the rain, as of the other elements, would be more interesting, could they be compared with the average

amount deduced from a series of observations made through a number of years.

The direction of the wind, as well as the amount of cloudiness and sunshine, besides being of much importance in determining the meteorological elements of the climate of a country, are of interest to the farmer in comparing them with the other elements with which it is intimately connected, and thus deducing rules for the prognostication of the weather.

METEOROLOGY IN ITS CONNECTION WITH AGRICULTURE.

PART II.—GENERAL ATMOSPHERIC CONDITIONS.

(Agricultural Report of Commissioner of Patents, for 1856; pp. 455-492.)

In the last Agricultural Report of the Patent Office I gave an account of the several systems of meteorology now co-operating in this country to advance the science, and also endeavored to show the importance of this branch of knowledge in its connection with agriculture. I propose in this Report and the subsequent ones to continue the subject, and to present some of the physical laws on which meteorology depends, the general principles at which it has arrived, and their application to the peculiarities of the climate of the United States. An exposition of this kind presented to the farmer through the Agricultural Report it is thought will serve to awaken a more lively interest in the subject, will tend to diffuse a knowledge of the advantages of general principles, and will convey information not readily accessible, and which in reality does not elsewhere exist in the condensed form in which it will be here given.

Perhaps no branch of science has given rise to more speculation or excited a greater amount of angry controversy than that relating to the nature and interpretation of atmospheric phenomena. The former may arise from the dependence of man for health and comfort on the state of the weather, and the latter from the limited sphere of individual observation to which the cultivators of this branch are generally confined. While the astronomer, without quitting his observatory (if situated near the equator) can watch the motions of all the heavenly bodies as they present themselves in succession to his telescope, the meteorologist can take cognizance only of the changes which occur immediately around him, and hence the origin of partial views and imperfect generalizations. Controversies in this science, as in most others, may frequently however be referred to the partiality we entertain for the products of our own minds. Truth, as has been properly said, belongs to mankind in general; our

hypotheses belong exclusively to ourselves, and we are frequently more interested in supporting or defending these than in patiently and industriously pursuing the great object of science, namely, the discovery of what *is*.

In the account of meteorology which it is proposed to give, the writer has no hypotheses or theories of his own to support, but will endeavor to confine his statements to the exposition of such principles as are generally recognized at the present day; and if hereafter it shall be found that views have been presented in this paper which cannot be sustained, he will point out in the subsequent Reports the errors which may have been committed. The expounder of science, unlike the politician, is at liberty to change his opinions when they are found to be at variance with the actual condition of things. Indeed, in the investigation of nature, we provisionally adopt hypotheses as antecedent probabilities, which we seek to prove or disprove by subsequent observation and experiment; and it is in this way that science is most rapidly and securely advanced.

Some parts of our subject, as will be seen, are intimately connected with leading questions of the day; and on this account it might be considered prudent to avoid allusion to them. But the great aim of science is the discovery of truth; and the proverbial veneration entertained for it by the human mind is a sure indication that truth, and the whole truth, will always be conducive to the real progress of nations or individuals, and that to present it simply as a proposition without special application is the best means of supplanting error. We hold in high veneration the plan of government established by the wisdom of our forefathers; but we cannot be blind to the fact that it required a peculiar theatre for its application, a wide territory of fertile soil and genial climate, well fitted to reward the labors of the husbandman and to promote the health of his body and the vigorous activity of his mind. Next to our political organization, under Providence our prosperity has mainly been promoted by the ample room afforded us for expansion over the most favored regions of this continent. It becomes therefore important

for us to ascertain the natural limits, if there are any, to the arable portion of our still untenanted possessions, and to determine, if possible, what parts of it are best fitted by climate and soil for the future operations of the husbandman. The data do not exist at present for the definite solution of this problem; but it is one object of the systems of meteorology now in operation in this country to collect the facts by which it may be fully solved. In the United States agriculture as a science has been up to this time of comparatively little importance; refined processes of cultivation are not required where the products of millions of acres of virgin soil can be gathered without skill and with comparatively little labor. It is only when the organic power and material which Nature has thus stored up in the primitive earth have been to a greater or less extent exhausted, that scientific processes must be adopted in order to secure the continued production of ample harvests. The time is at hand when scientific agriculture can no longer be neglected by us; for however large our domain really is, and however inexhaustible it may have been represented to be, a sober deduction from the facts which have accumulated during the last few years will show that we are nearer the confines of the healthy expansion of our agricultural operations over new ground than those who have not paid careful attention to the subject could readily imagine. We think it will be found a wiser policy to develop more fully the agricultural resources of the States and Territories bordering on the Mississippi, than to attempt the further invasion of the sterile waste that lies beyond.

The laws of nature are all simple and readily comprehended by a mind of ordinary capacity, when separately announced; but when the conditions under which they operate are varied, and a number of forces are called into action, the resulting phenomena frequently become so complex that their investigation transcends not only the ordinary logic of the most gifted mind, but even the more powerful analysis of the mathematician. It has been well said by Professor Benjamin Peirce, of Cambridge, that had the lot of man been cast

upon one of the outer planets of our system, the phenomena of the motions of the heavenly bodies, as viewed from that point, would have been so complex and apparently irregular, that our present state of civilization (resting as it does on the principles of science beginning with astronomy, the most perfect) would not have existed: man would never have arrived at the definite idea and the conclusive evidence of the universality of causation. In other words, that amid all the apparently confused and accidental occurrences which we observe, a few simple laws (constantly diminishing in number as our views become more extended) govern all events, whether they be those which we refer to order and succession, or those which in our ignorance we ascribe to to chance. Astronomy is the most perfect of all the sciences, not only because it has been longer studied, but more especially because it is the simplest exhibition of the laws of force and motion; and yet even in this science where all the data are furnished, the introduction of a few conditions renders a problem too complex for direct solution. For example, to determine the path described and the time of revolution of a single planet round the central body by the application of the laws of motion and gravitation is a simple problem, which was solved at an early period in the history of astronomy. When however a third body was introduced, such for example as the moon, in addition to the earth and sun, the problem baffled for a long time the skill of the first mathematicians of the age; and even yet a direct *à priori* solution of all the results which will be produced by the mutual action of a series of planets revolving round the sun has not been effected, and recourse is had to indirect methods of approximation. Had man confined his observations to the complex and multiform changes of the weather, the probability of his ever arriving at a definite law would be far less than even in the before mentioned case of astronomy; for, though we are assured that the motion of every atom of air is governed by the same laws which direct the heavenly bodies, yet the amount of perturbation and reciprocal action presented in the case of myriads of atoms renders the probability of a com-

plete solution of the problem of the currents of the atmosphere, even with the greatest possible extension of human science, extremely doubtful. We must therefore be content with approximations deduced from general principles combined with the results of extended, precise, and definite observation.

The history of meteorology illustrates the fact, that what may be termed popular observations and experience, without scientific direction, seldom lead to important rules. The uneducated sailor of to-day, after three thousand years of experience, firmly believes that he can invoke the winds and entice them from the caves of Æolus by a whistle. Most of the aphorisms in reference to the changes of the weather, though of venerable antiquity, merely relate to the greater or less degree of moisture in the atmosphere. They declare what has happened, that a change has already taken place in the air, but give no certain indication of what is to occur. In order therefore to the successful study of meteorology, the results of *systematic* observations are to be compared with the deductions from well established principles of science, and the converse; or in other words, deduction and observation should constantly go hand in hand, the former directing the latter, and the latter correcting the conclusions of the former.

In meteorology, as in all other branches of science, the important rule adopted by Newton should never be neglected, namely: "No more causes are to be admitted for the explanation of any phenomenon, or class of phenomena, than are true and sufficient." Though a general principle which is in strict accordance with the established laws of force and motion cannot be immediately applied to the explanation of an isolated class of phenomena, it is not, on that account, to be set aside for some new and unknown agent. We must look to further investigations for the light which shall enable us to perceive the connection. The undulatory theory of light connects so many facts, and has enabled the scientist to predict so many others which were previously unknown, that though a few outstanding phenomena may still exist they do not militate against our convictions of the truth of

the generalization which this theory so admirably expresses; and we may safely attribute the apparent want of agreement to our ignorance of some essential condition of the phenomena in question, or to some error in the logical deduction from our principles. The history of science abounds in apparent exceptions to general rules which when better understood become additional evidences in support of the general principle. The foregoing remarks will not be thought inapplicable on the present occasion by those who have studied the history of the progress of meteorology.

One of the most important general truths at which science has arrived by a wide and cautious induction, and which is the foundation of meteorology, is that nearly all the changes which now take place at the surface of the earth are due to the action of the sun. The forces which pertain to the earth itself—such as gravity, chemical affinity, cohesion, electricity, magnetism, &c.—are forces of quiescence; they tend to bring matter to a state of rest at the surface of the globe, from which it is only again disturbed by the solar emanation. All the elementary substances which constitute the surface of our planet, with the exception of the organic matter, have long since gone into a state of permanent combination. The rocks and various strata are principally composed of burnt metals. The whole globe is an immense slag, analogous to that drawn from the smelting furnace, surrounded by a liquid and an aerial envelope; the former in a state of ultimate chemical combination, and the active principle of the latter—the oxygen—finding nothing to combine with, except what has been released from a former combination by the action of the sun. If therefore the solar impulses were suspended, all motion on the surface of the planet would cease: the wind would gradually die away; the currents of the ocean would slacken their pace, and finally come to rest; and stillness, silence, and death would hold universal reign. We cannot however at present pursue this thought, but must confine our remarks to the effects of those impulses of the sun denominated *heat* in their connection with meteorology.

All the phenomena referable to heat from the sun acting under varying conditions will now (so far as they affect the climate of the United States,) be considered under two heads:

1. The effects of varying astronomical conditions, irrespective of atmospheric and other influences.

2. The effect of all conditions, other than astronomical, such as the influence of the air, the ocean, the land, &c.

I. Results of Astronomical Conditions.

The earth, in its annual revolution in its orbit round the sun, does not describe a perfect circle, but an ellipse, of which the sun occupies one of the foci; and hence we are nearer at one season of the year to this central luminary than at another. It is well established by mathematical investigation from astronomical data, that at the present historical period, the earth as a whole receives the greatest amount of heat during any one day in the year on the first of January, and the least amount on the 4th of July. The variation in the distance of the sun produces no effect on the different seasons; since the rapidity of motion or the less duration of proximity to the sun, just compensates for the greater intensity of the rays due to the nearer approach. Were it not for this, the eccentricity of the orbit would materially influence the heat of the seasons, since the fluctuation in the heating power of the sun's rays on this account amounts to one-fifteenth of the whole; and it does in reality increase the diurnal intensity for a few days in January, as is shown from the ardor of the sun's rays under a clear sky at noon in the southern hemisphere. One-fifteenth, says Sir John Herschel, is too considerable a fraction of the whole intensity of sunshine, not to aggravate in a serious degree the sufferings of those who are exposed to it without shelter, in the thirsty deserts of the south. The accounts of what is endured in the interior of Australia at this season, for instance, are of the most frightful kind, and seem far to excel what have ever been experienced by travellers in any part of the northern hemisphere.

Another astronomical deduction is that the point of the

earth's orbit which approaches nearest the sun is constantly changing its place, and in time the order will be reversed; the greatest amount of heat from this cause will be on some day in July, and the least in January. But this change is so slow, that no appreciable effect has been produced during the historic period. A slight variation also takes place in the distance of the earth and sun when nearest to each other; but this also is confined to such narrow limits, that it is entirely insufficient to account for the changes undergone in the earth's temperature, as indicated by fossil plants and animals, and cannot, on account of its slowness, have had any appreciable effect upon the temperature of any part of the earth since the first records of civilized man. If therefore it be true, as some suppose, that the seasons have changed in different parts of the earth within the memory of man, the effect must be due to other than to astronomical causes.

The earth is approximately a sphere, and consequently, the sun's rays strike it obliquely at all places, except those over which it is precisely vertical. The amount of variation on this account can readily be calculated; the sun's beam may be considered as a force, and resolved into two parts, one of which is parallel to the surface of the earth, and the other perpendicular to it, the latter alone producing the result. The intensity of the sun's beam will be the greatest at the equator, and will gradually diminish to the poles. It is true the sun does not continually remain vertical at the equator, but the average result in the course of the year, is nearly the same as if this were the case; since the greater amount of heat received while he is at the north just compensates for the less while at the south. The average temperature of any given place, in consideration of the obliquity of the rays which the earth would receive if uninfluenced by other conditions, can be obtained by multiplying its equatorial temperature into the radius of its parallel of latitude; or (in more technical language) into the *cosine* of the latitude.

From this formula, which we owe to Sir David Brewster, we have calculated the following table, which exhibits the astronomical and observed temperatures of the valley of the

Mississippi, along a line passing through the city of New Orleans:

Lat.	Astron. mean temp.	Observed temp. reduced.	Difference.
25°	74.32	74.50	+ 0.18
30°	71.01	69.00	— 2.01
35°	67.17	62.00	— 5.17
40°	62.81	53.00	— 9.81
45°	57.98	44.50	—13.48
50°	52.70	37.00	—15.70

The temperature of the equator is assumed to be 82°. The first column gives the latitude, the second the astronomical mean temperature, the third the observed temperature reduced to the level of the sea, as taken from the accompanying isothermal chart,* and the fourth column the difference between the last two. It will be seen that the difference between the calculated and the observed temperature in the lower latitudes is quite small; but as the latitude increases, the deviation becomes very great. This difference is due to other than astronomical causes, and by eliminating the latter we narrow the field of research.

Empirical formulas of much nearer approximation to the truth in high latitudes have been proposed, which will be noticed hereafter, our object at present being only to exhibit the difference between the astronomical results and those derived from actual observation.

Let us next consider the changes of temperature in different parts of the day and in different seasons of the year, produced by the varying obliquity of the sun's rays. If we assume a given length of sun-beam as the representative of the force, and then resolve this into two,—one perpendicular, the other parallel to the horizon,—the sum of all the perpendicular lines, from the rising to the setting of the sun on any day, will represent the whole intensity of the heat on a given place during that day; and in this way may be calculated the relative amount of heat received on different latitudes at different seasons of the year. From this estimate we shall find that the amount of heat received from the sun during a given day in summer, say the 16th day of June, at dif-

* [See Map, at page 72.]

ferent northern latitudes, is greater than that which falls upon the equator during the same time. This is exhibited in the following table, from the paper of L. W. Meech on the sun's intensity, in the 9th volume of the Smithsonian Contributions, [page 18]:

The sun's diurnal intensity at every ten degrees of latitude in the northern hemisphere.

1853.	Lat. 0°.	Lat. 10°.	Lat. 20°.	Lat. 30°.	Lat. 40°.	Lat. 50°.	Lat. 60°.	Lat. 70°.	Lat. 80°.	Lat. 90°.
Jan. 1-----	77.1	67.2	55.8	42.8	30.1	16.5	5.1	-----	-----	-----
Jan. 16-----	78.1	68.9	58.2	45.8	32.7	19.3	7.2	-----	-----	-----
Jan. 31-----	79.6	71.7	61.9	49.7	38.6	25.0	11.9	1.4	-----	-----
Feb. 15-----	81.0	74.7	66.6	55.6	45.1	31.9	19.0	6.4	-----	-----
Mar. 2-----	81.6	78.0	71.3	62.9	52.7	41.1	27.9	14.5	2.1	-----
Mar. 17-----	82.0	80.2	76.0	69.6	61.1	50.2	37.1	25.5	11.6	-----
April 1-----	80.8	81.4	79.5	75.3	68.9	60.2	49.9	38.0	25.6	20.5
April 16-----	79.0	81.7	82.0	79.5	75.1	68.6	61.1	51.4	44.0	44.6
May 1-----	76.9	81.5	83.7	83.6	80.8	77.1	70.9	64.6	64.3	65.3
May 16-----	74.7	80.8	84.7	86.7	85.7	83.3	79.7	76.8	80.3	81.5
May 31-----	73.0	80.1	85.1	87.8	88.9	87.8	85.7	86.8	91.0	92.4
June 15-----	72.0	79.6	85.2	88.4	90.1	89.9	88.8	91.7	96.1	97.6
July 1-----	72.0	79.5	85.0	88.5	90.4	89.5	88.4	90.8	95.1	96.6
July 16-----	73.0	79.8	84.7	87.5	87.6	86.5	84.1	84.3	88.3	89.7
July 31-----	74.7	80.4	83.9	85.1	84.5	81.6	77.3	73.4	76.2	77.4
Aug. 15-----	76.7	80.8	82.7	82.4	79.8	74.7	68.2	60.9	59.2	60.1
Aug. 30-----	78.5	80.7	80.6	77.7	72.1	65.5	57.3	47.7	38.8	38.9
Sept. 14-----	79.8	79.8	77.5	72.6	65.6	58.8	46.9	34.5	21.9	14.7
Sept. 29-----	80.5	78.4	73.8	67.0	57.8	47.0	36.2	22.5	9.0	-----
Oct. 14-----	80.7	76.4	69.7	61.0	50.2	38.2	25.7	12.6	1.0	-----
Oct. 29-----	79.9	73.5	65.0	54.6	42.5	30.1	17.5	5.2	-----	-----
Nov. 13-----	78.8	70.7	60.8	49.8	37.1	23.8	11.0	0.9	-----	-----
Nov. 28-----	77.5	68.3	57.3	45.3	31.8	18.9	6.8	-----	-----	-----
Dec. 13-----	76.9	66.9	55.4	43.0	30.3	16.3	4.9	-----	-----	-----

On the fifteenth of June the sun is more than 23 degrees north of the equator, and therefore it might be readily inferred that the intensity of heat should be greater at this latitude than at the equator; but that it should continue to increase beyond this even to the pole, as indicated by the table, may not at first sight seem so clear. It will however be understood, when it is recollected that the table indicates the amount of heat received during the whole day;

and though in a more northern latitude the obliquity of the ray is greater, and on this account the intensity should be less, yet the longer duration of the day is more than sufficient to compensate this effect, and to produce the result exhibited. This is an important fact, in comparing the agricultural capacity of different latitudes; for though there is absolutely more heat at the latitude of New Orleans during the year than at Madison, in Wisconsin, yet there is more heat received at the latter place during the three months of mid-summer than in the same time at the former place. An analogous but contrary result is exhibited in regard to the cold of winters, as will be seen by the table. It is from this principle that as we advance toward the equator, the extreme variations of the season become less and less. It is important to remark in this place that the foregoing tables exhibit the amount of heat actually falling upon the earth during the day as unmodified by any extraneous causes. They do not however exhibit the hottest portion of the season. This will depend upon another condition, which may be properly explained in this connection, though it is not classed under the astronomical causes. It is a well established principle that all bodies are radiating heat even while they are receiving it. If the amount received in a definite time is greater than that given off, the temperature will increase; on the contrary, if the amount given off is greater than that received, the temperature will diminish. The earth is constantly radiating heat into space, but only receiving it from the sun during the day. As the sun is declining towards the south, the daily amount received at length becomes less than that given off in the night, and hence the temperature begins to fall; and this diminution will continue until the two quantities again become equal, which will not be at the point where the greatest amount of heat is given off. On the twenty-first of June, in northern latitudes, the earth is receiving the greatest amount of heat, and hence it is becoming heated up most rapidly at this time. On the twenty-second it receives a less amount of heat, but the heating continues, since the gain is still greater than the loss; and this goes on until about the 25th of July, or

later, after which the radiation during the day and night together exceeds the amount received from the sun during the day, when the temperature begins to decline. The action is a little complicated, on account of the fact that the radiation increases with the temperature. A similar result is produced in the heating of the day, as will be seen from the following table of observations taken at every hour of the twenty-four, at Girard College, under the direction of Professor Bache :

MEAN DIURNAL VARIATION OF THE TEMPERATURE OF THE AIR AT
PHILADELPHIA.

Computed from observations in 1842, and from July 1, 1843, to July 1, 1845.

1 A. M.	2 A. M.	3 A. M.	4 A. M.	5 A. M.	6 A. M.	7 A. M.	8 A. M.	9 A. M.	10 A. M.	11 A. M.	12 NOON.
48·2	47·8	47·3	46·8	46·6	47·0	48·1	50·1	52·1	54·1	55·7	56·8

—
Minimum.

1 P. M.	2 P. M.	3 P. M.	4 P. M.	5 P. M.	6 P. M.	7 P. M.	8 P. M.	9 P. M.	10 P. M.	11 P. M.	12 NIGHT.
57·9	58·6	58·9	58·7	57·7	56·0	54·1	52·5	51·0	50·2	49·4	48·7

+

Maximum.

The result in the above table is somewhat affected by the greater humidity of the atmosphere towards morning, which prevents a greater radiation and fall of temperature, even after the rising of the sun.

II.—Results of other than Astronomical Conditions.

The deductions that have thus far been given are from established astronomical data; and unless some error has been committed in the statement, their correctness cannot be doubted by any person properly educated in the line of physical science. The effects produced by the air, the water, and the land, are however of a much more complicated character, and like the problem of the mutual action of all the planets on each other, have never yet been submitted to

a successful mathematical analysis. In the investigation of a phenomenon, it is not enough that we explain how it is produced; besides this, positive science requires that the explanation be true in measure as well as in mode, and indeed it is only when we can predict the exact amount of an effect, the principle being known and certain data given, that a phenomenon can be said to be perfectly analyzed. We have seen in the preceding paragraphs that the meteorological phenomena produced by astronomical causes admit of relative numerical expression; but in what follows we are obliged to content ourselves with the explanation in mode, and to refer to direct experiment and observation for the amount of the effect in measure. It is in this part of meteorology that so much uncertainty prevails, and in reference to which so much discussion, even of an excited character, has arisen. As was said before, the writer has no hypothesis of his own to advance and will therefore confine himself to a statement, and in some cases a brief examination, of such hypotheses relative to the effects of the atmosphere, the ocean, &c., in modifying climate as have been suggested, and which appear to be in accordance with established principles.

Effects of the Atmosphere in a Statical Condition.—Were it not for the aerial envelope which surrounds our earth, all parts of its surface would probably become as cold at night, by radiation into space, as the polar regions are during the six months' absence of the sun. The mode in which the atmosphere retains the heat and increases the temperature of the earth's surface may be illustrated by an experiment originally made by Saussure. This physicist lined a cubical wooden box with blackened cork, and, after placing within it a thermometer, closely covered it with a top of two panes of glass, separated from each other by a thin stratum of air. When this box was exposed to the perpendicular rays of the sun, the thermometer indicated a temperature within the box above that of boiling water. The same experiment was repeated at the Cape of Good Hope, by Sir John Herschel, with a similar result, which was however rendered more impressive by employing the heat thus accumulated in cooking the viands of a festive dinner.

The explanation of the result thus produced is not difficult when we understand that a body heated to different degrees of intensity, gives off rays of different quality. Thus if an iron ball be suspended in free space and heated to the temperature of boiling water it emits rays of dark heat, of little penetrating power, which are entirely intercepted by glass. As the body is heated to a higher degree, the penetrating power of the rays increases; and finally when the temperature of the ball reaches that of a glowing or white heat, it emits rays which readily penetrate glass and other transparent substances. The heat which comes from the sun consists principally of rays of high intensity and great penetrating power. They readily pass through glass, are absorbed by the blackened surface of the cork, and as this substance is a bad conductor of heat, its temperature is soon elevated, and it in turn radiates heat; but the rays which it gives off are of a different character from those which it receives. They are non-luminous, and have little penetrating power; they cannot pass through the glass, are retained within the box, and thus give rise to the accumulation of heat. The limit of the increase of temperature will be obtained when the radiation from the cork is of such an intensity that it can pass through the glass, and the cooling from this source becomes just equal to the heating from the sun. The atmosphere surrounding the earth produces a similar effect. It transmits the rays of the sun which heat the earth beneath; but this in turn emits rays which do not readily penetrate the air, thus effecting an accumulation of heat at the surface. The resistance of the transmission of heat of low intensity depends upon the quantity of vapor contained in the atmosphere, and perhaps also on the density of the air. The radiation of the earth therefore differs very much on different nights and in different localities. In very dry places, as for example in the African deserts and our own western plains, the heat of the day is excessive, and the night commensurably cool. Colonel Emory states in his Report of the Mexican Boundary Survey that in some cases on the arid plains there was a difference of 60° between the temperature of the day and that of the night. Indeed the air in this re-

gion is so permeable to heat, even of low intensities, that a very remarkable difference was observed on some occasions when the camp-ground was chosen in a gorge between two steep hills. The inter-radiation between the hills prevented in a measure the usual diminution of temperature, and the thermometer in such a position stood several degrees higher than on the open plain.

We shall next briefly consider the mechanical constitution of the atmosphere. The aerial ocean which surrounds the earth consists of atoms of matter self-repellant, which in proportion as the interior pressure is lessened, constantly tend to separate from each other and produce an enlargement or expansion of the whole mass. When the pressure is increased the mass sinks into a less volume, the atoms are brought nearer together, the force of repulsion is increased with the diminution of distance between the atoms, and a new equilibrium is attained. From this constitution of the air it immediately follows that the density of the atmosphere is greater near the surface of the earth than that at a higher altitude, since the lower stratum bears the weight of all those which are above it. The diminution in weight of equal bulks of air as we ascend is in a greater ratio than the height, since it diminishes on two accounts: first, because as we ascend in the air the number of strata pressing on us is less; and secondly, each succeeding stratum is lighter. From the law of this diminution of density a table may be formed of the pressure of the atmosphere at various heights, of which the following is an example:

Density of the air at increasing altitudes.

Miles above the sea.	Bulk of equal weight of air.	Density.	Height of barometer.
0	1	1	30.00
3.4	2	$\frac{1}{2}$	15.00
6.8	4	$\frac{1}{4}$	7.50
10.2	8	$\frac{1}{8}$	3.75
13.6	16	$\frac{1}{16}$	1.87
17.0	32	$\frac{1}{32}$	0.93

From this table it appears that one-half of the whole atmosphere is found within the upward limit of $3\frac{2}{3}$ miles, and one-third of the whole quantity beneath the average height of the Rocky Mountains: this fact has an important bearing on the influence of mountain ranges in modifying the direction of the winds.

The question occurs at this place, Why does the air grow colder as we ascend? The answer is that a pound of air, at all distances above the earth contains at least an equal amount of heat with the same weight taken at the surface, and that as the pressure is removed this air is expanded in bulk; consequently the heat is diffused through a greater amount of space, and hence the reduction of its intensity or temperature. To illustrate this, take a large ball of sponge and squeeze it into one quarter of the space which it naturally occupies; in this condition dip it into water, it will imbibe a certain quantity of the liquid, and when drawn out will be dripping wet; now let it expand to its natural dimensions, the water will be distributed through a large amount of space, and the sponge itself will appear comparatively dry. Squeeze it again into its former condensed state, and it will appear wet; suffer it again to expand, and the apparent dryness will be resumed. In a like manner we suppose that while the quantity of heat is the same, its intensity is increased by condensation into a smaller space and diminished by the converse process. In the foregoing illustration the amount of water contained in the sponge represents the amount of heat in the air, and the degree of wetness produced by condensation the intensity of the temperature exhibited in diminishing the bulk of air.

It follows from this that the blowing of a current of air over a high mountain, provided it descends again into the plain, does not necessarily diminish its temperature. When it arrives at the top of the mountain, it will become as cold as the circumambient air, not because it has lost any of its heat, but because that which it contained is now distributed through a greater space; when it descends again to the plain, it will suffer a corresponding diminution of bulk, on account

of the increased pressure, and with this the original temperature will be restored.

This principle, as we shall see hereafter, is of great importance in the study of the peculiarity of the temperature of the western portion of the territory of the United States. We have said that every pound of air, from the bottom of the aerial ocean to its surface above, contains at least an equal quantity of heat; and this was the inference of Dalton. From the investigations of Poisson and others it appears that the absolute quantity of heat, pound for pound, slightly increases rather than diminishes as we ascend; and this seems necessary to the stability of the equilibrium of the atmosphere as a whole. If the amount of heat were greater in the lower strata than in the upper, the equilibrium would be unstable, and an inversion would tend constantly to take place. An equal quantity of heat, (pound for pound,) as we ascend, would produce an indifferent equilibrium, while an increased amount in the order of ascent, would produce a stable condition of the atmosphere, such as that which really exists. The question however has not yet been fully settled, although it is an important one having a bearing on the explanation of many meteorological phenomena.

Another question of much interest is the exact law of diminution of temperature as we ascend into the air. Were this actually known, we could reduce to the same level all the observations which are made in a country; and thus, in addition to the astronomical effects, we could eliminate those due to altitude, and present the remainder as results which are due to the other conditions producing the peculiarities of climate. In order however to apply the law with precision in this way, it is desirable that it should be determined from observations made by ascents in balloons or at points of different heights on isolated mountain peaks. Relative observations made for this purpose on the top and at the base of mountain systems of considerable width and extent will probably give results involving the influence of the mountain surface itself, which in turn would be somewhat affected by the direction of the prevailing wind and

other causes. The progress of meteorology will call for an increased number of observations of the proper character, and for the repetition of the experiments with balloons, in different parts of the earth.

Celestial space, in which our sun and the earth and other planets of our system are placed, is known; from different considerations, to have a temperature of its own, which is supposed to be the result of the inter-radiation of all the suns and planets which exist in every part of the visible universe. The temperature of this space is estimated to be about -60° . This fact being allowed, it will follow (since the heat at the top of the air remains constant) that the rate of decrease of temperature as we ascend will be diminished with the decrease of temperature at the surface of the earth, and also that the rate of decrease will follow a slightly diminishing ratio. At all accessible elevations in the atmosphere however it may be considered as almost constant. In some cases the rate of diminution is interfered with by abnormal variations of temperature; for example, as we ascend into the region of the clouds, the latent heat evolved in the condensation of the vapor produces a local heat in the atmosphere beyond the natural temperature. In temperate latitudes it is usual to allow 300 feet of elevation for the reduction of temperature one degree of Fahrenheit's scale. This quantity was deduced from thirty-eight observations collected by Ramond. Boussingault found, from observation in the tropics, the diminution at 335 feet. Col. Sykes, from mountain observations in India, the diminution at 332 feet. Saussure ascertained the mean value in the Alps to be 271 feet. Gay Lussac's celebrated voyage gave 335 feet. And the result of several series of observations with the balloon by Mr. Welch, under the direction of the British Association, omitting the points unduly heated by the condensation of vapor, was about 320 feet. In the construction of the isothermal chart* we have adopted 333 feet, or three degrees to one thousand feet, as the rate of diminution, and find in comparing the temperature of different places of varying heights

* [See Map, at page 72.]

which have been reduced by it, that they afford very satisfactory corresponding results. We propose to give a fuller discussion of this part of the subject in another report.

Motions of the Atmosphere.—The repulsion of the atoms of the air is not only increased by a diminution of distance from being pressed closer together, but also by an addition of heat. From the latest and most reliable experiments on this point it is found that the pressure being the same, air expands $\frac{1}{491}$ part of its bulk at the freezing point for each degree of Fahrenheit's scale. Heated air therefore becomes specifically lighter, and tends constantly to ascend, being pressed upwards by the heavier circumambient fluid. The effect thus produced upon the air by the impulses from the sun is the great motive power which gives rise to all the currents of the atmosphere, from the gentle zephyr which slightly ripples the surface of the tranquil lake to the raging hurricane which overwhelms whole fleets, or destroys in a moment the hopes of the husbandman for an entire season. This fact is so well established by science that it is unnecessary to seek for any other *primum mobile* for the great system of constant agitation to which the aerial ocean is subjected.

Allowing the temperature of the equator, on an average, to be 82° F., that of the pole zero, and of the top of the air, or in other words, of celestial space, to be — 60°, and estimating the height of the atmosphere at 50 miles, it will follow from the law of expansion by heat, that the excess of elevation of the air at the equator will be upwards of four miles above that of the pole. Although this is not intended to present the exact amount of the aerostatic pressure, yet it will serve to show the great motive power constantly maintained by the influence of the solar radiation. In order to simplify the conception of the motions which result from this disturbing power, let us in the first place, suppose the earth to be at rest, and its whole surface of a uniform character, consisting, for example, of water. It is obvious from well established hydrostatic principles, that the air expanded as we have stated at the equator, would flow over at the top and descend, as it were, along an inclined plane towards the

poles, would sink to the earth, flow back to the equator below, and would again be elevated in an ascending current; and thus a perpetual circulation from either pole to the equator, and from the equator back towards the poles, along the several meridians of the globe, would be the continuous result. It is further evident that since the meridians of the earth converge, and the space between them constantly becomes less, all the air that rose at the equator would not flow along the upper surface entirely to the poles, but the greater portion would proceed north and south no further than the 30° of latitude; for the surface of the earth contained between the parallel of this degree and the equator is equal to that of half of the whole hemisphere. Portions however in the northern hemisphere, for example, would flow on to descend at different points further north; and of these some would probably reach the pole, there sink to the surface of the earth, and from that point diverge in all directions in the form of a northerly wind. Between the two ascending currents near the equator would be a region of calms or variable winds, influenced by local causes. The currents which flow over towards the poles would descend with the greatest velocity at the coldest point; because there the air would be most dense, or would have the greatest specific gravity.

According to the view here presented, a section of the atmosphere made by cutting through a meridian from pole to pole, perpendicular to the horizon, would exhibit two great systems of circulation; one from the north and another from the south to the equator below, rising at the latter place, and pouring over on either side to return again by longer or shorter circuits to the place whence they started. Such would be the simple circulation of the aerial ocean if no perturbing influences existed, and the whole science of meteorology would be one of comparatively great simplicity. But this is far from being the case. A number of modifying conditions must be introduced, which tend greatly to perplex the anticipation of results. First, the earth is not at rest, but in rapid motion on its axis from west to east.

Every particle therefore of the current of air as it flows towards the equator in the northern hemisphere would partake of the motion of the place at which it started, and in its progress southward it would reach in succession latitudes moving more rapidly than itself. It would thus as it were continually fall behind, and appear to describe on the surface of the earth a slightly curvilinear course towards the west. A similar result would be produced on the south side of the equator; and hence we have the first conception of the cause of the great systems of currents denominated the "trade winds," blowing constantly within the parallel of 30° from the northeast in the northern hemisphere, and from the southeast in the southern, towards the belt of the greatest rarefaction.

The motion however will require further consideration. The particles of air approaching the equator will not ascend in a perpendicular direction, as was first supposed, but as they rise will continually advance towards the west along an ascending plane, and will continue for a time their westerly motion in the northern hemisphere after they have commenced their return towards the north. They will however as they advance northward, arrive at parts of the earth moving so much less rapidly than themselves, that they will gradually curve around towards the east, and finally descend to the earth, to become again a part of the surface trade wind from the northeast. The particles will tend to move westward as they ascend: first, on account of their momentum in that direction; and secondly, because, as they reach a higher elevation, they will have less easterly velocity than the earth beneath. They will also be affected by another force, as has lately been shown by Mr. W. Ferrel, due to the increase of gravity which a particle of matter experiences in travelling in a direction opposite to that of the rotation of the earth. The last mentioned cause of deflection will operate also in a contrary direction on the atoms when they assume an easterly course.

The result of the complex conditions under which the motive power acts in such a case would be to produce a sys-

tem of circuits inclined to the west; the eastern portion of which would be at the surface, and the western at different elevations even to the top of the atmosphere. To give definiteness to the conception, let us suppose a series of books to be placed side by side on edge, pointing to the north; these books would represent the planes in which the currents of the air would circulate in the northern hemisphere, were the earth at rest; but if the earth is supposed to be in motion, then the books must be inclined to the west, so as to make an acute angle with the horizon, and overlap each other like the inclined strata in a geological model. If on each leaf of each book a circuit of arrows be drawn, then will the assemblage of these represent the paths of the different particles of the atmosphere. The currents of air however would not be in perfect planes, but in surfaces which could be represented by bending the leaves to suit the curvature of the earth. In this manner would be exhibited the general motion of the wind, which has been determined by actual observation.

The greater portion of the circulation would descend to the earth within 30 degrees of the equator, giving rise to the trade winds; a portion would flow further north, and produce the southwest winds; another portion would extend still further northward, descend towards the earth as a northwest wind, and so on. The air which descends in the region of the pole would not flow directly southward, but, on account of the rotation of the earth, would turn towards the west and become a northeasterly current. At first sight it might appear that the north wind which descends from the polar regions would continue its course along the surface until it joined the trade winds within the tropics; but this could not be the case, on account of the much greater western velocity this wind would require from the rapidly increasing rotary motion as we leave the pole. There would therefore be three distinct belts in each hemisphere, namely, the belt of easterly winds within the tropics, the belt of westerly in the temperate zone, and the belt of northwesterly at the north. The existence of these belts has been clearly

made out by Professor James H. Coffin in calculating the resultant of all the winds of the northern hemisphere, after having eliminated the effects of extraneous action, and thereby exhibiting the residue as the result produced by the general circulation.

Another condition however must be introduced. These belts would not be stationary, but would move laterally towards the south or the north, according to the varying positions of the sun at different seasons of the year. Their breadth would also vary; because they would be crowded into a smaller space towards the pole in the winter, and expanded into a wider space in the summer.

To trace with precision the path which would be described by a particle of air in its circuit, while under these varying perturbing influences, transcends the power of unaided logic, and could only be accomplished (if at all) by means of the most refined mathematical artifices. This problem has lately been presented (it is believed) as one of the prize questions of the French Academy of Sciences. Were it however solved with all the conditions that have been assigned, this would not be sufficient; since there is another cause of disturbance, perhaps more active than any yet enumerated, namely, the condensation of the vapor which arises from the surface of the ocean and is carried to different parts of the earth by the currents described. We owe to Mr. Espy, of this country, the principal development of the action of this agent in modifying and controlling atmospheric phenomena. The heated air which ascends at the equator is saturated with moisture, which it has absorbed in its passage over the northern and southern oceans. As it ascends above the surface of the earth it meets continually with a diminished temperature; and as the sun daily declines into the west, a considerable portion of it is converted into water which returns to the surface in the form of rain. The greatest effect of this action is immediately beneath the sun; and hence the belt of inter-tropical rains oscillates to the north and south with the course of the sun in its annual changes of declination. A portion however

of the same vapor is probably carried by the upper current far beyond the tropics, and deposited in fertilizing rains even at the extremities of the polar circles.

The condensation of the vapor which ascends in the equatorial regions evolves an astonishing power, in the form of heat, accelerating the upward motion of the air, and modifying in a greater degree than almost any of the causes we have heretofore mentioned, the primary motion due simply to the difference of heat between the poles and the equator. To understand this, it is sufficient to refer to the great amount of heat contained in a given amount of steam; and for illustration let us suppose the following simple experiment: A quantity of water at the temperature of melting ice is placed in a vessel over a lamp, which is so adjusted as to impart one degree of heat to the water in each minute of time. If the process is properly conducted, the heat will continue to increase, and, in accordance with the supposition we have made, the water at the end of about twelve hundred minutes will be all converted into vapor. If the process has been so conducted that a degree of heat has been given to the liquid in each minute of time, the steam will evidently contain about twelve hundred degrees of heat above the zero of Fahrenheit's scale. The greater portion of this will be in what is called a "latent" state; but it will all re-appear, as is well known from abundant experiments, when the vapor is re-converted into water. From these data it is easy to prove mathematically that every cubic foot of water which falls on the surface of the earth in the form of rain leaves in the air whence it descended sufficient heat to produce at least 6,000 cubic feet of expansion of the surrounding atmosphere beyond the space which the vapor itself occupied. The ascensional force evolved by this process must evidently be immense, when we consider the great amount of rain which falls within the tropics. A similar power is evolved whenever rain falls; and this principle, which has been so ably developed by Mr. Espy, is undoubtedly a true and sufficient cause of most of the violent and fitful agitations of the atmosphere which have so long puzzled the scientific

world. It however in its turn will probably require the consideration of modifying conditions in its applications; and while at present the data are known with sufficient precision to warrant the assumption of the evolution of the immense force we have mentioned, they are not in all cases sufficiently well determined to enable us to predict, with numerical accuracy, the results which have been shown to proceed from them. The same principle of condensation of vapor and evolution of heat is fertile in the explanation of the approximate cause of rain: for example, so long as the wind blows over a surface of uniform height and temperature, there is no cause to induce it to precipitate its vapor; but if in its course it should meet a mountain, the slope of which it is obliged to ascend, the vapor will be condensed on the windward side by the cold due to the increased vertical height. The latent heat will be evolved, the circumambient air will be abnormally heated, and an upward motion will ensue, towards which air will flow with increasing velocity to restore the equilibrium of the ascending column. In this way Mr. Espy explains very satisfactorily the fact that the wind blows over the desert of Sahara to supply the diminished pressure occasioned by the rains over the windward side of the Himalaya mountains. The same principle is immediately applicable to the explanation of the rainless districts in South America, Mexico, and other portions of the earth. The air, as it ascends on the windward side of the mountains, deposits its moisture; and if the elevation is sufficiently high, it will pass over in a desiccated condition.

The idea that mountains attract vapor is not founded on any well established principle of science. Molecular attraction extends only to imperceptible distances, and the attraction of gravitation is too feeble a force to produce results of this kind. The evaporation of water, and the transfer and subsequent condensation of the vapor in other parts of the earth, is undoubtedly the most active cause which produces the continual and apparently fitful changes of the weather.

We have stated that within the torrid zone there exists a

belt of rain, produced by the partial condensation of the vapor which ascends with the air of this region ; and since the sun between the 21st of March and the 21st of June passes from the equator to $23\frac{1}{2}$ degrees north, and then makes a similar excursion as far south, the rainy belt follows his course, and hence all countries within the tropics must have a periodical rainy season.

The air also which flows over to the north, and which, as we have seen, descends to the earth in the westerly belts of wind, carries with it a portion of vapor, and deposits it in the form of rain ; and hence there is a tendency to a rainy and dry season beyond the tropics, which oscillates north and south with the varying motion of the sun. This tendency to regularity of rain is in many places masked or neutralized by the configuration of the country. It is however distinctly marked on the western coast of the United States and of Europe, as well as in various other places in the north temperate zone. Oregon and California have their rainy belt, which descends to the south in the winter, and again returns in the spring. In Lisbon, the number of rainy days in December is 15, to 2 in July ; in Palermo, 17 in December, to $2\frac{1}{2}$ in July. In Algiers, which is also north of the tropic, but farther south, from the average of ten years, there are 18 rainy days in January, and on the other hand, only a single one in July. Another fact of interest with regard to the extra-tropical belt of rain is that it commences sooner at greater elevations above the surface : for instance, at the peak of Teneriffe, the rainy season commences at the top a fortnight earlier than at the bottom ; so that while rain is falling in abundance on the summit, the country in the vicinity of the mountain, at the level of the sea, is enjoying sunshine and a balmy atmosphere. According to Mr. Espy's views, the latter results from the radiant heat given off by the condensing vapor above. The sun however descending still farther to the south brings down the rain belt to the level of the earth in this latitude, and the rainy season then commences. Similar phenomena have been observed on the higher parts of the Coast range of mountains of California ; and indications

of a like action are witnessed on the higher peaks of the Appalachian chain. Besides the causes of the general perturbations of the atmosphere, which we have thus given in considerable detail, some authors have added magnetism and electricity, and others have indeed attributed some of the principal effects we have mentioned to these agencies; but the present state of science does not warrant us in considering these as true or sufficient causes, except in the case of thunder storms, and perhaps tornadoes, in which the electricity evolved by the action of the storm itself may modify some of the results. Electricity however probably plays a subordinate part; since it is itself a consequence, and not a cause.

Terrestrial magnetism has not been shown in any case to affect meteorological phenomena; it is a force which never produces translation, but merely direction of the needle. The air in its natural condition is not magnetic in the proper sense of the term, any more than a piece of steel wire is so before the power has been developed in it by a magnet.

We are not allowed in strict scientific investigations, to explain a phenomenon by referring it to any agent, unless we show, in accordance with the laws of that agent, that it is capable of producing the result; and consequently magnetism is here not admissible.

Currents of the Ocean.—We have seen the effect of the unequal heating of different parts of the earth by the sun in giving rise to great gyrations of air; and it must be evident that there is a tendency to produce a similar result in the aqueous envelope of the globe. Let us first suppose the ocean to cover the whole earth to a uniform depth, and to be uninterrupted by continents. If the earth were at rest and the heat of the surface at the equator could extend down sufficiently into the depths of the water, the latter would be expanded and would stand higher in the equatorial regions than in those of the poles; a current therefore, as in the case of the air, would be established toward the north and south, from the equator, which would be cooled in its passage, would sink to the bottom, and return again to its

starting point, to commence the same course anew. If we now suppose the earth, as in the case of the atmosphere, to be put in motion around its axis towards the east, the bottom currents, or those flowing towards the equator, coming from a part of the earth moving slower to a part going faster, would fall behind, and thus assume a westerly direction. They would therefore ascend obliquely in a westerly direction towards the surface, flow back towards the pole, (in their course curving constantly towards the east,) and as they cooled would sink down towards the bottom, to return again to the equator. Different portions of the upper surface of the current, as in the case of air, would continue their northerly course obliquely, and descend at intervals, some reaching nearly to the poles.

The result of the whole of this action would be a series of gyrations to the north and south, with the upper portion turned towards the west, forming a continuous circuit at the equator round the whole earth in a westerly direction, and a circuit in each temperate zone from the west. This would be the result, if the water could be heated to a sufficient depth; and accordingly it is considered by some that heating the water is the principal cause of the currents of the ocean,—on which account I have so described it. Yet though doubtless a true—I do not consider it a sufficient—cause; but I would ascribe the currents of the ocean mainly to the action of the winds in the belts of the equator and in the two temperate zones.

The constant westerly winds on either side of the equator would tend to produce a westerly current around the earth, provided no obstructions existed to its free course; but if, instead of considering the earth as entirely covered with water, we suppose the existence of two continents, extending from north to south, forming barriers across the current we have described, and establishing two separate oceans, similar to the Atlantic and Pacific, then the continuous current to the west would be deflected right and left, or north and south, at the western shore of each ocean, and would form four immense circuits, namely, two in the Atlantic,

one north and the other south of the equator, and two in the Pacific, similar in situation and analogous in direction of motion. For a like reason there will be a tendency to produce a similar whirl in the Indian ocean, the current from the east being deflected down the coast of Africa, and returning again into itself along a southern latitude on the western side of Australia. Besides these great circulating streams, the water supplied by all the rivers emptying into the Arctic basin, as well as that from all the precipitation in this region, returns to the south, and by the motion of the earth must tend westwardly in a current along the eastern shore of each continent between it and the stream flowing to the north. Similar currents, but more diffuse and less in amount, must constantly flow from the Antarctic regions.

We do not mean to assert that these whirls can be continuously traced on the surface of the ocean, though by attentively examining the maps their general outline may be marked out. We wish to convey an idea of the general tendency of the motions of the aqueous covering of the globe—the central thought, as it were, on which they depend. The regularity of their outline will be disturbed by the configuration of the deflecting coasts and the form of the bottom of the sea, as well as by islands, irregular winds, difference of temperature, and above all, by the annual motion of the sun as it changes its declination. The effect of these currents in modifying the climate of different parts of the world has long been recognized, though the detail of the mode in which this is produced has not until recently been pointed out. The Gulf Stream of the North Atlantic carries the warm water of the equator beyond Iceland and the northern extremity of Europe, and it may even be traced to the shores of Nova Zembla. Without its influence the climate of Norway, Great Britain, and the western coast of Europe would be as cold as that of the corresponding parallels of latitude on the North American continent. In like manner, the great circuit of the waters of the Pacific conveys the warmth of the equator along the eastern coast of Asia to Kamtchatka, and gradually cooling in its course, descends

along the northwest coast of the North American continent, to receive a new accession of heat and be again conveyed to the north. The total result of this circulation together with those of lesser influence in the northern hemisphere, is shown in the annexed polar projection, in which the series of irregular lines, marked 50° , 32° , 16° , and 0° , indicate the mean annual temperature of the points through which they pass, and are called the yearly isothermal lines, or lines of equal heat.



The darker line, marked 32° , indicates the boundary of the region within which the average temperature is below the freezing point. It will be seen at a glance that, instead of being circular in its outline, it has the form of an irregular elongated ellipse, the greater diameter of which is across the pole, from the southern extremity of Hudson's Bay to

the south of Lake Baikal, in Siberia. It extends some degrees lower to the south in Asia than in America. The shorter diameter of the ellipse is at right angles to the longer, and passes from near Behring's Straits, through the pole, to the open ocean west of Norway. Its longer diameter is nearly twice that of its shorter, and is in the direction of the greatest amount of land in the polar regions. This form of the curve and the peculiarities of the other curves are due principally to the currents of the Atlantic and Pacific oceans transporting the water from the equator to the north, and carrying with it the higher temperature. An elliptical dotted line will be perceived in the polar regions, the centre of which does not coincide with the geometrical axis of the earth, but is nearer the continent of North America than that of Asia, thus indicating that the coldest point on the earth's surface is a number of degrees south of the pole. It is true, this region has never been visited by man; yet knowing the law of the diminution of heat, and the form of the other lines, the smaller one can be drawn with considerable accuracy. It may be interesting to remark in this place that the mean temperature of the coldest part of the northern hemisphere has almost exactly the temperature of the zero of Fahrenheit's scale; a somewhat curious although entirely accidental co-incidence.

We have thus far almost exclusively confined our remarks to the general principles of science on which the phenomena of meteorology depend; we shall now give special attention to the application of these principles to the peculiarities of the climate of the continent of North America, and more particularly to that part of it which includes the territory of the United States. For this purpose it will be necessary to give a brief sketch of the topography and surface of the country.

Physical Geography of the United States.—The climate of a district is materially affected by the position and physical geography of the country to which it belongs. Indeed, when the latitude, longitude, and height of a place above the sea, are given, and its position relative to mountain ranges and the ocean is known, an approximate estimate may be

formed as to its climate. The North American continent extends across nearly the whole breadth of the nominal temperate zone, and has an average width of more than fifty degrees of longitude. The general direction of the eastern coast of the United States lies in a great circle passing through Great Britain. Hence, a ship, while sailing along this coast, is on its direct route to the British Isles. This fact—which is not clearly exhibited on the flat surface of a map, but is shown on the convex surface of a globe—has a bearing, not only on commerce, but also on the direction of the Gulf Stream, which conforms to the general direction and sinuosities of the coast. It will be seen by the map,* (to which frequent reference is here made), that the eastern coast of the United States exhibits three great concave curvatures; the first commencing at the extremity of Florida, and extending to Cape Hatteras; the second, from Cape Hatteras to Cape Cod; and the third, from Cape Cod to Cape Sable. These broad ocean bulgings, or bays, have a marked influence on the cold polar current which descends along the coast, and also, as has been shown by Professor Bache, on the great tide-wave of the Atlantic ocean, as it approaches our shore. At the southern extremity of the United States is the great elliptical basin containing the perpetually heated waters of the Gulf of Mexico, an enormous steaming cauldron continually giving off an immense amount of vapor which, borne northward by the wind of the southwest, gives geniality of climate and abundant fertility to the eastern portion of our domain. On the western side of the continent the coast presents, as a whole, an outline of double curvature, principally convex to the west in that part which is occupied by the United States, and concave further north. These bends of the coast-line and of the adjacent parallel mountain ridges affect the direction of the winds in this quarter and consequently of the ocean currents. The Gulf of California at the south, between the high mountains of the peninsula of that name and those of the main land, must also materially modify the direction of the wind in that region.

*[See Map, at page 72.]

The continent of North America is traversed in a northerly and southerly direction by two extensive ranges of mountains—the Alleghany system on the east and the Rocky Mountain system on the west. We give the latter name to the whole upheaved plateau and all the ridges which are based upon it. These two systems separate from each other more widely as we pass northward, and between them is the broad interval which, within the territory of the United States, is denominated the valley of the Mississippi; but in reality the depression continues northward to Hudson's Bay, and even to the Arctic ocean, giving free scope to the winds which may descend from that inhospitable region. It however may be divided into two great basins, one sloping towards the south, comprising the basin of the Mississippi, and the other sloping to the north, including the basins of Mackenzie's river and of Hudson's Bay, the dividing swell which may be traced along the heads of the streams having an elevation of about 1,200 feet. Our remarks must be principally confined to the portion of the continent south of the 49th degree of latitude.

The swell of land or watershed, on which the Alleghanies are situated, has an average elevation of at least 3,000 feet, although the ridges and mountains based upon it rise to a much higher elevation. The loftiest point is Clingman's Peak, of the Black Mountains in North Carolina. It has lately been measured by Prof. Guyot, and is found to have a height of 6,702 feet. The next greatest elevation is Mount Washington, the highest peak of the White Mountains, in New Hampshire, which, according to the same authority, has an elevation of 6,285 feet. The lowest depression in this watershed, with the exceptions to be next mentioned, is in Pennsylvania, and has an elevation of a little less than 2,000 feet. Further north the whole system is cut through by the valley of the Hudson nearly to its base, and also by the valley of the St. Lawrence. The latter, together with the basins of Lakes Ontario and Erie, forms a narrow trough between the Atlantic and the Mississippi valley, along which the flow of air may locally affect the climate. The position

of the Alleghany Mountains however does not so much affect the meteorology of the country as from the magnitude of the system we might at first suppose; and this results from the fact that their direction is from the southwest towards the northeast, which as we shall see hereafter, is the prevailing direction of the fertilizing wind of the United States. They do not therefore obstruct its course; it flows on either side of them and along the valleys between them. They do however in a considerable degree, modify the character of the westerly winds as felt upon the coast, depriving them of their moisture.

A reference to the map will show that the Rocky Mountain system occupies one-third of the entire breadth of the United States, and that the remaining two-thirds are divided into two nearly equal portions by the Mississippi river, beginning at its source. This great western mountain system of the North American continent, which produces the most important modifying influence on the climate of the United States, may be described as a broad, elevated swell or plateau of land, (the prolongation of the system of South America, to which the Andes belong,) extending northward in the general direction of the Pacific coast, with varying elevation and width to the Arctic circle. It occupies nearly the whole breadth of Mexico, from the Rio del Norte to the Pacific, and becomes still broader as it extends northward, occupying at the latitude of 40° , (as has just been said,) one-third of the breadth of the whole continent. Resting upon this great swell of land is a series of approximately parallel ridges, the principal of which are the Rocky Mountain ranges on the east and the Coast ranges on the west, with ridges of less magnitude between, the general direction of which is north, inclining towards the west. Between these ranges is a series of extensive elevated valleys of extreme dryness, and, in the summer, of intense heat.

As we proceed north from the high plains of Mexico, the base of the system declines to about the 32d parallel of north latitude, where its transverse vertical section presents the least amount of land above the general level. It has how-

ever an average elevation in the principal part of about 4,000 feet, and the lowest notch or pass in the ridge on the eastern side is 5,717 feet above the ocean. Along the 35th parallel the vertical section across the mountain system is considerably greater in width and elevation. The general height above the ocean is at least 5,500 feet, and the lowest pass of the principal ridge is here 7,750 feet. The section of the system between the parallels of 38° and 40° has an elevation of 7,500 feet, and the lowest notch in the principal ridge is 10,032 feet above the level of the sea. From this section, as we pass to the north, the altitude and width decline; and along the parallel of about 47° the mountain base is much contracted in breadth, and has a general altitude of 2,500 feet. The lowest pass however of the most elevated ridge of this section is 6,044 feet. We have no definite information as to the mountain base north of this line. It appears however to continue at a lower elevation, and consequently to produce less influence upon the climate of the country to the east of it than the portion within the boundary of the United States.

From the eastern edge of what we have called the mountain system—that is from the foot of the Rocky Mountain chain to the Mississippi river—a space comprising, as was said before, about one-third of the whole breadth of the United States, the surface consists of an extended inclined plain, which slopes eastward to the Mississippi and southward to the Gulf of Mexico, having at the greatest elevation, near the intersection of the parallel of 40° and longitude 105° , a height of upwards of 5,000 feet, whence it gradually declines to the Mississippi river to about 1,000 feet. At the parallel of 35° it has very nearly the same elevation; and thence it slopes to the bed of the Mississippi to about 450 feet, and south to the level of the sea at the Gulf of Mexico. This extended plain is traversed by a number of approximately parallel rivers flowing eastward and southward to the Mississippi river and the Gulf of Mexico, which have their rise principally in the mountain system, and are chiefly supplied by the melting of the snow and the precipitation of vapor

which takes place at the summit of the ridges. The rivers are sunk deeply below the general surface of the plain, and give no indication of their existence from a distance, except the appearance of the tops of the cotton-wood trees which skirt their borders. The surface towards the southeast is slightly diversified by a low range of mountains, denominated the Ozark, which probably have some slight influence on the local climate of Kansas.

General Character of the Surface.—The general character of the soil between the Mississippi river and the Atlantic is that of great fertility, and as a whole, in its natural condition, with some exceptions at the west, is well supplied with timber. The portion also on the western side of the Mississippi as far as the 98th meridian, (including the States of Texas, Louisiana, Arkansas, Missouri, Iowa, and Minnesota, and portions of the Territories of Kansas and Nebraska,) is fertile, though abounding in prairies and subject occasionally to droughts. But the whole space to the west, between the 98th meridian and the Rocky Mountains, denominated the Great American Plains, is a barren waste, over which the eye may roam to the extent of the visible horizon with scarcely an object to break the monotony. From the Rocky Mountains to the Pacific, with the exception of the rich but narrow belt along the ocean, the country may also be considered, in comparison with other portions of the United States, a wilderness unfitted for the uses of the husbandman; although in some of the mountain valleys, as at Salt Lake, by means of irrigation a precarious supply of food may be obtained sufficient to sustain a considerable population, provided they can be induced to submit to privations from which American citizens generally would shrink. The portions of the mountain system further south are equally inhospitable, though they have been represented to be of a different character. In traversing this region whole days are frequently passed without meeting a rivulet or spring of water to slake the thirst of the weary traveller. Dr. Letherman, surgeon of the United States army, at Fort Defiance, describes the entire country along the parallel of 35°

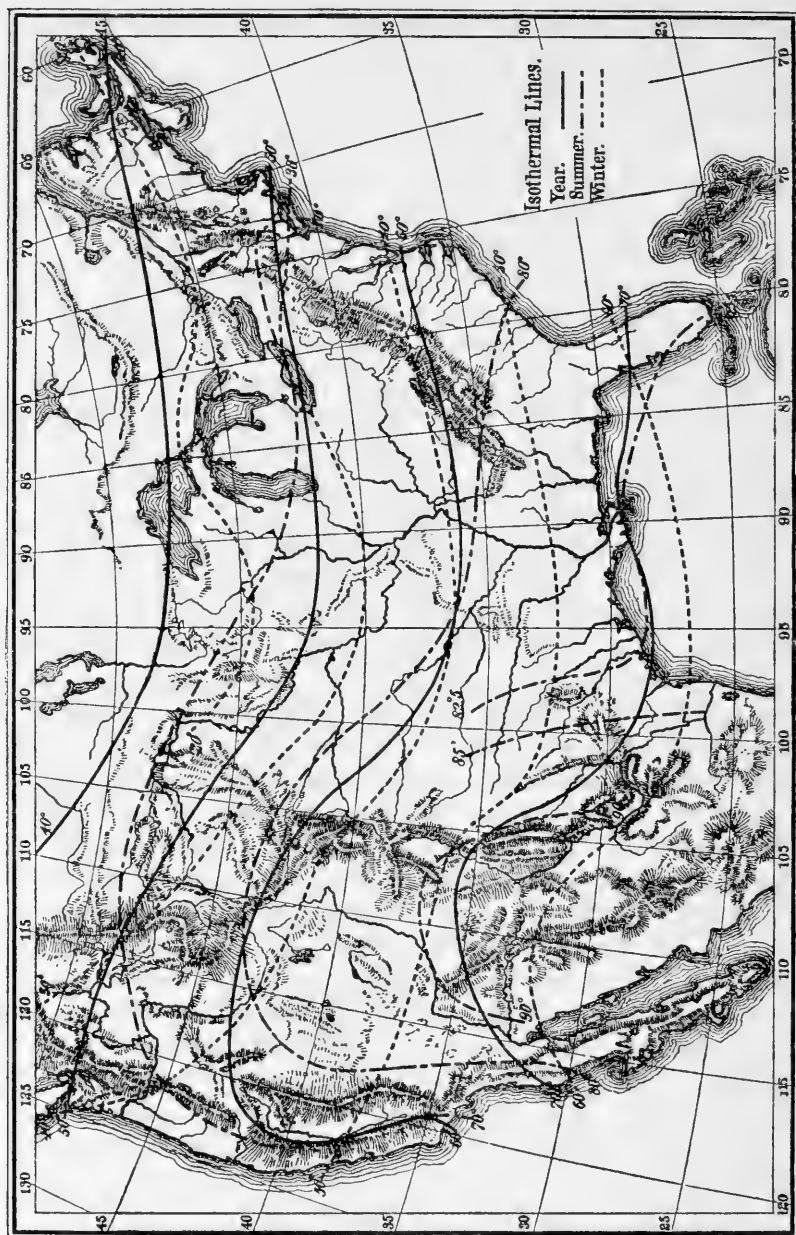
as consisting of a series of mountain ridges, with a general direction north and south inclining to the west, and broken in many places by deep cracks, as it were, across the ridge, denominated cañons, which afford in some cases the only means of traversing the country, except with great labor and difficulty. The district inhabited by the Navajo Indians has had the reputation of being a good grazing country, and its fame has reached the eastern portions of the United States; but taking the region at large, it will be found that with regard to abundance of natural pasturage, it has been vastly over-rated, "and we have no hesitation in stating," says the same authority, "that were the flocks and herds now belonging to the Indians doubled, they could not be sustained. There is required for grazing and procuring hay for the consumption of animals at Fort Defiance, (garrisoned by two companies, one of which is partly mounted,) fifty square miles; and this is barely sufficient for the purpose." The barrenness and desolation so inseparably connected with immense masses of rocks and hills scantily supplied with water are here seen and felt in their fullest extent. Dr. Antisell, geologist to one of the exploring expeditions, describes the country along the parallels of 32° and 33° as equally deficient in the essentials of support for an ordinary civilized community. On the west, within these parallels, occurs the Great Colorado desert, extending to the river of the same name, which empties into the Gulf of California. From the southern portion of the Colorado river, which is generally regarded as the eastern edge of the Colorado basin, the land rises eastward by a series of easy grades until the summit of the main ridge of the mountain system is gained, at a point about 500 miles east of that river. For the first 250 miles the ascent is across a series of erupted hills of comparatively recent date, and similar in constitution to the line hills and ridges which are dotted over the various levels of the basin country. The entire district is bare of soil and vegetation, except a few varieties of cactus. Over the greater portion of the northern part of Sonora and the southern part of New Mexico sterility reigns supreme.

At the mountain bases may exist a few springs and wells, and in a few depressions of the general level of the surface sloping to the Pacific may be grassy spots; but such are the exceptions. A dry, parched, disintegrated sand and gravel is the usual soil, completely destitute of vegetable matter and not capable of retaining moisture. The winter rains which fall on the Pacific coast, west of the Coast range of mountains, do not reach to the region eastward. This is partly supplied with its moisture from the Gulf of California, but chiefly by the southeast wind from the Gulf of Mexico, flowing up between the ridges of mountains. We hazard nothing in saying that the mountains, as a whole, can be of little value as the theatre of civilized life in the present state of general science and practical agriculture. It is true that a considerable portion of the interior is comparatively little known from actual exploration; but its general character can be inferred from that which has been explored. As has been said before, it consists of an elevated swell of land covered with ridges running in a northerly direction inclining to the west. The western slopes, or those which face the ocean, are better supplied with moisture and contain more vegetation than the eastern slopes; and this increases as we approach the Pacific, along the coast of which, throughout the whole boundary of the United States to the Gulf of California, exists a border of land of delightful climate and of fertile soil varying from 50 to 200 miles in width. The transition however from this border to a parallel district in the interior is of the most marked and astonishing character. Starting from the sea-coast and leaving a temperature of 65° , we may, in the course of a single day's journey in some cases, reach an arid valley in which the thermometer in the shade marks a temperature of 110° . We have stated that the entire region west of the 98th degree of west longitude, with the exception of a small portion of western Texas and the narrow border along the Pacific, is a country of comparatively little value to the agriculturist; and perhaps it will astonish the reader if we direct his attention to the fact that this line, which passes southward

from Lake Winnipeg to the Gulf of Mexico, will divide the whole surface of the United States into two nearly equal parts. This statement, when fully appreciated, will serve to dissipate some of the dreams which have been considered as realities as to the destiny of the western part of the North American continent. Truth however transcends even the laudable feelings of pride of country; and in order properly to direct the policy of this great confederacy, it is necessary to be well acquainted with the theatre on which its future history is to be enacted and by whose character it will mainly be shaped.

Temperature.—Let us now consider the distribution of temperature of the wide belt across the continent of North America which forms the territory of the United States. To illustrate this, attention is requested to the lines drawn from east to west across the small map so frequently referred to. These it will be seen, are of three kinds: first, the full line, indicating the mean or average temperature of the year; second, the broken line, denoting the mean temperature of summer; and third, the dotted line, that of winter. These lines are drawn through portions of the earth's surface having equal temperatures for the periods mentioned, and are plotted from the result of numerous observations. They do not however in all cases exhibit the actual temperature of the surface; for in order to show their relations and render them comparable with each other and with similar lines in other parts of the world, it is necessary that the observed temperatures in elevated positions should be reduced to that of the level of the sea; and in the construction of this map allowance has consequently been made for decreasing temperature of one degree for every 333 feet of altitude. The map therefore will present to the eye the lines along which the temperature of the air would be equal for the periods mentioned, were we to suppose the mountain ranges entirely removed and the air brought down to the level of the ocean.

These lines, at a glance, exhibit remarkable curvatures, particularly in the western portion of the United States, indi-



ating a great increase of temperature in this region beyond that of the eastern and middle portion. Let us first consider the dark lines representing the mean temperature of the year. These, and indeed all the lines, are given for each ten degrees of Fahrenheit. Too much complication would be introduced were lines drawn for intermediate degrees on so small a map, though such lines have been projected on a larger one from which this has been reduced.

The first dark line, beginning at the top of the map, is that of the mean temperature of 40° . It commences near the northern part of Nova Scotia, passes through Canada and the middle of Lake Superior, slightly diverging from parallelism with the line of 45° of latitude until about the 95th meridian, when it more rapidly curves northward and leaves the United States for the British Possessions at about the 103d meridian, passing out at the top of the map at the 110th. The next line of mean temperature is that of 50° . It commences a little south of Nantucket, passes almost directly west, nearly parallel to the line of the 40th degree of north latitude, to about the 95th meridian of west longitude, whence it curves more rapidly to the north, meeting the coast of the Pacific in about the 48th degree of north latitude, near Puget's Sound. It thus exhibits the fact that the mean temperature of a point near Rhode Island is the same as that of a point on the Pacific, at least six degrees of latitude further north. The next line of mean temperature for the year, given on the map, is that of 60° ; commencing near the mouth of Chesapeake Bay it inclines a little downward toward the 35th parallel of latitude until the meridian of about 98° , whence it rapidly ascends to the north, gains its greatest altitude at the 115th meridian, thence gradually declines southward to about the 125th, and thence, with a remarkably short bend, it passes parallel to the coast to about the latitude of 34° . By comparing the course of this line with that of the 35th parallel, it will be seen that the mean temperature is a little less near the Mississippi river than it is on the seaboard; but that in the great mountain system, in the same latitude as the mouth of the Chesapeake, the

temperature of a place is nearly equal to 70° instead of 60° , since the curve of 70° reaches almost as far north. The curve of the mean temperature of 60° , as has been stated, terminates on the shores of the Pacific, at about latitude 34° ; whereas, on the Atlantic, it commences at about 37° , indicating a lower temperature along the 35th parallel of latitude on the Pacific than on the Atlantic shore. The next is the curve of 70° . This commences in about latitude 28° on the coast of Florida, passes through New Orleans, and thence to a point on the Pacific in the latitude of 30° . It presents an upward curvature in that portion which passes through the Gulf, indicating that New Orleans is warmer than a corresponding place on the Atlantic, or on the shores of Texas. It thence curves rapidly to the north, though indicating the greatest temperature near the eastern edge of the mountain system. It terminates on the Pacific at a point at least two degrees higher than its point of commencement on the Atlantic, thereby indicating that along the 30th parallel the mean temperature is a little greater on the east than on the west side of the continent. It should be constantly borne in mind, that the temperatures in these descriptions are those which would be exhibited were the mountain system of the country removed and the whole reduced to the level of the ocean. This system of lines therefore exhibits the extraordinary fact that eliminating the effect due to elevation, there remains a cause of a remarkable degree of abnormal heating beyond that due merely to the latitude of the place. In other words, that at every point within the mountain system, whatever may be its elevation, the temperature is far above that of the same elevation of a point in free space having the same latitude, when compared with the eastern and western coast.

The broken lines indicate the temperatures of summer. The first of these given on the map is that of 70° and commences near Long Island, ascends rapidly towards the north, and then descends towards the large lakes, passing through Lake Erie; it reaches its greatest northern declination at about the 110th meridian, and thence turning nearly paral-

lel to the coast, meets the Pacific in the latitude of about 34° . The portion of this curve along the coast of the Pacific shows the remarkable fact that the summer temperature is nearly the same from latitude 32° to 45° , or through a distance of 13 degrees, the whole having the same temperature as that of 41° on the Atlantic coast. This curve also clearly exhibits the great effect which the vicinity of the lakes has on the temperature of summer. While the dark lines indicating the mean temperatures of 40° and 50° are not at all affected by their proximity to these large bodies of water, the mean temperature of the summer is materially reduced. We may here call attention to the fact that the dotted line, denoting the winter, suddenly bends up at the same place, indicating an increase of temperature due to the vicinity of the same reservoirs of water. The line of 80° commences near Charleston, South Carolina, and extends rapidly upward through the valley of the Mississippi, thereby indicating that the temperature of summer in the interior, along this parallel, is much higher than on the seaboard. The western portion of this curve also exhibits great intensity of summer heat in the mountain system, and a remarkable degree of uniformity along the coast range of mountains parallel to the Pacific. The short lines of $82^{\circ}5$ and 85° denote a high temperature of uniform intensity, extending to the north, and indicate the great summer heat of the western plain.

It will be seen, by examining the dotted lines, that the temperature of winter in the middle of the Mississippi valley, about the 95th meridian, is lower than on either the eastern or western coast; also, that the line of 30° , which is only two degrees below freezing, starts at the east end of Long Island, passes through Lake Erie, thence down to the 40th parallel, in longitude about 91° , and thence rapidly rises to the north, and leaves the United States at the 118th meridian. The line of 40° of winter temperature commences at the mouth of the Chesapeake, follows nearly the same general direction, and meets the Pacific Ocean near Puget's Sound, indicating the remarkable fact that this place and Norfolk, on the Atlantic, have about the same winter temperature. The line

of 50° is also similar to that of the last; also the line of 60° , which indicates in the Gulf of Mexico a lower degree of temperature in winter than exists on the Atlantic or Pacific coasts. In examining these winter lines attentively, it will be seen that the rise is not uniform from the 95th to the 105th degree, but the bend is most sudden about the 103d; which is probably caused by the occasional descent along this region of the polar winds to the Gulf of Mexico.

It has been stated that in reducing the lines to the level of the sea, 333 feet of elevation have been taken for each degree of Fahrenheit's scale. Therefore the actual temperature of any part of the United States may be readily determined, provided its elevation above the sea is known, by subtracting from the temperature given on the chart as many degrees as there are spaces of 333 feet in the elevation. Let us take, for example, the junction of the Kansas with the Missouri river, on the 95th meridian. This point, it will be seen by inspecting the map, is midway between the mean isothermal lines of 50° and 60° , and its temperature will therefore be approximately 55° . It has an elevation of about a thousand feet, which will give three degrees for the reduction; and hence its temperature will be about 52° .

On a little reflection it will be clear that it would have been impossible to draw these lines on the uneven surface of the earth. The variation of temperature due to height would mask that due to latitude and other climatological causes. For example, a greater elevation of mountain peaks at the south would represent a colder local temperature than regions further north, would entirely hide from view the results which are due exclusively to the peculiarities of conformation of the country, and would give no means of comparison.

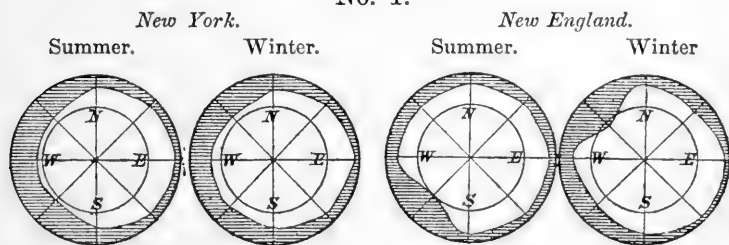
Winds of North America.—We have said that the whole mountain system of the western portion of the United States presents a remarkable abnormal elevation of temperature above the eastern and middle portions of the continent, and the question naturally presses itself upon us as to the cause of this surprising difference. The simple statement that the

western side of Europe is also warmer than the eastern side of Asia does not explain the phenomenon; it merely points out an analogy, but not a cause. It is evident that the position of the mountain system, and the direction of the ridges with reference to the prevailing winds, must have some connection with this phenomenon. In addition to this, the westerly aerial current, as it is principally derived from the equatorial regions, must in itself be warmer than the temperature due to the latitude of the belt in which it is moving. It will be well, therefore, before proceeding to this branch of the subject, to give a brief statement of some of the results which have been reached by deductions from actual observations in regard to this powerful agent in modifying climate. For the materials used for this purpose we are indebted to the valuable labors of Prof. James H. Coffin, of Lafayette College, the results of which have been published by the Smithsonian Institution.*

In order that the facts may be the more readily comprehended, and produce a more indelible impression upon the mind, since ideas received through the eye are the most definite and lasting, we shall represent the direction and amount of the wind by means of diagrams such as are exhibited in the accompanying figures. The lines indicated by the letters *N. E. S. W.* represent the cardinal points of the compass, and the breadth of shading along any of these lines the relative amount of wind in the course of a given period observed at a particular place.

Thus for example in No. 1, in the circle on the right hand

No. 1.

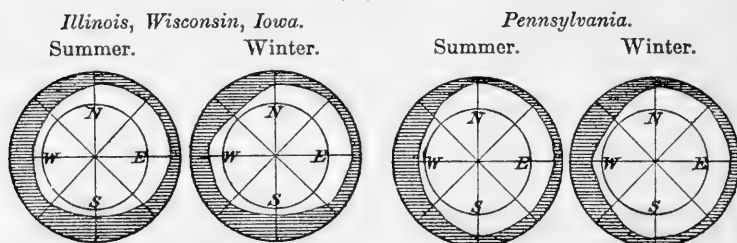


*["The Winds of the Northern Hemisphere." 4to. 198 pp. 13 maps and plates. Smithsonian Contributions to Knowledge; vol. vi.]

side, the shading represents the amount of wind from the different points of the horizon during the winter months in New England, from the average of a large number of observations at different places. Hence it will be seen that the predominant wind during the winter, in this part of the United States, is from the northwest; the next in amount is from the northeast and southwest, the eastern and southwestern portion of the horizon during the winter exhibiting but little wind. The next circle to the left shows the great preponderance of wind in New England from the southwest during the summer. The winds exhibited in the two circles combined will produce a general resultant from the west. The next circles to the left exhibit the amount of wind in summer and winter in the State of New York. In winter the greatest amount is from the northwest, and in summer from the southwest.

No. 2 presents the winds in Pennsylvania, and in Illinois, Wisconsin, and Iowa.

No. 2.



From these it will be seen that in Pennsylvania the wind is more westerly in winter than in New England, but still the greatest amount is from a point north of west. In summer the greatest amount is found a little south of west. During winter in the States of Illinois, Wisconsin, and Iowa, generally, the greatest prevalence is from the northwest, and in summer from the west and south. The maximum is a little east of south; the southwestern half however of the horizon in both seasons has the greatest amount.

The circles in No. 3 indicate that in Nebraska and Kansas the greatest amount of wind in the winter is from the

northwest, and in the summer from the southwest. In Oregon and Washington Territories the greatest amount of

No. 3.

Oregon and Washington Territories.

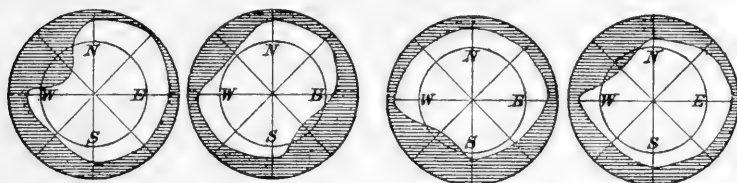
Summer.

Winter.

Nebraska and Kansas Territories.

Summer.

Winter.



wind in the winter is from the southeast, and the next greatest from the northwest, these two principally dividing the season between them. In summer a very large proportion is from the northwest, which is a remarkable inversion of the winds as observed in other parts of the United States. The principal current in winter being in the direction of the coast, from the southeast, consequently tends to mitigate the cold; while in summer it is in the opposite direction, and therefore tends to produce a similar effect in diminishing the intensity of the heat.

No. 4.

Texas and New Mexico.

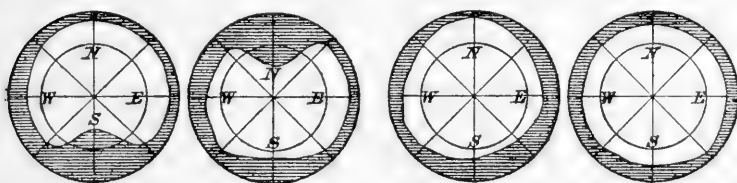
Summer.

Winter.

S. C., Ga., Ala., Miss.

Summer.

Winter.



In No. 4 the two circles to the right exhibit the general direction of the wind in South Carolina, Georgia, Alabama, and Mississippi; and those on the left, in Texas and New Mexico. In the former the winds in winter nearly equally divide the whole circumference of the horizon; in summer the south and southeast winds prevail. In Texas and New Mexico the wind in the winter is largely from the north, and often

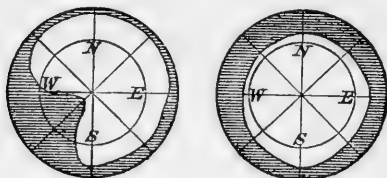
from the south; in summer its preponderance is greatly in favor of the south.

No. 5.

Lower California.

Summer.

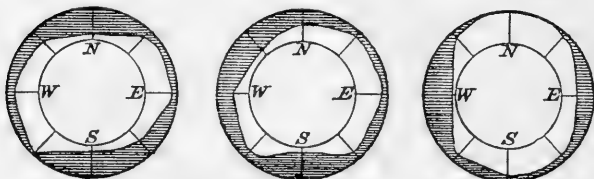
Winter.



No. 5 exhibits the winds of Lower California, which in winter are from all parts of the horizon; those from the north and west however preponderating. In summer it is almost entirely from the southwest.

The winds thus represented are surface currents, and are consequently much influenced by the position of mountain ranges. This is strikingly shown in No 6, which represents the mean annual wind at Hudson, Albany, and Utica, in the State of New York.

No. 6.

Hudson, N. Y.
8 years.*Albany, N. Y.*
12 years.*Utica, N. Y.*
12 years.

Hudson is in the valley of the Hudson river, a long, narrow glen extending in a north and south direction; and as the figure indicates, the winds are principally confined to the same course, blowing down the glen to the south in winter, and in the opposite direction in the summer. Albany is situated at the junction of the wide Mohawk valley with that of the Hudson, and the wind accordingly is from the northwest and from the south. Utica is in the valley of the Mohawk, which has a general east and west direction, the

influence of which is strongly marked by the prevailing winds. In a like manner the direction of the wind on the coast of the Pacific is modified by the trend of the coast and the parallel mountain chains. Almost every position at which meteorological observations are made is liable thus to be affected by the local topography; but the result of this is eliminated in a great measure by computing the average direction from a number of stations within a limited distance of each other. Yet, though in this way the opposite local influences in particular districts may be made to balance each other, those of great mountain systems still remain. These in turn however may be merged in a series of observations extending across continents, or entirely around the world. In this way, by collecting all the reliable observations which have been made on the winds in the northern hemisphere, so far as they were accessible to the Smithsonian Institution, Prof. Coffin has established the fact, before mentioned, that the resultant motion of the surface atmosphere between latitude 32° and 58° in North America is from the west, the belt being twenty degrees wide, and the line of its greatest intensity in the latitude of about 45° . This however must oscillate north and south at different seasons of the year with the varying declination of the sun. South of this belt, in Georgia, Louisiana, &c., the country is influenced at certain periods of the year by the northeast trade winds, and north of the same belt by the polar winds, which on account of the rotation of the earth, tend to take a direction toward the west. It must be recollected that the westerly direction of this belt here spoken of is principally the resultant of southwesterly and northwesterly winds alternately predominating during the year.

From what has been stated in regard to the general circulation of the atmosphere it would appear that these winds are due to the returning upper currents which flow over from the heated region of the equator, producing a southwest, a west, or a northwest wind, according to the distance to which they extend northward before they commence to descend to the earth. If the sun continued on the equator during the

year, and there were no obstacles to the free motion of these currents, they would be constant in intensity and direction around the whole earth; but the change in declination of the sun, and the obstacles opposed by continents and mountain chains, modify in an important degree the simplicity of this motion. When the sun ascends to the north, it carries with it the whole circulating system of the atmosphere, causes the northeast trade winds to invade the southern part of the United States, and the inferior currents, which give rise to the southwest wind, to flow in summer over a large portion of our territory. The latter, charged with the vapor from the Atlantic and the Gulf of Mexico, impart warmth and fertility to all parts of the surface on which they descend. The higher currents, which produce the west and northwest winds, flow in summer above us, to descend further to the north. Their course however is marked by the almost invariable direction of the upper clouds and of the summer thunder storms, which, in the greater part of the United States, pass from the west to the east. The curving course of the returned currents, when the sun is south of the equator, is perhaps best marked by the direction of the hurricanes, which exactly follow the path we have described as that of the particles of air in the general circulation so often referred to. This will be seen by examining the storm tracks on one of the maps of the lamented Redfield.

It is evident, from theory as well as from every day observation, that the currents of the belt of the northern hemisphere, in which the United States is situated, must be subject to many perturbing influences, and that this region is well entitled to the denomination of the zone of variable winds. While the great circulation which we have described is going on, particularly above us, every rain that occurs and every variation of temperature tends to disturb its regularity at the surface of the earth. According to the views here presented the following winds of the United States belong to the general circulation, namely, the southwest, west, northwest, north, and northeast; while those from the opposite quarters of the horizon are principally due to abnormal

atmospheric disturbances. We say principally, because a portion of the surface northeast trade wind in summer probably blows over Florida and the lower part of Louisiana. These views have been strengthened by a series of observations collected by M. De Doue, from which it is shown that the winds from the western half of the horizon, as indicated by the clouds, preponderate over those from the east, as indicated by the wind vane at the surface; or in other words, that there is a greater tendency to a movement, even in our latitude, in the upper strata of air from the western half of the horizon, and in the lower from the eastern—a result in conformity with the general principles we have endeavored to explain. The circulation in the region of variable winds may often be inverted, and the compensation take place by means of winds in different parts of the hemisphere. It must be evident from mechanical principles that to balance every current of wind which flows to the north over any parallel of latitude along any meridian an equal amount must flow back to the south either along that meridian or some other. If the compensation takes place at the same meridian, one current must flow above and the other below. If at different meridians, the compensating currents may both be at the surface or both above. The fact that very different temperatures prevail at different parts of the world at the same time under the same latitude favors the idea of Prof. Dove that the compensation does in many cases take place in the latter way. Mr. Espy supposes that our southwest wind is produced mainly by the descent of the return trade winds at about the 30th parallel, and by rains accompanied with an elevation of temperature, and consequently an ascent of air at the parallel of 58° or 60° , and that it returns again in an upper current over the belt we have described towards the south. That whatever air reaches the polar regions should descend there and flow southward, and then rapidly decline to the west, appears to be an evident consequence of well established laws. The rapid inclination of the air on account of the great increase of rotation in the surface of the earth in this latitude would

tend to produce a wind in a westerly direction along the parallel of 60° , which would conflict with the currents from the south, and thus produce a low barometer—a tendency to rain—and form a natural boundary between what may be denominated the polar winds and the belt of westerly winds, due, as we have supposed, to the returning trades. The region of the middle belt must be one of great irregularity, occasionally encroached upon by the polar winds of the north on one side and the inter-tropical winds of the south on the other, tending to restore the equilibrium in some cases in the mode suggested by Prof. Dove, and again in that proposed by Mr. Espy. We are however inclined to believe that all these are perturbations in the general circulation.

That the great western mountain system of North and Central America produces an important effect on these currents cannot be doubted, when it is recollected that one-third of the whole atmosphere is below its higher portions. It prevents the northeast trade wind from passing to the coast of the Pacific in about the latitude of 30° , and probably deflects northeastward a part of the lower portion of the upper return wind, giving more force and quantity to the southwest summer currents than they would otherwise have. This is the view adopted by Mr. Robert Russell, of Scotland, one of the most industrious and promising of the younger meteorologists of Europe, who visited this country about three years ago for investigating its climate and agriculture. It would appear from what has been stated before, that a northwest current most generally prevails in the higher regions, and that the southwest current is a more superficial one. According to Mr. Russell, all the disturbances of the atmosphere in this country are produced by the unstable equilibrium occasioned by the superposition of the northwest wind on that of the southwest; and this, we think, in connection with the evolution of heat, according to the principles of Mr. Espy, will account for all the violent commotions of our atmosphere, whether they appear in the form of winter storms, thunder gusts, or tornadoes.

METEOROLOGY IN ITS CONNECTION WITH AGRICULTURE.

PART III.—TERRESTRIAL PHYSICS AND TEMPERATURE.

(Agricultural Report of Commissioner of Patents, for 1857, pp. 419-506.)

We intend in this number of our contributions to Meteorology as applied to Agriculture, to give a more definite exposition of some of the general principles of science especially applicable to this subject than is usually met with in elementary works. And we are lead to this by numerous inquiries from correspondents in various parts of our country, whose interest in the study of meteorology has been awakened during the last few years. We trust that our essay will be acceptable to the agriculturist, since however remote from his pursuits the theoretical part of the communication may at first sight appear, a proper view of the relation of science and art will enable him to see that the one is dependent on the other, and that each branch of the study of nature is intimately connected with every other.

We take it for granted that the American farmer is capable of logical reflection; that he is not content with the ability merely to perform with facility agricultural operations, and to direct with skill the ordinary routine of his farm; but that he is also desirous of knowing the rationale or scientific principles of all the processes he employs. We have no sympathy with the cant of the day with reference to "practical men," if by this term is understood those who act without reference to well-established general laws, and are merely guided by empirical rules or undigested experience. However rapidly and skilfully such a person may perform his task, and however useful he may be within the limited sphere of his experience and in the practice of rules given by others, he is incapable of making true progress. His attempts at improvement are generally not only failures, involving a loss of time, of labor, and of materials, but such as could readily have been predicted by any one having the requisite amount of scientific information. It is the due combination of theoretical knowledge with practical skill

which forms the most efficient and reliable character, and it should be the object of the agricultural colleges which are about being established in various parts of our country to produce educational results of this kind.

It is not expected that the farmer is to be a professional scientist, but that he should be familiar with the general principles of all branches of knowledge which more especially relate to his occupation; and the wider the extent of his information the better. Above all, he should be qualified to form a just appreciation of the value of original scientific investigations, and be ready at all times to adopt the principles which they may unfold, so far as they may be applicable to his uses; and moreover, be willing to render a due acknowledgment for the benefits thus conferred, and to contribute in any way in his power to the necessary, if not liberal, support of those who seek without the hope of pecuniary reward, to advance the bounds of human knowledge and of human power. The number of those in any age and in any country, who successfully investigate nature and discover new truths which form valuable contributions to the existing stock of knowledge, is comparatively small. The successful labor of the hands is much easier than that of the head; and therefore those who have actually proved by what they have done that they possess the ability to enlarge the field of science should be especially cared for, and their energies husbanded and directed to the one pursuit to which they may have devoted their attention. Unfortunately however there has always been in England and this country a tendency to undervalue the advantages of profound thought, and to regard with favor only those investigations which are immediately applicable to the wants of the present hour. But it should be recollected that the scientific principles which at one period appear of no practical value, and are far removed from popular appreciation, at a later time, in the further development of the subject, become the means of individual prosperity and national wealth.

About fifty years ago, Sir Humphry Davy moistened a

small quantity of ordinary potash, and submitting it to the current of a powerful galvanic battery, observed a number of brilliant particles burning and exploding on the surface. With the intuitive perception of a highly philosophical mind, he saw at once in this experiment a fact of the deepest significance,—the verification of a previous *a priori* hypothesis, namely, that potash and the other alkalies and alkaline earths were not simple substances, as they had previously been considered, but metals compounded with oxygen. This discovery, which had an important bearing on the whole science of chemistry but which had no interest for the popular mind, has in the course of time, revolutionized many of the processes of art, and will furnish the means, in various ways, of adding to the comforts and conveniences of life. Within the last two years a French chemist has discovered a process of decomposing one of these alkaline earths, (namely, the clay which forms the basis of the soil of the farmer, and which hardened by fire constitutes the brick to build his tenement,) and of obtaining from it a metal as light as glass, as malleable and ductile as copper, and as little liable to rust as silver.* These discoveries were made by men whose lives were devoted to the abstract study of nature; they were not the results of accident, but were logical deductions from previous conceptions of the mind verified and further developed by the ingenious processes of the laboratory. It may be safely said, that for every one individual who is capable of making discoveries of this kind, there are at least a thousand who can apply them to useful purposes in the arts, and who will be stimulated to undertake enterprises founded upon them by the more general and powerful incentive of pecuniary reward. When the process of procuring aluminum (or the metal from clay) shall have been perfected, and some enterprising citizen shall have established a great manufactory for the production of the article for general use, he will confer a benefit on his country, be entitled to credit, and will probably re-

* [Aluminum though first separated by Woehler in 1828, and more perfectly in 1845, was first made available by Deville in 1855.]

ceive the desired remuneration. But should the names of the chemists who originally made the discovery of the principles on which this public benefit depends be forgotten? Ought not their labors in enlarging the bounds of knowledge to be properly valued, and their names held in grateful remembrance? If living, should they not be afforded the means of extending their investigations, without the distraction of mind attendant on the efforts to obtain a precarious livelihood for themselves and families?

In truth we must say—not in the way of complaint, but for the purpose of drawing attention to the fact and with the hope of somewhat changing the condition of things in this respect—that in no civilized country of the world is less encouragement given for the pursuit of abstract science than in the United States. The General Government has no power under the Constitution to directly foster pursuits of this kind; and it is only by an enlightened public opinion, and the liberality of wealthy individuals, that a better condition of things can be hoped for.

The great facts of the future of agriculture are to be derived from the use of the microscope, the crucible, the balance, the galvanic battery, the polariscope, and the prism, and from the scientific generalizations which are deduced from these by the profound reflections of men who *think*, in contra-distinction to the efforts of those who *act*. The intelligent farmer should be able (as we have already said) properly to appreciate the value of scientific discoveries; and for this purpose his studies should not be confined merely to rules or empirical receipts, but should comprehend also the general principles on which they are founded.

Though some of the points we shall discuss in the following essay may appear at first sight to be of too abstract a character to be comprehended by a casual reader, yet they will be found on attentive perusal, to be easily understood by a person of ordinary intelligence. But it may be well here to call attention to a fact frequently overlooked, that there is a great difference between *reading* and *study*, or between the indolent reception of knowledge without labor, and that

effort of mind which is always necessary in order to secure an important truth and make it fully our own.

Constitution of Matter.

Laws of force and motion.—All the objects which are presented to us in the material universe, and all the changes which we observe taking place continually among them, whether those which immediately surround us or those which we perceive at a distance, either by the naked eye or by means of a telescope, are referable to two principles—*matter* and *force*. By matter, we understand the substratum of that which affects our senses; and by force, that which produces the changes which we constantly observe in the former. The idea of force was probably first suggested to us by our muscular exertions: and indeed the original meaning of the term is a muscle or tendon; the Latin *vis* (force) being probably derived from the Greek *ἰς*, or *ἴς*. But we cannot imagine a force without some bodily substance by which, or against which it is exerted; the two ideas therefore of matter and force are co-existent in the mind, and on a clear and definite conception of them depends that precise relation of the phenomena of nature denominated *science*. Though the *essence* of force and matter may never be known to us, we can study the laws by which they are governed, and adopt such a conception of the constitution of matter as will enable us to generalize a vast number of facts; to connect these with each other, or with a central thought; to perceive their dependencies, and thus in some cases to control phenomena; to relieve the memory, and call into play the reasoning powers; and finally, to predict new facts, the existence of which had never yet been proved by actual experience. But such a generalization must be based on the well-established principles of the laws of force and motion, and be in strict accordance with accurately ascertained facts in the various branches of physical inquiry, in order that it may be an exact expression of the apparent cause of the phenomena, and that the prediction from it may be true in measure as well as in mode.

The laws of force and motion, to which we have alluded, may be expressed as follows:

LAWS OF FORCE.

1. Every particle of matter, at a sensible distance, attracts every other particle with a force varying inversely as the square of the distance. In the phenomena of electricity and magnetism, repulsion is also exhibited, acting in accordance with the same law.

2. Particles of matter at insensible distances, attract and repel each other with great energy, the attractions and repulsions appearing to alternate with minute changes of distance.

LAWS OF MOTION.

1. *The law of inertia.*—A body at rest tends to remain at rest, and when put in motion by the application of any force tends to move forever in a straight line with a uniform velocity.

2. *The law of the co-existence of motions.*—A body impelled at the same moment by several forces in different directions, will at the end of a given time be in the same position as if the forces had each acted separately.

3. *The law of action and re-action.*—When a force acts between two bodies of different masses, their momenta will be equal and opposite.

These laws were first given to the world in a definite form by Sir Isaac Newton in his *Principia*. They are ultimate facts of science, of which no satisfactory explanation is given; but by adopting them, as we do the axioms of geometry, and reasoning downward from them, all the great truths of modern astronomy have been evolved, as well as many of the facts of the molecular action of bodies.

Atomic Theory.

In connection with the laws of the forces and motion of matter, given above, we shall venture in this essay to express some of the widest generalizations of the present day in the form of what is called the *atomic theory*. This was the original conception of an imaginative Greek philosopher,

but in his mind it did not take that definite character which it has since assumed under the influence of inductive science. It was with him the vague and indefinite product of the imagination, unconditioned by the actual phenomena of Nature. It was adopted by Newton, who employed it with much success in the different branches of his investigations; but in modern times it owes its greatest development and range of application, to Dr. John Dalton, of Manchester, England, and still later principally to Mr. James Joule and Professor William Thomson. By means of it we are enabled to present in a single line a series of facts which could not otherwise be expressed in many pages, and also to exhibit to the mind the connection of a series of phenomena which could not, without this aid, be definitely conceived. It is intimately connected with all branches of physical science, and (strange as it may appear) particularly with agriculture; and we may therefore be excused for presenting it in its broadest applications, and with considerable detail.

According to this theory every portion of the whole universe, or at least that part of it which is accessible to us by means of the telescope, is occupied by atoms inconceivably minute, hard, and unchangeable, definitely separated from each other by attraction and repulsion. This assemblage of atoms constitutes the substance of the material universe; and to their attractions and repulsions, the forces by which they are actuated, is referable all the power or energy which produces the changes to which matter is subjected.

These atoms, thus endowed, form a plenum throughout all space, constituting what is called the ætherial medium, and in it, at wide intervals from each other, are isolated masses of grosser matter, which constitute our world, the planets, the sun, and stars. These also consist of atoms of another order, or of groups of atoms, with spaces between them, wide in comparison with the size of the atoms, which spaces are pervaded by the minuter atoms of the ætherial medium. These bodies move in the medium without encountering any sensible resistance.

The various isolated bodies of the universe act upon each

other by means of the force of gravitation, and also by tremors or vibrations in this medium, radiating in every direction from each body as a centre.

The atoms of matter are thus separated by intervals; and before we proceed further it will be necessary to consider more particularly this separation. It must be recollected that the hypothesis we are presenting is not the mere creature of the imagination, but is based upon a generalization of actual observation on the different states of grosser matter. We shall therefore commence with the consideration (as an example) of the constitution of the air. This we assume to consist of atoms, each endowed with attracting and repelling forces. That these atoms are not in contact with each other, will be evident from the fact that if we apply a sufficient pressure to a quantity of air taken at its greatest known rarity, it may be compressed into at least one ten-thousandth part of its primitive volume. The sum of the magnitudes of the void spaces is therefore, in this case, at least ten thousand times greater than the sum of the material parts, whatever be their nature. In order to explain this we are obliged to suppose that each atom is endowed with a repulsive force similar to that possessed by one pole of a magnet for a similar pole of another magnet. And this repulsion increases with the diminution of distance between the atoms. It is feeble when the volume of air is expanded to its fullest extent, and exceedingly powerful when highly compressed. Whatever weight we may put on the top of a piston fitted to a cylinder filled with air will be sustained by the repulsion of the atoms. The piston will descend until each atom is brought precisely to that state of proximity to the next that the repulsive energy between the atoms just balances the weight on the piston, and thus the most delicate equipoise is afforded by the air. The slightest extraneous force is sufficient to disturb the equilibrium, which is again restored by a series of decreasing oscillations.

If the atoms of the air however are removed to a much greater distance, the repulsion entirely ceases, and attraction of gravitation takes its place. If it were not for this, the

atmosphere would fly from the earth by the repulsive energy of its own atoms. We may therefore consider every atom of matter endowed with the property of obedience to the laws of force and motion; with inertia, by which it cannot change its place without the application of force, and when in motion cannot stop this motion without the application of an equal force in the opposite direction; and with attraction and repulsion, by which any two atoms placed at ever so great a distance from each other, will tend to approach each other with a force increasing inversely as the square of the distance. When these atoms approach very near to each other they cease their motion, and if pressed nearer than this point repel each other. And it appears from experiment and observation that there are several alternations of attraction and repulsion at distances too minute however for our senses, and only indicated by certain phenomena. Repulsion exists between the atoms of the densest bodies. Platinum, for example, which is 21 times heavier than water, and 257,000 times heavier than hydrogen, is still condensable. It may be compressed into a smaller space; and since the shrinking takes place equally in all directions, it follows that the atoms of this substance, as well as those of all gross matter, are not in contact. Indeed, when the hardest bodies are violently impelled against each other, and each is indented by the other, they do not come into actual mathematical contact, but are mutually impressed by the repulsive energy, which, vastly increased by the diminished distance, produces the visible effect.

All matter therefore is porous, whether in the gaseous, liquid, or solid condition. The pores may be conceived to be of different orders, namely, pores between the atoms, between the molecules or assemblages of atoms, and between the still larger particles. Gold itself is rendered brittle by being exposed to the fumes of sulphur, and solid iron is converted into steel by absorbing a large quantity of carbon, to which inter-penetration it owes its quality of hardness.

In the case of atmospheric air and other gases the repulsive energy alone is exhibited in most of the mechanical phe-

nomena, while in solid bodies both the attractive and repulsive are evident. Thus, if we place a heavy weight on the top of a vertical iron bar its length will be infinitesimally diminished. If the weight be removed, the atoms, by repulsion, will spring back to their original distances, and this may be repeated any number of times with the same result, provided the weight is not so great as to cause any permanent change which consists in a new arrangement of the atoms. If we now suspend the bar from one end, and apply a weight to the other, the bar will be minutely elongated; and if the weight be removed, the atoms, by their attraction, will return to their normal position. In this state the atoms are at the distance which constitutes a neutral condition. If pushed together, they fly apart whenever the compressing force is removed; and if drawn in the direction of the length of the body, they are brought into the region of attraction, and tend to bring the bar back to its original length when the elongating force is remitted.

This constitution of matter may be represented by a series of balls separated from each other by helical springs. If we attempt to elongate this bar the springs will be drawn out. When we attempt to compress the mass the several spires of the springs will be compressed closer together, and an action similar to repulsion will be produced.

This repulsion of the atoms is further demonstrated by the elasticity of a body, or the force with which it tends to restore itself to its former condition when disturbed by any extraneous force. The elasticity for instance of a rod of tempered steel is exhibited when we bend it. It tends to return to its first form in obedience to two forces. The atoms on the convex side, after the rod has been bent, are slightly separated, and are therefore in the region of attraction, while those on the concave side are brought nearer, and thus tend to repel each other. If this be the case, there should be a line somewhere near the middle of the bent rod, in which the atoms are neither compressed nor distended; and that such a neutral line does really exist can be shown by polarized light, which enables us, when the experiment is made on a

rod of transparent glass, to look into the interior of the elastic body and observe the changes there produced.

The difference between the compressibility of air and of steel depends upon the difference in the repulsion of the atoms in the two cases. But in the latter, as well as in the former, there is the most delicate balance of forces; for though a bar of good steel resists the weight of 60,000 pounds to the square inch tending to separate it in the direction of its length, yet the atoms may be thrown into vibration by the minutest force; and this is the case with all solids. A single tap with the end of a penknife on the table of the large lecture room of the Smithsonian Institution is sufficient not only to throw into vibration every particle of air in the room, but also every particle of the solid parts of the edifice. The agitation of the air is proved by the sound, discernible in every part of the room, and the vibrations of the solid parts also by the transmission of sonorous waves with even less loss than in the air.

The repulsion of which we have spoken, and which takes place only at minute distances, though these may be exceedingly great when measured by the size of the atoms, appears to be an essential endowment of matter, and is exhibited as well between the atoms of the ætherial medium as between those of air and other grosser assemblages of matter.

All bodies (as a general rule) are enlarged by an increase of temperature. But this result, as we shall endeavor to show, is not from an increase of the original repulsion, but from an energetic vibration imparted to the atoms, which tends to separate them and produce the phenomena improperly ascribed to an imaginary fluid called heat.

The medium of radiation.—We are obliged to assign to the ætherial medium a similar constitution to that possessed by grosser matter; namely, that it consists of inert atoms at great distances from each other relative to their own size, and each kept in position by attracting and repelling forces. Through this medium impulses or minute agitations are transmitted in celestial space, from planet to planet, and from system to system, which tremors or waves constitute light,

heat, and other emanations received by us from the sun. That is to say, the solar emanations are not matter, but motion communicated from atom to atom, beginning at the luminous body and diffused in widening spherical surfaces, enlarging in size and diminishing in intensity to the farthest conceivable portion of space.

The atoms of the ætherial medium are assumed to be perfectly free to move in all directions so that the earth and denser bodies experience no retardation as yet measurable; though lighter bodies, such as comets, apparently exhibit an effect of this kind for the same reason that a flock of cotton is more retarded in falling through the air than a piece of lead. At first sight it might appear paradoxical that atoms, which are kept in position by powerful attraction and repulsion, should yet be perfectly movable among each other; but this condition is observed in liquid water, the particles of which, though they exhibit perfect mobility, yet repel and attract each other with immense force. This arises from the fact that every atom beneath the surface of a fluid is equally attracted and repelled on all sides by the surrounding atoms, and is therefore perfectly free to move. Not so however with the atoms at the surface, for they are attracted downwards without a counteracting force to attract them upwards, and hence great resistance is manifested when we attempt to separate them.

The author of this essay has shown from conclusive experiments that the attraction of water for water is as great as that of ice for ice,* and that the difference of the two conditions consists in the perfect mobility of the atoms in the former case, and not in the neutralization of cohesion, as is generally supposed. If we attempt to draw up from the surface of water a circular disc of metal, say of an inch in diameter, we shall see that the water will adhere and be supported several lines above the general surface. This adhesion, on account of the perfect mobility of the atoms, is due alone to the attraction of the atoms of the external film and not to those of the whole mass which is elevated. This experi-

[* Proceedings of American Philosophical Society. See *ante*, vol. I, p. 217.]

ment, which is frequently given in elementary books as a measure of the feeble attraction of water for itself, is improperly interpreted. It merely indicates the force of attraction of a single film of atoms around the perpendicular surface, and not of the whole column elevated. The difference then of liquidity and solidity principally consists in the mobility of the atoms.

The immobility of the atoms of solids probably depends on their being assembled in larger groups, forming crystals, tissues, fibres, &c., and when force is applied to separate them they all resist together. In breaking a piece of steel for instance by extension, all the parts throughout the cross section of the mass simultaneously resist separation, and hence the great tenacity and rigidity of this substance: and between this and pure water other substances may be found having intermediate consistencies.

We have said that the atoms of the ætherial medium pervade those of all other bodies, and this postulate is analogous to the inter-penetration of the particles of different substances between each other.

If a piece of copper plated with silver be heated to redness the latter metal will be absorbed into the former. Water absorbs a large portion of air, and between the atoms of the air itself there may exist an indefinite number of other gases. Melted silver poured into water gives out a large portion of oxygen, which it had previously absorbed from the air in its liquid state.

If we suppose solid bodies to be composed of a series of groups of atoms, the larger in succession formed from the smaller, the vacuity in all cases may far exceed the solidity.

Let us now consider more minutely the nature of the emanations from the sun, (light, heat, &c.) in connection with the doctrine of atoms. And in order to this we shall make comparisons between the phenomena of light and heat, and those of sound, passing by analogy from the palpable and well-known cause of familiar phenomena to that which is apparently not as readily accessible to our investigations, but which when properly understood is equally

satisfactory in the explanation, prediction, and control of the phenomena.

Analogy of heat and sound.—If a heavy cannon be discharged at the distance of five or six miles, we shall see the flash almost instantaneously, and in about half a minute after the window will be violently agitated.

What is the cause of this agitation? No substance shot from the gun has reached us, for the same effect may be perceived on all sides. The simple and true explanation of the phenomenon is that the atoms of air just around the mouth of the piece were for an instant violently pressed outwards by the blast of powder; these atoms were pressed against the next layer, and these against the next, and so on until the impulse reached the distant window.

Each atom makes a short excursion or vibration, moving but little from its first position, and it is not therefore matter which proceeds from the cannon and produces the distant effect, but a propagation of motion from atom to atom.

The atoms are endued with inertia, and time is therefore required, even though immense force may be applied, to give them full motion. And again, the atoms are not in contact, but are kept at a distance by repulsion, which increases when the atoms are pressed nearer each other. Hence the second layer of atoms does not begin to move with full velocity at the precise moment when motion commences in the first.

The effect would be similar to that which would take place in a series of balls kept apart from each other by helical springs interposed. If a blow were given to the first ball, so as to drive it nearer to the second, the motion would not be instantaneously communicated; the second would resist a change of state, and would not move from its position until the spring was considerably bent. And in this way time would be required to propagate motion from the first ball to the second, from the second to the third, and so on throughout the series.

If a series of lighter balls were substituted for the first, the springs remaining the same, it is evident the motion would

be transmitted sooner, because the inertia would be in proportion to the weight of the balls. Hence sound is transmitted more rapidly in lighter than in heavier gases; in hydrogen its velocity is greater than in carbonic acid.

Again, we may suppose the stiffness of the springs to vary, or in other words, the repulsion between the atoms to become greater or smaller. If the springs become stiffer, then it is evident the motion will be transmitted sooner, for if the springs were infinitely rigid, or what is the same if a perfectly solid body were interposed between the balls, then the first ball could not move without at the same moment giving motion to the last. Hence if we increase the elasticity of a medium and at the same time diminish the size of its atoms any required velocity can be attained. Now though the flash is apparently perceived at the same instant at different places on the surface of the earth, yet we know from the most satisfactory evidence that this is really not the case, and that light and heat, as well as sound, require time for their propagation. Every impulse at the sun requires about eight minutes before it is felt at the distance of the earth.

The analogy between light and sound does not cease here; and to exhibit the resemblance still further, let us suppose a large bell placed in mid-air to be struck a single blow with a heavy hammer; we know that the lower rim of metal will be thrown into a state of vibration; it will be compressed into an elliptical form, the shorter axis in the direction of the blow. The elasticity will bring it back to its normal state, and will then carry it beyond in the other direction; and thus the part of the bell which is struck will continue to move backward and forward rapidly for a considerable time, which would be indefinitely prolonged were the experiment made in a perfect vacuum, and were no change produced in the atoms of the metal. In open air however the motion becomes feebler and feebler, and after a few minutes dies away and entirely ceases. The principal cause of this diminution is evidently the imparting of the motion of the metal to the immediately surrounding atoms of the air, and these to the next, and so on. It

is evident that at the moment the rim of the bell is going from the spectator, a tendency to a vacuum would be produced, and the atoms of the first layer of air will follow the metal by their elasticity, thus producing a rarefaction into which the atoms of the second layer of air will rush; and this will advance from layer to layer until it reaches the ear of the observer. But before it has got far on its way, the side of the bell will return, and will condense the air in contact with it, and send a positive impulse in the same direction with the first. These two impulses, travelling with equal velocities, and the one immediately succeeding the other, form an undulation.

The effect may be strikingly illustrated by water in a long trough. If a small block of wood of the width of the trough be suddenly drawn out of the liquid at one end of the trough the water in immediate contact with the block will flow in to fill the vacuum; the water next will flow into the space thus left, and so on, a hollow or negative wave will be propagated from one end of the trough to the other. If the same block be suddenly thrust down into the water, the effect will be as if a quantity of water had been suddenly added. The liquid will rise at the side of the block, and in its fall another wave will be elevated outside of it, and so on continually, a positive wave or one of elevation, will be transmitted to the farther extremity of the reservoir.

If the two motions of the block be made, one immediately succeeding the other, a compound wave or an undulation will be the result. The transfer in this case is again that of form and not of substance. The atoms of water remain in place, as will be evident by placing bits of wood on the surface; they will rise and fall, but will not advance as the wave passes. This is an illustration of an undulation, but not an exact representation of a sound wave, which consists in a slightly alternate backward and forward motion of each particle between the bell and the observer.

An undulation of sound therefore consists of two parts—a condensed and a rarefied part; and hence when two series of undulations of the same wave length follow each other at

a distance of half an undulation, they neutralize each other, the protuberance of the one undulation exactly filling as it were the hollow of the other; or to express it more accurately, the rarefied and condensed parts of the two waves will neutralize each other, and in this way silence may be produced by two intense sounds. From analogy therefore, if light also consists of waves, two series might be brought together, so as to produce darkness. Both these inferences are fully borne out by experiment.

If we observe the effect of the sound waves upon a distant object, (such for instance as a delicate membrane stretched over a hoop and strewed with sand,) we shall find that on sounding an instrument the sand will be violently agitated: and if the vibration is in unison with any of the strings of a neighboring piano, they will give forth an audible sound.

It may be well to stop one moment to inquire in what this unison consists. It is well known that a string of a given length performs all its vibrations in the same time. Now if the impulses from the sounding body reach a string of such a time of vibration that the effect of the second impulse may be added to that of the first, or while the string is moving in the same direction as that given it by the first impulse, then the sounding will take place, or the string will be aroused into a motion harmonious with that of the sounding body. But if the impulses are not timed exactly to the vibrations of the string, they will meet the latter in its forward as well as in its backward movement, and thus tend to neutralize the effects of each other.

In the case of light and heat, the luminous or heated body is supposed to be in the condition of the bell during its sounding. The ætherial medium is the analogue of the air, and the vibrations of the optic nerve that of the tympanum of the ear.

Further, in the case of heat, when the vibrations from the sun impinge upon the surfaces of solids and liquids, the ætherial medium within the interstices of these bodies, and also the atoms of gross matter, are put in a state of harmonious vibration, and thus give rise to the phenomena

of the heat of temperature or expansion. When, as we have previously indicated, the vibrations of the atoms of solids become sufficiently violent to throw them beyond the sphere of cohesion, the matter is converted from a solid into an aeriform condition.

But the question naturally arises, What is it that puts in vibration the luminous body (a candle, for instance) and keeps it for several hours in this constant state of agitation? The answer is, the continued rushing together of atom after atom of the carbon and hydrogen of the candle, and those of the oxygen of the surrounding air. An action of a somewhat similar kind, we must infer from analogy, is constantly producing impulses of a like character at the surface of the sun..

From the analogies of light, heat, and sound, we might infer, since there are different lengths of waves of the latter which give rise to the different notes of music, that there are different lengths of waves of the ætherial medium producing different sensations in us, and different effects upon gross matter. And this furnishes a ready explanation of the well-known phenomena of the different colors of the spectrum, and also of the less familiar but equally remarkable phenomena of the different kinds of radiant heat, as well as of the chemical and phosphorogenic emanations from the sun.

That there may be different forms of waves transmitted through the same medium will be evident from inspecting the following figure, and considering the motions of the atoms which may be produced by a single impulse.



If we strike for example the atom *a*, it will be driven towards the second atom, and the second towards the third, the third towards the fourth, and so on; the motion will be transmitted along the central line of atoms to the other ex-

tremity. But while this motion takes place through the centre line of the assemblage of atoms, the motion of *a* will also bring it nearer to the atoms *b* and *c*, on either side; and these will therefore be repelled from their positions of quiescence, and lateral waves in which the atoms vibrate transversely to the direction of the ray, will be produced. It is probable that both kinds of vibration are transmitted through the ætherial medium, and perhaps both also through the air; but such is the constitution of our eyes that they can only perceive the results of those of the second kind, and such the constitution of our ears that they can only take cognizance of those of the first. The transverse vibration of light and heat was a happy conception of Dr. Thomas Young, (one of the discoverers of the key to the Egyptian hieroglyphics,) and was applied by himself and Fresnel to the explanation of a large and interesting series of facts classed under the name of polarization of light and heat.

Besides the invisible emanation from the sun, which gives us the sensation of heat, there are others equally invisible which produce other effects. Indeed it is possible that there are an indefinite number of waves, differing in length and perhaps in form, though many of these must be so minute as to produce no appreciable physical effect at the distance of our planet. If a beam of light be decomposed by a prism, it is well known that it will be separated into parts, producing different colors. Now if we subject to this spectrum a piece of paper which has been soaked in a solution of nitrate of silver, we shall find that the salt of silver will be decomposed, and the paper will be blackened by the reduced metal. But the interesting part of the experiment is that the blackening will be more intense at a point in the prolongation of the spectrum, which is entirely in the dark. There is then in a sunbeam, besides light and heat, a ray which may be separated from the former by a prism, which produces chemical decomposition, and is hence called the chemical ray. I need scarcely remark that it is this ray, and not that of light, which produces the picture in the photographic and daguerrean processes.

Again; it is well known that if we expose a diamond for an instant to the rays of the sun, and then convey it to a dark place, we shall see it glow with a pale phosphorescent light; but this effect, long familiar as it has been to the natural philosopher, is now known to be the result of an emanation differing in some essential particulars from all the other emanations which we have mentioned. To prove this, it is sufficient to place the diamond under a plate of transparent mica, a substance which transmits freely light, heat, and the chemical emanation. This will screen the diamond; and the glowing, which was before very striking, will not now be produced. That this effect is not the result of the absorption of a ray of light will be evident when we mention the fact that a diamond will glow when placed under a thick plate of smoky quartz, which intercepts both light and chemical emanation, but freely transmits what is denominated the phosphorogenic ray. These results are all in accordance, in a general way, with the constitution of the ætherial medium which we have presented.

Light and heat appear to differ only in the lengths of the waves, which become shorter and more intense as the temperature of the source of emanation increases; though in some cases, as in that of luminous phosphorus and the light of the glow worm, it is emitted freely from bodies of low temperature. It is possible that light from these different sources may possess different physical properties.

Electricity.—The phenomena of light, of heat, of the chemical and phosphorogenic emanations have all been referred to vibrations of the ætherial medium, and all the facts which have thus far been observed are in accordance with this generalization. The question however naturally arises as to what explanation we can give of the multiplied and various phenomena constantly presenting themselves to us in connection with the changes which are taking place around us in nature, or which exhibit themselves to the chemist and physicist in their investigations of the minuter reactions which are brought about by their agency, and which are classed under the general name of electricity. It is a

recognized principle of philosophy to adopt no other causes for the explanation of phenomena than are true and sufficient; and although the existence of the ætherial medium may by some be doubted, yet to me it appears as certain as any fact can be which rests upon inferences drawn from observed phenomena. The wave motions which we refer to it, and which exactly agree with the observed facts, are precisely such as are produced in gross matter under the action of the laws of force and motion, and therefore we have nearly the same reason for believing in the existence of this diffused substance as in that of gross matter itself. Besides, the tendency of science is to reduce rather than increase the number of agencies to which effects are referred as causes. We shall therefore assume that the ætherial medium is also the agent by which the phenomena of electricity are produced, but the facts classed under the head of electricity cannot be explained on the principle of wave-motions, and we must therefore seek for some other probable mechanical action from which they may be rationally deduced.

Electrical phenomena may be referred to two great classes, statical and dynamical, or such as appear to be produced by the repulsive action of a fluid at rest, and by the same fluid in a state of motion. In some cases we have action at a distance on surrounding bodies which develop new and permanent properties so long as the conditions remain the same; and in other cases effects which exactly resemble those of a transfer—not of a property, but of actual substance, from one body to the other. Now these phenomena may be referred to an accumulation of the ætherial medium in one portion of space, and a corresponding diminution in the adjacent space. If the particles of the ætherial medium, when thus accumulated, act at a distance on other portions of the same medium we shall have a rational exposition of the phenomena of statical electricity; and in the restoration of the equilibrium of the medium, or in its return to its normal condition, we have a plausible cause of the dynamic effects belonging to the same class. But how is this disturbance of the equilibrium of the ætherial me-

dium produced? The answer is, by the agency of gross matter. From the refraction of light and the various effects of heat we must infer that the ætherial medium is intimately connected with gross matter; and although the latter may move in it without disturbing the equilibrium, yet when two pieces of gross matter are rubbed together an accumulation of the atoms of the ætherial medium may take place on the one and a deficiency on the other. According to this view there can be no electrical excitement in celestial space; for there gross matter does not exist, without which the medium cannot be coerced or the equilibrium disturbed. It is not supposed, in accordance with this hypothesis, that there is an absolute vacuum produced in the medium, but that a condensation exists in a given spot, and a corresponding rarefaction in the space around it. The degree of this condensation and rarefaction may be exceedingly slight in comparison with the whole elastic force of the medium, and therefore it is not essential to the truth of the hypothesis that any very perceptible changes should be produced in rays of light passing in close approximation to electrified bodies.

This hypothesis is adapted to the theory of either one or two fluids. In the second case the ætherial medium must be supposed to consist of two kinds of atoms, the separation of which gives rise to the phenomena observed; and in the first that it consists of but one kind of atom, and that the effects observed are due to its being in excess in one body, and in deficiency, at the same time, in another.

In a new investigation of the discharge of a Leyden jar, by the author of this essay, the facts clearly indicated the transfer of a fluid from the inside to the outside, and a rebound back and forward several times in succession, until the equilibrium was attained by a series of diminishing oscillations.

The magnetic phenomena may be referred to an assemblage of electrical currents, according to the theory of Ampère, or to a peculiar arrangement of the ætherial atoms within the magnetic body.

The electro-magnetic phenomena appear to be due to the action of the atoms of gross matter combined with that of the ætherial medium.

We cannot here go into an exposition of the facts of electricity and magnetism, but will merely point out one inference from the hypothesis we have given, namely that electricity is not in itself a primary source of motion or mechanical energy, tending to produce change by a kind of spontaneity, (as is frequently supposed,) but is the effect of a disturbance and subsequent restoration of an equilibrium, which disturbance has been produced by the application of an extraneous force. This conclusion may also be arrived at, without reference to the hypothesis, from the study of the facts themselves, which clearly demonstrate that the electrical equilibrium (whatever may be its nature) is never disturbed by its own action, but the manifestation is always the effect of the application of some other power, and is the mechanical equivalent of such disturbing cause.

Crystalline forms.—We will now consider the grouping of the atoms which is intimately connected with the various properties of different kinds of bodies. When the atoms of gross matter are suffered to approach each other, without disturbance or agitation, and from an aeriform or liquid condition to gradually assume the solid form, they exhibit beautiful geometrical figures, familiarly known under the name of crystals. For example if a quantity of common salt be dissolved in water and the liquid be suffered to evaporate in a still place, beautiful crystals of a cubical form will be found in the vessel; or if ordinary saltpetre be dissolved in warm water and suffered to cool, regular six-sided crystals will be obtained. If these crystals be reduced to an impalpable powder and again dissolved in hot water the same result will again be produced, provided the liquid be not in excess.

The most interesting illustration of crystallography to the meteorologist is that exhibited in snow and hoar frost. These generally consist of stellar figures in one plane, with rays and branches of rays, all making angles of 60° with

each other, and under different conditions of the atmosphere are exceedingly varied and beautiful. To explain these figures in a general way let us suppose three separate atoms to be within the sphere of mutual attraction and free to move; they will approach until they come within the sphere of repulsion, and will then evidently be found in the same plane at the angular points of an equilateral triangle, since each must be at the same distance from each of the other two. If a fourth atom be suffered to approach in the same manner it will also arrange itself at an equal distance from each of the three others at the apex of a regular triangular pyramid of equal and similar faces. The next symmetrical arrangement which could take place would be in case a fifth atom were added; and if this were situated on the other side of the base of the pyramid a regular six-sided figure would result. We see from these examples that regular geometrical forms are the necessary effect of the undisturbed grouping of the atoms, though it is impossible to deduce all the facts from considerations as simple as those we have given above. To adapt the hypothesis to the facts of the case we are obliged to assume that crystalline forms are not the result of the approximations of single atoms, but of molecules of more or less complicated structure.

Though the exact representation of the groupings of particles of different kinds of matter has exercised the ingenuity of a number of investigators, the theory is still in a very imperfect condition. It offers however a rich harvest for scientific culture, and a number of interesting conclusions have been deduced from the crystallographic study of bodies, particularly by M. Gaudin. We are obliged to suppose that the primary molecules which enter into crystals are themselves of a geometrical shape, due to the arrangement of the ultimate atoms of which they are composed, and such forms are called the primitive forms of the crystalline molecules. These primitive molecules vary in form and size, as we shall see hereafter, and they vary also in these respects, in some cases of their combinations. If the two salts we mentioned in the commencement of this division of our subject—namely,

saltpetre and common salt—be dissolved together in a sufficient quantity of water, and the liquid be suffered gradually to evaporate, they will be found at the bottom of the vessel in separate crystals. The cubes of common salt can readily be distinguished from the long-sided prisms of saltpetre, and when these are chemically analyzed, each is found to be exclusively composed of its respective substance. Not a single atom of the saltpetre is found in the crystal of salt, nor one of the latter in the former. The same effect takes place if magnesia and saltpetre be dissolved in hot water and the solution be suffered to cool. The case however is altogether different when sulphate of magnesia, and sulphate of nickel or sulphate of zinc are crystallized together, from the same solution. The separation of the two substances does not take place as in the former instance; the individual crystals formed will contain both sulphate of zinc and sulphate of magnesia, or sulphate of nickel and sulphate of magnesia, and this in every possible proportion, according to the relative amounts of the two salts in solution. Now if we compare a crystal of sulphate of magnesia with a crystal of sulphate of nickel, we find they have identically the same crystalline form: there is no perceptible difference in their angles, edges, or solid angles. And since a large crystal is built up of an aggregation of small ones of the same form, it is evident that the primitive molecule of sulphate of nickel must have the same form as that of the sulphate of magnesia; and therefore that in forming a large crystal they may be mingled together in the way we have just described, provided they are of the same size, or perhaps some multiple of the same size, for it is evident that it would be impossible to build a wall of symmetrical structure with bricks of different angular forms and sizes, since the parts would not fit or exactly fill the spaces. We must therefore conclude that though the ultimate atoms of bodies may be spherical, the groupings of them, which form the primitive crystallizing molecules, are of different geometrical shapes and sizes.

The atomic weights or combining proportions.—Though the primordial atoms may all be of the same weight and size,

and the different kinds of matter the result of the different forms in which they are grouped, yet in the present state of science there are sixty-one substances which are classed by the chemist as simple bodies, and which must continue thus to be classed until they shall be actually de-composed into two or more separate components. If these bodies consist of elementary atoms, or of groups of atoms, always of the same number and form, it will follow that all combinations of them will take place in definite and fixed proportions. For example, it is known that one part of hydrogen by weight unites with eight parts of oxygen to form water, and this liquid, whenever found, always contains the same proportion of these ingredients. But there is another compound of oxygen and hydrogen, of which the components are in the ratio of one to sixteen, and this result is precisely that which might have been anticipated from the theory of atomic combination. In the first case, if the atom of hydrogen weigh one, (for instance, one millionth of a grain,) and the atoms of oxygen eight, (eight millionths,) then any amount of combination will have the same proportion. The combinations then will be one to eight, one to sixteen, and if another combination of oxygen and hydrogen exist, it will be in the ratio of one to twenty-four. In the first instance, it is one atom to one; in the next, of one atom to two; in the third case, it would be one atom to three. This is also beautifully shown in the union of oxygen and nitrogen, of which there are five different compounds, as exhibited in the accompanying table.

Names of Compounds.	Weight.		Ratio.	
	N.	O.	N.	O.
Protoxide of nitrogen (nitrous oxide)-----	14	8	1	1
Binoxide of nitrogen (nitric oxide)-----	14	16	1	2
Hyponitrous acid-----	14	24	1	3
Nitrous acid-----	14	32	1	4
Nitric acid-----	14	40	1	5

A glance at this table will show the justice of the remark of M. Dumas, that granting matter to be atomic it must necessarily combine as it is found to do in this instance. We refer to any work on chemistry for a table of atomic weights, and shall only give here those of the atoms which form the principal part of animal and vegetable bodies, namely, hydrogen, carbon, oxygen, and nitrogen:

	Atomic weight.
Hydrogen -----	1
Carbon -----	6
Oxygen -----	8
Nitrogen -----	14

To these, in lesser quantities, are added sulphur, 16; phosphorus, 32. We may say therefore that the whole atomic system of animal and vegetable physiology depends principally on the four numbers 1, 6, 7, 8. Wherever the substances above mentioned are found in combination in any of the three kingdoms of nature, they always combine according to these numbers, or multiples of them—a statement which contains in a single line a truth of the widest significance; which has rendered chemistry an almost mathematical science, and its applications to agriculture an art of the highest value and yet of comparatively easy attainment. To facilitate still more the use of this generalization, the atoms are expressed in abbreviated language. Thus water is represented by HO—that is, one atom of hydrogen, 1, and one of oxygen, 8, making nine for the weight of the liquid. Two atoms of water would be represented by 2 HO; carbonic acid by CO₂, or one atom of carbon, 6, and two atoms of oxygen, 16; making for the atomic weight of the acid 22. Nitric acid is represented by NO₅, and ammonia by NH₃, and nitrate of ammonia by NO₅+NH₃; indicating, in the formation of nitric acid, five atoms of oxygen and one atom of nitrogen, and in that of ammonia, three atoms of hydrogen to one of nitrogen. The attainment of a knowledge of this notation is easy, while the use of it is exceedingly convenient.

Atomic volumes.—The spheres of repulsion of different chemical atoms, or rather molecules, are probably different;

and as we may consider these spheres as constituting the size of the atoms, in reference to the space which they occupy in combination, their magnitudes may be calculated with a view to ascertain whether any similarity can be found in the properties and action of bodies having equal atomic volumes. To explain how this may be done, let us suppose we wish to know the number of atoms in a given volume of matter of which the whole weight is known, and also the weight of a single atom; we shall then evidently have the required number of atoms by dividing the weight of the one atom into the weight of the whole. Now if we know the number of atoms in a body of given size, we can find the size of each atom by dividing the bulk of the whole by the number of atoms; but since we can only ascertain relative atomic weights and volumes, we suppose the volume of the mass to be unity, and the weight of the same to be the specific gravity, or weight relatively to that of water. If we then divide the atomic weight into the specific gravity, we shall have the relative number of atoms; and if we divide this number into 1, or what is the same thing, invert the fraction and divide the atomic weight by the specific gravity, we shall have the relative atomic volume. We find in this way that there are groups of simple bodies having nearly the same atomic volume, and that, when crystallized in the same form, one may be substituted for the other, giving rise to compounds of similar forms, and in some cases of similar properties, though of different chemical constitution; and on the other hand, by the differences in the grouping of the same atoms bodies may be formed having entirely different properties.

It frequently happens that in the union of different bodies in the gaseous state a condensation takes place, and the volume of the compound molecule is not equal to the sum of the volumes of atoms of which it is composed; and in other cases the reverse effect has place, and an expansion is the result.

The following table, from Faraday's lectures,* exhibits the

*[The subject matter of a course of Six Lectures on the non-metallic Elements. Lect. iv.—Nitrogen; p. 206. 16mo. London. 1853.]

reference to this point have been classed under the head of electro-chemistry; and in this case, as in every other subdivision of our general subject, we have merely indicated a group of phenomena, each of which has occupied the attention of a number of scientists, and in some cases during a long term of years.

Until recently it was supposed that the physical qualities of bodies must depend on the nature of their elements, or in other words upon their chemical composition; but a great many substances have been discovered composed of the same elements in the same relative proportion and yet exhibiting physical and chemical properties entirely distinct one from the other. For example, according to Liebig, the oil of turpentine, the essence of lemon, oil of balsam of copaiba, oil of rosemary, oil of juniper, and many others differing widely from each other in their odor, in their medicinal effects, in their boiling points, in their specific gravities, all contain the same elements, carbon and hydrogen, and in precisely the same proportion. The crystallized part of the oil of roses, a volatile solid, of which the delicious fragrance is so highly esteemed, is a compound body containing exactly the same elements and in the same proportions as the gas employed in lighting our streets.

Such bodies are called *isomeric* (literally, of *equal parts*), and the phenomena are classed under the head of isomerism. These remarkable facts can only be accounted for by the different groupings of the atoms. They exhibit as it were the economy of Nature in producing the most multiform effects from combinations of the simplest principles, and almost revive in us the dreams of the alchemists relative to the transmutation of matter.

Combinations of this kind are generally of a very unstable character and the atoms can sometimes be made to change their positions by an impulse from without, or by the addition of heat, and to combine again, forming other substances having entirely different properties.

The changes we have mentioned are those of bodies which are formed of groups of many chemical atoms; but a fact of

a similar character has been observed with reference to bodies belonging to the class which the chemist calls simple or elementary, because they have not as yet been decomposed. Of these bodies we may mention oxygen, chlorine, sulphur, and phosphorus. They all assume under certain conditions entirely different properties to such an extent as almost to lose their identity. Oxygen, when exposed to a series of sparks of electricity, is converted into a substance called ozone, of which we shall speak more fully hereafter. Sulphur, exposed to a temperature of 226° F., is melted, and if maintained in fusion at a temperature not exceeding 300° , and then suddenly thrown into water, will be found to have suffered no change; if however the fusion be continued above 300° , the material becomes black and almost solid, and if it now be poured into water it maintains its dark color, and assumes a consistence of heated glue or softened India rubber. In this condition its medical and other properties are changed. Sulphur is also capable of assuming two different crystalline forms belonging to two primitive classes entirely distinct. Phosphorus undergoes a similar change, and chlorine, after exposure to the light, exhibits new properties. Phenomena of this kind are classed under the head of *allotropism* (literally, of *another turn or fashion*).

Organic Molecules.

The groups of atoms which we have thus far been considering are principally those which have been formed under the influence of what is called the chemical force, and result from the ordinary attraction of the atoms. These are comparatively simple groups; but there is another class of groups of atoms of a much more complex character, which are formed of new combinations of the ordinary atoms under the influence, or (we may say) direction of that mysterious principle called the *vital force*. We are able to construct a crystal of alum from its elements by combining sulphur, oxygen, hydrogen, potassium, and aluminum; but the chemist has not yet been found who can make an atom of sugar from the elements of which it is composed. He can

readily decompose it into its constituents, but it is impossible so to arrange the atoms artificially, as in the ordinary cases of chemical manipulation, to produce a substance in any respect similar to sugar. When the attempt is made, the atoms arrange themselves spontaneously into a greater number of simpler and smaller groups or molecules than is found in sugar, which is composed of molecules of high order, each containing no less than 45 atoms of carbon oxygen and hydrogen.

The organic molecules, (or atoms, as they are called) are built up under the influence of the vital principle, from inferior groups of simple elements. These organic molecules are first produced in the leaves of the plant under the influence of light, and subsequently go through various changes in connection with the vital process. After they are once formed in this way, they may be combined and re-combined by different processes in the laboratory, and a great variety of new compounds artificially produced from them.

But what is this vital principle which thus transcends the sagacity of the chemist and produces groups of atoms of a complexity far exceeding his present skill? It is generally known under the name of the "*vital force*"; but since the compounds which are produced under its influence are subject to the same laws as those produced by the ordinary chemical forces, though differing in complexity; and since in passing from an unstable to a more stable condition in the form of smaller groups they exhibit, as will be rendered highly probable hereafter, an energy just equivalent to the power exerted by the sunbeam under whose influence they are produced, it is more rational to suppose that they are the result of the ordinary chemical forces acting under the *direction* of what we prefer to call the *vital principle*. This is certainly not a *force*, in the ordinary acceptation of the term, or in that in which we confine this expression to the attractions and repulsions with which material atoms appear to be primarily endowed. It does not act in accordance with the restricted and uniform laws which govern the forces of inert matter, but with fore-thought, making provision far in ad-

vance of a present condition for the future development of organs of sight, of hearing, of reproduction, and of all the varied parts which constitute the ingenious machinery of a living being. Matter without the vital influence may be compared in its condition to steam, which undirected is suffered to expend its power in producing mechanical effects on the air and other adjacent bodies, marked with no special indications of design; while matter under its influence may be likened to steam under the directing superintendence of an engineer, which is made to construct complex machinery and to perform other work indicative of a directing intelligence. *Vitality*, thus viewed, gives startling evidence of the immediate presence of a direct, divine, and spiritual essence, operating with the ordinary forces of nature, but being in itself entirely distinct from them.

This view of the subject is absolutely necessary in carrying out the mechanical theory of the equivalency of heat and the correlation of the ordinary physical forces. Among the latter, vitality has no place, and it knows no subjection to the laws by which they are governed.

All the constituents of organic bodies are formed of organic molecules, and as we have said, ~~are~~ of great complexity and are readily disturbed and resolved into a greater number of lesser groups. Thus the constitution of cane sugar is represented by C_{12}, H_{22}, O_{11} , making in all 45 atoms. Organic bodies are therefore in what may be called a state of power, or of tottering equilibrium, like a stone poised on a pillar, which the slightest jar will overturn; they are ready to rush into closer union with the least disturbing force. In this simple fact is the explanation of the whole phenomena of fermentation, and of the effect produced by yeast and other bodies, which being themselves in a state of change, overturn the unstable equilibrium of the organic molecules and resolve them into other and more stable compounds. Fermentation then simply consists in the running down of organic molecules from one stage to another, changing their constitution, and at last arriving at a neutral state. There is however one fact in connection with the running down of

the organic molecules which deserves particular attention, namely, that it must always be accompanied with the exhibition of power or energy, with a disturbance of the æthærial equilibrium in the form of heat, sometimes even of light, or perhaps of the chemical force, or of that of the nervous energy, in whatever form of motion the latter may consist. It is a general truth of the highest importance in the study of the phenomena of nature that whenever two atoms enter into more intimate union, heat or some form of motive power, is always generated. It may however be again immediately expended in effecting a change in the surrounding matter, or it may be exhibited in the form of one of the radiant emanations.

Balance of Nature.—The term balance of organic nature was first applied, we think, by Dumas to express the relations between matter forming animals and vegetables, and the same matter in an inert condition. We shall apply the term “balance of nature” in a more extended sense, and include within it the balance of power, as well as the transformations of matter. The amount of matter in the visible universe is supposed to remain the same, though it is subject to various transformations, and appears under various forms,—now built up into organic molecules, and now again resolved into the simple inorganic compounds. The carbon and other materials absorbed from the air by the plant is given back to the atmosphere by the decaying organisms, and thus what may be called a constant balance is preserved. But this balance (if we may so call it) does not alone pertain to the matter, but also to the energy which is employed in producing these changes. It may disappear for a while, or may be locked up in the plant or the animal, but is again destined to appear in another form and to exert its effects perhaps in distant parts of celestial space.

To give precision to our thoughts on this subject let us suppose that all the vegetable and animal matter which now forms a thin pellicle at the surface of the earth were removed—that nothing remained but the germs of future organisms buried in the soil and ready to be developed when the proper

influences were brought to bear upon them. Let us further suppose the sun to cease giving emanations of any kind into space. The radiation from the earth, uncompensated by impulses from the sun, would soon reduce the temperature of every part of the surface to at least 60° below zero; all the matter and liquid substances capable of being frozen would be reduced to a solid state; the air would cease to move, and universal stillness and silence would prevail.

Let us now suppose that the sun were to give forth rays of heat alone; these would radiate in every direction from the celestial orb, and an exceedingly small portion of them, in comparison with the whole, would impinge against the surface of our distant planet, would melt the ice first on the equator, then on the more northern and southern parts of the globe, and finally their genial influence would be felt at the poles. The air would be unequally rarefied in the different zones, the winds would again be called forth, vapor would rise from the ocean, clouds would be formed, rain would descend, and storms and tempests would resume their sway.

If the sun should again intermit its radiation all these motions would gradually diminish and after a time entirely cease; the heat given to the earth would in part be retained for awhile, but in time would be expended; the water would slowly give out its latent heat and be again converted into ice. Something of this kind takes place in the northern and southern parts of the earth during the different periods of summer and winter. Since the mean temperature of the earth does not vary from year to year, it follows that all the excess of heat of summer received from the sun is given off in winter, and hence the impulses from this luminary which constitute all the energy producing the changes on the surface of the earth, merely lingering awhile, are again sent forth into celestial space, changed it may be in form, but not in the amount of their power. The solar vibrations have lost none of their energy, for the water has returned to the state of ice, and the surface of the earth is again in the same condition in which it was before it received the solar impulse.

The energy of the solar vibrations communicated to the ice modifies its cohesion, converting it into the liquid state, and the ice again becoming solid gives out the same amount of heat in a less energetic form. Even the motive power of the wind is expended by the friction of its particles in producing a portion of the heat which gave rise to its motion, and this also is radiated into celestial space.

But the most interesting part of our inquiry relates to the effects which the radiation alone of heat from the sun would have on the vegetable germs buried in the soil. If these germs were enclosed in sacs filled with starch and other organic ingredients, stored away for the future use of the young plant, as in the case of the tuber of the potato, or the fleshy part of the bean, as soon as the sun penetrated beneath the surface in sufficient degree to give mobility to the complex organic molecules of which these materials consist, (the proper degree of moisture also supposed to be present,) germination would commence. The young plant would begin to be developed, would strike a rootlet downward into the earth, and elevate a stem towards the surface furnished with incipient leaves. The growth would continue until all the organic matter in the tuber or sac was exhausted; the further development of the plant would then cease, and in a short time decay would commence.

But let us dwell a few minutes longer on the condition of the plant and the tuber before the downward action becomes the subject of consideration. If we examine the condition of the potato which was buried in the earth, we shall find remaining of it nothing but the skin, which will probably contain a portion of water. What has become of the starch and other matter which originally filled this large sac? If we examine the soil which surrounded the potato, we do not find that the starch has been absorbed by it; and the answer which will therefore naturally be suggested is, that it has been transformed unto the material of the new plant, and it was for this purpose originally stored away. But this, though in part correct, is not the whole truth; for if we weigh a potato prior to germination, and weigh the young

plant afterward we shall find that the amount of organic matter contained in the latter is but a fraction of that which was originally contained in the former. We can account in this way for the disappearance of a *part* of the contents of the sac, which has evidently formed the pabulum of the young plant. But here we may stop to ask another question: By what power was the young plant built up of the molecules of starch? The answer would probably be, by the exertion of the vital force; but we have endeavored to show that vitality is a *directing principle*, and not a mechanical power, the expenditure of which does work. The conclusion to which we would arrive will probably now be anticipated. The portion of the organic molecules of the starch, &c., of the tuber, as yet unaccounted for, has run down into inorganic matter, or has entered again into combination with the oxygen of the air, and in this running down, and union with the oxygen, has evolved the power necessary to the organization of the new plant.

If we examine the skin of a potato, we shall find it perforated by innumerable holes, through which the oxygen penetrates into the interior to enter into combination with the starch, (or in other words, to burn it by a slow combustion,) and through which the carbonic acid and vapor of water again find their way into the atmosphere. We see from this view that the starch and nitrogenous materials, in which the germs of plants are imbedded, have two functions to fulfil; the one to supply the pabulum of the new plant, and the other to furnish the power by which the transformation is effected, the latter being as essential as the former. In the erection of a house, the application of mechanical power is required as much as a supply of ponderable materials.

But to return to our first supposition. We have said (and the assertion is in accordance with accurate observation) that the plant would cease to increase in weight under the mere influence of heat, however long continued, after the tuber was exhausted. Some slight changes might indeed take place; a small portion of pabulum might be absorbed from the earth; or one part of the plant might commence to decay,

and thus furnish nourishment to the remaining parts; but changes of this kind would be minute, and the plant, under the influence of heat alone, would in a short time cease to exist.

Let us next suppose the sun to commence emitting rays of *light*, in addition to those of heat. These, impinging against the earth, would probably produce some effects of a physical character; but what these effects would be we are unable, at the present time, fully to say. We infer however that the light, not immediately reflected into space, would be annihilated; but this could not take place without communicating motion to other matter. It would probably be transformed into waves of heat of feeble intensity.

Let us now suppose, in addition to heat and light, the chemical rays to be sent forth from the sun. These would also produce various physical changes, the most remarkable of which would be in regard to the plant.

The carbonic acid of the atmosphere, in contact with the expanding surface of the young leaves, would be absorbed by the water in their pores, and in this condition would be decomposed by the vibrating impulses which constitute the chemical emanation. The atoms of carbon and oxygen, of which the carbonic acid is composed, would be forcibly separated; the atoms of oxygen would be liberated in the form of gas, and the carbon be absorbed to build up, under the directing influence of vitality, the woody structure of the plant. In this condition the pabulum of the plant is principally furnished by the carbonic acid of the air, while the impulses of the chemical ray furnish the primary power by which the de-composition and the other changes are effected. This is the general form of the process, leaving out of view minute changes, actions, and re-actions, which must take place in the course of organization.

All the material of which a tree is built up, (with the exception of that comparatively small portion which remains after it has been burnt, and constitutes the ash,) is derived from the atmosphere. That this is so can be proved by growing a plant in perfectly pure flint sand, to which a

minute quantity of foreign substance is added, and sprinkling with distilled water. In this case the plant will yield the usual amount of carbon or charcoal, although there was none in the soil in which it grew.

In the decomposition of the carbonic acid by the chemical ray a definite amount of power is expended, and this remains (as it were) locked up in the plant so long as it continues to grow; but when it has reached its term of months or years, and some condition has been introduced which interferes with the balance of forces, then a reverse process commences, the plant begins to decay, the complex organic molecules begin to run down into simpler groups, and then again into carbonic acid and water. The materials of the plant fall back into the same combinations from which they were originally drawn, and the solid carbon is returned in the form of a gas to the atmosphere whence it was taken. Now the power which is given out in the whole descent is, according to the dynamic theory, just equivalent to the power expended by the impulse from the sun in elevating the atoms to the unstable condition of the organic molecules. If this power is given out in the form of vibrations of the ætherial medium constituting heat it will not be appreciable in the ordinary decay—say of a tree, extending as it may through several years; but if the process be rapid, as in the case of combustion of wood, then the same amount of power will be given out in the energetic form of heat of high intensity. This heat will again radiate from the earth, and in this case, as in that we have previously considered, the impulse from the sun merely lingers for a while upon the earth, and is then given back to celestial space changed in form, but undiminished in quantity. It may continue its radiating course through stellar space until it meets planets of other systems; but to attempt to trace it further would be to transcend the limits of inductive reason, and to enter those of unbridled fancy.

In the process we have described, the carbon, hydrogen, and other substances which are absorbed from the atmosphere are returned to this great reservoir to be used again,

and it may be to undergo the same changes many times in succession. The earthy materials are again returned to the earth, and all the conditions, as far as the individual plant which we are considering is concerned, are the same as they were at the beginning. The absorption of power in the decomposition of the carbonic acid gas, and its evolution again when the re-composition is produced of the same atoms, is precisely analogous to that which takes place in forcibly separating the poles of two magnets, retaining them apart for a certain time, and suffering them to return by their attractive force to their former union. The energy developed in the approach of the magnets towards each other is just equal to the force expended in their separation.

By extending this reasoning to the vast beds of coal which are stored away in the earth, we are brought irresistibly to the conclusion that the power which is evolved in the combustion of this material, now so valuable an agent in the processes of manufacture and locomotion, is merely the equivalent of the force which was expended in de-composing the carbonic acid which furnished the carbon of the primeval forests of the globe; and that the power thus stored away millions of years before the existence of man, like other pre-ordinations of Divine Intelligence, is now employed in adding to the comforts and advancing the physical and intellectual well-being of our race.

In the germination of the plant a part of the organized molecules runs down into carbonic acid to furnish power for the new arrangement of the other portion. In this process no extraneous force is required: the seed contains within itself the power and the material for the growth of the new plant up to a certain stage of its development. Germination can therefore be carried on in the dark, and indeed the chemical ray which accompanies light retards rather than accelerates the process. Its office is to separate the atoms of carbon from those of oxygen in the decomposition of the carbonic acid, while that of the power within the plant results from the combination of these same elements. The forces are therefore antagonistic, and hence germination is more

rapid when light is excluded; an inference borne out by actual experiment.

Animal Organism.

Besides plants, there is another great class of organized beings, viz: animals; and as we commenced with the consideration of the seed in the first case, let us begin in this with the egg. This (as is well known) consists of a sac or shell containing a mass of organized molecules formed of the same elements of which the plant is composed, viz: carbon, hydrogen, oxygen, and nitrogen, with a minute portion of sulphur and other substances. Indeed this material is derived exclusively from the animal kingdom. Without attempting to describe the various transformations which take place among these organized molecules, a task which far transcends our knowledge or even that of the science of the day, we shall merely consider the general changes which occur of a physical character.

As in the case of the seed of the plant, we presume that the germ of the future animal pre-exists in the egg, and that by subjecting the mass to a degree of temperature sufficient perhaps to give greater mobility to the molecules, a process similar in its general effect to that of the germination of the seed commences. Oxygen is absorbed through some of the minute holes in the shell, and carbonic acid constantly exhaled from others. A portion then of the organic molecules begins to run down, and is converted into carbonic acid and, possibly, water. During this process power is evolved within the shell,—we cannot say, in the present state of science, under what particular form; but we are irresistibly constrained to believe that it is expended under the direction again, of the vital principle, in re-arranging the organic molecules, in building up the complex machinery of the future animal, or developing a still higher organization, connected with which are the mysterious manifestations of thought and volition.

In this case, as in that of the potato, the young animal as it escapes from the shell weighs less than the material of the

egg previous to the process of incubation. The lost material in this case as in the other has run down into an inorganic condition by combining with oxygen, and in its descent has developed the power to effect the transformation we have just described.

We have seen in the case of the young plant that after it escapes from the seed and expands its leaves to the air, it receives the means of its future growth principally from the carbon derived from the de-composition of the carbonic acid of the atmosphere, and its power to effect all its changes from the direct vibratory impulses of the sun. The young animal however is in an entirely different condition; exposure to the light of the sun is not necessary to its growth or existence; the chemical ray by impinging on the surface of its body does not de-compose the carbonic acid which may surround it, the conditions necessary for this de-composition not being present. It has no means by itself to elaborate organic molecules, and is indebted for these entirely to its food. It is necessary therefore that it should be supplied with food consisting of organized materials, that is of complex molecules in a state of instable equilibrium, or of power. These molecules have two offices to perform, one portion of them, by their transformations, is expended in building up the body of the animal, and the other in furnishing the power required to produce these transformations, and also in furnishing the energy constantly expended in the breathing, the pulsations, and the various other mechanical motions of the living animal. We may infer from this that the animal in proportion to its weight before it has acquired its growth will require more food than the adult unless all its voluntary motions be prevented; and secondly, that more food will be required for sustaining and renewing the body when the animal is suffered to expend its muscular energy in labor or other active exercise.

The power of the living animal is immediately derived from the running down of the complex organized molecules of which the body is formed, into their ultimate combination with oxygen in the form of carbon, water, and ammonia.

Hence oxygen is constantly drawn into the lungs, and carbon is constantly evolved. In the adult animal when a dynamic equilibrium has been attained the nourishment which is absorbed into the system is entirely expended in producing the power to carry on the various functions of life, and to supply the energy necessary to perform all the acts pertaining to a living, sentient, and it may be, thinking being. In this case, as in that of the plant, the power may be traced back to the original impulse from the sun, which is retained through a second stage, and finally given back again to celestial space, whence it emanated. All animals are constantly radiating heat, though in different degrees, the amount in all cases being in proportion to the oxygen inhaled and the carbon exhaled. The animal is a curiously contrived arrangement for burning carbon and hydrogen, and the evolution and application of power. In this respect it is precisely analogous to the locomotive, the carbon burnt in the food and in the wood performing the same office in each. The fact has long been established that power cannot be generated by any combination of machinery. A machine is an instrument for the application of power, and not for its creation. The animal body is a structure of this character. It is admirably contrived, when we consider all the offices it has to perform, for the purpose to which it is applied, but it can do nothing without power, and that, as in the case of the locomotive, must be supplied from without. Nay more, a comparison has been made between the work which can be done by burning a given amount of carbon in the machine, man, and an equal amount in the machine, locomotive. The result derived from an analysis of the food in one case and the weight of the fuel in the other, and these compared with the quantity of water raised by each to a known elevation, gives the relative working value of the two machines. From this comparison, made from experiments on soldiers in Germany and France, it is found that the human machine, in consuming the same amount of carbon, does four and a half times the amount of work of the best Cornish engine. The body has been called "the

house we live in," but it may be more truly denominated the machine we employ, which furnished with power and all the appliances for its use, enables us to execute the intentions of our intelligence, to gratify our moral natures, and to commune with our fellow beings.

This view of the nature of the body is the furthest removed possible from materialism; it requires a separate thinking principle. To illustrate this, let us suppose a locomotive engine equipped with steam, water, fuel,—in short, with the potential energy necessary to the exhibition of immense mechanical power; the whole remains in a state of dynamic equilibrium, without motion or sign of life or intelligence. Let the engineer now open a valve which is so poised as to move with the slightest touch, and almost by mere volition, to let on the power to the piston; the machine now awakes, as it were, into life. It rushes forward with tremendous power, it stops instantly, it returns again, it may be at the command of the master of the train; in short, it exhibits signs of life and intelligence. Its power is now controlled by mind—it has, as it were, a soul within it. The engine may be considered as an appendage or a further development of the body of the engineer, in which the boiler and the furnace are an additional capacious stomach for the evolution of the power; and the wheels, the cranks and levers, the bones, the sinews, and the muscles by which this power is applied.

There is however one striking difference between the animal body and the locomotive machine which deserves our special attention, namely, the power in the body is constantly evolved by burning (as it were) parts of the materials of the machine itself, as if the frame and other portions of the wood-work of the locomotive were burnt to produce the power, and then immediately renewed. The voluntary motion of our organs of speech, of our hands, of our feet, and of every muscle in the body is produced, not at the expense of the soul, but at that of the material of the body itself. Every motion manifesting life in the individual is the result of power derived from the death (so to speak) of a part of his

body. We are thus constantly renewed and constantly consumed, and in this consumption and renewal consists animal life. When the proper balance between these two processes is destroyed the derangement and death of the body ensue. The rational, directing, thinking, willing soul, analogous to that Divine intelligence manifested in all the works of Nature, dissolves its connection with matter, and finds in another, and perhaps successive conditions, an immortal existence.

In this great perpetual circle of change nothing is lost. The earthy matter absorbed by the roots of the plant is given back to the earth in the ejections and decay of the animal body; the carbon, the hydrogen, the nitrogen, are returned to the air whence they were drawn; the solar impulses by which all the transformations were effected, are restored unaltered in quantity to the celestial space; and in the case of man, the soul, fraught with the moral effects of its connection with matter, returns to its Divine Creator, the source of all power, moral, intellectual, and physical.

Mechanical Energy.

The last remarks will lead us naturally to the subject of mechanical energy and the correlation of physical forces, a comparatively new class of ideas, which is at present occupying the attention of some of the first men of Europe and this country. Indeed, one reason which has induced us to adopt the atomic theory in this essay is, that we might give the clearest and simplest view of these new and interesting ideas, as well as some of the deductions which have been made from them. The fact has been long conclusively established in the minds of scientific men, that matter cannot be annihilated, except by the almighty fiat of Him who called it into existence; and the idea has been lately adopted, that the natural forces associated with matter, namely, the attractions and repulsions, are also as indestructible as the matter itself; moreover, the tendency of scientific speculation at the present day is to the conclusion that all energy, as it is called, or that which produces the changes in the material

universe, is due to the movements produced by attraction and repulsion of the atoms in passing from a primordial state of instability to one of final stability or relative rest. It must be evident to any person who is acquainted with the simplest principles of mechanics, that in a universe in which all the atoms are in equilibrium, or have approached each other as nearly as possible, there can be no spontaneous motion. Such a universe must ever remain, in all its parts, a dead, inert, and lifeless mass. It can only be awakened to life and motion by the application of power from without. Mechanical energy is only exhibited while two atoms are rushing together; when they have united in combination, they exhibit an apparent neutralization of all power to produce change in themselves or other bodies.

"Fill," says Professor Faraday, "an India-rubber bag with a mixture of oxygen and hydrogen in the proportion of 8 parts to 1 by weight; and blowing with it a number of soap-bubbles in a large dish, apply a lighted taper to the bubbles and observe the result. It is a violent deafening explosion, attended with the evolution of light and heat, giving evidence of tremendous power. But now we come to the result of this explosion, which is water—*nothing but water*. To me the whole range of natural phenomena does not present a more wonderful result than this. Well known, and familiar though it be, a fact standing on the very threshold of chemistry, it is one over which I ponder again and again with wonder and admiration. To think that these two violent elements, holding in their admixed parts such energy, should wait until some disturbance is effected, and then rush furiously into combination, and form the bland and un-irritating liquid water, is to me, I confess, a phenomenon which awakens new feelings of wonder as often as I view it."*

Wonderful as this may appear, it is but a simple illustration of a general law. The power exhibited was in the momentum produced by the energetic action of the two atoms on each other, and the consequent high velocity with which they rushed into union. The noise produced was due to the intense agitation given to the air; the light and heat to the

*[Faraday's Six Lectures on the non-metallic Elements. Lect. iii, pp. 175, 176.]

agitation of the ætherial medium; and these together are equal to the energy generated by the reciprocal motion of the atoms. If by any means a force were applied to separate the atoms to the same distance at which they were at first, this force would be just equal to that due to the rushing together of the atoms. Two atoms separated, and in a condition to be violently drawn together, are said to be in a state of *energy* or *power*; but when they have entered into combination, they are then in a state of inertness. The same may be said of a weight elevated above the surface of the earth. A certain amount of muscular power must be exerted to overcome the attraction of gravitation, and to raise the weight to the given height, say ten feet. It is then in a state of power, or in a condition to produce permanent changes in matter, and other effects which we technically denominate "work."

The energy developed in the weight may be employed to drive a pile into the ground, or it may be made to turn a mill and grind corn; but the work done in these two cases, when properly measured, will be the same, and just equal to that expended in elevating the weight. If the weight be raised to double the height, twice the force will be expended in accomplishing this effect, and the weight in its descent to the earth will also do a corresponding amount of work. The explanation of the development of the energy exhibited in the fall of a body from a height will be plain when we consider that gravity acts on the mass with a force proportioned to the number of pounds in weight at every point in its descent; and if we suppose that in the first this attraction gave it a certain velocity, and gravity were then to cease, the body, on account of its inertia, would continue to descend with this velocity to the end of its course. But if the attraction continues to act, new impulses are imparted at every instant, and the velocity will continually increase until it reaches the ground, where it will produce an effect which is the equivalent of the power accumulated in its descent. The mechanical energy of matter therefore is measured by the distance of the atoms into the intensity of the attraction at the differ-

ent points of their path of approach. If the atoms of any part of the material universe are in the condition of the atoms of oxygen and hydrogen after they have united to form water—that is, in the closest approximation and a complete neutralization of their affinities—the matter in this portion of space will be entirely inert, and unless disturbed by extraneous force, no change can take place among its parts. Matter wanting that peculiar characteristic which eminently distinguishes mind, namely, spontaneity of action, all will be in perfect quiescence.

From the researches of the geologist, the chemist, and the physicist, we are enabled to assert that such is the condition of our earth and its attendant satellite. All the chemical elements which are found in the crust of the globe have gone into a state of permanent quiescence. The metals and oxygen have united to form oxides, and these with the acids to form other stable compounds; and were it not for the disturbing influence of the impulses from the sun, the present system of continued change, of growth and decay, of storms and of calms, would cease, and the whole surface of our planet would exhibit a dreary desolation of darkness and stillness, of silence and death. Indeed as it is, the changes and ever-varying phenomena in which we are so much interested, and a knowledge of which constitutes the highest earthly wisdom, are confined to an almost infinitesimal pellicle at the surface of the earth. Organic matter is found but a few feet below the surface of the soil, and plants cannot exist in the ocean beyond the depth to which the rays of the sun penetrate. But this state of things has not always existed. It is conclusively proved by the past history of the globe, as written upon the rocks which form its outer strata, that its atoms were once in a state of intense agitation, or in other words, that the globe was in a condition of high temperature, and that the vibrations have been imparted to the surrounding ætherial medium and and thus radiated off into space. We arrive at this conclusion, not only from an examination of the condition of the strata, but from the fact that wherever we penetrate beneath

the surface, beyond the depth of the influence of external climate, the temperature uniformly increases at the rate of about 1° F. for every 50 feet. Our globe then consists of a mass of matter which has been gradually cooled from a state of intense heat, and at its surface has arrived at a condition of equilibrium, the heat which its surface gives off into space being just compensated by that received from the sun. The permanency of our temperature therefore depends upon that of the great central luminary of our system itself. But whether

"The sun himself shall fade, and ancient night
Again involve a desolate abyss,"

must be left for future consideration.

The ideas which are here given had their origin in the attempts which were made to produce self-moving machines. The possibility of such contrivances appeared to be sanctioned by the apparently spontaneous motion of men and lower animals. The idea that these motions were the results of the chemical action of food had not yet entered the mind; and it was only after many fruitless attempts, and the expenditure of much thought, time, and labor that the conclusion was at length arrived at that a machine is a mere instrument for the application and modification of power or energy, and that in no case can it do more work or produce more changes in matter, or in other words, it can break apart no more atoms than are equivalent to the power which has been applied to it. The same amount of power which we apply at one extremity of a machine, properly estimated, is equal to the sum of the resistances at the other, and the two precisely balance each other. From considerations of this kind we arrived at the conception of the correlation of the physical forces and the re-conversion of the equivalent of one into that of the other.

We may do the same work by heat properly applied, or by a fall of water, or by muscular energy. For example, a disc of iron may be made to revolve rapidly with a mill driven by a fall of water, and if this is allowed to rub with some pressure against another iron plate a great amount of

friction will be produced; the mechanical collision of the surfaces will set the atoms of the plates in that state of vibration which constitutes heat, and which, if unobstructed, will be communicated to the surrounding ætherial medium and radiated to adjacent bodies or off into celestial space. But if detained and applied it may be used to produce changes in matter, such as the boiling of water, the driving of a steam engine, and other objects. Now, if it were possible to collect and concentrate all the impulses of the heat vibrations, and apply them without loss by means of a machine to the elevation of water, the quantity thus raised and the height to which it is raised would be precisely equal to the height and quantity of water, the fall of which produced the first effect. Similarly, if by a steam engine we put in motion the plate of a large electrical machine and disturb the equilibrium of the æther, condensing a portion of it in one part of space and rarefying it in another portion, the force which would be exerted in the restoration of the equilibrium, or in the electrical discharge, would be just equal to the amount of energy exerted in producing the coerced condition. If in this case the coerced equilibrium is retained for a day, a year, or a century, so long the amount of energy expended to produce it will, as it were, be locked up but not lost. It will be ready to appear and do work as soon as the detent which prevents the commencement of motion is removed. As a further example of this, suppose a heavy weight to be elevated by steam power to the top of a high pillar, and there placed on an equipoise, so that the least force applied may overturn it and enable it to commence its fall. In its descent it will receive at every instant a new impulse from gravity, and when it arrives at the ground it will expend its accumulated energy in penetrating the surface and in the production of heat, sound, and tremors of the earth. When the weight is resting on the top of the pillar, ready to fall off with the slightest touch, it is said to be in a state of potential energy; and when it has almost reached the earth and is moving with the full velocity of the fall, it has converted its potential energy into actual power.

The general conclusion which has been arrived at is that the different physical energies—whether called chemical action, heat, light, electricity, magnetism, muscular motion, or mechanical power, are all referable to the disturbance of the equilibrium of the atoms and the subsequent restoration due to their attractions and repulsions; and that all these forms of energy are in one sense convertible into each other, or in other words the force generated in the restoration of the equilibrium in one case is sufficient to disturb it, though in a different form perhaps, in another. We must guard against the erroneous idea which some have inconsiderately adopted, that one form of power can be actually converted into another, as heat into electricity, or the converse. The theory of energy merely declares that the power exhibited in the electrical discharge is the equivalent of the muscular energy expended in charging the battery, and not that muscular energy is converted into electricity.

The origin of heat produced by friction for a long time perplexed the most sagacious philosophers. Our celebrated and ingenious countryman, Count Rumford, caused a quantity of water to boil for several hours by the heat generated in boring a cannon; and after the process was ended, he found that the borings and the cannon contained as much heat as at the commencement of the experiment. From this result he boldly proclaimed that heat was not matter, but the vibrations of the atoms of matter, and that in his experiment the heat was generated by the friction of the drill on the metal.

Later researches have constantly tended to strengthen the probability of this view, and even to establish the general fact, that when mechanical power is produced by the expenditure of heat, a quantity of heat disappears, bearing a fixed proportion to the power produced; and conversely, that when heat is produced by the expenditure of mechanical power, the quantity of heat produced bears a fixed proportion to the power expended. Thus in the case of a steam engine doing no work, the quantity of heat given

out in the waste-pipe would be just equal to that received into the boiler, provided there were no loss from conduction and radiation; but in the engine drawing up water, for example, a quantity of heat is actually annihilated in doing the work. The vibrations of the atoms which constitute heat are stopped in giving motion to the piston-rod. Conversely, if the water which has been pumped up to an elevation were made in its descent to produce heat by means of revolving disks, the amount generated would be just equal to that which disappeared in the other case.

For practical purposes it is therefore of great importance that the ratio of equivalents of heat and mechanical power should be accurately determined, and for this purpose James P. Joule, of Manchester, has made a series of most delicate and beautiful experiments on the heat evolved by the revolution of paddle-wheels in baths of water, mercury, or oil. Motion was given to the paddle-wheels by a known weight descending from a given height; the amount of heat was found to be precisely the same with a given expenditure of mechanical power, whether the wheel revolved in water, mercury, or oil, proper allowance being made for the different densities and the different capacities of these bodies for heat. In this way, he found that the fall of a weight of one pound through 772 feet, or what would be the equivalent, the fall of a weight of 772 pounds through one foot, is just sufficient to raise the temperature of one pound of water one degree of Fahrenheit's scale. Seven hundred and seventy-two pounds falling through one foot is therefore considered as the unit of the working power of heat; and in honor of the investigator who has thus enriched modern science with one of its most valuable means of calculation, applicable to every part of physical research, it is denominated "Joule's unit." By it we are enabled to express in terms of the descent of a weight the equivalency of all the forces of Nature, and thus to reduce the mechanical conception of their relations to its greatest simplicity, and to apply mathematical reasoning to a variety of problems heretofore excluded from the province of this great logical instrument, so essential in

the deduction of effects from complex relations. The descent of a weight is chosen, because it is perhaps the most familiar, and of the easiest conception and application. The value of a fall of water is always estimated by the quantity of liquid multiplied by the height through which it descends. If we multiply these together, and divide by 772, we shall have the number of degrees of heat that this will impart to a pound of water; and conversely, by knowing the number of degrees of heat as measured by the number of pounds of water raised one degree, we shall have the number of pounds of water which can be elevated to a given height by a perfect machine; and when such effects are submitted to this calculation, we find that the steam engine, in its most improved form, is far from utilizing all the heat applied to it; by far the greater portion is expended in the separation of the atoms of water in radiation, in overcoming friction, and in the production of vibration and useless motion.

Mr. Joule also established the relations of equivalence among the energies of chemical affinities of heat, of combination, or of combustion, of electrical currents in the galvanic battery, and in electro-magnetic machines, and of all the varied and interchangeable manifestations of caloric action and mechanical force which accompanies them. A series of experiments has also been made on the heat of animals, which is found to be the equivalent of the chemical combination of the food and the oxygen which they inhaled.

The influence which investigations of this kind are to have on the future history of mechanical arts and the production of labor-saving machines, and on the increased power of man in controlling the innate forces of matter, it is impossible to estimate.

“The food of animals is either vegetable, or animals fed on vegetables, or ultimately vegetable after several removes. Except mushrooms and other fungi, which can grow in the dark, are nourished by organic food like animals, and like them absorb oxygen and exhale carbonic acid,—all known vegetables get the greater part of their substance (certainly all their combustible matter) from the decomposition of carbonic acid and water absorbed by them from the air and

soil. The separation of carbon and of hydrogen from oxygen in these de-compositions is an energetic effect equivalent to the heat of re-combination of those elements by combustion or otherwise. The beautiful discovery of Priestley, and the subsequent researches of Sennebier, De Saussure, Sir Humphry Davy, and others, have made it quite certain that those de-compositions of water and carbonic acid only take place naturally in the day-time, and that light falling on the green leaves, either from the sun or an artificial source, is an essential condition without which they are never effected. There cannot be a doubt but that it is the dynamical energy of the luminiferous vibrations which is here efficient in forcing the particles of carbon and hydrogen away from those of oxygen, towards which they are attracted with such powerful affinities, and that luminiferous motions are reduced to rest to an extent exactly equivalent to the potential energy thus called into being. Wood fires give us heat and light which have been got from the sun a few years ago. Our coal fires and gas lamps bring out, for our present comfort, heat and light of a primeval sun, which have lain dormant as a potential energy beneath the seas and mountains for countless ages." (Prof. William Thomson.)

A striking example of the transformation, as it were, of the force of motion into heat is exhibited by an article of apparatus now in the cabinet of the Smithsonian Institution and devised by M. Leon Foucault, of Paris. Between the poles of a strong electric-magnet a heavy metallic disc is made to rotate, and although the revolving body does not touch the magnet, yet its motion is stopped by it in a few seconds. The momentum of the disc which is thus overcome gives rise to heat; for the re-action of the magnet produces a current of electricity, and in the resistance to this the heat is generated. A body in motion is in a state of power, and it cannot come to rest without producing some effect on the surrounding matter. The ultimate effect in this case is an agitation of the atoms of the metal.

Condition of the Earth in Space.

Having given a general view of the atomic theory in its widest generalizations, we now propose to consider its application to the physical phenomena of our globe. For this pur-

pose we will briefly recall some of the elementary facts of astronomy.

The earth is a globe very slightly flattened at the poles, isolated in space, supported upon nothing, and only connected with other bodies of the universe by the all-pervading force of attraction. In this free space it turns upon itself with a regular motion around an ideal axis which pierces its surface at two opposite points or poles, which have never sensibly varied their position. It also moves in space describing around the sun in the course of a year a slightly elliptical curve called its orbit. But this movement of translation around the sun does not interfere with the rotation of the earth around its axis; for in accordance with the second fundamental law of motion, two motions of this kind may exist in a body at the same time. If the earth's axis were at right angles to the plane of its orbit, but slight variations would be found in the temperature at its surface in different periods of the year. The axis is not however thus placed, but is inclined at an angle of about twenty-three and a half degrees to the plane above mentioned; and this fact, which at first sight might appear of little consequence, in reality produces all the alternations of seasons, and is connected with all the changes of climate of the surface of the globe. Gradual changes of climate cannot be produced by a change in the axis of rotation, as some have supposed, since this would alter the whole form of the earth, and produce other changes incompatible with the facts of observation.

The position, the form, and the movement of the earth are similar to those of the other planetary masses which we see isolated in space under the form of globes, turning around on an axis within themselves, and around the sun in elliptical curves. While we observe that the earth is the centre of the orbit of our moon, we see that four moons turn around Jupiter, seven around Saturn, and six around Uranus. A planet, with the moons which accompany it, form what is called a *planetary system*, and all the planets taken together, with the sun, constitute what is denominated the *solar system*. In this system the earth occupies the third place from the

sun, from which it is removed ninety-five millions of miles at its mean distance. Neptune occupies the most distant limit, and is more than thirty times farther removed than the earth from the principal centre of influence. But these distances, though greatly beyond our definite conceptions, are nothing in comparison with the intervals which separate the sun from the fixed stars. These bodies like the sun are self-luminous, and are without doubt centres of planetary systems; but they are at such an inconceivable distance that light itself, which requires but eight minutes to reach us from the sun, occupies years of time in its journey from the nearest of them. But all the stars which are visible to the naked eye form only a single group, which, if viewed at a sufficient distance, would appear in the heavens as only a luminous cloud or spot, and would resemble the nebulous patches which we perceive here and there in different parts of the heavens by the aid of powerful telescopes. This universe, unbounded (at least to human intelligence)—is composed of isolated groups of stars, and perhaps of orders of arrangement still more elevated. In this magnificent assembly our nebula is only a spot in the infinity of spots; our sun is only a star in the midst of the stars of the group to which it belongs; and among the planets which revolve around our sun, the earth is one of an inferior order.

Starting from the grouping of gross atoms, which we have previously given, and extending the analogy, the thought has been expressed that our earth might be compared to an atom; the earth and moon to a compound atom; the whole system to a molecule; and our sun, and all the stars of the group to which it belongs, as the great solid of solids, and thus in one conception embracing the whole material universe. But to limit our speculations we may inquire whether the infinity of stars by which we are surrounded has any influence upon the climate and temperature of this earth.

Influence of the stars.—It is well known that at one time the stars were supposed to influence human destiny, and though astronomy has discarded most of the pretensions of

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her progenitor—astrology, yet in this instance, modern science has shown that the stars have really a physical influence upon our earth and on every other planet of our system. If from any point in space a line be extended in thought in any direction, it will ultimately meet a radiating body; and hence every point in space must be constantly traversed from all directions with radiating impulses which give it a definite and fixed temperature. For example, our sun sends a ray to every point of the universe, and every other sun sends a ray to the same point, and the sum of all these rays will constitute the temperature of that point. We say the *temperature* of that point, by which we mean the effect which would be produced on a thermometer if put in that place; not that there is any temperature in celestial space, for this, as we have seen, belongs to gross matter, and is produced by the motion of its atoms. The term however is convenient, and we shall continue to use it.

If the radiating power of the suns remained without change, then the temperature of each point in space would be unchangeable. From this consideration it follows that the planetary space in which our earth is moving has in one sense a fixed temperature (independent of the heat of the sun,) derived from all the other suns of the universe; and this temperature, as we shall hereafter see, has a marked influence on the temperature of the globe.

We shall return to this subject again, and at present shall merely state that at the polar regions of our earth during the months of winter, the space immediately contiguous to the surface is screened from the heat of the sun, and consequently the earth by its radiation, must fall in temperature nearly to that of celestial space. A similar screening takes place in succession on all parts of the earth's surface during the night; and as the loss of heat by radiation depends (as we shall see) upon the temperature of the space into which the rays are sent, every part of the earth's surface must be affected more or less by the temperature of interplanetary space; and if this were to vary, though our sun might continue constant in its emanation, the average terrestrial temperature would be subject to a change.

We cannot however explain the effect of the temperature of planetary space upon our earth until we have further considered the subject of heat.

Heat of the Earth.

The temperature of the earth is derived from three sources, namely, the *original* heat of the earth, the heat of celestial space, and the heat of the sun. Before however giving an account of the heat derived from these sources, we shall consider the character of radiant heat, as developed by the researches of Melloni and others.

Radiant Heat.—The impulses which are received from the sun, as we have seen, are far from being simple in their nature. We know that a beam from this luminary consists of at least four different classes of emanations, namely, of light, of heat, of chemical action, and of phosphorogenic effect. We also know that the first class, that of light, consists of a number of different emanations which produce in us the sensations of the different colors of the spectrum, and from analogy we might have inferred that the heat emanations also consist of a number of rays, possessing different properties, and producing at the surface of the earth different physical and perhaps physiological effects.

Let us begin with heat of the lowest intensity, or that which is supposed to be composed of waves of the greatest length; for example, the radiation from a canister of hot water suspended in mid-air. If this have a temperature in the least degree above that of the surrounding bodies, they will increase in temperature, while the vessel itself will slowly cool. The rapidity of cooling will gradually diminish in a geometrical ratio, as the temperature of the canister approaches that of the surrounding bodies, and they will finally arrive at a state of dynamic equilibrium. The canister at this point does not cease to radiate, but continues to send impulses in every direction, receiving as many impulses from the surrounding bodies, (including the air,) as it sends off from its own surface.

The heat from this source possesses peculiar properties. First, it is readily absorbed by all bodies in proportion to some peculiarity of the texture of their surface, but is *wholly independent of the color*; or in other words this kind of heat, unlike light, is absorbed by light-colored substances as well as dark, and this fact would be in accordance with the hypothesis assumed, which supposes these two emanations to consist of waves of different lengths, and perhaps of slightly different form. Secondly, this kind of heat is incapable of passing by direct radiation through many media which are freely traversed by light, such as glass, alum, and many other transparent substances, while it is freely transmitted through polished plates of rock-salt, and partially through many other bodies, some of which are impervious to light. The former class of bodies is called *athermanous*, the latter *diathermanous*.

Let us now suppose the radiating body to be one which can be increased in temperature until it becomes red-hot. At a certain stage of incandescence, other rays than those described as capable of exciting heat begin to be given off along with the former, which are distinguished by different properties. First, they tend to be absorbed by all bodies in proportion to the darkness of their color, and approximate in this respect to the property of light. Secondly, they possess a property of transmissibility without diminution, through all transparent substances, through colorless media, and in various proportions through colored media, according to the nature of the latter.

While bodies heated below redness give off exclusively rays of the first class, (though approaching in character those of the second as the temperature is increased,) incandescent bodies simultaneously give off both species.

As the intensity of heating still further increases, rays of less and less length are given off, until they arrive at the limit of the perceptibility of the sense of vision, and only render their existence manifest by chemical and phosphorogenic effects.

The following table exhibits some of the results which

Melloni obtained by experimenting with different sources of heat and different substances.

Relative absorbing power by different substances of different kinds of heat.

SUBSTANCES.	Naked flame.	Incandescent platinum.	Copper, at 75° F.	Copper, at 212° F.
Lampblack.....	100	100	100	100
White lead.....	53	56	89	100
Isinglass.....	52	54	84	91
Indian ink.....	96	95	87	85
Shellac.....	43	47	70	72
Polished metal.....	14	13.5	13	13

As an illustration of the effects of radiant heat of different kinds, we may mention the fact, long observed, of the melting of snow near the trunks of trees and other dark-colored bodies. That this effect is not due to the natural heat of the plant is evident from the fact that it is equally exhibited around the stumps of dead trees, and dark-colored objects of an entirely different character. The rays of heat from the sun, (as before stated,) possessing similar properties to those of light, are absorbed by dark substances, and freely reflected from light ones. The facets of the small crystals of snow reflect this heat almost entirely, while it is absorbed by the dark surface of the wood of which it raises the temperature, thus producing a new source of emanation. The heat however given off from the wood, is that of long waves of low intensity, which is equally absorbed by light and dark bodies; hence it enters the snow, raises its temperature, and converts it from a solid to a liquid condition. We may imitate this action by supporting at a little distance above a surface of new fallen snow a piece of pasteboard, both sides of which have been covered with lampblack, and the whole freely exposed to the sun's rays. It will be found that the melting of the surface within the shadow is much more rapid than that exposed to the direct rays of the sun. The same result may be produced by the rays from an argand lamp. Having filled a square box with new fallen snow

slightly packed, and all above the rim having been removed by means of a ruler, so as to present a uniformly plain surface, the box is turned on its side opposite the lamp, and the pasteboard interposed. In a short time the plain surface of snow will be hollowed out beneath the disk, and at the end of half an hour the cavity will be several lines deep at its centre. When the same experiment is repeated by substituting for the lamp an iron ball heated to about 400° F., the phenomena present themselves in a reverse order, that is to say, the melting of the snow would be more abundant where the direct rays impinge on the surface, than where they are intercepted by the interposed disk, and instead of a hollow, a protuberance would be produced at the centre of the shaded portion. If we substitute in this experiment for the black disk of pasteboard one covered with white lead, the heat will not be absorbed, but will be reflected as from the snow itself.

Another example of the transmission and, as it were, transformation of radiant heat from the sun is afforded in the high temperature produced by the ordinary hot-bed of a garden. The solar rays, consisting of short vibrations, readily pass through the glass cover, and are absorbed by the dark ground, the atoms of which they put into more rapid vibration, and these in turn give rise to new emanations, which consisting of long waves are arrested by the glass, and the temperature of the enclosed space is thus constantly increased. It is also on the same principle that the radiant heat of a stove does not pass out into space through the windows of a house, though a considerable portion of the radiant heat from an open fire would be lost in this way.

We may apply the foregoing principles to explain the accumulation of heat at the surface of the earth. The transparent envelope which covers the surface of our planet is not entirely diathermanous; and though it transmits freely the intense rays of the sun it stops those of the long vibrations. The surface of the earth is then in the condition of the ground under the glass of the hot-bed; it is constantly absorbing and receiving heat of high intensity, and constantly radiating heat of intermediate intensity. Let us suppose all

heat removed from the earth, and the sun suddenly allowed to shine upon it. In this case, all the rays which traversed the atmosphere and reached the earth would be absorbed. None would be radiated into space until the temperature of the surface was so elevated that the rays emitted from it could permeate the atmosphere.

The surface of the earth at first would therefore receive more rays than it gave off. Its temperature would increase and with each increase of temperature a greater number of rays would be produced of such intensity as would enable them to permeate the atmospheric envelope, and finally an equilibrium would be attained in which the rays sent off in a given time would be just equal in number to those received.

The point of temperature at which this equilibrium would take place will depend on the height and permeability of the atmosphere. If the aerial envelope offered no impediment to the escape of heat of the lowest intensity, the equilibrium would take place at so low a temperature that all bodies capable of freezing would be perpetually in a solid state. If on the other hand the atmosphere were more dense than it is, or in other words, more impervious to rays of a higher intensity than those which now pass through it, the temperature of the surface of the earth would increase until the heat given off would again be equal to that received. The new equilibrium would be permanently retained, and the whole average temperature of the surface of the globe would be elevated.

Heat from the Stars.—The temperature therefore of the surface of a planet depends upon the nature of its atmosphere, provided the heat which falls upon it is derived from a source of high temperature: now radiations from the stars are of this character, since they come from self-luminous bodies, which are probably suns of other systems. The radiations from them can therefore readily pass through our atmosphere, and excite heat vibrations in the surface materials of the earth. The intensity of these vibrations must increase until it becomes so great that the radiations produced can permeate the aerial covering, and in this way

even the heat of the stars may so accumulate as sensibly to contribute to the temperature of the earth. Though at first sight it may appear that the effect from this source must be exceedingly feeble, yet when we reflect that the heat of the stars comes from every part of the whole concave of the heavens, while that of the sun proceeds from a disk which occupies only the five-millionth part of the whole sky, we may be inclined to attribute to the stellar radiation a much greater importance than without this reflection we should ascribe to it.

M. Pouillet, of Paris, has made a series of very ingenious researches on the subject of the temperature of space, and has arrived at very unexpected results. He employed in his observations an instrument to which he gave the name of "actinometer," or ray-measurer. It consisted of a cylindrical box of polished silver, about eight inches in diameter, and five in height, enveloped in swan's-down, and enclosed in an outer cylinder, so as to prevent as much as possible the effect of the temperature of the circumambient air. The box was filled with several layers of swan's-down, so supported as not to press upon each other. In the centre of the upper surface of the open box was placed the bulb of a thermometer, the stem projecting horizontally. A cylindrical border was raised round the edge of the box, to cut off the lateral rays, and at such a height that two-thirds of the whole sky could be seen by an eye at the point occupied by the bulb. The thermometer thus enclosed was turned during the night to the zenith, and exposed to the radiation from the clear sky. The temperature of this thermometer and one exposed to the air at four feet from the ground was observed hourly.

If the heat of the surrounding air were entirely excluded from the enclosed thermometer, it is evident that it would only be affected by the radiation from celestial space, and from the atoms of the air in the column between it and the top of the atmosphere.

Of these two sources of radiation one, namely that of celestial space, would be constant and remain the same during

the whole night, as well as different nights, while the other, namely the radiation from the air, would vary from hour to hour, since it depends on the varying temperature of the atmosphere.

By obtaining a series of observations in different states of the atmosphere an assumption could be made as to the fixed temperature of space which, when subtracted from the temperature observed, would give the radiation of the column of the atmosphere.

Since it was impossible to cut off all the heat from the instrument except that which it received from the sky and air above, and since it was exposed to but two-thirds of the celestial hemisphere, some correction was necessary to reduce the observed temperature to the true one. This was found by making an artificial sky, formed of a zinc vessel about forty inches in diameter, the bottom coated with lampblack, and the whole filled with a refrigerating mixture. Beneath this the "actinometer" was vertically placed at such distances as to expose it successively to one-quarter, one-third, and two-thirds of the hemisphere; and by repeating these experiments with different temperatures of the artificial sky, it was found that if from the temperature of the surrounding air $\frac{3}{4}$ of the lowering temperature of the actinometer were taken away, the temperature of the artificial sky would be obtained, since the same ratio would obtain in the case of the real sky. In order to find therefore in all future experiments the temperature which the actinometer ought to assume under the radiation from space and the air above, it was only necessary to subtract the degree given by the instrument from the temperature of the surrounding air and multiply this by $\frac{9}{4}$. From a series of observations thus corrected, he found for the fixed part of the temperature given by the instrument, or in other words the temperature of space, a value of -142° C. or -222° F. This temperature is much lower than that obtained before from considerations of a more theoretical character. M. Pouillet however thinks that it cannot be far from the true temperature of celestial space, since a thermometer placed upon the coldest part of the earth and

exposed to the clear sky, always falls by its own radiation several degrees lower than the temperature of the air; which it would not do if the temperature of space were not lower than -60° , since as it approached that temperature at places near the pole, the extra cooling from exposure to the sky would be very little. Mr. Espy concludes, from theoretical data, that the estimate of Pouillet is near the truth.

Pouillet finds, from the data given above, that the total quantity of heat which space transmits in the course of a year to the earth and atmosphere, would be sufficient to melt a stratum of ice upon our globe of 85.28 feet in thickness. From other investigations of a similar character, which we shall presently describe, he finds that the quantity of solar heat received by the earth in the course of a year is sufficient to melt 101.68 feet of ice. From these two sources together then the earth receives a quantity of heat sufficient to melt 187 feet of ice. These results are of so unexpected an amount that though obtained by instruments and methods which are apparently unexceptionable, they have not fully obtained acceptance, and the subject is therefore still open for further examination.

Terrestrial temperature.—If the earth were exposed in space without an envelope and without receiving radiation from any source, it would sink to the zero of temperature, or that at which the atoms would cease to vibrate, and this, according to the mechanical theory of heat, would be about 500° below the freezing point of Fahrenheit's scale.

If the earth were exposed without an envelope to the temperature of space it would, according to the results obtained by Pouillet, fall to -222° of the same scale.

With the present envelope and stellar radiation it would stand at -128° . The heat necessary to make up the actual temperature of the earth beyond this degree is due to the sun's accumulated heat under the envelope.

Pouillet has also made a series of researches on the absolute amount of heat from the sun. He used in his investigations an instrument to which he gave the name of pyr-heliometer (measurer of the heat of the sun). It con-

sisted of a flat cylindrical vessel, the top of which was of thin silver, of about four inches in diameter and six-tenths of an inch in height or thickness. It was filled with 100 grammes of distilled water, and in the middle of this liquid was placed the bulb of a thermometer with a fine bore and a long stem projecting downward in the direction of the axis of the cylinder through its lower surface.

The observations were made in the following manner: The upper surface of the vessel, coated with lampblack to render it absorbent of heat, was turned directly towards the sun, the water being kept in a state of constant agitation in order to equalize the heat. The increase of temperature received from minute to minute in the course of five minutes was noted. The vessel was then placed in the shade while its face was exposed to a portion of clear sky near the sun, and the loss of temperature from minute to minute during five minutes was again noted. A little reflection on the principles of the interchange of heat, according to which bodies are constantly radiating even while they are receiving heat from other bodies, will render it evident that in order to find the amount of temperature communicated by the sun in a minute of time we must add the loss of temperature during the shading of the instrument to the gain of temperature noted during the direct exposure to the sun, for while the instrument was receiving heat from the sun it was at the same moment radiating heat to that body. To find from the indications thus obtained the absolute amount of heat which falls on the face of the vessel in one minute of time we must make a correction for the absorption of heat by the metal, and allow for the specific heat of the water, that is, the relative quantity necessary to elevate a pound of this liquid one degree of Fahrenheit's scale. In this way the quantity of heat which falls on a given surface, (say a square foot,) perpendicular to the solar beam at the surface of the earth is determined. But this quantity is not all that would be given to the same surface were the atmosphere removed, or if the same experiment were made at the outer limits of the aerial covering of the globe. A portion of the heat is

absorbed and another portion reflected from the atoms in its passage through the air, and in the solution of the problem under consideration it became necessary to know the amount of loss from this cause. To ascertain this the experiment was made while the sun was on the meridian and at different degrees of elevation even down to near the horizon. The diameter of the earth, the approximate height of the atmosphere, or the length of the column of air traversed by the ray which passes from the zenith, and also the angle of elevation of the sun being given, the lengths of the several lines through the atmosphere traversed by the respective rays were readily calculated; and if we suppose that the amount of heat received at the outer limit of the atmosphere is invariable it is not difficult to determine the part which is absorbed. The numbers obtained by observation consisted of two quantities, a constant and a variable one; the former being the heat of the sun, and the latter the amount absorbed in passing through the different lengths of atmosphere.

From these data the amount of heat received from the sun on a square centimetre at the limit of the atmosphere, (and which it would equally receive at the surface of the earth if the air did not absorb or reflect any of the incident rays,) was ascertained to be 1.7633 units of heat in one minute of time: equivalent to 11.376 units per square inch, in the same period. It was also found that the atmospheric absorption of the rays directly from the zenith was comprised between eighteen and twenty-five-hundredths of the whole, even in cases where the sky was perfectly clear.

The quantity of heat which the sun sends to one centimetre of the earth's surface during one minute of time by its perpendicular action having been determined, it was not difficult to ascertain the total quantity of heat received by the whole illuminated hemisphere in the same time. Indeed this quantity is nearly the same as that which would fall on the plane of a great circle of the earth. From this can be readily deduced the amount of heat which would be distributed over the entire surface of the earth during a year;

and this was determined to be 231,675 units falling on each square centimetre of the whole surface. Calculating the amount of ice which this quantity of heat would melt, Pouillet obtained a thickness of 30·89 metres, or a little more than 101 feet; that is, if the total quantity of heat which the earth receives from the sun in the course of a year were uniformly distributed over all points of the globe, and were employed without loss in dissolving ice, it would melt a stratum having the above thickness.

The data given by these experiments enabled him to solve another problem which would appear even of a more transcendental character: that is, the amount of heat given off by the whole surface of the sun in a given time. For this purpose it is only necessary to consider the sun as the centre of a spherical enclosure, the radius of which is the distance from the earth to the sun;* and it must be evident that on each square centimetre of the concave surface of this vast sphere as much heat is received as on a square centimetre at the surface of the earth. If then the number 1·7633, before obtained, is multiplied by the number of square centimetres in this spherical surface the absolute quantity of heat given off by the sun during a given time will be ascertained. Or by reference to the visual angle subtended by the sun, the number expressing this quantity for each minute of time may be stated as 84,888 thermal units for each square centimetre of the solar surface.

If this quantity of heat emitted by the sun were exclusively employed in dissolving a stratum of ice, applied to the solar surface, and enveloping it on every side, it would melt in one minute a stratum of 11·8 metres thick; and in one day a stratum of 16,992 metres, or about $10\frac{1}{2}$ miles.

These results cannot be considered more than approximations, though in the progress of science, they may be rendered much more precise, and may be applied to solve many problems relative to the physical phenomena of the earth and our solar system.

*The proportional amount of the entire solar radiation intercepted by the terrestrial hemisphere is $1 \div 2$ 300,000 000.

Original heat of the earth.—Besides the smaller influence of celestial space, and the governing one of the emanations from the sun, there is another source of terrestrial heat, which, though it at present produces scarcely an appreciable effect upon the temperature of the surface, was once powerfully active in effecting geological changes, and in so modifying the surface of our planet as to give rise to the diversities of surface constituting mountains, seas, and continents, which now determine the varieties and peculiarities of our present climates, and may in the future be of vast practical value in its applicability to the wants of life. We allude to the internal heat of the earth.

That the earth was once at least in a liquid condition by heat, can scarcely be doubted, when all the cumulative evidence in favor of the hypothesis is considered.

First. Self-luminous bodies are met with in every part of the visible universe, and if we follow the strict inductive process, allowing no more causes than are true and sufficient, we must admit that these bodies are intensely heated. It is therefore not impossible that the earth itself may have been at one time a self-luminous star.

Second. The surface of our moon, though it now gives little or no indication of heat, appears when viewed through a powerful telescope almost covered with the craters of extinct volcanoes; and hence we may infer that it has cooled down from a high temperature to its present condition.

Third. Every portion of the earth's crust exhibits the remains of igneous action, and the facts of geology are inexplicable on any other hypothesis than that of the past high temperature of our globe.

Fourth. On every part of the earth where the experiment has been made, starting from the point where the sun's influence ceases, there has been found an increase of temperature as we descend toward the centre, at the rate of about a degree for every fifty feet.

Fifth. On different parts of the earth's surface springs of hot water are found bursting forth.

Sixth. There are on the surface of the earth several hun-

dred volcanoes, which occasionally emit heated materials, and in some cases incandescent lava.

Seventh. The oblate form of the earth is on an average that which would be due to the rotation of a liquid mass.

From all these facts we may now safely admit as a definite theory, the hypothesis which was at first a mere antecedent probability, namely, that the earth was at one time in a highly heated state, and that its interior, even at the present moment, is still at a very elevated temperature. If we apply this hypothesis to the facts of geology as they are generalized and arranged at the present day, we have a complete explanation of the whole; or if there be any outstanding phenomena not yet included in this generalization, their number is so small in comparison to those included in it, that they may reasonably be left for the present until further discovery shall throw more light upon their character. The great principle of universal gravitation was not abandoned though at one time several facts in regard to the motion of the moon could not be referred to it. The same consideration applies to moral subjects as well as to those of science.

Equilibrium of the Atmosphere.

The aerial covering which surrounds our earth may be compared to an ocean, of which the bottom is composed of land and water, which has a definite surface above, probably agitated by tidal waves of great extent and magnitude. Although nearly eight hundred times lighter than water at the surface of the earth, yet it possesses a very appreciable weight, since a cubic yard of it weighs about two pounds, and consequently when moving with high velocities it produces great mechanical effects upon bodies subjected to its momentum.

This ocean, unlike the aqueous ones belonging to our earth, diminishes in density very rapidly as we ascend, and finds its limit at that elevation at which the repulsion of the last layer of atoms added to the centrifugal force of the earth's rotation is just balanced by the attraction of gravitation.

In order to simplify the conditions and to give precise ideas of the mechanical equilibrium of the atmosphere we will at first suppose it to be a body consisting of simple atoms, which though they obey the attraction of the earth repel each other. This repulsion increases, as we have said in our exposition of the atomic theory, with a diminution of the distance of the atoms—a fact which may, perhaps, be best illustrated by a portion of air confined by a movable piston in a tube closed at the bottom, as in the case of the ordinary fire syringe, the well known instrument used for igniting tinder by means of the condensation of a portion of air. If such an instrument be placed under the receiver of an air-pump, and the pressure of the atmosphere be removed from it, the air which is contained under the piston will expand; and if the tube be sufficiently large this expansion will continue until the repulsive energy of the atoms under the piston is just equal to the weight of the piston itself. If we now double the weight of the piston it will descend until the air is compressed into half its first volume. At this point a new equilibrium will take place between the weight of the piston and the repulsive energy of the atoms. If another addition be made to the weight of the piston it will descend through another distance, and in all cases the compression will be inversely proportioned to the weight applied; but the density of the air, that is, the weight for a given quantity increases as the bulk diminishes, and therefore in all cases of a gas the density or the number of ponderable atoms in a given space will be inversely proportioned to the pressure applied.

This fact was discovered independently by an English and a French philosopher, and is generally known by the name of the discoverers, namely, the law of Boyle and Mariotte, but perhaps more frequently it bears the name of the latter.

The same law applies to all other gases within certain ranges. In the case of atmospheric air, within the limit of experiment it appears to hold without variation, or if any, with a very minute one, when great pressure is applied in connection with a great reduction of temperature. In the

case of carbonic acid, the range of distance of atoms is much less in which this law is found; for by mechanical pressure the gas is converted into a liquid, a sudden change taking place in the intensity of the repulsion of the atoms at this point. Vapor of water, separated from the liquid which produced it, obeys the same law as that of air; but in this instance the range of atoms is still more limited than in that of carbonic acid, and with a slight pressure, and at the ordinary temperature of the atmosphere, the vapor is converted into a liquid.

The atmosphere being subject to the law of Mariotte, we shall now proceed to inquire what will be its condition of equilibrium or rest.

First. If we suppose the whole atmosphere surrounding the earth to be divided into a series of strata of equal weight, as thin as may be necessary, and separated by ideal surfaces perpendicular to the plumb line, these surfaces will rest upon each other, and be in a state of equilibrium when each part of the same stratum is of the same density.

Second. In order to a stable equilibrium, the density of each stratum must diminish from below upward.

Third. The upper stratum must be below the point where the centrifugal force, derived from the rotation of the earth, becomes equal to the weight of the air at this point.

If the first condition is not fulfilled, (that is if the equality of the density of the strata be not the same at all points,) the heavier parts will flow below those which are less dense, and buoy them up in the same manner as the heavier liquid sinks below the lighter one; and it is evident that if the upper strata were heavier than the lower ones, an unstable equilibrium would be produced which the slightest agitation would overthrow.

Lastly, if the atmosphere extended upward above the point where the centrifugal force equalled the weight of the gas, the whole atmosphere, strange as it may appear, would fly off into void space. To explain this, it is necessary to demonstrate the important though paradoxical fact which results as a logical consequence of the law of Mariotte,

that the total height of an atmosphere surrounding a planet does not depend upon the quantity of gas of which it is constituted. To prove this, let us imagine a vertical column, say an inch square at the base, filled with air of a given density extending to the top of the atmosphere. Let us suppose this column to be divided into portions an inch high throughout its whole length by movable planes, and into each one of these portions double the quantity of air to be introduced. The lowest portion, namely, the first inch, will not be enlarged by this condition; for though twice as many repellant atoms are introduced into the same space, tending to repel upward the first dividing plane, yet this plane will be pressed downward by twice the weight, because twice the number of atoms have been introduced into all the strata above.

The same reasoning may be applied to all the successive strata until we come to the very highest. On this no additional weight is placed, and it would therefore expand until the diminution of its elasticity just equals its own weight, and at this point the equilibrium will take place. If however this point should be just at the place of equilibrium where the weight of the atom would be overcome by the centrifugal force, the upper film would be removed, another would expand into its place, and another, and another, until the whole atmosphere would be withdrawn. This, as we have said, is a logical consequence of the extension of the law of Mariotte, and has been applied by Dalton and others to determine the heights of mixed atmospheres, or of atmospheres of different densities. But the height of the atmosphere is probably far below the point where the weight of the atom is equal to the force of gravity, since this may be found by calculation to be at about 5.6 times the earth's radius from the surface at the equator, or about 22,400 miles. If we suppose the column to be formed of a lighter gas, as for example hydrogen, the atoms of which have the same repulsive energy as those of air, then the column will be inversely proportioned to the density at the surface, and from this we can readily calculate the relative heights of atmos-

pheres of different gases, having different densities at the surface of the earth. These heights will evidently be inversely as the densities, or in other words the specific gravities, of the same gases under the same pressure. If the specific gravity of hydrogen be represented by 1, that of nitrogen in round numbers will be 15, that of oxygen 16, and that of carbonic acid 22, and the total heights of atmospheres of these gases will be inversely as these numbers; or if we call the height of an atmosphere of oxygen 60, then the heights of atmosphere of these gases will be as follows:

Gases.	Specific gravity.	Height of atmospheres.
Hydrogen -----	1	960
Nitrogen -----	15	64
Oxygen -----	16	60
Carbonic acid -----	22	44

In the foregoing the repulsive energy has been considered as increasing in conformity with the law of Mariotte, directly as the pressure and without regard to the increase of repulsion caused by heat; but if we suppose that the repulsion of the atoms of the lower stratum is increased by heat, they will be farther separated, and the space occupied by them enlarged. But if the heat extends upward through the whole, each of its parts will be uniformly expanded, and hence the relative height of atmospheres of different grades will not be altered by an increase of heat, provided this increase is the same in each gas. The absolute heights will however be increased $\frac{1}{490}$ part for each degree of Fahrenheit's scale above its volume at the freezing point.

In order to obtain or determine an equilibrium of the atmosphere when the natural repulsion of the atoms is increased by heat, each stratum as we ascend must at least contain the same amount of caloric. In this case, if a quantity of air be removed from a lower to a higher position, it will expand on account of the reduced pressure, and the same amount of heat being now diffused through a larger space, the intensity of its action or its temperature will fall, and

thus a reduction of sensible heat will be observed as we ascend in the atmosphere. The equilibrium we have described would not however be a stable one, and hence the upper strata of the atmosphere contain more heat per pound than the lower.

Until about the middle of the last century, the atmosphere was supposed to consist of one simple homogeneous substance, and after modern chemistry had discovered it to be a compound, the ingredients were thought to be chemically united. It was also supposed, until the researches of Dalton proved the contrary, that the vapor of water found in the atmosphere was dissolved in it, as one liquid is dissolved in another.

Dalton was the first to advance the proposition that the atoms of different gases neither attract nor repel each other; and though each offers a slight mechanical obstruction to the free motion of the other, yet if sufficient time be allowed, each will arrange itself as if the other did not exist; or in other words while the atoms of the same gas repel one another, those of different gases exert no action of this kind, and are in fact statical though not dynamical vacuums each to the other. The fundamental fact on which this theory is based is the following: If two wide-mouthed jars be placed, one on the other, mouth to mouth, the lower one being filled with oxygen or heavy gas, and the upper one with hydrogen, the lightest of all gases, and thus suffered to remain, after a short time it will be found that the two gases will be thoroughly mingled through both jars; the light gas will descend and mix with the heavier, while the heavier will in turn ascend and mix with the lighter. There will be no increase or diminution of bulk of the two gases after they have thus mingled. In order to explain the mixing of gases, three hypotheses may be assumed:

First. We may suppose that the atoms have an affinity for each other in their gaseous state. But if this were the case, from general analogy there should be a diminution of the bulk; the number of centres of repulsion would be diminished, and also the intensity of the action of each would be at least partly neutralized.

Secondly. We may suppose that the two classes of atoms repel each other, but in this case no mixture could take place; the heavier gas would remain in the lower vessel, while the lighter one would occupy the upper position.

Thirdly. If we suppose the atoms of the two gases have no action on each other, but are free to obey their own repulsions, then the atoms of each gas will expand into the void space of the interstices of the other, and the diffusion indicated by the experiment will be produced.

It follows from this hypothesis that the bulk of the mixture should remain the same before and after the mingling takes place. Let us suppose each vessel to contain a foot of gas, and that the repulsive energy is sufficient to sustain a weight of 15 pounds to the square inch; and let us suppose the interior of the vessel containing the hydrogen is a vacuum. Then it is evident that the oxygen in the lower vessel, being relieved from the pressure of the atmosphere, will expand and fill both vessels, and by the law of Mariotte, its elastic force or repulsive energy will be reduced to one-half or $7\frac{1}{2}$ pounds to the square inch. The same will take place with regard to the hydrogen. It will expand downward and fill both vessels, and its elastic force will be reduced to one-half or to $7\frac{1}{2}$ pounds to the square inch. If therefore the gases are vacuums to each other, they will each expand into the other and form a mixture of two gases, the pressure of each of which against the sides of the vessel will be $7\frac{1}{2}$ pounds to the square inch, and consequently the whole pressure will be 15 pounds.

The theory of Dalton is in exact accordance with all the facts, though it may be difficult to conceive of atoms, such as those of oxygen and hydrogen, as being without action on each other particularly when highly compressed. Indeed, Mr. Dalton in the latter part of his life was inclined to refer this seeming want of repulsion to the fact of the different sizes of the atoms, or in other words to the difference in the spheres of their repulsive energies. If two classes of atoms were thus mingled with each other, it is evident that they could not be in equilibrium until the one was generally

diffused through the other; this would give a ready explanation of the diffusion of the two gases through each other in close vessels. But it does not seem to us to be applicable to the explanation of free atmospheres co-existing on the surface of the earth, as appears to be the case, particularly with reference to the gases and aqueous vapor of the atmosphere.

I have dwelt upon this point because very erroneous ideas are frequently entertained as to the theory of Dalton, which, whatever may be its truth, has had a very important bearing on the progress of meteorology. By one class of writers on the subject it has been the basis of all investigation, and by another it has been too much neglected. All our hygrometrical calculations relative to the amount of water in the air rest upon it. While there remains but little doubt that if the air, as a whole, were at rest, and sufficient time were given for the establishment of an equilibrium, the several ingredients would arrange themselves in accordance with this theory; yet, since the atmosphere is constantly agitated with currents, and diffusion is carried on more rapidly through this agency than that from the self-repulsion of the atoms, we can only suppose that there is merely a constant tendency (particularly in the lower strata of the atmosphere) to assume the statical condition indicated by the theory.

Composition of the Atmosphere.—At the level of the sea and at all accessible heights our atmosphere principally consists of a nearly invariable mixture of two permanent gases, oxygen and nitrogen, and a number of variable substances, of which we enumerate carbonic acid, nitric acid, ammonia, hydrogen, mineral powders, animal and vegetable matter, odoriferous substances, and above all a considerable quantity of water in a state of invisible vapor, and that of partial condensation in the form of cloud. Indeed, it must be a reservoir of all the emanations which arise from the decomposition of animal and vegetable matter, and which are given off from all substances in minute quantities under the application of heat. Though the variable portions of the atmosphere form but a small percentage of the whole mass, yet they exert an important influence on animal and

vegetable life, and deserve the special attention of the agricultural chemist.

Analysis of the Air.—But before proceeding to give an account of these, it may be well to pause here for a moment to describe the simplest method by which the constitution of the air may be approximately analyzed. For this purpose we introduce into a large glass vessel filled with ordinary air a small quantity of limpid lime water, or better still, baryta water, and having closed the vessel agitate the liquid. All the soluble substances, including the carbonic acid, will be absorbed. The latter will unite with the lime or baryta water and form insoluble carbonates, which may afterwards be separated from the water, dried and weighed, and the amount of carbonic acid thus determined. To obtain the amount of vapor in a given quantity of air the latter is drawn through a tube containing chloride of lime, a substance which has a great affinity for moisture. The increase of weight found after the process will indicate the amount of water in the portion of air submitted to the experiment. The volume of this air may be readily ascertained by attaching the tube containing the chloride of lime to the upper part of a vessel, say a cubic foot in capacity, filled with water, from which the liquid is suffered to run out by an orifice at the bottom; an equal bulk of air will enter through the tube containing the chloride, and when all the water has run out, the vessel will be filled with air, or in other words, one cubic foot of the moist atmosphere will have passed through the drying tube. The quantity of aqueous vapor is more variable than that of the carbonic acid.

After having separated the water and carbonic acid, in order to ascertain the amount of oxygen and nitrogen in a cubic foot of air, we burn in the mixture a piece of phosphorous, which combines with every atom of the oxygen, forming a soluble substance called phosphoric acid, which is absorbed by the water, leaving the nitrogen in a separate state. Other and more refined methods are frequently employed, but this will serve to indicate in a general way the mode in which the results are obtained. In this manner,

we find that the atmosphere consists of 20.01 parts of oxygen to 75.29 of nitrogen in volume, or 23.01 parts, by weight, of oxygen and 76.39 of nitrogen. These numbers are not precisely those which would result from a chemical union, as was at first supposed, namely, one volume of oxygen and four of nitrogen. They are not also entirely invariable, but are found to differ slightly at different places at the level of the sea. Observation has not shown any appreciable variation from year to year, though it is not improbable that during the geological periods changes have taken place in its proportions as well as in its amount. The quantity of carbonic acid is found, by the mode we have described, to vary from the $\frac{4}{1000}$ to $\frac{6}{1000}$ of the weight of the whole.

Oxygen, as we have seen in the exposition of the atomic theory, is a very energetic element widely diffused through nature, and performs an important part in the transformations of inert matter into plants and animals, and back again into carbonic and other inorganic compounds. The nitrogen also is an important element in vital economy, and is associated with all the most instable organic compounds. Its atoms appear to exert a great repulsive energy on each other; and hence, when confined in a solid state by surrounding atoms of other substances, the slightest jar will overturn the instable equilibrium, and produce a violent explosion.

Carbonic acid is a transparent substance that is produced when charcoal is burnt in air or oxygen, and is composed of one atom of the former to two of the latter, or three parts of one to eight of the other by weight. It furnishes the carbon of the plant, and though it exists in small quantities in the atmosphere, animal and vegetable life could not be continued on the surface of the globe without it. The quantity of carbonic acid contained in the air varies between the hours of night and day, the quantity being at its maximum towards morning, and its minimum towards the middle of the day. In this respect it follows a law analogous to that of the heat and moisture of the atmosphere. A part of this variation may be referred to the absorption of carbonic acid

by plants during the day, though this cannot be the principal cause; a more efficient one is probably the varying quantity of moisture, which may serve as a kind of vehicle for its transportation to and from the ground. There is also a great difference in the amount of carbonic acid in different places, perhaps in different countries, and it is possible that a part of the variations of fertility, the other conditions being the same, may in some cases be referred to this cause. We find, from experiment, that vegetation is favored by the increase of this ingredient until, according to Sausure, we arrive at the proportion of eight parts to one hundred, which is eighty times more than the ordinary quantity existing in the atmosphere. The same portion would entirely extinguish the life of the red-blooded air-breathing animals. It is on this fact that some geologists have founded the hypothesis that the luxuriant vegetation which existed on the earth during the coal period was due to an atmosphere charged with carbonic acid, and the amphibious character of the animals existing at that period would seem to favor this supposition.

M. Chevandier has shown that one square mile of forest land produces annually 441 tons of fixed carbon in the wood, (*Comptes Rendus*.) and Liebig increases the quantity to as much as 504 tons to the square mile. The same author also shows that all other vegetable productions yield nearly the same quantity of carbon to the square mile. Now a prism of air extending to the upper limits of the atmosphere, and having a base of one square mile, contains 4,260 tons of carbon, from which it results that the annual consumption of carbon by thrifty vegetation amounts to about one-ninth of all the carbon of the atmosphere which rests upon it. (Gasparin; vol. II.)

From this it might at first sight appear that the carbonic acid of the air ought rapidly to diminish, and in a few years to be entirely exhausted; but, as we have seen, the carbon thus extracted is not lost to the air, but lent as it were to the organized matter of the globe; for by the process of combustion and decay an equal amount of the same substance is

restored to supply the place of that previously abstracted, and the whole quantity of carbon in the atmosphere remains nearly the same from age to age, the measurable variations being only perceptible during the lapse of the ages which constitute a geological period. When we consider however the great amount of coal consumed at the present day in the mechanical arts and locomotion, it would appear that the amount of carbonic acid is increasing in the atmosphere; but when we compare with this the improvements made in agriculture, and the stimulus thus afforded to the growth of plants and animals, the effects of these artificial conditions would apparently nearly balance each other. There is another source of abstraction of carbonic acid from the atmosphere, namely, that which takes place through the agency of animal life in the production of coral; but this again may be probably balanced by the carbonic acid emitted from the various active volcanoes of the globe. We do not however by these remarks attempt to establish the fact that in all parts of nature there is an exact compensation, and that our globe has always remained in the state in which it now exists, but that the great changes which affect our planet are exceedingly gradual, and the conditions may be considered constant during the age of individuals, or even of nations.

Should the carbonic acid of the air sensibly increase over the limits before mentioned, the vegetation of the earth would, as we have seen, become more luxuriant, and animal life degenerate into a lower type. If on the other hand, the carbonic acid should be diminished, the reverse would probably take place, vegetable life would become less, and animals would either correspondingly diminish in number, or they would assume a higher type. M. Flourens supposes that the amount of organic life on the surface of the globe has remained the same through all periods, though exhibited under different forms, but this would be dependent upon the permanency of the amount of organizing force from the sun.

Saline matter in the atmosphere.—Air from the surface of the ocean contains a portion of the saline ingredients which in positions near the sea, and in some cases further inland,

produce a marked effect upon the character and condition of vegetation. Dr. Dalton found, at Manchester, one part of salt in one thousand parts of rain water. Brandes found in rain water, in Germany, besides common salt, chlorate of magnesia, sulphate of magnesia, carbonate of magnesia, chlorate of potassium, sulphate of lime, oxide of iron, oxide of magnesia, and salts of ammonia, the greatest part of these being ingredients of sea-water. This explains the fact that certain plants do not grow luxuriantly near the ocean unless screened by a fringe of trees or houses, or protected in some other way. Near the ocean, a number of garden plants cannot be made to grow unless placed near a fence which intercepts the wind from the ocean. We might infer from this that the saline matter is carried mechanically by the air, and not diffused through it, as in the case of vapor. We are informed by Mr. Browne that a gentleman at Nahant has succeeded in raising pears to perfection by protecting the trees on the ocean side by a high brick wall, perforated at intervals with comparatively small openings, sufficient however to keep up the ventilation.

Mineral matter in the atmosphere.—There is also constantly diffused through the air a considerable quantity of mineral substances, in a state of impalpable powder. This is carried up by the ascending columns of air which are constantly rising under the varying heat of the different portions of the ground due to the influence of clouds and the various conditions of the surface, and is brought down in the rain which falls in the beginning of a shower. The presence of this material at all times, is rendered evident when a ray of light enters a small hole in the window shutter of a darkened room. By some, it has even been conceived to be an essential ingredient of the atmosphere. The amount of this is much greater than we might be led by casual observation to suppose. It falls upon the decks of vessels in mid-ocean, and forms dry clouds, which were observed by Prof. Piazzì Smyth, at the height of several thousand feet, upon the side of the Peak of Teneriffe.

Its constant presence in the atmosphere furnishes an explanation of the occurrence of a minute quantity of mineral matter in the composition of certain plants which is not found in the soil in which they grow.

Pollen of plants.—At certain seasons of the year, the pollen of the pine tree and other plants is carried to immense distances, and after a thunder-storm is often found on the surface of water in our rain casks and from its yellow color is frequently mistaken for sulphur.

Ozone.—Another substance which of late years has been discovered in the atmosphere by the indefatigable labors of Prof. Schönbein, the inventor of gun-cotton, is known by the name of "ozone," which is supposed from all the researches made upon it to be oxygen in a peculiar condition, in which its affinity for other substances or combining power is highly exalted. When a stream of frictional electricity is made to flow from the point of the prime conductor of an ordinary machine, a peculiar odor is perceived, due as is supposed to the oxygen of the air assuming an altered condition, and hence it has been inferred that ozone consists of oxygen with an extra dose of electricity.

M. Clausius however has advanced another hypothesis which appears to be in accordance with other facts, namely, that an ordinary atom of oxygen, of which the atomic weight is eight, is in reality a molecule composed of two atoms, and that under the influence of electrical repulsion these atoms are separated, and in the unneutralized affinity, consequent upon this separation, the increased avidity of combination is evinced.

Whatever be the nature of ozone, it is certain that it possesses great powers of combination with many other substances, and thus tends to produce chemical effects. It is probably produced on a large scale in the atmosphere, on the same principle by which it is obtained in the laboratory, namely, by the electrical discharge in the form of lightning from the clouds.

The test for ozone consists of one part of iodide of potassium, ten parts of starch, and one hundred parts of water,

boiled together for a few minutes. A thin coating of this preparation applied to writing paper with a brush, being exposed to an atmosphere containing ozone, is rendered blue from the evolution of the iodine. In order to bring out the blue color distinctly, it is necessary to dip the paper in pure water.

Besides the action of the electrical spark, ozone may be produced by the action of phosphorus on atmospheric air, provided moisture is present. It is also produced in the gas evolved in the galvanic decomposition of water. But by whatever process obtained, it always presents the following properties:

First. It is a gaseous body of a very peculiar odor, approaching that of chlorine when intense; when diluted, it cannot be distinguished from what is called the electrical odor.

Second. Atmospheric air strongly charged with it renders respiration difficult, causes unpleasant sensations, and by its action on the mucous membrane produces catarrhal affections. It soon kills small animals and undiluted must be highly deleterious to the animal economy.

Third. It is insoluble in water.

Fourth. It is a powerful electro-motive substance.

Fifth. It discharges vegetable colors.

Sixth. At common and even low temperatures it acts powerfully upon metals, producing the highest degree of oxidation of which they are susceptible.

Seventh. It destroys many hydrogenated gaseous compounds.

Eighth. It produces oxidizing effects upon most organic substances.

But the question of the greatest general interest regarding it is a physiological one. It is not found in places abounding in miasma, and from its energetic powers of combination it is thought to decompose the organic molecules of which this effluvium is supposed to consist, and hence observations in regard to it are highly desirable.

Dr. Smallwood, near Montreal, who has made an extended

series of observations upon ozone, concludes that its presence in the air does not depend upon temperature but moisture. He has observed traces of it when the thermometer was at 20° F. below and at 80° above zero. But in general it was present in large quantities during the fall of rain and snow, which may account for its greater prevalence near the sea-shore than elsewhere. It appears to exist in great quantities in dew, and to this fact has been attributed the remarkable rusting effect produced on iron when exposed to this form of precipitation of water.

Malaria, or miasma.—In certain places, there is diffused through the air an exceedingly minute quantity of a substance which has a powerful effect on the human system, and frequently offers in such districts a serious obstacle to the cultivation of the soil. It is this which gives rise to intermittent fevers and perhaps to maladies of a more malignant character. This substance is found in marshy and low places where animal and vegetable matter of an aqueous character is in a state of decomposition, but the winds which pass over these places transport the malarious effluvia to a distance and thus render whole tracts of country unhealthy.

The corpuscles of this substance appear to adhere to the molecules of water, and are elevated with the latter by the ascending currents of air to heights which vary in different countries. Around the Pontine marshes, in Italy, the malaria disappears at the height of from seven hundred to one thousand feet, while in South America, according to Humboldt, it is found at an elevation of three thousand feet; usually however its effects are exhibited with intensity at a much lower elevation than that first mentioned. It is also observed that humid air which transports miasma is deprived of this noxious material in passing through trees, and that in many cases, in the same neighborhood, a screen of foliage is sufficient to produce a marked difference between two places otherwise similarly situated. Double screens of fine gauze also placed in the windows of sleeping rooms answer a similar purpose, and should be resorted to in all cases as a precaution wherever there is danger of disease

from this cause. It is probable that the diffusion of malaria in still air, as in the case of vapor, is exceedingly slow, and hence anything that tends to interrupt the current will much retard its transmission. It is asserted that in some cases near the focus of emanation it is less deleterious than at places at a considerable distance. It would appear from this to ascend vertically with the columns of heated air and to be afterwards wafted horizontally to a distance, and there impinging on the first elevation produces its effects; or perhaps this opinion has arisen from the screening influence of objects near the source.

Miasma in perfectly dry air is in such small quantities as not only to be inaccessible to the investigation of science, but also insufficient to seriously affect human life. It is otherwise however in air cooled by the radiation of the evening and night. It appears then to be precipitated into the lower strata of the atmosphere with the mass of humidity with which it is probably connected, and when this is again evaporated at sunrise, it carries up with it the miasma in its ascending movement. At this time it is taken into the system by swallowing, respiration, and possibly by absorption through the pores of the skin, in sufficient quantities to manifest its deleterious effects. In malarious districts therefore caution should be taken against exposure to the evening precipitation and morning evaporation of the humidity of the atmosphere. Ground which has been a long time under water retains during a series of years the property of emitting the effluvia. The virgin soil in which decaying vegetable matter has accumulated for years, when first exposed to the action of the air by the labor of the pioneer, gives off a large amount of malarious effluvia; care should therefore be taken in the settlement of a new country not only to select a proper location, but also to protect the houses by a border of trees, particularly on the side against which the prevailing wind impinges. And it is to be regretted that good taste, as well as the comfort of an agreeable shade, does not more frequently induce the husbandman to spare some of the original products of the forest which are found near

the spot on which he erects his dwelling. It is also stated that plants in active vegetation, as in the case of sunflowers, absorb deleterious effluvia; but whether this effect is produced independently of the screening we have mentioned has not yet been settled. In the fertile regions of the tropics where heat and moisture abound—for example, the valley of the Amazon—and where vegetation is luxuriant, the malarious effluvium is at its maximum; while in dry countries with less vegetable life, such as those west of the Mississippi, it is not found. Nature thus is not indiscriminately benevolent to civilized man; in his uncivilized condition different races are confined to different districts, and the influences which affect one are inoperative on the other. It is only by investigating the causes of these differences, and thus in some cases arriving at the means of controlling them, that the civilized man becomes a citizen of the world, and within certain limits is enabled to overcome the natural enemies to which in his primitive ignorance he is exposed.

The difficulty of investigating the nature of miasma has induced some to believe its effects due to variations of temperature and moisture; but this is not sufficient to explain all the phenomena, as places very different in this respect vary greatly in their sanitary condition. The quantity of material (whatever it may be) which constitutes malaria is too minute to be immediately detected by the eudiometer, the instrument usually employed to analyze air. M. Moscati, in order to collect it in considerable quantities, employed a glass globe filled with ice, on the surface of which the aqueous vapor of the atmosphere was constantly precipitated. He found that the water thus collected in infected places was of a white color, inodorous, slightly alkaline, and after standing a short time lime-water and acetate of lead produced in it a light precipitate. It contained animal matter, ammonia, and chlorate and carbonate of soda. The effect of this water upon animals has not (so far as we know) been tested, though it is said that sheep which feed upon grass covered by the morning dew in infected districts are subject to peculiar maladies.

The presence of organic matter may be detected in the process just described by dropping into the water a little sulphuric acid and by afterwards evaporating the fluid we will obtain traces of carbon. If the experiment, for example, be made in a slaughter-house, comparatively a large amount of this substance will be obtained; and yet from abundant observation it is known that the animal effluvia to which the butcher is constantly exposed are not of a morbid character, since the followers of this occupation are proverbially healthy. It would appear from this fact that the hurtful miasma is of vegetable not of animal origin. That collected by Regnault had the odor of burnt plants when incinerated. The same investigator asserts that a marshy odor does not always indicate feverish infection, and that in malarious districts it was above all to be feared at times when the air appeared pure and inodorous. From all the facts then, it appears most probable that the substance called miasma is an organized body, endowed with life, and first generated in the decomposition of aquatic vegetation; that its introduction into the circulation of animals is a real inoculation affecting especially the nervous system; finally, that when it commences itself to decay in the open air it ceases to be deleterious, though it gives rise to disagreeable odors. This investigation opens a wide field for chemical research, to which the later improvements in the art of analysis may perhaps be successfully applied. Whatever may be the cause of the disease spoken of experience has indicated the following precautions for those exposed to its influence:

1st. In malarious districts, going out before the dew has evaporated, should be as much as possible avoided.

2d. Before exposure to the morning air breakfast should be taken, or some slightly exciting drink, such as coffee or tea, rather than spirits. The former produces a healthful exhilaration, which prevents an attack of the miasma, while the re-action which succeeds the exhilarating effects of the latter tends to favor the absorption of the poison.

3d. Flannel garments should be worn next to the body, as these tend to stimulate the skin and prevent the deleterious effect.

4th. The use of disinfectants, though perhaps less energetic in destroying miasma than in decomposing odors, should not be entirely neglected; and for this purpose a small quantity of chloride of lime may be found beneficial. It is said that the flashing of gunpowder in a room answers the same purpose.

5th. Screens of trees should be planted to interrupt the damp and warm wind from the focus of the emanation.

6th. During warm weather, when ventilation is more necessary, the doors and windows should be provided with screens of fine gauze.

7th. Boiled water should be used in preference to any other, or pure rain water, or that which has fallen some time after the rain commences, to which add a small portion of vinegar or acetic acid.

8th. In cool evenings of summer, the dampness of the house should be dissipated by a blazing fire upon the hearth.

It appears that the malarious influence is produced at a certain temperature, and that it is favored in marshy places by the heating of the water in shallow pools. It has been recommended to divide such places by deep parallel ditches or narrow canals at right angles to the direction of the prevailing wind, the earth being thrown up on the side in the form of dykes, which are to be planted with rapidly growing trees or large shrubs. The ditch collects the water in too large bodies to be much heated, and this liability to become warmed is further lessened by the shade of the trees. The latter also serve as a series of screens to intercept any malaria which may arise.

Nitric Acid.—If sparks of electricity are passed through a tube containing atmospheric air, the oxygen and nitrogen, which do not combine under ordinary circumstances, will chemically unite and form nitric acid. This union is supposed to be the result of the production of ozonized oxygen, which promptly unites with the nitrogen on account of its increased combining energy. The nitric acid thus formed combines with ammonia, which is also found in the atmosphere as an original though a variable constituent, and forms

nitrate of ammonia. To the atmosphere is also probably due the nitric acid which forms the nitrate of lime, from which the nitrate of potash, the principal ingredient of gunpowder, is produced in the soil containing the base. We have in this instance another confirmation of the conservation and transformation of power. The discharge of the electricity in the heavens expends a portion of its energy in producing a change in the condition of oxygen which in its turn attracts and imprisons (as it were) a portion of nitrogen—a substance which of all others, appears to possess the greatest repulsive energy, and the violent breaking loose again of this from its combination exhibits its power in the explosion which ensues. In this way the bolt of Jove may be said to be partly transformed into that of Mars, and the thunder of war to be but a reverberation of that of the heavens.

Odors.—The observations which have been made during the photographic process have revealed the fact of the existence in the air of the vapors of metals and other substances which though so minute as to have escaped particular attention are yet sufficient to interfere materially with the operations necessary to the production of perfect pictures. Almost all metals heated to redness give off effluvia perceptible by the sense of smell.

The diffusion in the air of the odoriferous principle of plants and other substances is a subject worthy of more attention than it has yet received. The wide diffusion of an almost infinitesimal quantity of matter in these cases may well excite our astonishment. A single grain of musk has been known to scent a room for twenty years, and without apparent reduction of the original material. To produce this result, the minuteness of the atoms must be beyond the conception of the imagination. From the influence which chlorine has upon animal and vegetable odors, it is probable that hydrogen is an essential part of their composition. The atmosphere itself, when pure, is inodorous; but the absence of perceptible odor may be due to the fact that our sense of smell ceases in some cases to indicate an odor after having been for a certain time subjected to its in-

fluence; for example, the nauseous effluvium which arises in some processes of the arts becomes often insensible to the operator, and the same may be said in regard to the effect of animal effluvia on the inmates of crowded and ill-ventilated houses. The sense of smell, like our moral faculties, thus becomes blunted by misuse or improper association.

Matter in the aeriform condition is generally transparent, though different gases exhibit occasionally different colors; even the atmosphere possesses this property in a slight degree, as is evident in the fact of the slightly blue appearance of distant objects.

From all that we have said, it appears that the aerial ocean, like the aqueous one, is a vast reservoir, principally composed of two ingredients of nearly constant proportions, and a number of adventitious materials which in some cases, though in very minute quantities, have a marked influence on animal and vegetable life. There is however another variable ingredient, (previously alluded to in a general way,) which by its production and condensation, is the agent to which nearly all the fitful variations in our atmosphere are to be ascribed. I allude to the aqueous vapor of the atmosphere. But before proceeding to consider this, it will be necessary to treat more fully of some of the principles of heat and its influence on the climates of the earth.

Maxima and Minima of Temperature.—A certain degree of heat is necessary to give mobility to the sap of plants, and this differs in each species of plant. Vegetation is accelerated and becomes luxuriant, provided it is furnished with a corresponding amount of humidity to compensate for the evaporation as we increase the quantity of heat. It is therefore important to determine the average amount of heat in different places; but for this certain precautions are indispensable. It is not the direct heat of the sun that we at first wish to ascertain, but that of the air. Hence it is necessary to suspend the thermometer to a badly-conducting body, and the instrument itself should not have so great a volume as would prevent its readily taking the temperature of the atmosphere. If the bulb is large and the stem small,

the degrees may readily be divided into small fractions; but in this case the thermometer will fall behind in its indications, since if the temperature be increasing, some time must elapse before the instrument can arrive at this new condition; and in case it be falling, a similar tardiness will be exhibited. If on the other hand the bulb be very small, the degrees will be of less length; but since there is little of the fluid to be heated or cooled, it will more readily take the temperature of the circumambient air. For determining however the mean temperature of a place, the thermometer should not be too small, since in that case it will be more easily affected by the heat of the body during observation, and at the same time it may be affected by an accidental or fitful stream of air, and thus give too high or too low an indication. One of the ordinary size in which the bulb is about half an inch in diameter, is preferable.

For a similar reason the thermometer ought not to be suspended in immediate contact with a large solid conducting body, for example a stone or brick house, since this will retain the effects of a term of heat perhaps for several hours after the temperature of the air has changed. It should be suspended from an imperfectly conducting material, such as wood, and so situated that the air may circulate around it on every side. It should also be screened from the direct radiation of the sun, and from the reflection of surrounding bodies; for if this be not done it will indicate the average of all the impressions received, and not simply the temperature of the air. The thermometer therefore ought to be placed in the shade on the north side of the house, but a few feet above the level of the ground, in an unobstructed place; and indeed it has been recommended to suspend it between two large parallel horizontal discs of wood, which will protect it from the earth below, the sky above, and every influence except that of the stratum of air in which it is situated. Instead of this however, we may enclose it in lattice-work, easily permeated by currents of air, and painted white on the outside to reflect back the more intense rays of heat which may accidentally reach it.

If our instruments consist of a maximum and a minimum self-registering thermometer, exposed to the air in the way we have indicated, it will be sufficient in order to obtain the average temperature of the day approximately, to note the temperature of each but once in twenty-four hours. If we then add together the maximum and minimum, and divide the sum by two we shall have approximately the average temperature; but this is not precisely the quantity required for meteorological and agricultural purposes, or that which enables us to judge of the heat of different days or different periods, since the thermometer may at different times of the day be suddenly elevated or depressed and not reach its maximum and minimum gradually, as is usually the case.

To determine these points with more precision, and the average temperature of the air during the day, we must observe the thermometer at very short intervals; for example every quarter of an hour. If we add these into one sum and divide by ninety-six we shall have the mean or average temperature of the day. Before division however caution is to be observed in combining the observations taken in winter, or when the temperature sinks below zero, to subtract the sum of the observations with the *minus* signs from the sum of those with *plus* signs.

In running our eye down the column of a series of observations of this kind, we can mark not only the maximum and minimum temperature for the day, but also the time at which they occurred. If we continue these observations, during the month of thirty days for example, we shall obtain thirty maxima and as many minima, and an equal number of mean temperatures. If we now add these thirty observations of the same kind together, and divide by the number thirty, we shall obtain the maximum, the minimum, and the mean of the month. Similar observations continued throughout the year and thus combined will give us the mean of all the maxima, of all the minima, as well as the general means of all the three hundred and sixty-five or three hundred and sixty-six days of which the year may be composed.

There is still another way of combining these observa-

tions. We may take, for example, the mean of all the temperatures of mid-day for the month or the year, or of any other hour of the twenty-four, and from this obtain the mean temperature of all hours of the day and night. Finally, instead of limiting our observations to a single year we may extend them to a series of years, in order to determine more accurately the mean temperature of a given place, all accidental variations of particular years and seasons being reasonably supposed to balance each other. It is by this admirable invention of extended averages that order and regularity are deduced from phenomena which appear to be under the influence of no fixed laws, and that we are enabled to arrive at permanent and constant quantities, by eliminating those which are irregular and variable.

A series of observations continued during the day and night through a number of years, or even a single year, involves an amount of labor which few men of science can afford to bestow upon meteorology; and few have the industry and perseverance necessary to so prolonged and tedious an effort. This task however has been performed under the direction of several persons in this country, namely, Prof. Dewey, in Massachusetts; Capt. Mordecai, at the United States Arsenal, near Philadelphia; Prof. Bache, at Girard College, Philadelphia; Prof. Snell, at Amherst; and Col. Lefroy of Toronto; not to mention the names of a large number of persons who have executed the same work in Europe. Could it be repeated in a number of different places in this country, the results would be of essential importance in correcting the ordinary observations made at fixed hours of the day.

To illustrate these observations and the uses to which they may be applied, we shall select a series made since 1816, at the Observatory of Paris, by M. Bouvard, at six different epochs of the day, namely, from nine o'clock till mid-day, and from three to nine in the evening, the other hours being given by interpolation:

Hours.	Temperature.		Hours.	Temperature.	
	° C.	° F.		° C.	° F.
Midnight.	8·5	47·30	1 P. M.	14·1	57·38
1 A. M.	8·1	46·58	2	14·47 max.	58·05
2	7·7	45·86	3	13·91	57·04
3	7·4	45·32	4	13·4	56·12
4	7·13 min.	44·83	5	12·8	55·04
5	7·5	45·50	6	12·2	53·96
6	8·2	46·76	7	11·6	52·88
7	9·2	48·56	8	10·8	51·44
8	10·3	50·54	8½	10·67 mean.	51·21
8½	10·67 mean.	51·21	9	10·19	50·34
9	11·21	52·18	10	9·7	49·46
10	12·1	53·78	11	9·1	48·38
11	12·9	55·22			
Noon.	13·5	56·30	Mean-----	10·67	51·21

From this table we see, first, that the annual mean temperature at Paris is $10^{\circ}67$ C., or $51^{\circ}21$ F. Second, that the minimum is near four o'clock A. M., and the maximum about two o'clock P. M. Thirdly, which follows from the last, the air is heated during ten consecutive hours, and is cooled during fourteen hours. Fourth, that we fall into a small error in deducing the mean temperature from the maximum and minimum of the day, the true mean being $10^{\circ}67$; while the other is $10^{\circ}8$. Fifth, that the mean temperature is at 8 h. 20 min. in the morning and 8 h. 20 min. in the evening. From this it is evident that, in order to find the mean temperature of the year, it is sufficient to observe the thermometer each day at twenty minutes past eight in the morning and at twenty minutes past eight in the evening; but if our object is to obtain the mean for each month of the year, it is necessary to change the hour in question, since it is found that for January, 1 o'clock A. M. is the proper hour, for July, 7 o'clock A. M.; and for all the other months, intermediate hours. The epoch of the mean experiences similar changes in the evening.

Having discussed the variations of the temperature of different hours, it now remains to speak of the monthly variations. From twenty years' observations at Providence, Rhode Island, the following result has been obtained by

Professor Caswell, of Brown University. This gentleman has made a series of observations extending through upward of a quarter of a century, and has presented the whole to the Smithsonian Institution for publication.

Temperature of Providence, Rhode Island; by Prof. A. CASWELL.

Years.	Months.												Year
	Jan.	Feb.	Mar.	Apr.	May	Jun.	Jul.	Aug.	Sep.	Oct.	Nov.	Dec.	
1838-----	32.5	17.9	35.1	40.8	53.5	68.2	75.0	71.0	61.4	47.2	35.3	25.8	47.0
1839-----	26.3	27.9	34.9	46.7	56.0	62.2	71.7	67.9	61.1	51.5	37.3	30.6	47.8
1840-----	18.6	32.9	36.0	47.5	57.3	67.6	72.2	70.9	58.5	51.3	39.2	27.7	48.3
1841-----	30.5	25.1	35.1	42.2	54.1	68.6	70.0	69.2	63.2	45.8	37.3	32.7	47.8
1842-----	30.8	34.4	39.7	46.3	53.4	64.2	71.8	68.3	59.3	50.9	38.7	30.2	49.0
1843-----	34.2	22.4	28.7	45.3	54.4	64.3	68.8	69.8	61.3	49.3	37.6	30.9	47.2
1844-----	20.2	28.2	36.3	50.6	58.5	64.6	68.4	67.8	59.6	49.9	39.1	32.2	47.9
1845-----	30.7	28.5	41.3	44.6	54.2	64.8	69.0	68.2	57.5	50.7	42.5	24.9	48.1
1846-----	27.3	21.7	39.4	46.3	53.2	60.7	67.5	71.2	66.0	50.2	44.6	29.8	48.2
1847-----	29.3	28.7	32.3	43.0	54.3	65.6	71.3	68.7	62.3	49.8	45.8	39.6	49.2
Mean of 10 years-----	28.0	26.8	35.9	45.3	54.9	65.1	70.6	69.3	61.0	49.7	39.7	30.4	48.1
1848-----	32.3	27.4	33.3	46.8	58.8	66.2	70.2	79.4	59.7	51.2	37.8	37.3	50.0
1849-----	24.6	22.3	37.0	43.7	54.2	67.5	70.6	69.9	60.5	50.9	47.3	31.2	48.3
1850-----	30.5	32.2	34.0	43.1	52.3	67.2	72.4	67.8	60.7	52.9	43.5	29.3	48.8
1851-----	29.8	32.1	38.5	46.3	56.4	64.2	70.6	67.7	61.0	53.7	36.9	25.5	48.5
1852-----	23.9	28.6	34.7	41.8	57.1	67.7	72.4	66.6	62.6	52.4	39.7	37.8	48.8
1853-----	28.4	30.5	36.0	44.4	57.0	66.9	70.8	69.2	62.5	49.4	42.6	28.6	48.9
1854-----	26.4	25.6	33.1	42.9	57.7	65.9	72.9	68.6	61.4	52.9	40.7	26.5	47.9
1855-----	30.0	21.1	32.6	44.1	54.7	65.3	72.9	67.9	61.9	52.4	42.0	32.3	48.1
1856-----	19.3	22.7	27.8	46.8	53.5	67.7	72.1	69.8	63.2	50.2	39.4	25.5	46.5
1857-----	16.3	32.7	32.2	41.0	52.8	62.0	69.9	66.8	60.3	50.5	42.3	34.6	46.8
Mean of 10 years-----	26.1	27.5	33.9	44.1	55.4	66.1	71.5	69.4	61.4	51.7	41.2	30.9	48.3
Mean of 20 years-----	27.1	27.1	34.9	44.7	55.2	65.6	71.0	69.3	61.2	50.7	40.5	30.6	48.2

It appears from this table that the coldest month is January, and the warmest are July and August, which are nearly the same. The mean temperatures of April and October are nearest to the mean of the year. In the two periods of ten years each, at Providence, the difference between the mean temperatures is but two-tenths of a degree; the differences also between the mean temperatures of the several months

in the two decades scarcely differ a degree in the whole series. If the times were further extended the agreements would probably be closer, the instruments remaining the same. These facts illustrate the truth of what we have previously said relative to the deduction of definite results from the most complex and variable elements, and the permanency of the mean temperature of a given position; the sum of the variations consisting in oscillations on either side of the mean, which in the aggregate neutralize each other.

It is known from extended observation that the same weather exists at the same time over a large extent of country. For example, during a cold winter, it is comparatively cold over the whole of France; and in the State of New York, though the temperature be different in different places, a cold January will be cold over the whole state; hence a table carefully made at any one place will serve to indicate the relative temperature of others in the same district.

We see from the foregoing table that the greatest heat of the day at Paris happens at 2 o'clock, while we know that the solar rays are most intense at 12 o'clock. We have in a previous report given an explanation of this phenomenon, namely, that the earth is constantly radiating heat into space and receiving it from the sun the whole time it is above the horizon; the temperature therefore will constantly increase while the amount of heat received is greater than that given off. The greatest amount of heat received in a minute is at 12 o'clock, and hence the increase of temperature at this time will be the greatest; but the earth after 12 o'clock still continues to receive more heat than it gives off, and hence the temperature of the air will still continue to increase, though at a less rapid rate, until about 3 o'clock in our latitude. The radiation into space from the earth and the absorption from the sun about balance each other, and the temperature will then remain stationary at its maximum point during some time, the loss and gain being equal. After this the loss is greater than the gain, and this goes on continually until the setting of the sun, when the radiation is entirely uncompensated and cooling takes place, at first with a sudden

accelerated velocity, and then gradually diminishes in intensity until daylight, when the earth has arrived at the minimum of temperature. After this, again, the earth begins to receive more heat than it loses, and the temperature of the air constantly rises again until 3 o'clock. If the earth were to radiate heat as rapidly at night as it does in the day the minimum temperature would be at about 9 o'clock in the morning; but on account of the diminished radiation with diminished temperature, the compensation takes place about the rising of the sun. When the radiation towards the sky is prevented by a transparent covering which admits the radiation from the sun, as in the case of a house lighted by windows in the roof, the maximum temperature takes place at a much later period of the day; and indeed were the radiation to the sky entirely stopped the temperature of the earth would increase indefinitely.

Temperatures below the surface.—At a certain depth below the surface of the earth there is a stratum of invariable temperature, the depth of which augments with the latitude, and in our climate is from about 100 to 115 feet. In general the temperature of this stratum appears to be a little more elevated than the mean annual temperature of the surface, and this excess appears to increase with the latitude. This stratum, it is evident, cannot be a regular surface, since it must necessarily partake in a considerable degree of the varying contour of the external surface of the earth. The first observations which were made upon this subject were in the cellars of the Observatory at Paris, at the depth of $67\frac{1}{2}$ feet below the surface. They extend over a period of more than fifty years, and show an invariable temperature of $53^{\circ}28$ F. The thermometer used in these observations was a most delicate one, constructed by Lavoisier, and it in no instance showed a variation of one-tenth of a degree Fahrenheit above or below $53^{\circ}28$; and even these variations, small as they are have been traced to accidental causes.

Below the surface of the ground, and at a depth of from 65 to 80 feet, but few observations have been made, and these have been principally applicable to the middle latitudes of

the northern hemisphere. From all the observations Pouillet gives the following deductions:

1. The *diurnal* variations are not perceptible at depths greater than about 40 inches.

2. The mean *annual* temperature of the different strata differs little from the mean annual temperature of the air.

3. The differences between the maxima and minima of the different strata decrease in a geometrical progression, while the depths increase in an arithmetical progression.

4. From all the observations it appears that at a depth of from 26 to 29 feet, the annual variation is only $1^{\circ}8$ F.; at from 49 to 52 feet, it is but $0^{\circ}18$ F.; and at a depth of from 65 to 81 feet, it becomes only $0^{\circ}02$ F.

5. At the depth of about 26 feet, or where the variation is 2° F., the seasons are precisely reversed; that is, the maximum temperature occurs about the 1st of January, and the minimum about the end of June.

Effect of heat on plants.—We have stated that all the transformations of matter going on around us, the power exhibited in the growth of the plants, in the functions and motions of animals as well as in the winds,—are referable to impulses received from the sun; but the mere continuance of the heat of a body at a certain temperature does not produce a continuous change in it; for example, a piece of metal, when kept at the same temperature, may remain unchanged for years, provided the intensity of the heat is not sufficient to melt it. In order therefore that heat may do work, or effect a permanent change in matter, it is necessary that it be applied by means of some mechanical arrangement analogous to a machine. In most cases, an intermediate agent (such as steam or heated air) is employed in connection with the machinery, and we have a striking natural arrangement of this kind in the organization of the plant. If the stem of a plant were solid, and did not consist of minute cells filled with evaporable liquid, the heat of the atmosphere, so long as it were constant, could produce no change. To understand this, let us suppose a tube of glass with a minute bore (for instance the tube of a broken thermometer) to have

its lower end placed in water, the liquid will rise perhaps to an inch above the general level of the liquid in the vessel, and here it will remain. The cause of this ascent is the attraction of the glass for the liquid and the liquid for itself, and is familiarly known under the name of capillarity. A perpetual flow of water can never be produced by this action since if we cut off the tube before-mentioned, leaving but three-fourths of an inch above the water, the attraction of the glass will draw the liquid up to the very top, but will not permit it to run over, because the same attraction which suspends it will prevent it from overflowing. The atom of water at the top of the tube will be attracted as much downward by the glass as the next one below will be attracted upwards; hence an equilibrium will ensue.

If however we apply heat to the upper surface, which will evaporate the water, a new portion will be drawn up to restore the equilibrium; and if this process be continued, a constant current will be maintained, and a definite amount of mechanical work will be performed. If the liquid contain different substances in solution, these will be retained, it may be in a solid form, and in this way a solid substance may be brought up and deposited at the end of the tube. If across the lower end of the tube a porous membrane be stretched, and if the liquids above this, and that in the vessel below, be of a different quality, which would necessarily result on account of the evaporation mentioned, then the ascensional power would be very much increased by the process called endosmose. Without considering at present this action very minutely, we may apply the principles we have here given to the means by which heat becomes a motive power in building up a plant. The stem of a tree is an arrangement analogous to an assemblage of minute tubes, such as we have described, terminating in leaves above, from the surface of which constant evaporation is going on, and a current of liquid ascending called crude sap, which consists of water containing in solution the various substances imbibed by the roots, and elaborated by the leaves. The tubes are not continuous, but are elongated cells analogous to a glass tube, the ends of which are closed with porous membrane.

We can scarcely doubt that by this arrangement the motive power which gives rise to the circulation of the sap is the heat derived from the atmosphere and the direct rays of the sun. But a small part however of the material of which the plant is mainly built up, (namely carbon) is elevated from the roots. This is furnished, as we have before stated, by the de-composition of the carbonic acid absorbed from the atmosphere into the pores of the leaves, and there resolved by the chemical ray of the sun. It is at this place that the liquid brought up by evaporation is elaborated into true sap, under the principle of vitality, which being carried downward through the cells by endosmose, serves by secretion to build up new cells, and thus to increase every part of the plant. The rapidity of evaporation will depend, the amount of heat being the same, upon the quantity of vapor already in the atmosphere; and hence with the same degree of temperature the amount of work performed would appear to be greater in a dry than in a moist atmosphere; but since the carbonic acid which is decomposed is probably absorbed by the water in the leaf, too rapid an evaporation will retard rather than increase the useful effect.

But little is known of the minutiae of this process, or how far the results may be influenced by other causes than those actually observed. We are assured however by observation, that beyond a certain degree of heat, a given plant cannot have a healthy condition, and also below a certain temperature, which is still above freezing, the sap of plants ceases to have an active if any circulation.

Heat necessary for the growth of plants.—The hypothesis was early advanced that for each plant a certain amount of heat is requisite in order to its developement from one stage of growth to another; for example, in the case of *wheat*, from the time it begins to sprout until it arrives at its full maturity, a definite quantity of heat is required, other conditions being the same, though the time in which it may be furnished may be different in different instances. Different methods however have been proposed for estimating this heat. Reaumur, who first advanced the hypothesis of the definite amount

of heat, as well as late writers on the subject, has proposed to calculate it by multiplying the number of days in which the plant is passing through its growth by the mean temperature of each day; while M. Quetelet, of Brussels, who has made more experiments on this subject than any other person, thinks that the heat ought to be measured, not by the simple product of the sum of the temperatures of the several days but by the sum of the squares of the temperatures of these days. He deduces this rule from the consideration that if heat be due to vibration, the impulses from it ought to do work in proportion to the square of the intensity, and not simply in proportion to the intensity. For example, a cannon ball moving with twice the velocity will penetrate a wall four times as far,—moving with three times the velocity, nine times as far,—and so on, in proportion to the square of the velocity. In accordance with this, let S represent the amount of heat required to produce the full development of the plant, and t and t' be the mean temperatures of the several days; then will $S=(t)^2 + (t')^2 + (t'')^2$, &c. It follows as a consequence of the law of the square of temperatures that alternation of temperatures within certain limits may produce greater effect than a uniform temperature. For example, if on three consecutive days the temperatures were 70° , 60° , and 80° F., and on three other days, 70° , 70° , 70° , though the average heat is the same, the effect of the former will be slightly greater than that of the latter; since the sum of the squares of the first is 14,900 while that of the latter is 14,700.

From *a priori* considerations there can be no doubt that to produce a given amount of organization a definite amount of power must be expended; but we are unable to say in the present state of science how much of the power which may disappear is lost in producing other than useful effects. Also, in the foregoing investigation it might reasonably be supposed that the mean heat of the day, in part, should be derived from the heat of the sun, and not alone from that of the air. The upper surface of a plant will be heated by the direct rays of the sun, while the lower will be exposed in the shade to the heat of the air. It has therefore been proposed

to employ the temperature obtained from the mean of the observed thermometer in the sun and in the shade during the day. To render this principle of use in practice, a series of observations in different seasons of the year, on the temperatures of thermometers in the sun and in the shade would be necessary. Besides this, since vegetation is comparatively but little advanced at night, the length of the day should be taken into account, which in the neighborhood of the equator is 12 hours, and in the vicinity of the polar circle, nearly 24 hours. Another correction is necessary in order to obtain strictly comparative results, namely, that which is due to the fact that different plants begin to show signs of vitality in the spring at different temperatures.

Allowing the truth of the proposition of the definite amount of heat required for the full development of each plant, we have a ready explanation of the fact that some grain will come to maturity in climates of very different temperatures, the less intensity of heat being compensated for by the longer duration of the day. Though each species of plant may require a definite amount of heat for its perfect maturity, yet this is by no means the measure of the power expended in the organization, though it may bear a definite ratio to it. The chemical ray of the sun decomposes carbonic acid, and thus furnishes the greater part of the material of which the plant is composed, and in the process of germination and assimilation, probably furnishes a portion of the power necessary to carry on these processes.

The following table is selected from the memoirs of M. Quetelet, of Belgium, and contains the times of leafing, blossoming, and fructification of plants found in this country as well as in Europe. The selection has been made at my request by Dr. L. D. Gale, of Washington, and it is hoped that the times will be compared with those pertaining to the same periods of the developments of the same plants in different parts of this country.

The observations from which the original table was constructed were made in the garden of the Royal Observatory, at Brussels, and according to the author, they may be ap-

plied not only to Belgium but also to the whole of Europe, due regard being had to the differences of latitude and elevation between Brussels and other places. The correction for each degree of latitude is four days for each degree, to be added or subtracted accordingly as the place is to the north or south of Brussels. The correction for elevation is a retardation also of four days for every 330 feet above Brussels, which is itself about 195 feet above the level of the sea. It must be understood that these corrections are only approximate, for we are obliged to abstract the consideration of the nature of the soil, the exposure of the plant, and the more or less continental locality, that is the greater or less distance from the sea.

Plants that grow in Europe and in the United States, whether indigenous or introduced—experiment continued ten years; by M. QUETELET, of Brussels.

NAMES OF PLANTS. (<i>Time of leafing.</i>)*	Mean time.	Earliest.	Latest.
<i>Acer pseudo-platanus</i> , a maple-----	April 20	April 7	April 28
<i>Æsculus hippocastanum</i> , horse chestnut-----	April 6	March 27	April 27
<i>Amygdalus Persica</i> , peach-----	March 28	March 4	April 19
<i>Berberis vulgaris</i> , barberry-----	March 22	Feb. 26	April 14
<i>Betula alba</i> , white birch-----	April 9	March 27	April 20
<i>Bignonia catalpa</i> , catalpa tree-----	May 1	April 17	May 19
<i>Cratægus oxyacantha</i> , English hawthorn-----	March 23	Feb. 25	April 16
<i>Clematis viticella</i> , Italian clematis-----	March 25	Feb. 23	April 20
<i>Daphne mezereum</i> -----	March 13	Feb. 23	April 4
<i>Fraxinus nigra</i> , black ash-----	April 26	April 15	May 5
<i>Gleditschia ferox</i> , honey-locust tree-----	May 9	April 30	May 26
<i>Juglans nigra</i> , black walnut-----	April 28	April 19	May 10
<i>Lonicera Tartarica</i> , Tartarian honeysuckle-----	March 6	Jan. 30	April 5
<i>Magnolia grandiflora</i> , gr. flower magnolia-----	April 19	April 4	April 29
<i>Morus alba</i> , white mulberry-----	May 2	April 21	May 15
<i>Philadelphus coronarius</i> , mock orange-----	March 18	Feb. 23	April 13
<i>Populus alba</i> , white poplar-----	April 12	April 1	May 1
<i>Populus balsamifera</i> , balm of Gilead-----	April 5	March 14	April 22
<i>Prunus cerasus</i> , cherry laurel-----	April 6	March 27	April 21
<i>Prunus domestica</i> , common plum-----	April 2	March 6	April 23
<i>Prunus spinosa</i> , sloe, black thorn-----	April 1	March 1	April 23
<i>Pyrus communis</i> , common pear-----	March 30	March 10	April 22
<i>Pyrus malus</i> , apple-----	March 30	March 12	April 20
<i>Rhus typhina</i> , staghorn, sumach-----	April 19	April 1	May 7
<i>Ribes grossularia</i> , gooseberry-----	March 8	Feb. 18	April 3
<i>Ribes rubrum</i> , red currant-----	March 17	Feb. 25	April 8
<i>Ribes nigrum</i> , black currant-----	March 17	Feb. 24	April 8
<i>Robinia pseudo-acacia</i> , white locust-----	April 23	April 9	May 10
<i>Sorbus aucuparia</i> , mountain ash-----	April 7	March 18	April 21
<i>Tilia Europæa</i> , European linden tree-----	April 7	March 18	April 22

* Latitude of Brussels.

Table of Plants—Continued.

NAMES OF PLANTS. (<i>Time of flowering.</i>)	Mean time.	Earliest.	Latest.
<i>Acer pseudo-platanus</i> , a maple-----	April 28	April 19	May 10
<i>Achillæa millefolium</i> , yarrow-----	July 13	July 5	July 30
<i>Aconitum napellus</i> , monkshood-----	June 1	May 15	June 12
<i>Æsculus hippocastanum</i> , horse chestnut.	May 3	April 23	May 16
<i>Amygdalus Persica</i> , peach-----	March 20	Feb. 27	April 8
<i>Amorpha fruticosa</i> , common false indigo	June 12	May 28	June 24
<i>Anthemis cotula</i> , mayweed-----	June 5	May 6	June 19
<i>Berberis vulgaris</i> , barberry-----	May 4	April 18	May 20
<i>Betula alba</i> , white birch-----	April 8	March 22	April 22
<i>Cratægus oxyacantha</i> , English hawthorn.	May 4	April 16	May 23
<i>Clematis viticella</i> , Italian clematis-----	June 29	June 2	July 14
<i>Daphne mezereum</i> -----	March 15	March 3	April 2
<i>Lonicera Tartarica</i> , Tartarian honeysuckle	May 9	April 23	May 23
<i>Magnolia grandiflora</i> , gr. flower magnolia	April 16	March 8	April 25
<i>Morus alba</i> , white mulberry-----	May 22	May 15	June 3
<i>Philadelphus coronarius</i> , mock orange---	May 23	May 11	June 4
<i>Populus alba</i> , white poplar-----	March 23	Feb. 28	April 20
<i>Populus balsamifera</i> , balm of Gilead----	March 23	Feb. 28	April 20
<i>Prunus cerasus</i> , cherry laurel-----	April 16	April 2	May 4
<i>Prunus domestica</i> , common plum-----	April 16	March 27	May 3
<i>Prunus spinosa</i> , sloe, black thorn-----	April 7	March 2	April 30
<i>Pyrus communis</i> , pear tree-----	April 13	March 9	May 2
<i>Pyrus malus</i> , apple-----	April 25	April 12	May 8
<i>Rhus typhina</i> , sumach-----	July 13	July 5	July 25
<i>Ribes grossularia</i> , gooseberry-----	April 3	March 12	April 22
<i>Ribes rubrum</i> , red currant-----	April 2	March 18	April 22
<i>Ribes nigrum</i> , black currant-----	April 14	March 28	April 30
<i>Robinia pseudo-acacia</i> , white locust-----	May 30	May 17	June 12
<i>Sorbus aucuparia</i> , mountain ash-----	May 2	April 16	May 15
<i>Tilia Europæa</i> , linden tree-----	June 9	May 15	June 17

NAMES OF PLANTS. (<i>Time of fruit.</i>)	Mean time.	Earliest.	Latest.
<i>Acer pseudo-platanus</i> , a maple-----	Oct. 30	Oct. 25	Nov. 3
<i>Amygdalus Persica</i> , peach-----	Aug. 22	Aug. 5	Sept. 11
<i>Prunus cerasus</i> , cherry laurel-----	June 11	May 30	June 24
<i>Pyrus communis</i> , common pear-----	Aug. 26	July 28	Sept. 14
<i>Ribes grossularia</i> , gooseberry-----	June 25	June 16	July 8
<i>Ribes rubrum</i> , red currant-----	June 15	June 6	June 29
<i>Ribes nigrum</i> , black currant-----	June 15	June 8	June 27

Heat on different surfaces.—The amount of heat which falls upon a given surface depends upon the inclination to the different points of the horizon. A field, for instance, in our latitude sloping towards the south, receives a greater, and one towards the north a less amount of heat; moreover, the former obtains more than an equal extent of ground parallel

to the horizon, and the latter, as in the other case, much less. A field also which slopes in an easterly direction receives less heat than another inclined towards the west, inasmuch as more reaches the latter, since the maximum heat of the day takes place after the sun has passed the meridian; as it is, each of these enclosures gets a less amount than one of equal extent parallel to the horizon.

Estimate of temperature by rings in trees.—It frequently happens that permanent records are found of the past condition of our globe in the impressions retained in the rocky strata, and that the yearly occurrences of certain phenomena such as the annual deposit from the overflowing of rivers. Such records may be rendered available in determining the time of actions which may have long since ceased, or which continue to the present day. It is well known that the trees of our latitude increase in size by the deposition of an additional layer annually between the wood and the bark, and that a transverse section of such a tree presents a series of concentric though irregular rings, the number of which indicates the age of the tree. The relative thickness of these rings depends on the more or less flourishing state of the plant in the year in which they were formed, and therefore indicates the relative state of heat and moisture during the same period. Furthermore each ring in some trees may be observed to be subdivided into others during the same year, indicating that the vegetation was advanced or checked at intervals during the season. Furthermore it has been found by observation that even the motion imparted to a tree by the wind has an influence on its growth, giving to its trunk an oval form, the longer direction of which will be that of the prevailing wind. A thin slice therefore cut from a large tree at right angles to its axis, carefully polished and varnished, forms a natural record of the weather well calculated to call forth admiration and to impart instruction. It is scarcely necessary to remark that the year should be carefully identified, corresponding to a given circle, in order that the whole might be properly numbered.

Mr. Babbage has proposed an ingenious application of this

principle for carrying back the series of records by means of trees which are found in the deep bogs of different parts of Great Britain. By searching for corresponding thick or thin rings in the outer circumference of one tree and in the inner of another, a number of trees may be arranged in a series, and thus the record extended back into the geological periods. Whatever may be the practical value of this plan, it is certainly ingenious and worthy of attention. Since the trees found in bogs are, we may suppose, the regular and consecutive productions of the primitive forests, they would probably represent the successive vegetation of a series of centuries.

The remains of plants found in the rocky strata indicate that the same diversity of weather and the same changes of seasons existed in the past geological ages as at the present time. By carefully studying the rain marks on sandstone, the direction of the wind during storms in the ancient periods may be determined; and this will probably be found the same as in thunder showers of the present day. The remains of plants and animals of a tropical character found abundantly in the northern regions assure us that the temperature of the surface of the whole globe has undergone remarkable changes.

Effect of different surfaces.—The rays of heat from the sun which strike the earth are partly reflected into space and partly absorbed by the surface in producing an elevation of temperature. The absorbent and reflective powers are complementary to each other, and vary greatly in different substances, and as we have seen according to their color and texture. Lampblack possesses this power of absorption in the greatest degree; and if we represent this by 100, that of common glass will be 90, and that of polished metallic surfaces about 6. Consequently, the latter have a high reflective power, while that of lampblack and other dark substances is very small. This is a matter of interest to the agriculturist, since the amount of heat which may be received by a given surface will depend very much upon its color; and indeed in some cases, charcoal or other dark sub-

stance has been strewed over the ground to increase its absorptive power.

The following table by M. Schubler is copied from Becquerel, and gives the greatest elevation of temperature obtained by different soils exposed to the direct rays of the sun, while the surrounding air was at about 78°.

Maximum of temperatures of various earths exposed to the sun, by SCHUBLER.

KIND OF EARTH.	Maximum temperature of the superior layer, the mean temperature of the ambient air being 77° F.	
	Moist earth.	Dry earth.
	°	°
Silicious sand, yellowish gray-----	99·05	112·55
Calcareous sand, whitish gray-----	99·10	112·10
Argillaceous earth, yellowish gray-----	99·28	112·32
Calcareous earth, white-----	96·13	109·40
Mould, blackish gray-----	103·55	117·27
Garden earth, blackish gray-----	99·50	113·45

The differences of temperature exhibited by the two columns are due to the heat expended in the evaporation of a portion of the water in the moist earth, while the differences between the substances are to be ascribed principally to the colors, though the texture may have some effect.

Absorptive power is connected with that of emission; and those bodies which possess the greatest absorptive power for heat of a low intensity, also possess the greatest emissive power for heat of the same kind. But the preceding remarks have reference to the rays from the sun and not to those of dark heat, and here I must stop to recall the fact which is frequently neglected, even by scientific men, namely, that color has no effect upon the absorption or emission of rays of low intensity. For example, if we pass our hands over a sign-board on which dark letters upon a white ground are exposed to the sun we can readily perceive with our eyes shut the difference of temperature; but this would not be the case were the board exposed in the dark to the heat of

a stove of a temperature below redness. Furthermore if the same board were exposed to the clear sky and suffered to cool by its own radiation no difference of temperature would be observed in the different parts of its surface, except a very slight one, which might be due to the difference of the radiating power possessed by the substances of which the black and white paints are composed. On this subject Prof. Bache, the Superintendent of the Coast Survey, has made a series of very interesting experiments. He found that canisters of tinned iron painted externally of different colors and filled with heated water, required the same time to cool through a given number of degrees. The facts in regard to this point may be generalized by saying that color has no influence whatever upon the emissive power of different bodies, but that its influence is confined to the reception of rays of high intensity, or those which approximate in quality to the luminiferous emanations. Hence a black or a white dress is equally cool in the night, though in the sunshine the darker one would absorb the greater amount of heat.

Besides the color, the humidity of the soil has great influence upon the temperature it acquires, a portion of the heat being expended in evaporating the water. We have seen the statement somewhere that the average temperature of whole districts in Great Britain has been elevated one degree by the system of drainage adopted in that country.

In addition to the preceding causes, there are two others which affect the temperature of the soil, namely, conduction and capacity for heat. In a porous, badly conducting substance the heat which may escape from the surface is not readily supplied from the interior, and hence such bodies are long in cooling. Again, different bodies contain very different amounts of heat at the same temperature, and hence one body may take a much longer time to cool down to the same temperature through the same number of degrees than another. That two different bodies of the same weight at the same temperature possess different amounts of heat may be shown by first heating say a pound of each in boiling

water, and afterwards plunging them separately into equal amounts of cold water of say 32° F. It will be found that the heat which they severally impart to the water in the two cases will be very different.

The following table, also from Becquerel, gives the relative retention of heat by different soils, (that of calcareous sand being one hundred,) and also the time of cooling of cubes of 3.2256 inches (550 cubic centimeters) of the different earths.

Table of retention of heat, by BECQUEREL.

KIND OF EARTH.	Capacity for heat, that of calcareous sand being 100.	Time required by 33 cubic ins. of earth to cool from $144^{\circ} \cdot 5$ to $70^{\circ} \cdot 2$, the tem- perature of the sur- rounding air being $61^{\circ} \cdot 2$.
		<i>hours.</i>
Calcareous sand-----	100.0	3.30
Silicious sand-----	95.6	3.27
Argillaceous earth-----	68.4	2.24
Calcareous earth-----	61.8	2.10
Mould-----	49.0	1.43

Effect of Cold.

While the periodic temperature of a given place depends upon the position of the sun in its course, the abnormal hot and cold periods, or terms, as they have sometimes been called, are due principally to winds from certain directions. The cold terms in this country generally begin in the northwest and advance southerly and easterly, and are accompanied with winds from the north and northwest. We do not however intend in this place to discuss these abnormal variations of temperature, but to consider the effect of cold on different bodies, including plants and animals. We shall first consider its effects on a surface of water.

Effect of cold on water.—When the surface of water is exposed to a low temperature, the upper stratum is cooled, becomes specifically heavier and sinks. A lower portion then comes to the surface which in its turn is cooled, be-

comes heavier, and again gives place to another stratum, to pass through the same process. This continues till the column of water originally included between the surface and the bottom is reduced to a temperature of about 39° F., at which point the fluid ceases to shrink, or in other words to become heavier, but on the contrary, expands with every diminution of heat until it becomes entirely solidified. After it has assumed a solid condition, it follows the law observed by other solids and shrinks with every subsequent fall of temperature. After the water of a given reservoir has arrived at a temperature of 39° , since it does not increase in weight, it continues to float on the surface, and is rapidly cooled down to 32° , or the point of congelation. Before however it can be converted into a solid at this temperature, it is necessary to abstract from it a large amount of latent heat.

To render this plain, let us suppose a lump of ice, taken at zero, and with the bulb of the thermometer in it, placed under such conditions that it shall receive from surrounding bodies one degree of heat in one minute of time. We shall find in thirty-two minutes the thermometer will come up to the freezing point; but here we shall observe that the mercury ceases to rise, although the supply of heat remains the same, and it will continue stationary during one hundred and forty minutes, or until all the ice is melted, after which it will again begin to rise, and continue its upward march until the water begins to boil, when a second stationary point will be reached. The heat which continued to flow into the ice during the stationary period, was necessary to convert it from a solid to a liquid state, and inasmuch as it does not affect the thermometer, it has been called latent or concealed heat. Water at 32° therefore contains 140° of heat more than ice at the same temperature.

In the freezing of water, a reverse process takes place, and 140° of heat have to be abstracted before the liquid is converted into a solid. Freezing is therefore comparatively a slow process, independently of the previous cooling down of the whole mass in the reservoir to 39° , and the upper film to 32° . For example, if on the exposure of a stratum of water

at a temperature of 20° above freezing, to the air below 32° , it requires twenty minutes to reduce it to the point of congelation, one hundred and forty minutes will be required to solidify it—or seven times as long.

In melting the ice, the same amount of heat has to be absorbed, so that a large extent of deep water becomes a regulator of temperature, preserving the air immediately over it at near 32° , though the atmosphere in the vicinity during the winter may be far below zero; conversely in the spring, though the temperature of the same latitude may be 60° or even 80° , that of the air immediately over the water will be near 32° . It is evident from these facts that the deeper the reservoir, the longer will be the continuance of low temperature required to freeze the surface, and the longer the time necessary for melting it again. These principles are illustrated in our great lakes. The greatest known depth of Lake Superior is 792 feet, and soundings of 300, 400, and even 600 feet are not uncommon. In the coldest weather, the water over these deeper places is above 32° , and does not freeze, while over the shallow parts a coating of ice is formed, which gradually cooled by the slow diffusion of the water underneath, retains its solidity until the last of June. Indeed, ice is sometimes found at the surface in the middle of July. At this period of the year, or a little later, the smaller ponds of water in the vicinity have a temperature of 72° to 74° . Lake Erie, being much shallower, sometimes freezes entirely across, and becomes in summer heated throughout its extent to nearly the temperature of the supernatant air. At the beginning of September, 1857, the temperature of Lake Huron was 56° , while that of the water from Lake Erie, which passed over the falls of Niagara, was 72° , precisely that of the air.

All bodies, as we have previously said, in passing from a liquid to a solid state, tend to assume a regular geometrical arrangement called crystals. This is particularly observable when the process has been slow, and undisturbed by agitations and tremors. The form peculiar to each substance is exhibited when a portion only of liquid has assumed the

solid state, as in the case of the shooting of spicules across the surface of water in a metallic basin exposed to the cold. It will be found on inspection that the filaments of ice arrange themselves at definite angles of either 60° or 120° , and that the triangular openings are bounded by sides making the same angles with each other. In reference to crystallization, there is an important law to be borne in mind, namely, that the axis of the crystal always tends to be at right angles to the surface of the cooling mass. For example, if a quantity of melted zinc be poured into a cylindrical hole in cold sand, and the bar thus formed be broken across, the crystals will be found to be arranged in the form of radii, with their bases in the circumference; and in some cases there will be found a cylindrical hole along the axis, from which the metal has been drawn away by the shrinking at the time of cooling and crystallization. A precisely analogous arrangement takes place in the freezing of water, which may be observed by placing a quantity of this liquid in a globular glass vessel, and submitting it to a temperature of some 10° below freezing. We shall find then that the crystallization will begin at all sides of the globe, and proceed gradually towards the centre, expelling before it all the air, and most of the foreign substances which may be contained in the water. If the cold be continued, the freezing will proceed toward the middle, until finally the process would end by collecting at this point a quantity of air surprising in amount. Before this takes place however, the glass vessel will be broken by the expansion of the ice. The crystallization at the upper surface of the water will be somewhat irregular at first; the spicules of ice around the margin will tend to shoot out at right angles to the surface of the glass; but after a pellicle has formed over the top of the fluid, this will serve as a point of attachment, and the crystallization will go on, as in the other case, at right angles to the surface; the air bubbles will be driven down before it, and if the freezing be very gradual the air will be entirely expelled, and the ice assume a perfectly transparent and homogeneous structure. If the freezing be more rapid, the air

which has been expelled from the higher stratum will be caught by that next below, and in this way we shall have a series of air-bubbles extending downwards to the surface of the unfrozen water.

Accustomed as we are to see bubbles of air rise in the water, it would appear at first sight that the bubbles seen in ice come up from the water below; but from actual observation in the manner we have described, it is clearly proved that the bubbles are composed of air which had been absorbed at the surface of the water and expelled downward from stratum to stratum in the process of freezing.

The ice then over a lake or pond consists of crystallized water, of which the axis of crystallization is at right angles to the surface and the principal cleavage in the same direction. It results from this that in the thawing of the ice in spring it tends to resolve itself into innumerable prismatic crystals at right angles to the surface, and is liable to be disintegrated by a strong wind in a single night, thus producing the phenomena of a sudden disappearance of ice over a large surface, a fact which has been erroneously attributed to its sinking, an evident impossibility, since the minutest portion of crystallized water is specifically lighter than the same substance in a liquid form. General Totten several years ago arrived at the same conclusion as to the sudden disappearance of ice which I have demonstrated in the experiments before mentioned.

Ice before it tends to give way becomes pervious to water, which is readily transmitted through the interstices of the crystals; hence those who are accustomed to travel on frozen lakes or rivers are aware of the fact that so long as the water of the melted snow does not pass through the surface of the ice underneath, it is safe and in a sound condition, though we must be careful not to confound this water with that forced up by hydrostatic pressure from below, on account of the bending downwards of the whole field.

A simple method has been proposed for determining the relative severity of different winters, by observing the thickness of ice. For this purpose a shallow vessel of water is

exposed to the air and the thickness of the ice produced measured each day. From what has been said it is evident—first, that the vessel should be made of wood or some other non-conducting substance, in order that the freezing may not take place at the sides; and second, that the water should be always of the same depth; for if there be two vessels of the same diameter, one containing more water than the other, the thickness of ice formed in the two will be different, unless the fluid in both is at the temperature of *thirty-two* degrees at the commencement of the exposure. If we would ascertain more accurately the measure of effect, the ice must be broken and its thickness measured or the amount weighed very carefully every day, for if we suffer it to accumulate we shall have a less result, since the first coat tends to screen the water, so that with the same temperature the process goes on more slowly. This method is very simple, and when properly employed furnishes reliable data for determining the relative intensity of different winters. By simply measuring the thickness on a lake or pond from year to year we may approximately arrive at a similar result. But as we have said the upper stratum screens the lower ones, and a knowledge of this fact has been taken advantage of in some parts of New England to increase the quantity of the ice for economical purposes. To this end water is suffered to flow over a surface of ice already frozen, and thus by frequently repeating the operation a much greater aggregate thickness of ice is produced. Ice made in this way is more porous however and contains more air than that formed by ordinary freezing, since all the air evolved from the strata after the first must be retained by the next below.

The more solid the ice, the longer it will resist thawing; first, because it contains more water under a given external surface, and second, because a portion of radiant heat is always absorbed at any surface, whether it be external or internal; for example, if we expose a piece of ice containing a bubble of air to a source of radiant heat, we shall find that the bubble will gradually enlarge, thus proving an internal melting to be going on. In the preservation of ice for

domestic purposes it is therefore important that it should be gathered in masses as thick and large as possible. The lower side of the ice, as a general rule, contains more impurities than the upper, since the process of crystallization tends to expel all the foreign ingredients downwards; and hence a storehouse filled with thin ice will contain more impurities, and, on account of the multitude of bubbles and amount of surface exposed, will melt much sooner than if well packed with thicker blocks. The temperature of ice moreover may be reduced considerably by exposure for some time to the weather, when below the freezing point, and thus the value of its cooling effect be enhanced. This diminution of temperature however is continued only by the slow conducting power of the ice, and though it may retard considerably the melting of the mass, we think the effect is scarcely perceptible in ice transmitted to warmer climates. We have never found a thermometer, inserted in a hole in the centre of blocks of Boston ice, in the city of Washington, to sink below 32°. In filling the ice-house however and in compacting the mass, advantage should be taken of the coldest weather.

In the preservation of ice the smaller the amount of surface exposed between the several parts, and the greater the amount accumulated in a given place, the longer it will resist melting; for the tendency to become liquid will be in proportion to the surface exposed, since the heat which produces this effect must pass through the surface; for example, in a cubic block of ice, measuring one foot on each edge, there are six surfaces exposed, each one foot square. Now if we cut this same block into two parts, by a plane parallel to one of the sides, we shall present two additional faces each a square foot in extent, and the aggregate amount of surface exposed will be increased in the ratio of six to eight. For a similar reason, if we have two ice-houses of like form, the one ten and the other twenty feet in diameter, the capacity will be in the ratio of one to eight, while their surfaces will be as one to four; hence the tendency to resist melting will be in direct proportion to the diameters of reservoirs of similar forms.

Of all geometrical solids, a sphere is that which contains

the greatest amount of space in a given surface. All other conditions being equal, we should choose this form of excavation for preserving ice; but on account of the difficulty of lining a pit of this shape, we may select the next most economical form, which is the cylindrical. It is scarcely necessary to mention in this connection the fact that, in order to succeed in preserving ice, it should be well protected from the surrounding earth and air by strata of non-conducting materials, such as straw, powdered charcoal, or saw-dust, the greater the thickness of which, the better the purpose in view will be answered. The house should also (as an additional precaution) be shaded above by trees, and have the cover painted white, to reflect back the more intense rays which may reach it indirectly. Moreover the ice should not be suffered to rest upon the bare ground below, but on double floors, between which a non-conducting substance is placed, communicating by holes with a deep pit or drain through which the water from the melted ice may percolate.

We have stated that water at $39^{\circ}\cdot 1$ begins to expand, and that this expansion increases until solidification takes place. The force exerted by this expansion is immensely great, being sufficient to burst a cannon or to cause water to pass in the form of a fine frost through the pores of solid metal. When however this expansion is opposed by a sufficient external pressure the water is not converted into a solid at thirty-two degrees, but assumes this condition at a lower temperature; a piece of ice therefore at thirty-two degrees subjected to a great pressure ought to be converted into a liquid; and this may serve to explain a fact frequently noticed, that pieces of ice thrown upon each other adhere at the points of contact—the percussion changing these surfaces from a solid to a liquid, which immediately afterwards solidifies again. But this cause is scarcely sufficient to explain the very remarkable fact that if two lumps of ice be placed so as to present two flat surfaces and these be pressed together they will unite as one mass; and this will take place even in hot water while the external surface is rapidly melting. The pressure necessary to bring them into contact would no

doubt tend to produce the effect we have already mentioned, though it is not improbable that the melting of the ice, as in the case of the evaporation of water, tends to reduce the temperature slightly below 32° . Prof. Tyndall, of the Royal Institution, has recently made an interesting series of experiments on the plasticity of ice. He finds that it may be bent and moulded into a variety of forms by subjecting it to pressure, particularly when near the melting point, and has very ingeniously applied this property to the explanation of the stratified appearance of some of the glaciers. If pressure is applied to any plastic substance in which are disseminated globules of air or irregular patches of other material, the mass will assume a lamellar structure at right angles to the direction of the compressing force; and in this way the laminated appearance which is exhibited after the confluence of two separate streams of ice which exert a great pressure upon each other is explained.

It is well known that when alcohol and water are mixed together the attraction of the two bodies is so great that a diminution of bulk and a consequent rise of temperature ensue. The same affinity exists between ice and alcohol; but when these are mixed, strange to say, a considerable *diminution* of temperature is the result; and those who habitually or otherwise mingle these two ingredients as a beverage, are sometimes surprised to find the fragments of ice frozen in a solid mass to the spoon by which the mixture is stirred. When two liquids having an attraction for each other are mingled together and a diminution of bulk ensues, heat must be evolved on account of the power generated by the approach of the atoms. For an analogous reason, when the attraction between the atoms of two bodies is diminished a quantity of heat must disappear; hence when a solid is dissolved in a liquid for which the attraction is not very intense, a quantity of heat disappears or cold is the result. In the case of the alcohol and ice, the cold produced by the liquefaction of the solid greatly exceeds the heat which might be produced by the union of the water and the alcohol. When the affinity however is very great, as between nitric

acid and copper, then the heat of the chemical combination of the two substances far exceeds the cold due to the liquefaction of the solid, and a high temperature in the mixture is the result.

On the same general principle is explained the melting of ice by sprinkling the surface of it with salt,—a process sometimes resorted to for clearing the sidewalks after an intense cold has succeeded rain. The union of salt and ice produces a liquid the freezing point of which is many degrees below that of water; and hence on their contact in a solid state, liquefaction necessarily ensues; and this in accordance with the general law must be attended with a great reduction of temperature in the surrounding bodies; on which fact depends the application of salt and snow to artificial freezing, as in the manufacturing of ice-cream. In places where ice is scarce the same principle may be applied to produce a much greater reduction of temperature from a smaller quantity of this substance. Three parts of ice and one of salt mixed together in a thin vessel will reduce the temperature of a large quantity of water; and since the same salt may again be obtained in a solid form by exposing the solution to the sun we think such a freezer might in some cases be economically employed.

The artificial production of ice in hot countries on a scale sufficient for domestic use, has of late it is said been successfully accomplished. An attempt of this kind was made a few years ago at New Orleans, by means of the rapid evaporation of water, but the cold produced in this way being small the process was not sufficiently economical to enable the manufactured article to compete in price, in that city, with the abundant supply of ice imported from New England.

Another process, which is said to be more effectual, is that of a Mr. Harrison, of England, and consists in the evaporation, liquefaction, and re-evaporation of ether. If the bulb of a thermometer covered with cotton and wet with ether be exposed to the atmosphere, the cold produced by evaporation will cause the mercury to descend many degrees below the freezing point; and if the evaporation be made to take

place under the receiver of an air pump, a much greater reduction of temperature will be produced.

Although we have not seen any account of the apparatus for reducing to practice the plan above referred to, we can readily imagine an arrangement which would produce the result. For this purpose, it would be sufficient to put the water to be frozen in thin tightly closed vessels, and place them in a large receiver containing ether, the latter being connected with an air pump, of which the upward stroke should exhaust the atmosphere, and the downward stroke re-condense the vapor in a separate vessel, to be again let into the freezing receiver, and so on.

The establishment of the ice trade, for which the present age is chiefly indebted to an enterprising citizen of Boston, must have a beneficial effect upon the sanitary condition of the world. The white man is especially adapted by his physical organization to the temperate regions, and succumbs to the intensity of the prolonged heat of the tropics unless through the agency of science he is enabled to ameliorate the effects of the ardent rays of a nearly vertical sun. An abundant supply of ice not only adds to the comfort of the European in India, but is indispensable to the continuance of his health. The use of this article will probably be very much extended, and by a suitable system of ventilation applied to the cooling of the air of apartments in a manner analogous to that of heating them during the rigor of winter at the North.

The expansion of a quantity of water passing into a solid state will be in the direction of least resistance, and hence we find a bulging up in the centre of the ice in a pitcher; but if the freezing be continued the thickening of the ice in this direction will produce a re-action in other directions, which causes the rupture of the vessel. This expansion, as we have stated before, only takes place while the water is in the act of solidifying; and it is not the stratum of ice first formed which causes the bulging up in this case, but the expansion of the water beneath. This is fully explained by the plastic character of ice before mentioned. If the bulg-

ing up however be too great, cracks are produced at the most elevated parts.

After a quantity of water has been solidified it ceases to expand; and with a still further diminution of temperature shrinks, in accordance with the law to which all solid bodies are subjected. Indeed it is now known that most liquid substances which pass into the solid state enlarge their volume at the moment of transition, and that the phenomenon exhibited by ice is only a conspicuous illustration of a general rule. Ice once formed is found to shrink more rapidly with a diminution of temperature than any other substance on which experiments have yet been made.

The expansion of water and shrinking of ice serve to explain a variety of phenomena presented in the operations of nature and the processes of the arts. Those who reside near the borders of rivers or fresh-water lakes are often startled during cold winter nights by explosions apparently as loud as those of discharges of heavy ordnance. These are produced by the rupture of long lines of ice—the gradual shrinking of which has been going on during the reduction of temperature tending to bring the whole mass into a state of tension, which is relieved by the sudden giving way along the line of least strength. I am informed by Captain M. C. Meigs, who has paid particular attention to the cracking of ice on Lake Champlain, that it most frequently takes place in the narrower parts of the lake—the shrinking of portions on each side of this line of least resistance tends to separate the two masses. The water sometimes rises in the cracks thus formed, a new freezing takes place, and when the weather moderates and the field expands to its original dimensions, it becomes too large for the area it covers, and long ridges are thrown up.

A similar effect is sometimes produced on the surface of damp ground subsequently frozen. During the winter of 1856 and 1857, we received accounts of injury done to several brick houses by the separation due to the shrinking of the surface, passing through the foundation of the edifice, and extending up along the walls. We might infer from the

principles already stated that the line of separation would in preference pass through a house, as this is the direction of least resistance, for the cellar may be considered as a line of fissure between the two masses of earth, or a crack already commenced.

During a very cold night when the temperature is rapidly diminishing, and the ground covered with snow slightly encrusted on the surface by previous thawing and freezing, a continued series of minute explosions may be heard depending in frequency and loudness upon the thickness or thinness of the crust. In some cases it resembles a crackling, and at others a series of distant though not loud or sharp explosions.

There is a phenomenon connected with ice in rivers which has given rise to much discussion as to its cause. I allude to the freezing which takes place at the bottom of running streams, where in some cases the ice remains until it is separated by its buoyancy and rises to the surface. It presents a peculiar angular appearance, and is sometimes known by the name of *anchor ice*. Its formation appears to be an exception to the general rule of the freezing of water, which on account of the decreasing density usually takes place at the surface. It was at first supposed that it was due to the radiation of heat through the clear water above; but Arago has shown that this explanation cannot be the true one, since rays of low temperature cannot pass through water, and hence no such radiation can take place. A more probable explanation has been given, I think, by the same author, in referring it to the fact that still water can be reduced below the freezing point without congealing, and that it will immediately be converted into ice if a bit of solid matter be thrown into the vessel in which the experiment is made, which may serve as a nucleus for the crystallization. When water in this state is passing through a rapid channel it is mixed together and the coldest as well as the warmest part is brought into contact with the bed of the stream, the materials of which acting as a point of rest serve as a basis of crystallization.

Peculiar mechanical effects are sometimes produced by alternations of thawing and freezing,—as for example in the case of water pipes constructed of lead or other malleable metal. To render this plain let us suppose a lead pipe one foot in length to be filled with water, and after being hermetically sealed at each end exposed to a low temperature; the expansion would merely stretch the pipe, the extension not being sufficient to burst it, and no continuation of cold or increase of its intensity would produce any further effect, as this would merely cause the ice to shrink; neither would thawing and re-freezing produce any effect, since the water would merely return to its original volume, and the ice again expand to the same extent as before; but if the pipe communicated with a reservoir of water, so that when the thawing took place, the whole space, enlarged by the previous freezing, were again filled with water, a second freezing would produce another enlargement of its internal capacity, and a third thawing and freezing, under the same circumstances, would repeat the process until at length the sides of the tube would give way.

Effect of cold on plants.—Plants filled with sap and exposed to a low temperature are variously affected, according to the character of the plant, the duration of cold, and the season of the year at which it occurs. A sudden cold will tend to burst the cells. The velocity of the motion of the sap depends principally on the amount of evaporation from the leaves and stems, and this diminishes with temperature, all other things being the same; hence there is a certain degree of cold at which the sap ceases to flow, and the functions of the plant are suspended.

The different parts of the same plant are killed at different temperatures below 32° ; the more succulent and tender growths suffer first, and the woody portion, or that in which the sap is better defended by non-conducting materials, last. A sudden fall of temperature, (even though it be extreme,) if of short duration, may not penetrate to the sap and produce freezing. It would also appear that the sap of different plants congeals at different temperatures, and it is highly probable

that other changes than those of a mechanical character are produced; but on this subject much research is required, and every intelligent farmer may add important materials to our stock of knowledge by carefully recording the observations he may make relative to the reduction of temperature, and its continuance, by which certain plants are destroyed.

It is shown by repeated observations that alternations of freezing and thawing are more hurtful to the tender plant than a uniform continuation of cold; whether this is produced by an action analogous to that we have described in reference to the water-pipe, or is due in part to other changes we are unable to say. When however the sap of a plant killed by frost is examined with a microscope, we find in it portions of destroyed tissue. It has also been observed that air may sink a few degrees below the freezing point without injury to the plant, provided the air at the time be very dry. It would seem from this that the freezing of the vapor and the production of the minute crystals which constitute hoar frost are in a degree essential to the effect.

As a general deduction from chemical and mechanical principles, we think no change of temperature is ever produced in plants without the concurrence of actions such as here indicated. Hence, in mid-winter, when all vegetable functions are dormant we do not believe that any heat is developed by a tree, or that its interior differs in temperature from its exterior further than it is protected from the external air. The experiments which have been made on this point, we think, have been directed by a false analogy. During the active circulation of the sap and the production of new tissue, variations of temperature belonging exclusively to the plant may be observed; but it is inconsistent with general principles that heat should be generated where no change is taking place.

Effect of cold on animals.—All animals, so long as life continues, generate heat, and have temperatures peculiar to themselves. In the higher class of air-breathing animals this temperature varies within comparatively slight limits under the influence of motion, rest, or of external circum-

stances; and a reduction of temperature by the application of external cold produces, as is well known, a sluggish condition, which finally terminates in death. The effect of external cold can be prevented by artificial covering, or it may be obviated, in the case of domestic animals, by an extra allowance of food. The sagacious farmer is aware of the fact that a well-sheltered enclosure for cattle is not only a humane but an economical provision.

Many observations have been made on the temperature peculiar to different animals, and a considerable number of observations recorded of a less scientific character in regard to the effect of the variations of temperature to which they may be subjected without permanent injury. The most astonishing fact, and one which could scarcely be believed if we were not in this country familiar with it, is that many cold-blooded animals can be actually frozen, and be to all appearance dead, and yet be revived by gradually thawing in water near the freezing point.

Fish, as we are assured on credible authority, are often brought to our northern markets from a great distance in a frozen condition, and may be restored to life by the process we have mentioned.

This is a subject, as it appears to me, of high interest in a physiological point of view, and would richly repay the application of well-devised systems of investigation. Can it be possible that the animal is frozen entirely through, and that every vital act is suspended? To what degree can a like result be produced on warm-blooded animals, and how far can the state of hibernation be prolonged without death to the individual? Will it ever be possible, in the case of any of the higher mammalia to so maintain the unstable equilibrium of constitution as to prevent decay, and at the same time to preserve in a latent state the vivifying principle? Though investigations on this point would be interesting we can scarcely hope to realize from them one of the fancies of Dr. Franklin, that of sending representatives of one age down to another to keep alive more actively the sympathies of the present with the past.

Effect of cold on the ground.—The depth to which ground is frozen in some places from year to year, is also an indication of the severity of the seasons; the effect of cold will penetrate very differently however in dry and moist soil; in the first it will depend entirely on the conducting power of the material, and in the second, it will also depend upon the amount of water to be congealed. The conducting capacity being the same, the depth to which the given degree of cold will penetrate will be much greater in dry than in wet soil, on account of the great amount of latent heat given off by the water before it is solidified. In dry conducting soil the propagation of cold downwards may continue some time after the surface of the ground has become considerably heated.

In a conducting body all parts tend to an equilibrium of temperature. If the upper end of a vertical iron bar be heated and then removed from the source of heat, it gradually becomes cooled, while the other parts increase in temperature, until gradually an equilibrium is established; conversely, if we cool the upper end of the bar, it will take heat from the next lower part; and this from the next, and so on, until the cooling reaches the extreme end, which will be cooled last. If, before the cooling has reached the lower end, we heat the upper part, the next below will be heated, and so on, proceeding downwards; thus waves, as it were, of heat and cold may be sent through the length of the bar, becoming less and less in intensity as they descend. In this way explanations have been given of the phenomenon of caverns colder in summer and warmer in winter,—the cold wave due to a lower temperature requiring six months to reach the point of observation.

The freezing of the ground in certain soils is hurtful to vegetation; the frozen stratum expanding irregularly from below heaves up the surface, and frequently loosens or breaks the roots of the plant. A covering of snow is a protection, since this substance from its flocculent nature and the air entangled in it is a bad conductor of heat. As a general rule during cold weather a thermometer in air on the snow

will exhibit a lower temperature than one under the same material at the surface of the ground. This effect however is not entirely due to the screening influence of the covering, but in part to the fact that the intense rays of the heat of the sun as well as those of the light of the same body penetrate the crystals of the snow as they do the glass covering of a hot-house, and being absorbed by the dark ground beneath elevate the temperature. For the same reason in bright days the snow next to the slate roof of a house is seen to melt, while the upper surface remains unaffected.

There is a singular phenomenon observed during the spring of the year in damp, sandy places, which has attracted much attention, namely, the ice-columns which spring from the earth during cold nights, elevating small gravel-stones on their tops, and raising as it were above its usual level the general surface of the ground. These crystals have been carefully studied by Professor John Le Conte, and appear to be due to the law we have before mentioned of the axis of crystallization being always at right angles to the surface of cooling, as well as to the attraction of the water for itself and the consequent excluding effect of all extraneous bodies. The water of which these crystals are formed is drawn up from below by capillarity; is frozen as it comes up to the surface in vertical prismatic crystals; a new portion is drawn between the basis of the crystals first formed and the ground, which is also frozen; and so the process is continued until stopped by the failure of moisture, or the increase of the temperature due to the advancing heat of the day.

The next subject in order of which we intended to treat is that of the vapor of water in the atmosphere; but this is of so important a character in its connection with all the phenomena of the fitful changes of the weather, and the peculiarity of climate, as well as with the agricultural products of a country, that justice cannot be done to it within the limits assigned to meteorology in this Report, and therefore we shall defer it until next year. *

* [Forty-three pages of Meteorological Tables following this part are omitted in the present re-print.]

METEOROLOGY IN ITS CONNECTION WITH AGRICULTURE.

PART IV.—ATMOSPHERIC VAPOR AND CURRENTS.

(Agricultural Report of Commissioner of Patents, for 1858, pp. 429-493.)

In the preceding articles on Meteorology, it has been shown that the great motive power which gives rise to the various currents of the aerial covering of our globe is the unequal distribution of the heat of the sun; the elevated temperature of the equatorial regions heating the air causes it to ascend and flow over toward the pole, while the cold of the frigid zone produces a condensation of the air, which gives rise to downward currents in that region, and a spreading out there in all directions towards the equator.

The simplicity of this movement is first interfered with by the motion of the earth upon its axis, which gives to all the currents flowing toward the equator a curvature to the west, and to all those flowing from the equator a curvature to the east. Another perturbing influence is the unequal heating of the several parts of the different zones of the earth, consisting as they do of alternations of land and water. But the great perturbing cause is the varying quantity of moisture which exists in the atmosphere, and which by its increase and diminution gives rise to the varying conditions of the weather, and produces the fitful and almost infinite variety of meteorological changes which occur at different times and in different places.

The present essay will be principally devoted to an exposition of the phenomena of the vapor of the atmosphere, including that of the various aqueous meteors, such as rain, hail, hurricanes, tornadoes, &c. The meteorology of North America, as well as its geology, is exhibited on a large scale, and affords one of the best fields on the surface of the globe for studying the general movements of the atmosphere. The subject has received much attention on this side of the Atlantic, and a number of laborers have devoted themselves to it with ardor and success; but we regret that the discussions

which unavoidably arise among different investigators, have not always been carried on with the calmness and moderation with which the pursuit of truth should always be conducted. Indeed, meteorology has ever been a source of contention, as if the violent commotions of the atmosphere induced a sympathetic effect in the minds of those who have attempted to study them.

We have stated in the previous articles that we have no hypotheses of our own to advocate; and while we attempt to reduce the multiplicity of facts which have been collected in regard to this subject to general principles, we shall aim at nothing but truth, and endeavor to select from the various hypotheses which have been proposed, such as in our judgment are well founded on the established laws of force and motion, and which give the most faithful and explicit expression of the phenomena. We shall be ready at any time to modify or change our views as soon as facts are discovered with which they are incompatible, and indeed we shall hold most of them as provisional truths which may serve to guide our inquiries and which are to be established, modified, or rejected by the results of subsequent induction. While the general principles of meteorology are well understood, the facts relating to it on account of the variations and multiplicity of condition are the most complex of those of any branch of physical science. It has been properly said that astronomy is the most perfect of all branches of knowledge because its elements are the most simple; and we may say, for a like reason, that meteorology is the least advanced because its phenomena depend upon the concurrence of so many and so varied causes.

Vapor of the Atmosphere.

The air at all times contains water in an elastic, invisible state, called vapor. To prove this it is sufficient to pour a quantity of cold water into a bright metallic or glass tumbler, the outside of which will become covered with dew. If the vessel were pervious to the liquid we might suppose the water which appears on the outside to come from within, but this

cannot be the case with a metallic or glass vessel, and the only source to which we can refer the dew is the atmosphere. The stratum of air immediately around the vessel is cooled by contact with its sides and a portion of its vapor reduced to water. The air thus cooled becomes heavier, sinks down along the side of the tumbler, and gives place to a new portion of which the vapor is also condensed; and in this way the process is continued as long as the temperature of the water is below that of the surrounding air. If the water which trickles down the side of the vessel is chemically examined, it will be found in some cases almost entirely pure, and in others contaminated by animal and other effluvia which are diffused in the atmosphere. If the experiment be made on different days and at different seasons we shall find a greater or less reduction of the temperature of the liquid within the tumbler is required in order to produce a deposition of the vapor. The greater the number of degrees of this reduction of temperature the greater will be the evaporation from a given surface of water, and the more intense will be the different effects which depend on the relative dryness of the air. If the experiment be made in summer we shall frequently find but a small reduction of temperature necessary to produce the deposition of moisture on the outside of the tumbler, and if we attend to the state of our feelings at the same time we experience that peculiar sensation which is referred to what is called the closeness or sultriness of the atmosphere, and which is caused by the large amount of vapor with which it is charged.

The phenomena of vapor by itself in a vacuum.—To understand even approximately the effects due to the vapor in the atmosphere it is necessary that we should first carefully study the phenomena of water in an aeriform condition as it exists by itself or separated from the atmosphere; and for this purpose we may employ the ingenious method devised by Dr. Dalton, of Manchester, England, to whose researches in meteorology and other branches of physical science we are more indebted than to those of almost any other individual of the present century. He employed in these researches a

glass tube of about 40 inches in length, closed at one end, and filled with dry and warm mercury. The tube thus filled was inverted with its lower end in a basin of the same metal, and thus formed an arrangement similar to that of an ordinary barometer, in which the pressure of the air, as is well known, forces up the mercury and keeps it suspended at an elevation of 30 inches, when the experiment is made at the level of the sea. The space above the mercury is a Torricellian vacuum; that is, a space void of all gross matter, save a very attenuated vapor of mercury, which can also be removed by a reduction of temperature below the 50th degree of Fahrenheit's scale, but the correction on this account is so small that it may be neglected. Into this vacuum Dr. Dalton introduced a very small quantity of water, by forcing it from a small syringe into the mercury at the base of the column, whence it rose to the surface and was attended with an immediate depression of the mercurial column, which, when the temperature of the room was at 60°, amounted to nearly half an inch. By this experiment it was proved that water at the ordinary temperature, when the pressure of the air is removed, immediately flashes into steam or vapor, and that the atoms of this vapor repel each other, thus producing an elastic force which depresses the column of mercury. In this experiment, the quantity of water introduced was but a few grains, yet it did not all flash into vapor, but a portion of it remained in the form of a thin stratum of liquid on the surface of the mercury. Its weight, however, was insufficient to produce the observed descent of the column, and its effect in this respect could readily be calculated, since its weight was known. The descent of the mercury was therefore due to the repulsion of the atoms of vapor, and the former afforded an accurate measure of the comparative amount of this force.

The tube, as we have stated, was 40 inches long; and since the column of mercury at first occupied but 30 inches of its length, the extent of the vacuum before the introduction of the water was 10 inches, and afterward $10\frac{1}{2}$ inches. That the depression of the mercury is an exact measure of the

elastic force or repulsion of the atoms of the aqueous vapor will be evident when we consider that if we remove the vapor the column will rise to 30 inches, and will then be exactly in equilibrium with the pressure of the external atmosphere, the two being in exact balance; but if after the introduction of the vapor the column is reduced half an inch in height, it is plain that the force which produces this effect must be just equal to the weight of this amount of mercury.

Dr. Dalton next diminished the length of this vacuum by plunging the lower end of the tube deeper into the basin of mercury, and thereby causing the upper end of the column to be projected farther into the tube; but this produced no difference in the height of the column, the top of which was still depressed to half an inch below the normal height of 30 inches. From this experiment we infer that the repulsion of the atoms of vapor cannot, like that of the atoms of air, be increased by external pressure; for when we attempt to

coerce them into a smaller space by external pressure, a portion of them is converted into water, and the atoms which remain in the aeriform condition exert the same amount of pressure as before.

Dr. Dalton next increased the temperature by surrounding the tube containing the mercurial column with a larger tube filled in succession with water of different temperatures; this produced for each temperature a difference in the depression of the height of the column; and when the water was at the temperature of 100° the depression instead of being half an inch was almost precisely three times as much.

Fig. 1 represents the apparatus employed by Dr. Dalton, in which *a* is the barometer tube filled with mercury to the height of *f*, and its lower end plunged into the basin of mercury *c*. The grad-

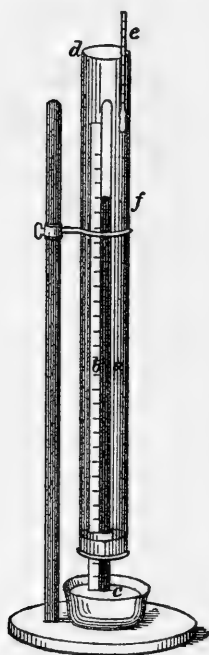


FIG. 1.

uated scale for measuring the height of the column is denoted by *b*. The larger tube around the barometer tube to contain the water of different temperatures is denoted by *d*. A thermometer *e* is inserted at its upper end by which to ascertain the temperature of the enclosed water and, consequently, that of the vapor within the barometer.

With this simple contrivance Dr. Dalton made a series of experiments to determine the repulsion of the atoms of steam; or in other words, the *elastic force of aqueous vapor*, corresponding to the different degrees of Fahrenheit's scale from zero up to the boiling point. To facilitate the operations and to allow for any changes that might take place in the pressure of the atmosphere during the continuance of the experiment, another tube was placed beside the first in the same basin, and the descent of the mercurial column of the first tube estimated from the top of that in the second, which to render the measure more accurate may be effected by means of a small telescope, sliding on a graduated rod, and movable in a horizontal plane.

By placing water of a given temperature within the outer tube and gradually cooling it after each observation, and finally filling the same tube with freezing mixtures, a table similar to the following was constructed. Dalton's experiments however have been repeated with additional precautions by other scientists, and particularly by M. Regnault, from whose work the annexed table has been compiled.

A—Elastic force of aqueous vapor, in English inches of mercury.

Degs. F.	0°.	1°.	2°.	3°.	4°.	5°.	6°.	7°.	8°.	9°.
°	<i>Ins.</i>	<i>Ins.</i>	<i>Ins.</i>	<i>Ins.</i>	<i>Ins.</i>	<i>Ins.</i>	<i>Ins.</i>	<i>Ins.</i>	<i>Ins.</i>	<i>Ins.</i>
0	0.043	0.045	0.048	0.050	0.052	0.055	0.057	0.060	0.062	0.065
10	0.068	0.072	0.075	0.078	0.082	0.086	0.090	0.094	0.098	0.103
20	0.108	0.113	0.118	0.123	0.129	0.135	0.141	0.147	0.153	0.160
30	0.167	0.174	0.181	0.188	0.196	0.204	0.212	0.220	0.229	0.238
40	0.248	0.257	0.267	0.277	0.288	0.299	0.311	0.323	0.335	0.348
50	0.361	0.374	0.388	0.403	0.418	0.433	0.449	0.466	0.482	0.500
60	0.518	0.537	0.556	0.576	0.596	0.617	0.639	0.662	0.685	0.708
70	0.733	0.758	0.784	0.811	0.839	0.868	0.897	0.927	0.958	0.990
80	1.023	1.057	1.092	1.128	1.165	1.203	1.242	1.282	1.323	1.366
90	1.410	1.455	1.501	1.548	1.597	1.647	1.698	1.751	1.805	1.861
100	1.918	1.977	2.037	2.099	2.162	2.227	2.293	2.361	2.430	2.501

The first column of the above table gives the temperature of the water and vapor in the Torricellian vacuum for every ten degrees; the second, the depression of the mercury, or the elastic force of the vapor, corresponding to the several degrees of temperature of the first column. The remaining columns give the depression of the mercury for the intermediate degrees, this arrangement being adopted to save space.

For example, if we wish to know the elastic pressure of vapor at the temperature of 70° ; by looking opposite to 70° , in the second column, we find 0.733 or nearly seven-tenths and a third inches of mercury. Again, if we wish the amount of repulsive force of the atoms of vapor at the temperature of 86° , we cast our eye along the line of 80° until it comes under the 6° , which is at the top of the table, and find 1.242 or very nearly an inch and a quarter as the height of a column of mercury which vapor of water will balance without being condensed into a liquid at the temperature of 86° .

By looking along the foregoing table it will be seen that equal increments of heat are attended with more than equal increments of elastic pressure. Thus while the elastic force of vapor at 20° is sufficient to depress the mercurial column a little more than one-tenth of an inch, at 40° it depresses it nearly two and a half times as much, at 60° five times, at 80° ten times, and at 100° nineteen times. The reason of this is not difficult to understand, since it is evident that the elastic pressure of the vapor must be increased by the action of two causes: First, by increasing the temperature the vapor tends to expand just as air would do under the same circumstances; and second, by the same increase of temperature a new portion of water is converted into vapor, which being forced into the same space, increases the density, and consequently the elasticity of the vapor which existed there before.

Dalton also showed that there is a remarkable difference between vapor which exists over water and vapor separated from the liquid from which it is produced. In the first case, as we have seen, every increase of temperature causes the formation of a new quantity of vapor which

serves to increase the density, and consequently the repulsive energy of the vapor previously existing. Hence, as we have shown before, the expansive power of vapor or steam increases in a geometrical ratio, while the temperature increases in an arithmetical ratio, that is, an addition of a few degrees of heat produces more than a proportional degree of elastic force. The case however is very different with vapor separated from the water from which it is produced; it then obeys the same law as atmospheric air and increases in elasticity with equal additions of temperature.

It has been stated in a previous article that the atmosphere increases its elastic force by one four hundred and ninetieth part for every degree of Fahrenheit above the freezing point; the vapor of water follows the same law.

These facts are readily proved by the apparatus exhibited in Fig. 2. So long as any water *e* remains

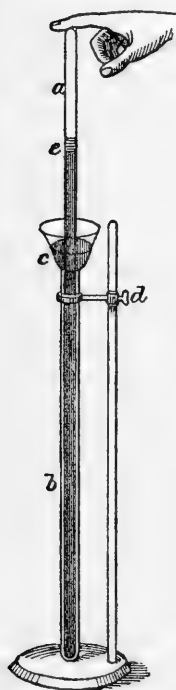


FIG. 2.

above the mercury in the tube *a*, the latter may be drawn up or pushed down into the reservoir without altering the height of the column of mercury *c e*. The higher the tube is drawn up, the more water will spring into vapor, while the tension or repulsive energy remains the same, as shown by the invariable height of the mercurial column. When the barometer tube is pushed down into the basin and the space above diminished, a portion of the vapor is converted into water, and this portion increases as the space is made to diminish. If however we draw up the tube so that all the water will pass into vapor, a further elevation of the tube will produce an elevation of the height of the mercurial column; the vapor will become rarified and its elastic pressure will consequently be diminished, and hence the increased length of the column of mercury. If sufficient cold and pressure could be applied to atmos-

pheric air, it is not improbable that a portion might be con-

verted into a liquid, just in the same way that an increase of pressure converts the vapor which fills the top of the barometer tube into water. This supposition is the more probable since several gases which were at one time considered permanently elastic have been reduced in this way to a liquid by the application of a powerful pressure, combined in some cases with a reduction of temperature.

The foregoing table is limited to 100° , and is sufficient for resolving problems relative to the hygrometrical condition of the atmosphere. It is however important for the use of the steam engineer that it should be extended to a much higher degree, and accordingly experiments have been made for this purpose by a number of persons, and particularly by M. Regnault, at the expense of the French government. From the table thus extended we may see that at the temperature of 212° the elastic force of vapor balances 30 inches of mercury, and is then just equal to the pressure of the atmosphere. This fact gives the explanation of the phenomenon of boiling, since the vapor formed at the temperature of 212° has just sufficient repulsive power to expand beneath the pressure of the atmosphere, and to pass up in volumes through the water, giving it the peculiar agitation known as boiling.

It is further evident from the same table that vapor is given off from ice even at zero, or 32° below the freezing point. If a lump of this substance on a cold day be placed under the receiver of an air pump, even when the apparatus is cooled down to zero, a portion of it will immediately spring into vapor, sufficient to fill the whole capacity of the cylinder when the air is withdrawn; and if this vapor in its turn be removed by working the pump another portion of the ice will pass into the state of vapor, and if the pressure of this be removed another quantity of ice will be evaporated; and if the pumping be continued sufficiently long all the ice will be dissipated in vapor without passing through the intermediate condition of water. Instead of continuing to work the pump in order to evaporate the ice we may produce the same effect by placing within the receiver a broad dish con-

taining sulphuric acid, which will absorb the vapor as fast as it is formed.

We may convince ourselves immediately of the evaporation of ice by exposing a given weight of it during a cold day in the shade while the temperature is below freezing. It will be found sensibly, though slowly, to diminish in quantity. The same effect is exhibited in the process of drying clothes in cold weather, which though they may be stiffened by the frozen water with which they have been wetted, soon become dry and pliable by the evaporation of the ice.

The apparatus of Dalton enables us to make the following experiment, which has an important bearing on some of the phenomena of meteorology. If, while the column of mercury is at the temperature, for example, of 60° , and a small quantity of water is resting on its upper end, the space above being filled with vapor due to this temperature, we place under the lower end of the tube beneath the surface of the mercury a small crystal of common salt, it will rise through the mercury by its specific levity, and be dissolved in part or whole by the stratum of water at the top. Now as soon as this solution begins to take place we shall see the column of mercury ascend; a portion of the vapor will be absorbed, and the tension of the remainder be diminished.

In this case the attraction of the salt for the particles of water neutralizes a part of their repulsive force and thus diminishes the weight of mercury the vapor can support. For the same reason salt water boils at a temperature several degrees higher than 212° , though the vapor produced in this case has only the elastic force of that due to pure water. From the foregoing we conclude that the quantity of vapor from the surface of the ocean is less and has less tension and density than that from the surface of fresh-water lakes at the same temperature.

The table which was furnished by Dalton, and has since been corrected by more refined experiments, is of great value in various branches of science. The very simplicity of the method employed is an evidence of scientific genius of the highest character, and is well calculated to excite our admi-

ration as well as to call forth our gratitude on account of the important truths which it reveals. Dalton, although a profound thinker, and thoroughly imbued with a love of science for its own sake, was eminently a practical man in the proper sense of the term. He had not only the sagacity to frame significant questions to be propounded to Nature, but also the ingenuity to devise simple means by which the answers to these questions would be given in terms the most precise and accurate.

The weight of vapor.—There are other important questions to be answered in regard to the same subject; and the first we shall consider is the relative weight of a given quantity of vapor in a space fully saturated at different temperatures.

The general method of ascertaining the weight of a given quantity of an aeriform fluid consists in weighing a vessel of known capacity when exhausted, and again when it is filled with the air or vapor of which the weight, or in other words the density, is desired. The difference of weights of



FIG. 3.

the vessel in the two conditions evidently gives the weight required. This may serve to give a general idea of the method of determining the *weight* of vapor; but it may be well to dwell a few moments on a more detailed account of one of the processes which has been actually adopted. This consists in employing an apparatus formed of a glass globe *a* (Fig. 3) screwed at *f* to the top of a barometer tube *e*. The capacity of the globe is previously ascertained by weighing it empty and afterwards filled with mercury. The difference of weight gives the weight of mercury sufficient to fill it, and from this it is easy to calculate its contents in cubic inches or parts of a cubic foot. Next, a small hollow bulb of glass *g*, is formed by the blow-pipe, and filled with a known weight of water. For this purpose the capillary tube *c* (Fig. 4, in which the bulb *g* is represented much enlarged) is plunged beneath a surface of water *b*, and the glass gradually heated by a spirit lamp *d*, by which the air

is partially expelled. It is then suffered to cool, when by the pressure of the atmosphere a quantity of water is forced up into the bulb. This is made to boil rapidly so as to expel along with the escaping steam all the air. The capillary end of the bulb being again plunged below the surface of the water, and the lamp withdrawn, the

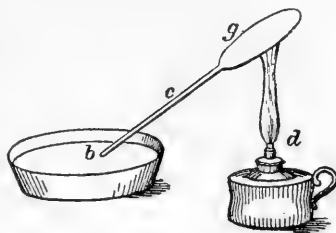


FIG. 4.

pressure of the atmosphere will now entirely fill the bulb with the liquid. The point of the capillary tube is then closed by melting it in the flame of the blow-pipe, and the bulb thus filled with water is again weighed. If from this last weight we subtract the weight of the glass we shall have the weight of the contained water. This bulb with its known amount of water is next placed in the glass globe *a*, (Fig. 3,) the long tube screwed in its place, and the whole apparatus filled with dry mercury and inverted in a basin *i* of the same metal. The mercury of course by its weight will descend from the glass globe into the tube, and sink until it becomes in equilibrium with the weight of the atmosphere, which as we have said before, will be about the height of 30 inches. The inside of the globe will then be a Torricellian vacuum, and the water if released from the small bulb in which it is contained would immediately flash into vapor by the unbalanced repulsion of its atoms; and we can readily release them from their confinement by directing upon the bulb for an instant a beam of heat from the sun by a burning glass. By this means the bulb will be broken, (particularly if formed of dark glass,) the water will be set free, and will be converted in part at least into vapor. The whole apparatus is then heated by plunging it into a water bath of which the temperature is gradually raised, or by heating the room in which the experiment is made, until all the water is converted into vapor. By carefully noting the temperature at which the liquid disappears, we have from the previous table the tension of the vapor at

this point; and since the weight of the steam which fills the globe is equal to the weight of the water originally contained in the small bulb, we have the weight of the vapor, and knowing the number of cubic inches of the capacity of the globe, we can easily determine the weight of a cubic foot of vapor at the temperature at which the experiment was made.

In this experiment care must always be taken to determine the exact temperature at which the water disappears; for if a portion of water remains in the liquid state we shall not have the true weight of the vapor; and we are assisted in determining this point by the fact that in gradually increasing the temperature of the apparatus we shall find that at the moment when all the water is evaporated the vapor will change its rate of expansion, and be governed by the same law as that of the expansion of dry air.

After having determined the weight of a given quantity of vapor, for example a cubic foot, by direct experiment according to the method we have described, the weight of an equal quantity of vapor at other temperatures may be determined by calculation. For example, the density of the vapor (as in the case of air) will be in proportion to its elastic force or the pressure to which it is subjected, if the temperature remained the same; hence from the table of elastic force already given, we may calculate the corresponding weights of a foot of vapor. The numbers thus obtained however must be corrected for the diminution of weight on account of the expansion due to increased temperature. In this way table *B* was constructed, in which the first column indicates the temperature of every ten degrees of Fahrenheit's scale; the second column gives the weight of vapor in Troy grains contained in a cubic foot of space; the remaining columns give the weight of vapor at intermediate degrees.

B—Weight of vapor in a cubic foot of saturated air, in grains Troy.

Degs. F.	0°	1°	2°	3°	4°	5°	6°	7°	8°	9°
°	<i>Gr'ns.</i>	<i>Gr'ns.</i>	<i>Gr'ns.</i>	<i>Gr'ns.</i>	<i>Gr'ns.</i>	<i>Gr'ns.</i>	<i>Gr'ns.</i>	<i>Gr'ns.</i>	<i>Gr'ns.</i>	<i>Gr's.</i>
0	0.54	0.56	0.59	0.62	0.64	0.67	0.70	0.73	0.77	0.80
10	0.84	0.87	0.91	0.95	0.99	1.04	1.09	1.13	1.19	1.24
20	1.29	1.35	1.41	1.47	1.54	1.60	1.67	1.74	1.81	1.89
30	1.96	2.04	2.12	2.20	2.29	2.37	2.46	2.56	2.65	2.75
40	2.86	2.96	3.07	3.18	3.30	3.42	3.55	3.67	3.81	3.94
50	4.08	4.23	4.38	4.53	4.69	4.86	5.02	5.20	5.38	5.56
60	5.75	5.95	6.15	6.36	6.57	6.79	7.02	7.25	7.49	7.73
70	7.99	8.25	8.52	8.79	9.08	9.37	9.67	9.97	10.29	10.61
80	10.94	11.29	11.64	12.00	12.37	12.75	13.14	13.54	13.95	14.37
90	14.81	15.25	15.70	16.17	16.65	17.14	17.64	18.16	18.69	19.23
100	19.79	20.35	20.93	21.53	22.14	22.77	23.41	24.06	24.74	25.42

This table we shall see is of great importance in practical meteorology, as it enables us to ascertain the weight of the vapor in a given portion of the atmosphere at different temperatures.

The latent heat of vapor.—There is another circumstance in regard to vapor which is of essential importance in understanding the part which it plays in producing the diversified changes of the weather, namely, the great amount of heat which it contains at different temperatures. It is well known that the quantity of heat that a body contains is not actually measured by the thermometer or the temperature which it exhibits; for example, if a cubic foot of air at 60° be expanded without receiving or losing heat its temperature will be much diminished, because the same amount of heat which was before contained in a given space is now distributed through a larger space. If an ounce of steam from boiling water, which indicates a temperature of 212°, be condensed in water at 60°, it will give out to the latter enough heat to elevate six times the quantity of water to the boiling temperature; that is, six times as much water through 152°, or the same amount of water 912°; or in other words after having given out more than 900° of heat in the act of being converted from a vapor to a liquid, it still retains a temperature of 212°. The heat which is thus evolved, and is not indicated by the thermometer, (as has been stated in our preceding article with reference to the

melting of ice,)* is called latent heat. In thus condensing a given quantity of vapor, from water at different temperatures in a given quantity of cold water and noting the elevation of temperature of the latter, it has been shown by Dalton and others that an ounce of vapor at all temperatures contains very nearly the same amount of heat, adding the latent and sensible heat together.

This constancy of the amount of heat arises from the fact that as we increase the thermometric heat a new portion of vapor is forced into the same space, its density increases, and the amount of latent heat is diminished; hence if the attenuated vapor from ice were received in a syringe and suddenly condensed until its density became equal to that of boiling water, its temperature would be 212° .

On account of the great amount of latent heat of vapor, heat must be absorbed from all surrounding bodies during the process of evaporation; and in all cases of the reverse process, that is of the conversion of vapor into water, an equal amount of heat must be given out. This absorption of heat by vapor at the place of its formation, and the evolution of an equal amount at the place where it is condensed into water is one of the most efficient means of varying the temperature of different portions of the earth from that which they would naturally acquire under the regular periodical variation due to the changes of declination of the sun.

In the evaporation of a cubic foot of water it is known from experiment that an amount of heat is absorbed equal to that evolved from the combustion of 20 pounds of dry pine wood, and consequently every cubic foot of rain water which falls from the clouds leaves in the air above an equal amount of extraneous heat, which tends to abnormally raise the temperature due to the elevation, and to produce powerful upward currents above, and horizontal motions of the air below. We may also recall in this place the fact that water, in passing from the state of ice to that of a liquid absorbs 140° of heat, which is again evolved in the act of freezing,

*[See *ante*, p. 195.]

and that this also is an efficient means by which colder portions of the earth are mollified in temperature.

In the explanations we have thus far given we have spoken of the increase of the repulsion of the atoms of water by an increase of heat. By this we mean the increased tendency which they have to separate from each other with a force which resembles simple repulsion, but which, if we adopt the vibratory theory of heat, will be due to the increased intensity of the oscillation of the particles. We have also employed the usual term "latent heat" to express the heat which disappears when a solid is converted into a liquid, or a liquid into a vapor—though this, according to the new theory of heat, would be expressed by the quantity of vibration or mechanical energy which is absorbed in the change of state of the body and which will re-appear when the reverse process takes place. To illustrate this suppose an upward impulse be given to a ball sufficient to throw it upon a shelf. In this case we may consider the mechanical energy as having been expended in producing this effect, although it is ready again to make its appearance and to do work when the ball is suffered to fall again to the level whence it was projected.

Vapor in air.—We are also indebted to Dr. Dalton for another important series of experiments which relate to the mingling of air and vapor. In the experiments before given the vapor was weighed, and its temperature and tension determined in a separate state and unmingled with the air. To ascertain the effect which would be produced on the tension of vapor when suffered to be exerted in a space already occupied with air of different densities, Dr. Dalton employed the same method of experimenting previously described. A barometer tube was filled and inverted, as before, in a basin of mercury, a quantity of air was then admitted, which rising into the Torricellian vacuum, pressed by its elasticity on the surface of the mercury and caused it to descend a given number of divisions of the scale which were accurately noted; a small quantity of water was next admitted, which rising to the top of the mercurial column was after a

few moments in part converted into vapor while the mercury was observed to be depressed. When the experiment was repeated with different quantities of air above the mercurial column and at different temperatures, produced by varying the heat of the water in the external tube, or which would amount to the same thing, by varying the temperature of the room, the remarkable fact was discovered that the depression of the mercurial column due to the introduction of the water was precisely the same at the same temperature as when the experiment was made with a vacuum; for example, at the temperature of 60° , whatever might be the elasticity of the air within the tube, the introduction of the water always gave an additional depression of half an inch. From this result the important fact is deduced that the tension or elastic force of vapor in air is the same as that of vapor in a vacuum; from which we might also infer that the quantity of vapor which can exist in a given space already occupied with air is the same as that which can exist in a vacuum at the same temperature. But this fact may be directly proved by an independent experiment. For this purpose let the globe *a*, Fig. 3, be filled with air, while the small bulb placed within contains a known quantity of water, and let the globe thus filled be screwed to the top of the barometer tube. If the apparatus be now partially filled with mercury so as to leave the globe nearly filled with air and the whole inverted with its lower end in a basin of mercury, the mercury will descend along the scale and will come to rest at a certain division, which will indicate the elastic force of the air in the globe; if next the stop-cock be shut and the small ball be broken by the heat from a burning glass the contained water will, in part at least, spring into vapor; and if we gradually heat the globe until all the water disappears and note the temperature at which this takes place, the globe at this moment will be filled with air at a known density and with in-



FIG. 3.

visible vapor of a known weight and temperature. If we calculate from the table *B*, (p. 225,) the amount of vapor which at this temperature existed in this globe while its interior was a vacuum, we shall find it precisely the same as the weight of that which the globe now contains when filled with air. If, for example, the globe be a foot in capacity and the small bulb contain 9.37 grains of water, the temperature at which the water disappears being 75° , by passing our eye horizontally along the table we shall find under 75° the same number of grains. This experiment conclusively proves that the same amount of vapor can exist in a space already filled with air as in a vacuum. The repulsive atoms of each however will be exerted against the sides of the vessel, and the resulting pressure will be the sum of the two; a fact which is proved by noting the height of the column *e*, which indicates the elastic pressure of the air in the globe before the vapor was admitted, and which, for example, we may suppose to be equivalent to the weight of 20 inches of mercury. If we now open the stop-cock the mercurial column will be depressed by the additional repulsion of the atoms of the vapor of water, and if the temperature be at 75° (as we have previously supposed) the depression will be 0.868 inches.

The same result may be obtained by the following method, which also gives us an independent means of determining directly the amount of vapor which exists in the atmosphere at a given time, and which may be employed for verifying the results obtained by other means. Let a tight cask furnished with a stop-cock near its lower part be entirely filled with water, and let the small end of a tube which has been drawn out in a spirit lamp be cemented into the vent-hole above, so that no air can enter the cask except through the tube. Let this tube be filled with coarsely powdered dry chloride of calcium—a substance which has a great affinity for moisture—and the upper end put in connection with an open vessel containing air entirely saturated with moisture, which can readily be effected by agitating a quantity of the liquid in the vessel from which the air is drawn. Let the

stop-cock be now opened and exactly a cubic foot of water be drawn into a measured vessel; it is evident that precisely a foot of air will enter the top of the cask through the tube and between the interstices of the pieces of chloride of calcium, the moisture will be absorbed, and its weight can be accurately ascertained from the increase of weight of the tube and its contents, which had previously been weighed for that purpose. By this simple experiment as well as by the one we have previously given we are enabled to conclusively prove that the weight of vapor contained in the air in a given space is the same as that which would exist at the same temperature in a vacuum. To render the result of this experiment absolutely perfect however a slight correction must be made on account of the expansion of the air and the vapor due to the increased repulsive energy of the compound over that of the air itself. This will be evident from a due consideration of what follows.

If into an extensible vessel, such as an India-rubber bag filled with air, a little water be injected, the bag will be suddenly expanded by the additional repulsive force of the atoms of vapor. Previous to the introduction of the water, the bag will be pressed equally on the outside and on the inside; on the former by the weight of the external atmosphere, and on the latter by the repulsive or elastic force of the atoms of the inclosed air; when the water is introduced and a portion of it springs into vapor the elastic force of the aqueous atoms must be added to that of the atoms of the air, and the interior will then be pressed outward with a force equal to the sum of the two repulsions. For example, if the experiment be made at 60° and the air at its normal weight, the outward pressure within the bag previous to the introduction of the water will be equal to 30 inches of mercury, but after the water is injected it will be 30 and a half inches; hence expansion will take place and the bag will be distended until by the separation of the interior atoms the repulsion is so much weakened that the pressure without and within will again be equalized. The amount of the increase in bulk will be given by the following propor-

tion: as the pressure of 30 inches of mercury is to the pressure of $30\frac{1}{2}$ inches, so is the original bulk of the India-rubber bag to its bulk after the introduction of the vapor.

From the preceding experiments and observations it is evident *that in free air the vapor exists as an independent atmosphere, being the same in weight and in tension as it would be in a vacuum of the same extent and of the same temperature.* That the same amount of vapor can exist in a space filled with air as in a vacuum at first sight appears paradoxical, but when we consider that a cubic inch of water expanded into steam at 212° occupies nearly 1,700 times the bulk which it does in the form of water, also that air may be compressed into a space many hundred times less than that of its ordinary bulk, it is evident that the extent of the void spaces is incomparably greater than the atoms themselves, and consequently it is not difficult to conceive that the atoms of the vapor have abundance of space in which to exist between the atoms of air and the atoms of air between those of vapor. Dalton announces this important truth by stating that air and vapor and almost all gases are vacuums to each other. This enunciation is a true expression of the state of diffusion which gases and vapors attain after the lapse of a given time, but it does not truly express the phenomena of the act of diffusion. In a perfect vacuum a given space is filled with vapor almost instantaneously, or with a rapidity which has not yet been estimated, but this is not the same in a space already filled with air. In this case, though the vapor ultimately diffuses itself through the air as it would in a vacuum, yet time is required to produce this effect; the result is as if there were a mechanical or some other obstruction to the free passage of vapor through the different strata of air, and indeed it would appear from the following experiments that a definite force similar to that produced by a slight attraction or repulsion is offered in the resistance of a given thickness of this medium: In the laboratory of the Smithsonian Institution a glass tube of about 3 feet in length, closed at its lower end, suspended vertically, and containing about an inch of water, has re-

mained for several years undisturbed in this condition without the least perceptible diminution in the amount of the liquid. In another experiment a pane of glass was removed from an external window of a room and the place of the glass supplied by a board, through the middle of which a hole of about an inch in diameter was made, and in this opening a tube was placed horizontally, one end being in the room and the other in the outer air. To each end of this tube a glass bulb was attached, air tight, the one within the room containing about an ounce of water, while the tube and the bulb on the outside were occupied with air. The temperature of the air within the room was on an average about 70° , while that of the air without was on an average nearly 32° , and although the experiment was continued for several months during winter not one drop of water was distilled over into the outer bulb. When however the latter was surrounded by a freezing mixture a small quantity of vapor did pass over and was condensed into water; and also when the vapor in the outer bulb was absorbed by introducing a quantity of strong sulphuric acid into this bulb the water in the other bulb gradually diminished in weight.

From these experiments it would appear that there is more than a mechanical obstruction to the transfusion of vapor through air, and that if the difference of tension of vapor in two vessels only amounts to a certain quantity no transfusion from one will take place to the other, or in other words for each inch or foot of thickness of a stratum of air a certain amount of unbalanced repulsive energy is required for transfusion. The rapid mingling of vapor with air is due in a considerable degree to the currents produced by the mixture itself and by variations of temperature.

From an application of the principle relative to the co-existence of vapor and air, above given, we are able by means of tables *A* and *B* to immediately ascertain by inspection the amount of vapor which exists at any time and in any place in a foot of air perfectly saturated with moisture and its tension; that is, which contains as much vapor as it can hold at the given temperature. If for example the tem-

perature of the saturated air be 75° , we would find opposite this, in table *B*, (p. 225,) the weight of 9.37 grains; and by merely knowing the temperature at other times and at other places we would be able to determine the relative quantity of the vapor under these different circumstances and to form a judgment as to the dryness or humidity of different localities; but since there is a constant resistance to the diffusion of vapor through the atmosphere it follows that the air is seldom at any time or in any place entirely saturated. It is on the contrary in the condition of air filling a vessel into which less water has been injected than that necessary to furnish sufficient vapor to fill the interstices between the atoms at the given temperature.

We have been provided by Dalton with a very simple process by which the amount of vapor in a given portion of air which is not saturated can be determined. For this purpose it is only necessary to procure a bright metallic tumbler, the thinner the sides of which the better, and partly filling this with water at the temperature of the air and gradually adding colder water, stirring the mixture all the while with the bulb of a delicate thermometer, note the temperature at the moment when dew begins to be deposited on the outside. This temperature is called the dew-point, from which we determine by the tables the tension and the amount of vapor in the surrounding atmosphere. To render this clear, suppose the amount and tension of vapor in the atmosphere to be that which would be produced by a temperature of 60° , the temperature of the air at the time of the experiment being 70° , the atmosphere in this case would not be saturated; but if we should gradually cool it down to the temperature of 60° , it would *then* be saturated, and the least diminution of temperature below this degree would cause a precipitation of vapor in the form of mist or dew, and this is what really takes place in regard to the vapor which immediately surrounds the sides of the tumbler. The introduction of cold water into the tumbler cools the surface, which in turn cools the air immediately around it, and when the diminution of temperature reaches the point at which

the air is just saturated the dew makes its appearance. Hence when the sides of the vessel are very thin the temperature noted by the thermometer within gives that of the dew-point without, and if we inspect the table for this temperature we find at once the corresponding tension and weight of vapor in that portion of the atmosphere in which the experiment was made.

It is not however upon the actual amount of vapor which the air contains at a given time or place that its humidity depends; but upon its greater or less degree of saturation. That air is said to be dry in which evaporation takes place rapidly from a surface of water or moistened substance. In an atmosphere entirely saturated with vapor, that is in one which is filled with as much vapor as the space which it occupies can contain, the vapor already in the air by its elastic force presses on the surface of the moist body and neutralizes the repulsive action of the water; if however the temperature be raised, the elastic force will be increased, and a new portion will be forced into the same space; the farther therefore the condition of any portion of air is from saturation the more rapid will be the evaporation from the moist bodies which it surrounds.

For example, a portion of saturated air at a temperature of 102° would contain vapor of an elastic force equal to a pressure of 2 inches of mercury. (See table A, p. 217.) If the same air however contained vapor of only the elastic force of 59° , (that is if the dew-point were at 59° ,) the elastic force would be half an inch, and consequently there would be a force unbalanced by the pressure of vapor equal to the pressure of a column of $1\frac{1}{2}$ inches of mercury. The dryness therefore of the air is estimated by the difference of the elastic force of the vapor due to the temperature of the air, and of the elastic force due to the tension of the dew-point.

In meteorological works generally, a portion of the atmosphere containing vapor equal in tension to that of the temperature of the air is said to be fully saturated, and its humidity is marked 100; but if the elastic force of the air as determined by the dew-point is only one-fourth of that

necessary to produce complete saturation, the relative humidity is marked 25. To find then the relative humidity at any time, we seek from the tables the tension of vapor due to the temperature of the air, and again its tension due to that temperature to which it must next be cooled down in order to produce precipitation, or full saturation, which temperature as we have seen is that of the dew-point. We then say, *as the tension of the first temperature is to 100, so is the tension of the other temperature to the percentage of saturation.* In this way comparative tables of relative humidity for different places are calculated from actual observation.

Instead of employing the method of the dew-point for ascertaining the quantity of vapor in the atmosphere, a process which is attended with some difficulty, particularly in cold weather, since in this case it is not easy to reduce the temperature of the water within the tumbler except by a freezing mixture sufficiently low to produce the deposition of dew, another process has been employed, called that of the wet and dry bulb thermometer.

In this process we note the temperature of the air by an ordinary thermometer, and again we observe the temperature to which in the same place a thermometer, whose bulb is covered with wet muslin, descends. If the air is perfectly saturated with moisture the two thermometers will indicate the same degree; but if the temperature is above that due to the elastic force of the actual amount of vapor in the air the evaporation from the moist bulb will cause it to descend, by the absorption of heat, a certain number of degrees below that indicated by the naked bulb.

M. Regnault has compared by direct experiment, the indications of the wet and dry bulb thermometer, with the actual amount of vapor contained in air at different temperatures and at different degrees of saturation, according to the method previously explained, and has in this way formed a series of tables by which the dew-point, the tension of the vapor, and the weight in a cubic foot can be ascertained. In order however that these indications may be relied upon, it is necessary that the observations be made with care, since the evaporation from the wet bulb will very

much depend, as we shall presently see, upon the motion or stillness of the air; and indeed we think that in all cases, in order to obtain comparable results, the bulb should be fanned, so as in every instance to give the same amount of agitation to the surrounding medium. This will be evident from what we have said of the slow diffusion of vapor of feeble tension in the atmosphere. A local atmosphere of vapor is soon formed around the bulb, which very much impedes evaporation and consequently the reduction of temperature.

Evaporation of water.—Water is constantly evaporated from the surface of the ocean; the amount however diminishes as we proceed from the equator towards the poles. It is also exhaling from the surface of the earth, but in less quantities. The daily, monthly, and yearly amount of evaporation from a given surface of water and different kinds of earth is one of the most important data in reference to engineering and agriculture which can be furnished, and we would commend the research in reference to it to the special attention of any person who can command the time and desires an opportunity of advancing our knowledge of the operations of nature. A series of experiments on the evaporation from water may be made by carefully noting the quantity which disappears daily from a surface of a square foot freely exposed to air and sunshine. The depth of the box, which may be of tin encased in wood, should be 6 inches, and the amount of water measured by a screw, the lower end of which tapers to a point, and on the upper end a divided circle is placed, so marked that the tenth part of the width of the screw or the one-thousandth of an inch may be estimated. Care should be taken to guard this surface from rain, and in high wind to estimate the amount of water which may be blown out; the latter may be approximately found by surrounding the evaporating vessel with a border of gray paper, on which each drop of escaping water will make a stain; the number and size of these spots being known, the amount of water blown out may be estimated from the result of previous experiments in which the known quantity of the fluid has been sprinkled over the same surface. It is well, in order to make certain corrections, to observe the

average temperature of the water during the day, and for this purpose a bulb of a thermometer is placed just below the surface of the liquid. In ascertaining the evaporation from different kinds of soil, a number of boxes of the dimensions above described, should be filled with different samples, supplied with a measured quantity of water, weighed from day to day, and the loss (which will give the evaporating capacity) accurately noted. To ascertain the amount of evaporation from the actual surface of the earth in the course of the year, the loss should be daily determined from a new portion of earth taken from the surface in its actual condition.

The annual amount of evaporation from a given surface of water in the interior of the country is greater than that of the rain which falls on the same surface, but the amount of evaporation from the surface of ground is generally less, particularly in mountainous districts.

The evaporation does not depend upon the position of the evaporating surface since a piece of moist paper pasted on a pane of glass loses the same amount of water in the same time, whether it be held horizontally or vertically. It does however depend very much upon the nature of the surface; for example, less must be given off in a given time from a surface of salt water than from a surface of fresh water; and also from the cohesion with which water adheres to solids, a less amount of vapor is produced in a given time from a given surface of moist earth than from water, as is shown by the following table, deduced from observations made by M. Gasparin, in France, at temperatures from 73° to 75° F., during the time specified:

Dates.	Evaporation from water.	Evaporation from earth.
	<i>Inch.</i>	<i>Inch.</i>
1st day of August -----	0.575	0.160
2d do. -----	0.534	0.098
3d do. -----	0.448	0.070
4th do. -----	0.468	0.051
5th do. -----	0.456	0.051
6th do. -----	0.429	0.047
7th do. -----	0.367	0.051

The surface of the earth in this experiment was at first completely soaked with water.

It is evident, on account of the slowness with which vapor diffuses itself through still air, that a much greater evaporation will be produced during a brisk wind, particularly if it be from a dry quarter, than during calm weather. If the vapor which is formed is allowed to accumulate over the evaporating surface, it will by its re-action retard the free ascent of the other portions of vapor; but if it be constantly removed as fast as it is formed the process will evidently go on more rapidly.

Vapor as we have seen contains a large amount of latent heat, and water cannot be converted into an aeriform state without the supply of the necessary quantity of this principle. Hence the higher the temperature, or the more freely the evaporating surface is supplied with heat, the greater will be the amount of vapor in a given time.

We have seen that water immediately flashes into vapor in a vacuum, and we might infer from this that the rarer the air, or the more nearly it approximates to a void, the less obstruction would it offer to the free production of vapor, and the correctness of this inference has been satisfactorily shown by direct experiment.

We owe to Dalton a series of precise experiments on the evaporation of water in air of different degrees of dryness and at different temperatures. He employed in his investigations a circular dish or pan 6 inches in diameter, about an inch deep, and suspended from the beam of a balance, by which the loss of water could be accurately ascertained from the variations of the weight in a given time. With this instrument he made a series of experiments while the air contained different quantities of moisture, the amount of which was ascertained by means of the dew-point method we have before described in a perfectly still place and with the apparatus exposed to a rapid draught of air. At the boiling point the evaporation in still air was 120 grains in a minute; in a gentle wind, 154 grains; and with a strong wind, 189 grains. A similar difference existed at the evaporating temperature

of 60° : in still air the evaporation was 2.1 grains in a minute; in a gentle wind, 2.7; and in a strong wind, 3.3. From all the experiments he deduced the important result that the amount of evaporation in all cases is proportional to the difference of the elastic force of the temperature of evaporation and that of the dew-point or the vapor actually in the air.

The empirical rule deduced from his table of results will serve approximately to calculate the amount of evaporation under the different conditions of temperature, dryness, &c., of the air, the temperature of the evaporating surface, and that of the dew-point being known. For still air multiply the difference of the tension of vapor due to the temperature of the evaporating surface, and of the vapor in the atmosphere, by 4, and this will express in grains the weight of the vapor given off from a circular surface of water of 6 inches in diameter in one minute of time. If a gentle wind be blowing multiply the same difference by 5, and if a high wind exists during the experiment multiply the same difference by 6. If for example the temperature of the evaporating surface be at the boiling point, and the temperature of the dew-point be 60° , we shall have 30 inches, the tension of the evaporating surface, and 0.5 for that of the tension of the vapor in the atmosphere at the time, the difference will be 29.5, which multiplied by 4 gives 118 grains. Again, if the temperature of the evaporating surface be 90, and that of the dew-point 70, then we shall have $1.4 - 0.7 = 0.7$. If we suppose a gentle wind blowing at the time this must be multiplied by 5, and we shall have $0.7 \times 5 = 3.5$ grains as the amount of evaporation per minute from a circle of 6 inches in diameter.

The formula of Dalton, in the absence of other data, may be considered a valuable approximation; still results derived from direct observations in different parts of the earth, as we have said before, are desiderata of great value.

Physical effects of vapor in the atmosphere.—Before considering the more important meteorological changes produced in the general condition of the atmosphere by the vapor which

it contains, we may discuss some of the minor physical phenomena connected with the process of evaporation and the existence of water in an aeriform condition.

Heat and moisture are the principal essential atmospheric agents in the production of vegetable matter, and where these are not found in sufficient quantities, however rich may be the soil in fertilizing materials, at least comparative if not absolute sterility must prevail. Unfortunately however, these conditions though so highly favorable to the production of the substances which administer to the necessities and conveniences of life, are not equally favorable to the condition of health of the more highly civilized races of men. Heat and moisture are also the essential conditions under which the deadly malarious effluvia exert their baneful influence—especially upon the white race; and though science may hereafter furnish the means of disarming them of their terrors, yet at present they require the rich harvests of fields which would otherwise be uncultivated, to be reaped by the labor of individuals of another race so different in their physical organization as to be apparently exempt from the effects of these aerial poisons. The fertile rice, cotton, and sugar fields of the southern portion of the United States, are cultivated by negroes not only with impunity but without impairment of their physical enjoyments of life.

The relative moisture of different countries is intimately connected with their condition as to healthfulness. While in the moist climate of Great Britain and that of some of the West India islands diseases of the lungs are prevalent, they are seldom known in the dry regions of Nebraska and Minnesota.

From the experiments of Dalton, as we have seen, the rapidity of evaporation is proportional to the difference of elastic tension of the vapor in the air and that of the evaporating surface. Meteorologists have generally adopted as the expression of relative humidity the ratio of the force of vapor in the air to the force which it would have were it perfectly saturated, or they sometimes adopt an equivalent expression by defining the relative humidity to be the ratio of the absolute

quantity of vapor which the air could contain at the given temperature, to that which it actually contains. According to this definition two places would be equally damp which are both half saturated with vapor, though the abstract quantity of vapor in the one case may be many times that of the other. Thus in winter when the temperature is very low and the absolute quantity of vapor in the air is exceedingly small, the air may have a maximum of dampness, that is to say, a very great relative humidity. Although this method of establishing the relative humidity of different places may correspond with variations in different phenomena, yet there are some effects which appear to depend not on the relative but on the absolute amount of humidity in the air. The conducting capacity for electricity (for example) appears to increase with the absolute amount of vapor in the air, and hence experiments with the electrical machine succeed much better in winter than in summer, though the relative humidity in both cases may be the same. Again, since the temperature of our bodies is about 98° , and as this may be regarded as the temperature of an evaporating surface, the difference of tension of vapor from the pores of the skin and that in the air must be very different in winter and in summer; and hence in the latter case, when the dew-point approaches the temperature of the body, we experience the sensation of the closeness and sultriness of the atmosphere.

On the other hand the intense cold which is felt on the Western plains in winter is due principally to the rapid evaporation from the pores of the skin—a result which can only be guarded against by a covering of close texture, such as the prepared skins of animals. In this connection we may mention a fact, which at first sight might appear to militate against the usages of civilized and refined life, namely, that dirt and grease are great protectors of the skin against inclement weather, and therefore, says Mr. Galton, “the leader of a party should not be too exacting as to the appearance of his less warmly clad followers.” Daily washing, if not followed by oiling, must be compensated by warmer clothing. A savage never washes himself in cold weather unless he

can give himself a clothing of grease. The tendency to evaporation from the skin during high winds must be opposed by a substance which will partially close the minute orifices. Warmly clad and protected from the cold of winter the civilized man can enjoy the luxury of washing which is denied to the naked savage.

Among other effects of evaporation connected with its reduction of temperature, should be mentioned the advantages derived from draining marshy soil, that the cooling due to the evaporation of the surface water, is thereby diminished. It is said that the mean temperature of certain parts of England has been perceptibly increased by the general introduction of this system of agricultural improvement.

The moisture of the atmosphere often affects our health and comfort by its deposition on the walls and other parts of our habitations. It is absorbed with great force and in large quantities into the pores of almost every substance, and is given out again when a change in the temperature or dryness of the air occurs. Building-stone and brick absorb a large amount, which may be transmitted by capillarity from without through a wall of considerable thickness and evaporated at the interior surface. The dampness however of a stone house is not principally due to this cause, but to the deposition of moisture from the air on the cold surface of the wall—precisely analogous to the formation of dew on the surface of a pitcher containing cold water.

If during a period of cold weather an apartment of a stone house has been closed, and on the recurrence of a warm day the windows are opened to air the room, the deposition we have mentioned takes place in abundance, and the result intended to be guarded against is promoted rather than diminished. If a fire be made in the room previous to opening the windows, so that the sides of the apartment may be made warmer than the air, the deposition will not take place. The effects both of the transmission and of the deposition of moisture can in a great measure be obviated by the means now generally adopted of lining the interior of the room with a thin coating of a non-conducting material separated

from the wall by a stratum of air. The surface of this material readily assumes the temperature of the air, and therefore does not allow of the deposition of much moisture. This internal lining, known by the name of furring, is usually composed of lath and plaster, but in some large buildings it is formed of a single thickness of brick, which prevents transmission of moisture from without, but does not fully obviate the tendency to deposition within, since a large amount of vapor is absorbed through the pores of the coating of plaster into the substance of the brick and again given out with a change of temperature.

The dampness of newly-plastered walls is in part due to a chemical action, which (paradoxical as it may appear) is not obviated by heating the wall. After a newly plastered room has been dried by an excess of artificial heat, it continues for a long time to give off vapor, and this is due to the chemical change going on while the lime in the plaster is in process of being converted from what is called a hydrate to a carbonate of lime. Perfectly dry slacked lime contains in chemical combination a portion of water, and when it is exposed to the atmosphere it absorbs carbonic acid from the air and expels the water in the form of vapor; hence, after a plastered wall has been thoroughly dried it ought to be exposed freely to currents of air, which may furnish the carbonic acid necessary to expel what may be called the solid water or that of chemical combination.

The water which is absorbed into the pores of stone by capillary attraction does not change its dimension. Mr. Saxton, of the Coast Survey, has shown that a rod of marble of 3 feet in length is not increased the ten-thousandth part of an inch by soaking it in water from a state of perfect dryness produced by heating it in an oven. The experiment was made on the marble of the Capitol, at the request of Captain Meigs, the superintendent of the extension of that national edifice. The absorption of moisture by organic substances however produces a change in their dimensions, which takes place with the exhibition of great force. The water is absorbed in great quantities at the ends of the

fibres of wood, and the principal expansion takes place in a direction at right angles to these fibres; it is also absorbed laterally between them, though in a less quantity. The warping of furniture is simply due to the exhalation of the water in the form of vapor from the pores of the wood and the consequent shrinking of the part from which the exhalation has taken place, while the other parts retain their original bulk. To prevent this it is necessary to imprison the vapor by a coating of an impervious substance, such as varnish or paint, or what is still better to expel the moisture by baking the wood and subsequently filling its pores with some resinous substance. It is important however to observe that when a substance is to be protected from moisture by a covering of paint or varnish, care should be taken to cover every part with the impervious mixture, for the moisture may be drawn in through even a nail hole and pervade the whole interior capacity of the wood.

Various instruments for indicating the moisture of the atmosphere without accurately measuring its changes have been constructed upon the principle of the absorption and consequent change of dimensions of different substances. An instrument, which has lately been very widely described in the newspapers under the erroneous name of a simple barometer, is composed of two shavings of light wood glued together so as to make a ribbon of double thickness; the fibres of one layer being at right angles to those of the other. The absorption of the moisture into the shaving in which the fibres are lengthwise tends merely to increase the width and not the length of the compressed ribbon, while the absorption of moisture into the shaving of which the fibres are transverse tends to increase the length of the ribbon and thus causes it to curl. The foregoing instrument belongs to the class denominated hygroscopes, intended simply to indicate the changes which take place in the vapor in the atmosphere without furnishing the means of measuring its precise amount. For this purpose various substances are employed, such as a stretched cord, a human hair deprived

of oily matter by washing it in ether, and the beard of the wild oat; the change in length of the first two and the twisting of the latter furnish the indications required.

Different materials absorb moisture in different degrees; a fact which is evident in passing along the sidewalk of a street at the beginning of a rain. While some of the bricks of which the pavement is composed are entirely wet at the surface others appear dry, because the water which has fallen upon them has been absorbed. It is scarcely necessary to add that after perfect saturation has taken place, and the surface is exposed to the heat of the sun, the appearance of wetness is exhibited in a reverse order. The relative absorptive power of different materials is frequently a matter of considerable practical importance, which can be readily ascertained by weighing equal bulks of the material previously dried in an oven, and again after having been thoroughly soaked under the pressure of several feet of water. The absorption of water and its subsequent expansion by freezing is the most efficient agency in the gradual destruction of the architectural monuments by which the ancients sought to impress upon the future a material evidence of their power and wealth.

Constitution of clouds.—Water in the state of vapor (as has been stated,) is perfectly transparent, and this may be conclusively proved, even of steam at a high temperature, by boiling water in a glass vessel with a long neck or by fastening a glass tube to the spout of a tea kettle. The vapor within the glass will be entirely invisible, and that peculiar condition called *cloud* will not be assumed till the transparent steam mingles with the cooler atmosphere and is partially condensed. The appearance of a cloud is also produced if a portion of transparent air is suddenly cooled, either by expansion or mingling with a portion of air of a lower temperature. Much speculation has arisen in regard to the nature or condition of water when in the intermediate state of cloud, and though the subject has occupied the attention of scientists for more than a century it is still not fully settled.

Saussure, the celebrated Swiss meteorologist, states that in

ascending the sides of a mountain into the region of the clouds he has seen globules of water as large as small peas floating in the air, which from their levity were evidently hollow spheres, similar to small soap bubbles. From this observation the idea became prevalent that the water of a cloud was in a vesicular condition, or in other words that cloud consists of minute hollow spheres of liquid water filled with air which is rendered more buoyant by the rarefaction due to the heat of the sun; and this opinion was strengthened by the fact that clouds do not give a decomposition of the rays of light sufficient to exhibit the phenomena of the rainbow. In what manner such a condition of water can be produced and how it can be retained, has not, so far as we are informed, been explained by any principle of science. A soap bubble soon becomes too thin to retain its globular form, and is resolved into the condition of soap water. Ordinary water is still more unstable and cannot be retained for an instant in a hollow spherical form. We shall therefore be on the safe side if we adopt an hypothesis apparently more in accordance with known and established principles, and if this does not furnish a logical account of all the phenomena we must wait until further research or light from collateral branches of science dispels the obscurity with which this point may be involved.

The suspension of the clouds can be explained by taking into account the extreme minuteness of the particles of which they are composed. In the case of mists which are sometimes formed at the surface of the earth and afterwards become clouds in being elevated into the atmosphere by a wind blowing between them and the earth, the particles are of such extreme tenuity as to be invisible to the naked eye, and their presence is rendered evident only by looking through a stratum of considerable thickness.

If particles of lycopodium (the sporules or seeds of the club-moss) are dusted upon a flat glass they exhibit a series of colors, when held between the eye and the light, produced by the interference of the waves of different rays of light. In order to produce this effect, the particles of lycopodium (as can be proved

mathematically) must not exceed the seven-thousandth of an inch in diameter. Now the particles of a cloud are sometimes known to present the appearance of similar colors, and therefore are not larger than those of the lycopodium. This extreme minuteness is sufficient to account for the suspension of clouds or the extreme slowness with which they descend. M. Maille of Paris has attempted to compare the volume of a particle of this size with that of a drop of rain water of about a tenth of an inch in diameter. He finds that it would require upwards of 200 millions of particles of cloud to make one drop of rain water of the size mentioned. We are prepared to admit the correctness of the conclusion when we reflect on the rapid increase of the volume of a sphere relative to the increase of its diameter. For example, if a series of spheres have diameters in the ratio of 1, 2, 3, 4, 5, 6, the volumes or weights of the spheres, provided they are of homogeneous material, will be represented by the numbers 1, 8, 27, 64, 125, 216. Indeed nothing is more deceptive than the estimate we form of the relative volume or weight of different solids by simply comparing their diameters. It requires but a very small increase in the diameter of an egg, for example, to double its weight. We know that the resistance of the air to the descent of a falling body is in proportion to the surface which it presents to the resisting medium. Now every time a drop of water is divided, a new surface is exhibited, and when the division is carried as far as that of the particles of cloud, the resistance must be so great that an indefinite length of time must be required to produce a descent of a few hundred feet.

The process of the formation of clouds will be described in a subsequent section; we may here however mention that the forms and aspects in which they are presented are indicative of the circumstances in which they are forming or dissipating, and hence the importance of giving special names to these forms in order that they may become objects of definite study. The first attempt at a descriptive classification of clouds was by Mr. Luke Howard in 1802. An account of this is given in all works on meteorology, and we need here

only give a brief exposition of his nomenclature. He divides clouds into three primary modifications: cumulus, stratus, and cirrus, with intermediate forms passing into one another under the names cumulo-stratus, cirro-stratus, cirro-cumulus; and lastly, a composite form, resulting from a blending or confusion of the others, under the name cirro-cumulo-stratus or nimbus.

1. *Cirrus*, consisting of parallel or diverging fibres, extended by increase of material in any or in all directions.

2. *Cumulus*, convex or conical masses, increasing upward from a horizontal base.

3. *Stratus*, a widely extended continuous horizontal sheet.

4. *Cirro-cumulus*, generally known as "mackerel sky," consisting of small rounded masses, disposed with more or less regularity and connection.

5. *Cirro-stratus*, consisting of horizontal or slightly inclined masses, undulating or separating into groups, giving the idea of a shoal of fish in the distance.

6. *Cumulo-stratus* consists of a blending of the cirro-stratus with the cumulus.

7. *Nimbus* is the cloud from which a continued rain falls.

A drawing of these different forms of clouds will be found in the instructions for meteorological observations published by the Smithsonian Institution.

Dew and hoar frost.—When a mass of moist air is brought in contact with a cold body its vapor is condensed into water and deposited in minute globules on the cooled surface, which constitute dew. If the temperature of the surface is below the freezing point the globules of water will be frozen into minute crystals of ice, which constitute hoar frost. For a long time the nature of these phenomena was entirely misconceived; the effect was put for the cause, the dew being regarded as producing the chill which accompanies its formation instead of the reverse. Dr. Wells of London, born in South Carolina, was the first who gave the subject a scientific investigation, and by a series of ingenious, accurate and

conclusive experiments furnished a definite explanation of all the phenomena. They are simply due to the cold produced in different bodies by radiation. As we have seen, the earth is constantly radiating heat into celestial space, and is constantly receiving it from the sun during the continuance of that body above the horizon. As long as the heat from the sun exceeds that radiated into space the temperature of the surface of the earth and that of the air in contact with it continues to increase; but when the two are equal the temperature remains stationary for a short time and then begins to decline as the heat of the sun, on account of the obliquity of the rays, becomes less than the radiation into space. The maximum of heat generally takes place between 2 and 3 o'clock in the afternoon, and the cooling from this point goes on until near sunrise of the next morning. As soon as the sun descends below the horizon the cooling of the surface of the earth takes place more rapidly if the sky be clear; the air in contact with grass and other substances which are cooled by this radiation will deposit its moisture in a manner analogous to that of the deposition of water on a surface of a metallic vessel containing a cold liquid. Although the atmosphere may contain the same amount of vapor, yet the quantity of dew deposited during the night in different places and on different substances is very unequal. It is evident that it must depend to some extent upon the quantity of moisture, since if the air were dry, no deposition could take place; and indeed it has been remarked that on some parts of the plains west of the Mississippi dew is never observed. It must also depend upon the clearness of the sky; for if the heavens be covered with a cloud the radiant heat from the earth will not pass off into celestial space, but will be partly absorbed by the cloud and radiated back to the earth. This is not a mere hypothesis but has been proved by direct experiment. The author of this article while at Princeton some years ago placed a thermo-electric apparatus in the bottom of a tube provided with a conical reflector, and thus formed, if the expression may be allowed, a thermal telescope, with which the heat of a cloud of the

apparent size of the moon was readily perceptible.* When this instrument was directed first to the clear sky in the vicinity of a cloud, and then immediately after to the cloud itself, the needle of the galvanometer attached to the thermo-electric pile in the tube always deviated several degrees. At first sight it might appear from this experiment that the heat of the cloud was greater than that of the transparent air in which it was floating, but this was not necessarily the case; the rays of heat from the apparatus when it was directed into the clear sky passed off into celestial space, while when the instrument was directed to the cloud they were absorbed and radiated back. It is probable however that the lower surface of the cloud is really a little warmer than the air in which it is floating, from the radiation of heat by the earth, while the upper surface is probably colder on account of the uncompensated radiation into space. But be this as it may, the counter radiation of the clouds prevents the sufficient cooling down of the bodies at the surface of the earth for the deposition of dew, or at least for the formation of a copious quantity. A haziness of the atmosphere (and it is probable a large amount of invisible vapor) will retard the radiation, and hence a still, cloudless night, without a deposition of dew, is considered as indicative of rain. The amount of deposition of dew will also depend upon the stillness of the atmosphere; for if a brisk wind be blowing, the different strata of air will be mingled together, and that which rests upon the surface of the ground will be so quickly displaced as not to have time to cool down sufficiently to produce the deposition.

Again the deposition will be more copious on bodies the surfaces of which are most cooled by the radiation. It is well known that different substances have different radiating powers. The following table from Becquerel exhibits the proportional tendency of different substances to promote the deposition of dew. The figures do not represent the relative emissive power, but the combined effects of emission and conduction :

*[See *ante*, vol. I, p. 233.]

1. Lamp black.....	100
2. Grasses	103
3. Silicious sand.....	103
4. Leaves of the elm and poplar	101
5. Poplar sawdust	99
6. Varnish.....	97
7. Glass	93
8. Vegetable earth.....	92

Polished metals are of all substances the worst radiators; they reflect the rays of heat as they do those of light, and it would appear that the escape of heat from the substance of the metal is prevented by internal reflection. In order that the surface of a body should cool down to the lowest degree it is necessary that it should be a good radiator and a bad conductor, particularly if it be in a large mass and un-insulated. Thus the surface of a mass of metal coated with lamp black, though it radiates heat freely, will not be as much cooled under a clear sky as a surface of glass, since the heat lost at the surface is almost immediately supplied by conduction from within. If however a very small quantity of metal such as gold leaf be suspended by fine threads, the dew will be deposited, because the heat which is radiated is not supplied by conduction from any other source, and hence the temperature will sink to a low degree.

M. Melloni has within a few years past repeated the experiment of Wells, and established the correctness of his conclusions; and has also added some particulars of interest. He found that the apparent temperature of the grass, which in some cases was 8° or 10° lower than that of the air at the height of 3 or 4 feet, was not entirely due to the actual cooling of the air to that degree, but to the radiation and cooling of the thermometer itself, the glass bulb of which is a powerful radiator. To obviate this source of error in estimating the temperature he placed the bulbs of his thermometer in a small conical envelope of polished metal of about the size of an ordinary sewing thimble. This prevented a radiation and by contact with the air indicated its true temperature. He found with thermometers thus guarded that the solid body was in no case cooled down more than 2° below the temper-

ature of the surrounding air, and that the amount of radiation was nearly the same at all temperatures. The explanation therefore of the great cold of the air between the blades of grass is as follows: By the radiation of the heat the grass is at first cooled two degrees lower than the air at the surface of the earth, and next the thin stratum of air which immediately surrounds the grass is cooled by contact to the same degree. It then sinks down and another portion of air comes in contact with the blade of grass, and is in its turn cooled to the same extent, and so on until all the air between the blades is two degrees lower than that of the air farther up. The radiation however continues, and a stratum of air from the mass already cooled is cooled two degrees more, which sinks down as before, and so on until the air between the blades is cooled to 4° below its normal condition; and in this way the process may be continued until the temperature descends to 8° or 10° below that of the stratum of air a few feet above. In this way we can readily explain the small amount of dew deposited on the tops of trees, since the air as soon as it is cooled sinks down toward the ground, and its place is continuously supplied by new portions of the atmosphere. To the same cause we may attribute copious deposition of dew on wool and other fibrous materials which, though they do not radiate heat more freely into space, yet entangle and retain the air between their fibres, and thus allow the cooling process we have described to go on. It would appear that spider-webs radiate heat freely into space, since they are generally covered with a large amount of dew; their insulated position prevents them from renewing their heat, but according to the above principle a much larger amount of deposition ought to be produced by the same material were it loosely gathered up into a fibrous mass. The fact of the screening influence of the clouds teaches us that a thin cloth or even a slight gauze supported horizontally over tender plants is sufficient to neutralize the radiation and to prevent injury from frost during the clear nights of spring or autumn. The same effect is produced by artificial clouds of smoke.

Since radiation from the surface of the earth is most intense on clear nights, when the moon is visible, many of the effects which are due to this cause have been referred to lunar influence; for example, a piece of fresh meat exposed to the moonlight is said to become tainted in a few hours; this may arise from the deposition of moisture on the surface of the meat due to the cooling from radiation. The moon itself however acts as a cloud and radiates back to the earth a portion of the heat which it received from the earth as well as a portion of that which it received from the sun; and hence Sir John Herschel has referred to this cause, with apparent probability, the origin of an assertion of the sailors that "the moon eats up the clouds." He supposes that they may be dissipated by the radiant heat from that body, which being of low intensity and but feebly penetrating the lower stratum of the atmosphere may serve to dissipate the clouds. Though a wrong explanation is generally given by the popular observer of natural phenomena, and though effects and causes are frequently made to change places in his explanations, yet it is true, as Biot has properly said, that the scientist who devotes himself assiduously to investigate the subject of popular errors will find in them a sufficient amount of truth to fully repay him for his labor.

Formation of fogs.—The difference between a fog and a cloud relates principally to the conditions under which they are severally formed. A fog has been aptly called a cloud resting on the earth and a cloud a fog suspended in the atmosphere. The circumstances under which a fog is usually produced are the following: Either the surface of the earth or water is warmer than the air or it is cooler. If the temperature of a river or of a damp portion of ground is higher than that of the atmosphere which rests upon it the warmer surface will give off vapor of an elastic force due to its temperature. Should the superincumbent air be extremely dry the vapor will diffuse itself up through it in an invisible form without condensation, and no fog will be formed until by the continuation of the process the air becomes completely saturated; and then if an excess of heat remain in the evapo-

rating surface the fog will be produced, and will increase in density and height so long as a difference of temperature continues. If however a wind be blowing at the time, so that successive portions of unsaturated air are brought over the place, no fog will be produced. A still atmosphere therefore is a necessary condition to the accumulation of fog.

The foregoing is the usual method in which fog is produced, for it is well known that in cold weather the surfaces of lakes and rivers are much warmer than the stratum of air which rests upon them.

It is however frequently observed that fogs are formed during still nights in low places when the surface of the ground is colder than the stratum of the atmosphere which rests upon it, and indeed we have shown that the temperature of the surface of the earth on a still and clear night is always lower than that of the air which is immediately in contact with it; and it is not easy, without further explanation, to see the reason why fogs should not always be produced in this case as well as dew. When the atmosphere is still the condensation of the vapor by the coldness of the surface is so gradual that the air is not disturbed, and the stratum immediately above the grass has relatively less moisture in it than that a few yards higher; hence no fog ought to be produced in this case, since all the precipitation produced is that which has settled directly upon the grass in the form of dew. In this case we may define the dew to be a fog entirely condensed into drops of water. The question still arises how under these circumstances can a fog really be produced. The answer is that another condition is required, namely that the surface cooled by radiation should slope to a lower level, as in the side of a hill or the concave surface of the sides of a hollow. In this case the superincumbent stratum of air of which the temperature has been lowered by contact with the cold earth, flows down the declivity by its greater weight into the valley below, and there mingling with the damp air which generally exists in such places, precipitates a part of its transparent vapor into visible fog. In this manner large hollows are sometimes seen in the morn-

ing filled with a mass of fog, exhibiting a definite and level surface presenting the appearance of a lake the shores of which are the surrounding eminences; and if a depression of sufficient depth occurs in any part of the circumference of the basin, through this the fog is seen to flow like a river from the outlet of a lake.

The explanation we have here given of the formation of fog in low places is also applicable to the phenomenon frequently observed of early frost in the same localities. As rapidly as the air is cooled on the sides of sloping ground it sinks into the valley below and its place is supplied by the warmer air above, which has not been subjected to the cooling influence. In the vicinity of Washington the hollows are sometimes found several degrees colder than the more elevated parts of the surrounding surface. Fogs are produced on the ocean when a gentle wind charged with moisture mingles with another of a lower temperature. The wind from the Gulf Stream mixing with the cold air which rests upon the water from the arctic regions, (which as before stated flows along close to the eastern shores of our continent,) gives rise to the prevalence of fog over the Banks of Newfoundland.

There is another atmospherical phenomenon which though it does not affect the hygrometer and is only indirectly connected with moisture, is generally classed with fogs. I allude to what is called dry fog—a smoky haziness of the atmosphere, which frequently extends over a large portion of the earth. The nature of these fogs is now pretty well understood, and more refined observations, particularly with the microscope, have served to dissipate the mystery in which they were formerly enshrouded. When a portion of the air in which the fog exists is filtered through water and the substance which is retained is examined by the microscope it is found to consist of minute fragments, in some cases of burnt plants, and in others of the ashes of volcanoes. It is surprising to what a distance the pollen of plants and minute fragments of charred leaves may be carried. Samples of substances which have been collected from rain water and ex-

amined microscopically by Professor G. C. Schæffer of Washington, at the request of the Smithsonian Institution, have been found to consist of portions of plants which must have come from a great distance, since the species to which they belong are not found in abundance in the localities at which the specimens were obtained. It is highly probable that a portion of the smoke or fog-cloud produced by the burning of one of our western prairies is carried entirely across the eastern portion of the continent to the ocean. On this subject Dr. Smallwood communicated a series of interesting observations to the American Association at their meeting in Albany in 1855. Particles of matter of the kind we have described are good absorbers and radiators of heat, and hence in the daytime they must become warmer than the surrounding atmosphere and tend to be buoyed up by the expansion of the air which exists in the interstices between them, while at night they become cooler by radiation than the surrounding air and tend to condense upon themselves the neighboring moisture, and consequently to sink to a lower level. It is on this account that the smoky clouds which are produced by the enterprising manufacturing establishments of Pittsburg and other western cities, sometimes descend in still weather to the surface of the earth and envelop the inhabitants in a sable curtain more indicative of material prosperity than of domestic comfort. From the density and the wide diffusion of these smoky clouds they must produce a sensible effect upon the temperature of the season of the year in which they occur. During a still night, when a cloud of this kind is over head, no dew is produced; the heat which is radiated from the earth is reflected, or absorbed and radiated back again, by the particles of soot, and thus the cooling of the earth necessary to produce the deposition of water in the form of dew and hoar frost is prevented.

So well aware of this fact are the inhabitants of some parts of Switzerland that, according to a paper by Boussingault, in a late number of the *Annales de Chimie*, they kindle large fires in the vicinity of their vine fields and cover them with brush to produce a smoke-cloud by which to defend the

tender plants from the effects of an untimely frost. Though the first announcement of the proposition by some of our earlier meteorologists that the peculiar condition of the atmosphere known as "Indian summer" might be produced by the burning of the prairies, was not thought deserving of any comment, yet the advance of science in revealing the facts just stated renders this hypothesis by no means unworthy of attention. A large amount of smoke existing in the atmosphere must have a very sensible effect in ameliorating the temperature of the season by preventing the cooling due to radiation; and although this may not be the sole cause of the peculiarity of the weather above mentioned, it may be an important consideration in accounting for the smoky appearance of the air and the effect produced upon the eyes.

In concluding this section we would commend to the attention of the microscopists of this country,—as a readily accessible and interesting field of research,—the subject of atmospheric dust. The atmosphere constantly holds in suspension a mass of particles derived from the mineral crust of the globe and from animals and vegetables, which by being deposited in undisturbed positions, serves as a record to be read by the microscope of changes alike interesting to the antiquarian and the naturalist. On this subject M. Pouchet has lately presented a paper to the French Academy of Science, in which he enumerates the particles of mineral, animal, and vegetable origin which he has found deposited from the atmosphere. Under the latter he mentions specially particles of wheat flour which have been found as an ingredient of dust in tombs and vaults of churches undisturbed for centuries. The dust floating in the atmosphere may readily be collected by filtering the air through a tube swelled in the middle, bent into the form of a syphon, partly filled with water and attached at the lower end to the vent-hole of a cask from which water is drawn, or simply by sucking through the air by means of the mouth.

Rain.—The discussion of the *rationale* of the production of rain will be given in a subsequent part of this article. We shall in this place however state some facts in regard to it

which are naturally connected with the general subject of the existence of vapor in the atmosphere.

The humidity so constantly supplied to the air by evaporation is returned to the surface of the earth principally in the form of rain resulting from the union of the very minute particles of water which constitute the mass of clouds. Without stopping to inquire into the cause of union in this place we may remark that we think it probably due to the further condensation of the vapor which first assumed the condition of a cloud. Rain, it is true, has been observed to fall from apparently a cloudless sky, but the occurrence is one of extreme rarity, and it seems possible that it is brought from a distance by wind at a high level.

A knowledge of the quantity of rain which falls in different portions of a country is important, not only with reference to agriculture, but also with reference to internal navigation, as well as to the application of hydraulic power, the occurrence of devastating floods, the water supply of cities, and the sanitary condition of a district.

Almost every portion of the earth on which rain falls is provided with natural drains that carry off the surplus water (above that which evaporates) to the ocean whence it came; and taking the earth as a whole the same amount of water must be returned to the ocean as was taken from it by evaporation.

Nearly the whole surface of the earth is divided into basins, each provided with a separate system of drainage. The boundaries of these basins can readily be traced on the map by drawing a line around between the heads of the streams, the waters of which find the level of the ocean through the channels of different rivers. Thus we have the great primary basins of the Amazon, the Mississippi, and the St. Lawrence, and the secondary basins of the Ohio, the Missouri, and the Tennessee, giving the latter name to those which pour their waters not into the ocean but into another river.

A knowledge of the amount of rain which falls on each of the subordinate basins supplying a river like the Mississippi

with the water which passes through it into the ocean, if transmitted by means of the telegraph, would be of the greatest value, in connection with previous experience as to the elevation of the water of the river corresponding to a given indication of the rain-gauge, in furnishing the means by which the effects of floods may be guarded against and the labors of the husbandman along the banks preserved, in many cases, from destruction. A single gauge in each subordinate basin would be sufficient to furnish valuable practical information of this kind, and in the case of the Mississippi River, (especially if applied to the basins on the eastern side,) would suffice to give premonitory indications of a sudden rise at the lower part of the river, since the water which is furnished from the western part of the valley of the Mississippi is more constant in its amount, or in other words not so subject to fitful variations.

The simplest method of measuring the rain, which any one may practice for himself, is to catch the water in a cylindrical vessel, like an ordinary tin pail, and to measure the depth in inches and tenths of an inch after each shower. It is hardly necessary to remark that the vessel should be so placed that it may not be screened by trees, buildings, and other obstacles from the wind which bears along the falling drops. The object of the investigation is to ascertain the number of inches of water which fall from the clouds on a given space in a given time—for example, a year or a season. It is well known that while the wind is blowing strongly the drops descend in an oblique direction, and gauges have been proposed which, by the action of the wind, would so incline their mouths as always to present them at right angles to the direction of the drops; but gauges of this kind would not give the indication required, which is that of the absolute quantity of rain which falls on a given horizontal extent of the surface of the earth.

A remarkable fact has been observed as to the amount of rain collected at different heights. It is a well known phenomenon, of which we shall give the explanation hereafter, that on the windward side of a mountain a greater amount

of rain falls annually than at a less height on an extended plain. The effect however, to which we now refer, is just the reverse, since it is found that less rain falls on the top of a tower, and even of an ordinary building, than at the bottom. This phenomenon is due in part at least to the fact that a drop in its descent through a foggy atmosphere, in which the rain is falling, catches in its path all the minute particles of water between the upper and lower stations. It cannot be due, except in a slight degree, to the condensation of the transparent vapor in the atmosphere which occupies the line of its descent, since the condensation of this would rapidly heat the drop of water, although its temperature were considerably lower than that of the air, on account of falling from a colder region. The principal cause of the difference is to be found in the effect of the wind in passing over and around the edifice on which the gauge is placed. The effect of this cause was first investigated by Professor Bache, of the United States Coast Survey, who made a series of observations with a number of gauges placed on different sides of the roof of a shot tower in Philadelphia. He found that different quantities of rain were collected by gauges thus placed.

To explain the effect of the wind, we may refer to what takes place when an obstacle like that of a large stone is found with its upper end just below the surface of a running stream. The water of the current will pass over and around the stone, and will rise above the general surface; there will exist a tendency to a partial vacuum on the sheltered side; the liquid in passing over and around the stone will be accelerated; the particles of water which pass around the stone, supposing it to be a cylinder, will traverse a space equal to the semi-circumference of the circle, while those moving along the general current, and not deflected, will pass through a space equal to the diameter of the same circle. A similar effect would be produced by the wind striking against a tower. The portion which passes around the top will be accelerated; that which strikes against the top will be deflected upward, and in both cases a diminution in the quantity of rain which falls on the top of the tower will be the result.

Suppose the wind is coming from the west, and striking with force against the side of the tower which faces that direction, it will be deflected upward, and thus retard the fall of rain on the near side of the roof of the tower, and precipitate it over the leeward side, while the portion of wind which passes around the circumference of the tower, near its top, will be accelerated, and will by the latter action impart its motion to the air on the north and south sides of the roof of the tower, which will cause the drops of rain to be crowded together on the leeward side.

The effect of the upward deflection of the wind and the acceleration of the rain, under conditions such as we have just described, are strikingly illustrated by the observations which were made on the high tower of the Smithsonian Institution. Three gauges were placed on the roof of this tower,—one on the west, one in the centre, and a third on the east side. Now if the prevailing wind be west, we should expect (if the theory which we have presented is correct,) that the west gauge would contain the smallest quantity of water, the middle one next, and the one on the east side the greatest; and this was found to be actually the case.

The action of the wind also materially affects the amount of water which falls in different gauges of different forms and sizes at the surface of the earth. It is well known that different gauges, which indicate the same amount of rain in calm weather, differ materially in the quantity of water which they collect in high winds. If the gauge be of considerable size, and project above the surface of the earth, the air will be deflected upward and accelerated around it, as in the case of the tower; nor is this result obviated by sinking the large gauge to the level of the earth, since in that case the current curves down into the gauge and tends to carry out a portion of the falling drops on the opposite side. From a series of experiments made at the Smithsonian Institution, and continued for several years, it is found that a small cylindrical gauge, of 2 inches in diameter, and about 6 inches in length, connected with a tube of half the diameter, to retain and measure the water, gives the most accurate results.

In still weather it indicates the same amount of water as the larger gauges, but when the wind is high it receives more rain, for on account of its small size the force of the eddy which is produced is much less in proportion to the momentum of the drops of water. This gauge, which has been copied from one introduced by Mr. James Stratton, of Aberdeen, may be still further improved by cutting a hole of the size of the cylinder into a circular plate of tin of 4 or 5 inches in diameter, and soldering this to the cylinder like the rim of an inverted hat, three or four inches below the orifice of the gauge.

The effect of the wind in disturbing the level of light snow in the vicinity of buildings illustrates the general principles which we have endeavored to explain. When a rapid current of air is obstructed by a building the acceleration of its velocity on the side of the eddy is marked by the removal of the snow to a considerable distance. Indeed all the phenomena we have mentioned in regard to rain are illustrated by the extraneous motion given to the particles of descending snow.

Constitution and phenomena of the compound atmosphere.—

From the principles we have endeavored to explain, we may now readily infer what would be the general effects if the earth were surrounded with an ocean of water and devoid of an atmosphere. At first sight it might appear that all the water of the ocean would immediately pass into vapor; but, on a little reflection, it will be seen that this would not be the case. A definite amount of vapor would be formed, which by its pressure on the surface of the water would prevent any further evaporation, provided the whole globe and the space around it were of uniform and constant temperature.

A portion of vapor would rise from the water and would expand as it rose until the upper atoms were so far separated that their repulsion would become insensible and they would be retained as an appendage to the earth merely by their weight. The upper layer of vapor would press on the next lower, and this on the next, and so on with accumulating

weight as we descend ; the aqueous atmosphere surrounding the whole earth would thus be found increasing in density as we approach toward the liquid surface. If the temperature of the earth and of the space around it were 60° F. it will be seen by table A, (p. 217,) that the pressure of this aqueous atmosphere at the surface of the earth would be equal to half an inch of mercury ; if the temperature were 100° it would be equal to 2 inches. This pressure however would be sufficient to prevent any further evaporation, unless, as we have said, an increase of temperature took place.

In order that such an atmosphere should be in equilibrium it would be necessary that the absolute amount of heat in equal weights and at different heights should be the same ; or in other words it should follow the same law as that of a gaseous atmosphere. There would however be this great difference between the two atmospheres, the one would be readily condensed by a diminution of temperature beyond a certain point into water, while the other would remain a permanently elastic fluid at all temperatures. If therefore the space beyond the atmosphere were colder than that which would be due to the diminution which would naturally take place in an aqueous atmosphere, a continual rain would be the result, the moisture would be constantly evaporated from the surface of the earth, and constantly condensed by the cold above. Now were it not for the gaseous atmosphere which surrounds the earth and offers a resistance to the ascent of the aqueous particles, we think such a condition would actually exist. We are inclined to this belief from the facts which have been stated indicating an exceedingly low temperature to the space beyond our atmosphere.

Be this as it may however, an atmosphere of this kind would be exceedingly unstable, and if any portion of the earth's surface were colder than another there would be a constant condensation at the coldest parts, and a constant evaporation at the warmest to restore the equilibrium. If for example the heat of the equatorial regions were 80° , and that of the polar regions at zero, the elastic force of the vapor at the former place would be 1 inch, while at the latter it

would be but 0.043 of an inch; hence an equilibrium could not exist, and there would be a continued series of currents from the equator to the poles, a perpetual condensation of vapor into water at the latter, and a constant evaporation of liquid into vapor at the former, for the supply of which a series of ocean currents would be established. A tendency to the same effect must exist in the compound atmosphere of air and vapor which actually surrounds our earth, but the resistance to the permeation of the vapor is so great that a considerable inequality of the elastic force of vapor continually exists in different parts of the earth.

Though there is a constant tendency to a diffusion of vapor from the equator to the poles, yet the greatest disturbance of the equilibrium of our atmosphere results from the diminution of temperature as we ascend in the atmosphere, and for the establishment of the principle on which this disturbance depends, and the consequences which flow from it, we are indebted to the laborious, persevering, and sagacious investigations of Mr. James P. Espy.

From observation it is well known that the air diminishes in temperature as we ascend, at the rate of about one degree Fahrenheit for each 100 yards or 300 feet. If therefore a portion of air be transferred from the surface of the earth to a height in the atmosphere, it will be cooled to the temperature of the stratum of air at which it arrives; but it is proper to observe at the beginning of the explanation that this cooling will not be due principally to the coldness of the space to which the mass of air has been elevated, but chiefly to its own expansion. If the air for example expands into double the space by being subjected to half the pressure, it is evident that the amount of heat which it contains will be diffused through twice the amount of space; and hence though the absolute quantity of heat remains the same, its intensity of action, or its temperature, will diminish and the substance will become much colder. This is a principle to which we have before alluded, and which will be frequently applied hereafter in the explanation of phenomena.

If in accordance with the foregoing an upward motion takes place from any cause whatever in a mass of air saturated with vapor, a precipitation must instantly follow. For example, if we suppose the moist air to be raised to the height of 1,000 yards, and if we further suppose the temperature at the surface to be 70° the temperature at the height of 1,000 yards will be 60° ; and if we inspect table *B* (page 225), at these numbers, we shall find opposite 70° 7.99 grains of vapor for each cubic foot; and opposite 60° , 5.75 grains of vapor for each cubic foot. In this case therefore nearly 2.24 grains of vapor will be converted into water and fall as rain. We see from this simple consideration that the mere upward motion of a portion of saturated air, from whatever cause produced, must give rise to a precipitation of vapor in the form of water. It may not be in sufficient quantity to come to the earth in the form of rain, but may remain in the air in the intermediate state of a fog or a cloud.

If the air be not saturated entirely with vapor no precipitation will ensue until it rise to the height at which it becomes by the diminution of temperature fully saturated. Suppose for example the air at the surface is 70° , and the vapor in it is that due to 65° ; then it is plain that it must be reduced in temperature 5° before precipitation commences, and this reduction will take place at the height of 500 yards, since, as we have just stated, the reduction of temperature is one degree for each 100 yards of ascent. And by this simple method Mr. Espy has shown that we may, on a given day, approximately estimate the height of the base of a cloud by merely knowing the dew point at the surface of the earth; for if we find that while the temperature of the air is 70° , there is required at the same time to produce a deposition of dew on the exterior surface of a tumbler, a reduction of temperature of 6° (for example) of the water within, the cloud would be 600 yards above the surface of the earth, because it will be necessary that the vapor should rise to that height in order that the whole mass may be cooled to the point of deposition. The bottom of this cloud will be horizontal,

because the precipitation begins at a definite temperature due to a definite height; its form will be that of a mushroom, bulging out and gradually increasing in altitude; in short, will be precisely that form of cloud which is denominated cumulus, and which may be seen during a moist warm day forming in a still atmosphere, gradually extending upward until the precipitation of vapor begins to be so copious that the particles of water coalesce and form drops of rain, which falling down directly through the base of the cloud, leave but a remnant of very attenuated vapor, which is blown away and forms, according to Mr. Espy, the cirrus or hair clouds.

We can also readily infer from the same principle that so long as a current of air moves horizontally over a plain of uniform temperature, no precipitation will take place; but if in its course it meets with a mountain, up the acclivity of which it will be obliged to ascend and thus come under a less pressure and lower temperature, a precipitation must ensue. We have in this way a natural explanation of the effect of a mountain in causing a cloud and a fall of rain, and need not refer the phenomena to the unscientific explanation of attraction so frequently given; we say unscientific, because the attraction of gravitation at a distance on an atom of vapor, is almost infinitely small, and could have no appreciable effect in drawing the clouds. If we suppose, in addition to the preceding case, that the air, after ascending to the top of the mountain and forming a cloud by the precipitation of its moisture, descends on the other side to the same level, it will arrive at the earth much dryer than it went up. If the height of the mountain is not sufficient to reduce the temperature enough to produce a rain, but merely a cloud, and if we suppose the current of air to continue its course, and to descend to the same level on the other side, it will, as it descends, become condensed as it comes under greater pressure; the temperature will increase for a like reason to that which caused its diminution in the ascent.

We have in this way an explanation of the paradoxical appearance of a strong wind blowing across the top of a

mountain, while a light cloud, which crowns its summit and perhaps hangs over its sides, remains apparently immovable. The truth is that this cloud, which appears stationary, is in reality a succession of clouds constantly forming and constantly dissolving. Every portion of air which ascends the mountain, tends (by its expansion and cooling) to form a new portion of cloud, and in its descent tends (by its condensation and increase of temperature) to dissolve a similar portion. The cloud is consequently forming on one side and dissolving on the other, and in this condition may aptly represent the dynamical equilibrium of the human body; which, by every expiration of breath is wasting away, and by every pulse of the heart is renewed.

What we have given may be considered as the more obvious inferences from the first and simplest propositions of Mr. Espy's theory. The phenomena as they occur in nature however are more complex, and another effect is produced by the upward motion of the air, which very essentially modifies the results; we allude to the great amount of heat which is evolved during the condensation of vapor into water. We have stated that the heat evolved from the combustion of 20 pounds of dry pine wood is absorbed by a cubic foot of water at the ordinary temperature of the air in its conversion into vapor, and it is evident that this vapor cannot be re-converted into water without giving out to the surrounding bodies an amount of heat equal to the combustion of 20 pounds of dry wood.

In order to give an idea of the importance of this principle, which is an essential element in the theory of Mr. Espy, it will be necessary to dwell somewhat longer on other points before considering more minutely the results to which it leads.

Statical equilibrium of the compound atmosphere.—Before proceeding to discuss the subject further, it will be necessary to consider the question, which appears to be in a very unsettled state, as to the effect of vapor in the atmosphere while in the act of diffusion. On the one hand, the resistance which air offers to the diffusion of vapor has been too much

disregarded, and on the other, we think too much effect has been attributed to this cause. It is customary in reducing the observations made at European observatories to deduct the elastic force of the vapor in the atmosphere at a given time from the height of the barometer, and to consider the remainder as the pressure of the dry air. This process would give a correct estimate of the pressure of the dry air, provided the gaseous envelope of the earth were a perfect vacuum to the vapor, and the latter were consequently regularly diffused through the space in accordance with its diminution of density due to a diminution of pressure and temperature as we ascend; but this we know to be far from the fact. In the balloon ascent of Mr. Welsh, on the 21st of October, 1852, the tension of vapor at the elevation of 800 feet was observed to be greater than at the ground, and at a height of 3,000 feet it was still greater. In an ascent of the same observer on the 17th of the previous August, the tension continued to increase until an elevation of 8,400 feet was reached.

To render this point more clear, we will for a moment consider the relation of tension and pressure. By the tension of vapor, (as has been seen,) we understand the elastic force or repulsion of the atoms combined with the action of heat by which they tend to enlarge the space in which they are enclosed, and to force down the mercurial column in the experiments by which table A (p. 217,) was constructed. At the temperature of 60° F. this elastic force is just balanced by a column of half an inch of mercury. Let us now consider the nature of tension in regard to the atmosphere; for this purpose let us suppose a piece of paper pasted over the mouth of a glass tumbler so as to be air tight. This paper, though of a very fragile texture, is not broken in by the superincumbent pressure of a column of air extending to the top of the atmosphere and pressing with a force equal to nearly 15 pounds on every square inch of the surface of the paper, because it is counteracted on the lower surface by an upward pressure due to the repulsive action or elastic force, that is to the tension of the inclosed air. The weight of the superincumbent column on the upper side of the paper is known as the weight

or pressure of the atmosphere, while the upward pressure on the lower side, due to the repulsion of the atoms, is designated indiscriminately by the terms *elasticity*, *elastic pressure*, *elastic force*, and simply the *tension* of the air.

The force analogous to the latter (in the case of vapor) is more generally known by the name of *tension*, though it is sometimes called *elastic pressure*. In the foregoing experiment, if the pressure of the superincumbent air is increased, the exterior surface of the paper will assume a concave form, the atoms of the inclosed air will be pressed nearer together, and their repulsive energy will be increased by the approximation of the atoms, and thus a new equilibrium will take place. If conversely the column of air above the tumbler is diminished in weight, the surface of the paper will assume a convex form, because the atoms within the tumbler being pressed with less force will separate to a greater distance, and the repulsion will be reduced by their separation, until a new equilibrium is attained between the pressure without and the repulsion within. In this case, variations of the elastic force or tension of the air within the tumbler become an exact measure of the pressure of the exterior column, provided the temperature remains the same; and it is upon this principle that the barometer called aneroid is constructed. It consists practically of a flat flask of thin metal, filled with air and hermetically sealed by means of solder; the motion of the sides of this flask, precisely analogous to that of the paper closing the mouth of the tumbler, is communicated by means of lever and wheel work to a hand, which indicates the variations of the tension of the inclosed air and consequently of the weight of the atmosphere.

Now if the aqueous vapor formed a separate or entirely independent atmosphere around the earth, the variations in its pressure would be accurately measured by the variation of its tension or elastic pressure at the surface; but since the vapor, on account of the resistance of the air with which it is entangled, is not uniformly distributed, its tension at the surface cannot give a true measure of its whole pressure. It is true that as a whole the weight of the atmosphere is in-

creased by the addition of every grain of water which rises in the form of vapor from the surface of the earth or ocean, but when the evaporation is copious in a limited space, as for example, over the surface of a pond of water, or a portion of the earth subject to sunshine while the regions around are obscured by clouds, the elastic force of the vapor tends to diminish the specific gravity of the aerial column and to produce a fall rather than a rise of the barometer. This is always the case while the vapor is in the act of diffusion; for the resistance of the atmosphere at the surface of the expanded volume of vapor may be considered as an elastic envelope against which, as in the case of the India-rubber bag to which we have previously alluded, the aqueous atoms press by their repulsion and tend to expand it, and therefore to increase their own volume as well as that of the inclosed atmosphere.

If the vapor ascended into the air without resistance, (as in a vacuum,) it would in all cases increase the weight of the latter, but on account of the resistance under the conditions we have just mentioned, the ascending vapor by its elasticity would lift up the atmosphere, tend to lessen its pressure, and thus temporarily to expand the air in the space included within the surface of the aqueous volume. It is therefore a difficult point to ascertain in the explanation of these phenomena, when we must consider the weight of the atmosphere increased, or when diminished—by the pressure of vapor.

It is evident from the experiments which have been made on evaporation under diminution of pressure of air, that the resistance to diffusion spoken of diminishes in proportion to the rarity of the atmosphere; and hence the vapor which exists at great elevations would be in a state of entire diffusion, and its presence would increase the specific gravity of a portion of air through which it is disseminated, instead of diminishing it.

We think erroneous conclusions have frequently been arrived at on account of a want of a proper consideration of this subject, and from too exclusive an attention to the expansive influence of the aqueous vapor in a confined space

on the one hand, and the increased pressure of the whole atmosphere by the addition of vapor on the other.

It is probable however that in portions of the earth in which the air is constantly saturated at a uniform temperature and at which the diffusion is permanently uniform, if the elastic force of the vapor is subtracted from the whole height of the mercurial column it will give the pressure of an atmosphere of dry air.

On the supposition that the vapor is uniformly distributed through the atmosphere, (which will not be far from the truth if considered with reference to the principal zones of the earth,) we can calculate the whole weight of water contained. If the water were at the boiling point its elastic tension or pressure would be equal to the pressure of the atmosphere, and in this case it would support 30 inches of mercury, or its equivalent, 407·4 inches of water; and since transparent vapor observes the same law of expansion and contraction by variations of pressure and temperature that dry air does, it is clear that we shall have the following relation for any other temperature, namely, as 30 inches is to the quantity of mercury expressing the elasticity of the air at any temperature, so is 407·4 inches of water to the whole weight of the aqueous vapor, provided the weight of vapor were the same as that of the air. It has however been proved by the experiments we have described that vapor is only five-eighths of the density of air, and therefore the quantity found by the foregoing relation must be reduced in this ratio.

If we assume that the dew-point is on an average 6° below the temperature of the air, and allowing the temperature of the tropical regions to be 82° , we shall have the following proportion,— $30 : 0.897 :: 407.4 : 12.181$. This last number must however be multiplied by $\frac{5}{8}$, and this will give us 7.613 inches. From this it will appear that if the atmospheric columns at the equator were to discharge their whole watery store the moisture precipitated would cover the earth to the small depth of 7.613 inches; and from a similar calculation we find that if the column of air resting upon the city of

Washington were to precipitate at once all its moisture, the quantity of water would be indicated by about 3 inches of the gauge. To supply therefore 30 or 40 inches of rain in the course of a year it is necessary that the vapor contained in the atmosphere should be very frequently renewed, and that consequently localities which cannot be reached by moist winds must be abnormally dry.

Effects of vapor on the general currents of the atmosphere.—From what has been previously stated it is evident that the atmosphere which surrounds the globe being composed of two portions, one of permanent elastic gases, and the other of a readily condensable vapor containing a large amount of latent heat, it must frequently be in a state of tottering equilibrium, liable to be overturned by the slightest extraneous forces, and in assuming a more permanent condition to give rise to violent commotions, and currents of destructive energy.

It has previously been shown that the equilibrium of a dry atmosphere depends upon the fact that each pound from the top to the bottom of an aerial column contains approximately the same amount of heat. If therefore a portion of air be caused to ascend (by mechanical or other means) to a greater elevation, it will expand, and its heat being distributed through a larger space, its temperature will fall to that of the new region to which it has been elevated, and be again in equilibrium. If on the other hand a portion of air be caused to descend, it will be condensed into a smaller space on account of the increased pressure, and its temperature will be raised to that of the stratum at which it has arrived. But this is not the case with moist air; for if by any means it be elevated above a given level, the coldness produced by its expansion will, as we have said, condense a portion of the vapor into water, and in this process the vapor will give out its latent heat to the surrounding air, and therefore the column in which this condensation has taken place will not be as cold as the surrounding atmosphere; consequently an upward force will still exist, the column will rise to a greater height, and a new portion of vapor

will be formed, and so on until all or nearly all the vapor will be converted into water. In this way the steam power, which has been accumulated from the heat of the sun, is expended in producing commotions of the atmosphere connected with all the fitful—and many of the regular—meteorological phenomena of the globe.

It may be objected to this part of the theory of Mr. Espy that the condensation of the vapor in the atmosphere would tend to contract it into a smaller space, consequently to render it heavier, and thus neutralize the effect of the expansion due to the evolution of the latent heat. The effect however from this cause is very small in comparison to that due to the expansion of heat; and this will be plain when we consider that the particles of vapor exist in the interstices of the particles of air, and in a close vessel tend to increase the volume only in proportion to their repulsive force, which compared with that of the air, is small. For example, if a quantity of dry air were inclosed in an India-rubber bag, at a temperature of 60° , at the level of the sea, its elastic pressure outward on the sides of the bag would be equal to the weight of 30 inches of mercury, while the elastic force of vapor would only be equal to half an inch of mercury; so that we should have the enlargement of the bag expressed by the last term of the following proportion: If 30 inches of mercury give one foot what will 30.5 give? In this case, which is an extreme one, we see it would give but a little more than 1 per cent., and hence the diminution due to extracting the vapor from a quantity of air is very small, and far less than the expansion due to the evolution of heat. This will be evident from the following calculation of the effect produced by the condensation of a pound of vapor into water: It is known from direct experiment that the condensation of one pound of vapor will raise 970 pounds of water 1° , or if it were possible to heat water thus high it would raise one pound of water 970° ; but the capacity of air for heat is only one-fourth that of water; therefore the condensation of one pound of steam would raise one pound of air $3,880^{\circ}$, or 10 pounds of air 388° . The above calcula-

tion is from Daniell's Chemical Philosophy, and is given as an illustration of the immense motive power due to the fall of a single pound of water in the form of rain. During a single rain, in 1857, water fell to the depth of 6 inches in the space of 36 hours, and considering merely the amount of ascensional power evolved by the condensation of the quantity of the liquid which fell on the roof of the Smithsonian building, it would be equivalent to a thousand horse-power exerted during one day.

From these considerations it is evident that the general currents of the atmosphere must be very much modified by the action of the vapor, and very different from those described in our previous essays as belonging to dry air. Indeed to such an extent are some of the general phenomena influenced by this cause, that the motive power of the atmosphere has been referred to other causes than the action of the heat of the sun; but in this case, as in most other exceptions to a principle deduced from a wide generalization—like that of the action of solar heat on our atmosphere, the facts when rightly understood and properly interpreted, serve but more firmly to establish the truth.

We shall now consider more minutely the effect of the formation and condensation of vapor in modifying the general circulation of the atmosphere. It has been shown in the previous articles, that if the earth were at rest in space, without revolution on its axis, heated at the equator and gradually cooled to a minimum point toward the poles, there would be a constant circulation of air from the poles, north and south, toward the equator. The air would rise in a belt encircling the whole earth, and flow backward towards the poles above. In this simple circulation, at every place on the surface of the earth, in the northern hemisphere for example, there would be a perpetual wind from the north flowing toward the equator, and above the same place at the surface of the aerial ocean there would be a return current constantly flowing from the equator toward the pole. It is evident however since the meridians converge and meet at the pole, that the space between any two be-

comes less and less as we depart from the equator; hence all the air which ascends at the equator could not flow entirely to the pole, but the larger portion of it would descend to the earth to return again to the equator, along the surface at some intermediate point, which would be, on an average, about the latitude of 30° , since the space included between this and the equator would be nearly equal to the remaining surface in each hemisphere. Again as we have seen, the simplicity of this system of winds would be interfered with by the rotation of the earth on its axis. On account of this rotation, as a general rule, when a current moves from the equator, in the northern hemisphere, for example, it would gradually curve to the east, and when it moves southward in the same hemisphere, it would curve to the west; the rapidity of curving in either case would increase as we approach the pole. On account of this curvature and deflection east and west of the upper and lower currents, together with the disturbance produced by the evolution of the latent heat, the simple system we first described will tend to separate, as we shall more fully see hereafter, into three distinct systems, which we have represented by *A*, *B*, and *C*, in the annexed figure.

Fig. 5 is a diagram intended to represent an ideal section

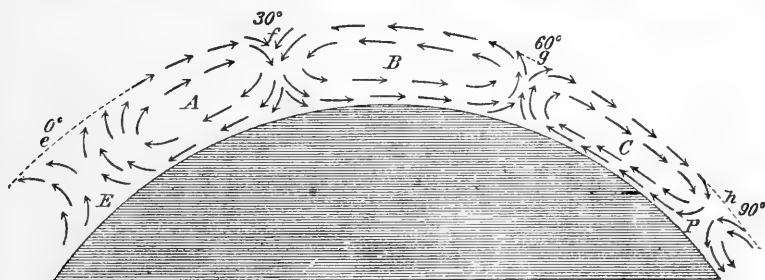


FIG. 5.

through a meridian of the northern hemisphere, showing the several systems of aerial circulation, commencing on the left at *E* (the equator), and completing the series on the right at *P*,—the north pole. Fig. 6 is a bird's eye view of the globe, designed to illustrate the prevailing direction of the

surface currents, particularly in the northern hemisphere. By comparing the two figures, it will be seen that the systems *A*, *B*, and *C*, of Fig. 5, correspond with the three zones of arrows in Fig. 6. To supply the air which ascends in the region near the equator, the current on each side, on account

Surface Winds of the Globe.

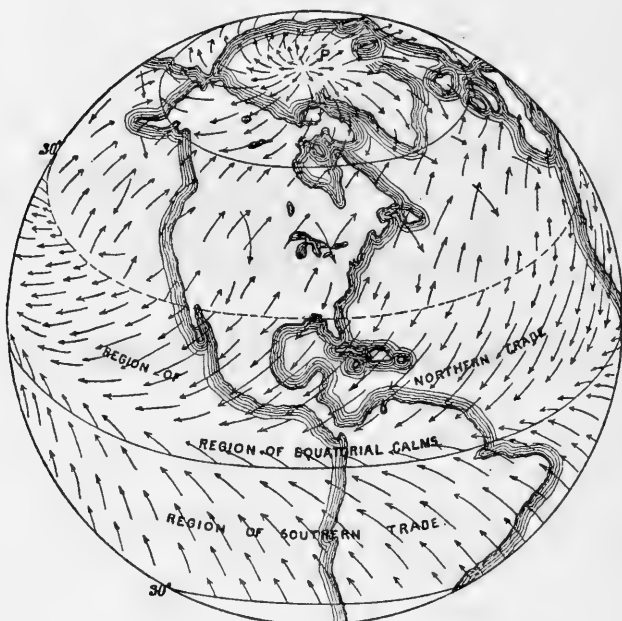


FIG. 6.

of the rotation of the earth, takes an oblique direction, (as we have seen,) flowing in the northern hemisphere from the northeast, and in the southern from the southeast. It continues its westerly motion as it ascends until it reaches its culminating point, and then flows backward in an opposite direction curving as it goes, toward the east.

The surface currents on either side of the equatorial region, (called the trade winds,) as they pass over the ocean constantly imbibe moisture, and deposit but little in the form of rain, since there is no obstacle on the level surface of the water to produce an upward current and the consequent

diminution of temperature essential to the formation of rain. They therefore carry their moisture to the belt of confluence, where in the ascent of the air it is precipitated, evolves its latent heat, and develops its ascensional power. To render the ascent of these currents more plain Fig. 7, may be considered a transverse section across the equator at the belt of calms.

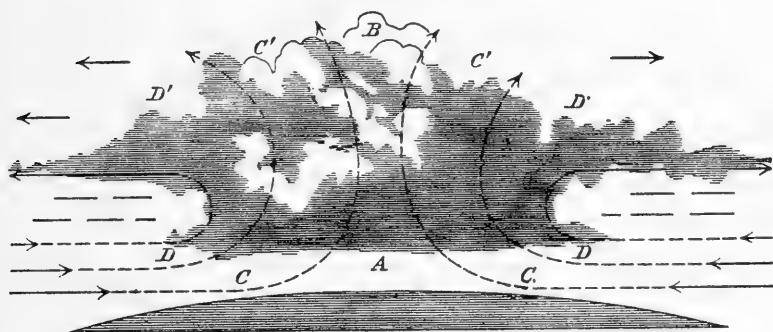


FIG. 7.

The air enters below on either side *D C*, rises upward in the middle space, and spreads out north and south above *D' C'*. As the air ascends it comes under less pressure, expands, becomes colder, and on this account condenses a portion of its vapor, which renders the air warmer and lighter than it would be if this evolution of "latent heat" did not take place. Hence the ascension continues, and the elevation to which the column attains is therefore much greater than it would be if the air were void of moisture. The condensation of the vapor takes place in the form of a large amount of rain which falls by its superior weight through the ascending air, *A, B*, and deluges the surface immediately below, in some places to such an extent that fresh water on the surface of the ocean has been found floating on the top of the salt water. Indeed more rain falls on the surface within this belt than on the whole earth beside. On either side of the rain belt a cloud will be formed by the spreading out of the ascending air mixed with vapor, as shown in the figure. The falling rain coming from a high elevation and

having consequently a low temperature, will cool the surface of the earth below that of the spaces on either side.

The pressure of the air in the ascending column will be less than that on the regions north and south, since a portion of its weight is thrown over on either side. This fundamental principle, which has been strangely mis-understood, will be rendered evident by the annexed figure 8, in which *A, B* represents the surface of the earth, and *a, b, and c, d,* (the several parallel lines above,) the surfaces of the strata in which

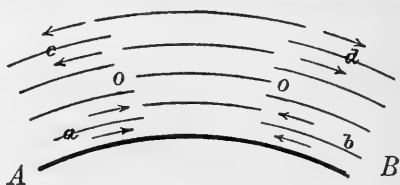


FIG. 8.

the air is supposed, for illustration, to be divided. The depth of these strata will be throughout the whole column increased, and the surface of the upper one will be elevated above the general surface of the atmosphere. Being unsupported, it will tend to flow over on the strata on each side; the surface of the next stratum below will also press outward with more force than it is pressed inward, and will consequently mingle with the air on each side, while the heavy air on each side below opposed by lighter air will press under the lower stratum and tend to elevate it. Between the bottom and the top there will be a neutral surface, marked *o*, which is in equilibrium.

In the middle space at the bottom of the ascending column (Fig. 7), the air will be nearly at rest, subject however to fitful squalls due to the falling rain, and hence this belt is known either as the belt of rains, or of equatorial calms. The width across the ascending belt is several hundred miles, and though particles of dust or infusoria which enter on the south side may occasionally mingle with the air which enters at the north and thus be carried northward by the upward current, yet the habitual crossing of the two, as some have supposed, and the constant transfer of the vapor of the northern hemisphere to the southern, and *vice versâ*, is in accordance with no established principle of nature, and therefore cannot be admitted even as a plausible hypothesis.

On account of the heat evolved, the air in the ascending belt receives an additional momentum which carries it considerably beyond the point of statical equilibrium, and consequently it descends with a greater velocity, which is further accelerated by the cooling to which it is subjected at this high altitude by radiating its heat into celestial space. In its descent it brings down with it (at about the average latitude of 30°) the air north of this latitude, giving rise to a reverse current, and thus producing two separate systems, *A* and *B*. (Fig. 5.) The air at the foot of the descending belt at the latitude of 30° will press with greater weight than that of the average of the atmosphere, hence in this belt at the surface of the earth the barometer will stand higher, and while the belt of rains is called the middle belt of low barometer, the belt of 30° is frequently known as the belt of high barometer. At the foot of this belt the air will be pressed out toward the north and south; southward to supply trade winds and the air which ascends at the belt of calms, and northward to form the current from the southwest, (as shown in Fig. 6,) which latter is the prevailing wind of the north temperate zone.

We have thus seen that there would be a tendency to separate into the two systems *A* and *B*. (Fig. 5.) There would also be a tendency in the remaining air to separate at the point *g*, giving rise to the polar system *C*. Were the air within the circle of 60° north latitude entirely isolated from the other part of the atmosphere, a circulation would take place in this such as is indicated by *C*, the difference of temperature between the surface of the earth at the circumference of this circle and the regions in the vicinity of the cold pole would be sufficient to produce such a circulation. The column of air in the polar region, on account of its low temperature, would be denser and consequently heavier than the surrounding air; it would therefore sink down and spread out in every direction from the centre of the column; the air would flow in above to supply the level, while the current below would become heated as it passed southward and rise as shown at the point *g*. In its ascent it would tend to

carry up with it the surface air of the system *B*, and thus conspire with the downward motion at *f* to produce the circulation shown in system *B*.

The upward current at *g*, (as in the case of the upward current at the equator,) will tend to diminish the pressure of the air and produce a low barometer and an abnormal fall of rain, which perhaps will be more effective in helping on the circulation of the system *B* than the mere mechanical effect of the uprising of the current of *C*. The current at the surface of the earth in the system *C*, as is shown in Fig. 6, will curve to the westward on account of the increased rotation of the earth, and will therefore be almost in direct opposition to the system *B*. If we attentively consider the effect of the rotation of the earth on the system *B*, we shall find that as the current passes along the surface to the northeast as indicated in Fig. 6, it will begin to ascend when it comes near the parallel of 60° , (retaining however its easterly direction,) will gently curve round and pass southward as an upward current, and flow toward the equator as an upper northwest current, shown in the figure by the few longer arrows, indicating a northwest wind. The system *A* is the constant circulation of the trade and anti-trade winds. The system *C* depends upon a similar cause as we have seen, and is for a similar reason permanent in its character. Though but comparatively few observations have been made in the polar regions, the character of this system does not rest upon mere inference from the general principles we have given, but is conclusively established by the immediate results of reliable data. Professor J. H. Coffin, in his valuable memoir on "The Winds of the Globe," published by the Smithsonian Institution, inferred the existence of this system independently of theoretical conclusions. From the reduction of all the observations he was able to obtain, he conclusively proved that the resultant wind from the pole is from a northeasterly direction; and the same result is established by the discussion of the interesting series of observations made during the last expedition of Dr. Kane. These observations, which

have been tabulated for the Smithsonian Institution, under the direction of Professor Bache, by Mr. Schott, of Washington, give the same direction to the northern current at the surface of the earth within the polar circle.

That the prevailing motion of the system *B* is in the direction exhibited by the arrows, is abundantly shown by the fact of the prevalency of the southwest wind, particularly in the summer, over the whole of the temperate zone; and that this upper current of the same system is southward and eastward, or in other words from the northwest, is attested by aeronautic observations in this country, and in Europe. The celebrated American aeronaut, Mr. John Wise, (from the experience of upward of two hundred balloon ascensions,) has stated to the writer, that while the current at the surface of the earth is from the southwest, at a variable elevation of two miles or less, the wind becomes nearly due west, and at a still greater elevation it blows from the northwest. The direction of the intermediate stratum is probably due to the resultant action of the two, and this would naturally result from the almost constant action of ascending currents, passing with every fall of rain from the lower to the upper. A similar testimony is given for Western Europe by the aeronautic experience of Messrs. Green and Mason. According to this, though the prevailing wind at the surface is from the southwest, at an elevation of 10,000 feet the current is invariably from some point north of west. Moreover, observations on the direction of the ashes of volcanoes prove the same direction of the upper current. In the summer of 1783 the smoke of an eruption of a volcano in Iceland was diffused over England, Germany, and Italy. From another eruption of a volcano in the same island, in 1841, the ashes were carried by a northwest upper current and deposited on the decks of vessels in the Irish Channel.

Though the *prevailing* direction of the currents of the system is given in *B*, (in Fig. 5,) yet the stability of this system is by no means equal to that of *A*, or even that of *C*, since in some cases its direction is apparently entirely reversed. The northwest upper current, mingling perhaps

with the polar current, descends to the surface of the earth, (particularly along the continent of North America,) and probably gives rise to the phenomena known by the name of "Northerners" and possibly also to the more violent north-east storms of the coast. While the reversal of this system takes place in one part of the earth, the more habitual motion may be continued in another, and in this way a mild winter in America, produced by a prevalence of south-westerly wind, may be accompanied with a severe winter, produced by northwesterly winds in some part of Asia, or Eastern Europe.

The belts and systems we have described are not stationary, but move north and south in different periods of the year with the varying declination of the sun. For example, the belt of rains is constantly almost directly under the sun, and moves north and south with the changing declination of that luminary, and thus divides the year in the tropical regions into two rainy and two dry seasons. The rain is produced (as has been abundantly shown) by the condensation of the vapor carried up by the ascending current of air; the dryness on each side of this belt is the result of the descent of the air which has been thrown out above, principally deprived of its vapor and increased in temperature both by the heat due to condensation and to that absorbed before it is thrown outward from the precipitated vapor. In the summer season, when the sun is on the northern side of the equator, the trade-wind system extends up on the ocean sometimes as high as 40° N. latitude. A similar movement takes place, but to a less extent, in the system of the temperate zone. From this movement it is evident that there is not only a variation of heat, but also of moisture and precipitation at different seasons of the year.

It is also necessary to mention that the belt of high barometer is interrupted across the continent of North America, and probably never passes farther north than the portion of the United States bordering on the Gulf. But on this point we cannot speak positively without more data and further investigation. It is certain however that on the Pacific side,

the belt of high barometer, (or that from which the air flows out on each side north and south,) in summer extends beyond the latitude of 40° , and thereby produces a wind from the north in this season of the year, while in winter it is found below Southern California, and thus gives rise along the coast and parallel mountains of the interior to a wind in the opposite direction, namely, from a southern point of the compass.

This is a sufficient explanation of the rain which falls at that season, since the currents from the south are laden with moisture which they deposit in their ascent along the slopes of the mountains towards the north.

On the drawing exhibiting the surface currents, (Fig. 6, p. 276,) the point *P* representing the geometrical pole, is not the centre of divergence of the aerial currents which settle down in this region. The latter centre is that of the cold pole, which probably on account of the unequal distribution of land and the currents of the ocean, does not coincide with the former.

Climate of the United States.

An application of the general principles we have given will enable us readily to comprehend the peculiarities of the climate of the United States, and to see how it must differ from that of other portions of the globe.

In order however to properly make this application, we must briefly recall what has been said in previous papers* on the circulation of the waters of the ocean, since they have a powerful influence in the distribution of heat and the modification of different climates of the earth. For the more definite comprehension of this, we have prepared a sketch of the western hemisphere, shown in Fig. 9, on which the direction of the principal currents of the northern oceans are denoted by arrows, and in explanation of these, we shall briefly recapitulate the general theory of the cause and motion of these currents.

If the equatorial regions of the earth were entirely covered

[See *ante*, pp. 59-62.]

with water, the trade-winds blowing on each side and acting on the water would produce a current toward the west, encircling the whole globe. But since the region of the equator is crossed by continents, the continuous current we have

Ocean Currents of the Western Hemisphere.



FIG. 9.

spoken of is broken up and deflected right and left into extended circuits; the water blown from the coast of Africa along the region of the equator westward is divided into two currents, as represented in Fig. 9, one directed northward, and the other southward, by the projecting part of South America. The northern branch, as shown by the arrows, passes through the Gulf of Mexico, and impelled by the action of the surface wind and the rotation of the earth, makes a complete circuit, returning into itself along the coast of Africa, leaving in the centre a large area of stagnant water covered with weeds, and known by the name of the Sargasso Sea. The entire course of the waters in this

extended circuit is completed in about three years. In the Atlantic Ocean a branch is sent off from this circuit, which passes northward, impinges on the western coast of Europe, and probably skirts the whole circuit of the polar basin, from which it passes out on the west side at Behring's Straits.

Two similar systems of currents exist in the Pacific Ocean; that in the northern hemisphere passing from Central America along the equator to the continent of Asia, is deflected northward along the coasts of China and Japan, and returns to the equator along the western coast of North America.

Besides these great circuits from the equator, cold currents descend from the polar basin. One of these is represented by the arrows with double barbs between the Gulf Stream and the eastern coast of the United States; and a similar one descends along the coast of China between it and the Gulf Stream of that region. These are in part derived from the water which is discharged into the polar basin from the several rivers of the north, and probably in part due to a return portion of the equatorial currents. They skirt the eastern shores of the continents, because currents from the north (on account of the rotation of the earth) tend to move westward, while those from the south tend to move eastward.

The effect which these great currents of the ocean, (evidently the natural results of the system of winds which we have described,) produce on the climate of the United States, compared with that of Europe, can readily be appreciated. The elevated temperature of the water in the Gulf of Mexico, (higher than that of the water in almost any other part of the globe,) is retained by the Gulf Stream until it reaches the shores of the polar basin. The southwest winds which accompany and blow over the Gulf Stream share its temperature, and impart their warmth and moisture to Western Europe, giving it a climate far more genial than would be due to the latitude. The southwest and westerly winds which prevail over the surface of the United States serve to bear the heat of the Gulf Stream from our coast, and even when an easterly wind is produced by local

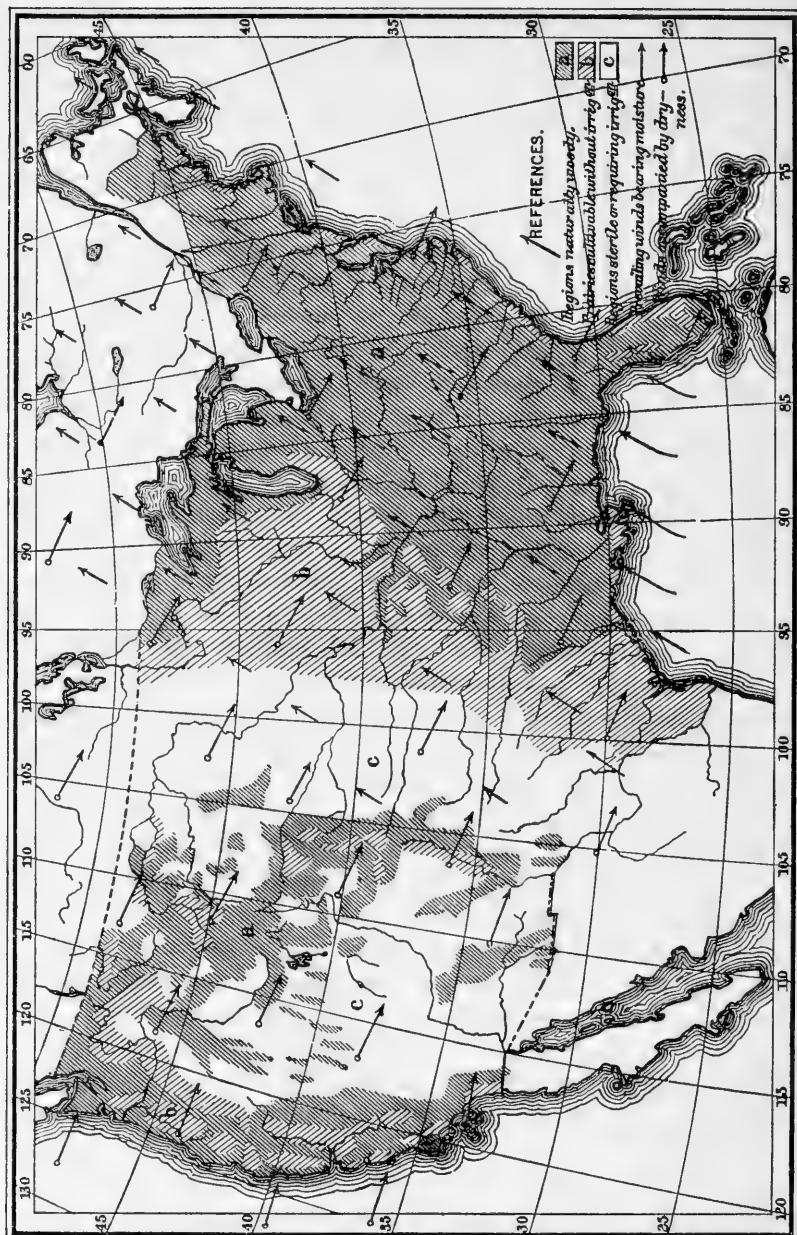
causes, which would bring the warm air of this stream to our shores, it is cooled by crossing the cold current we have mentioned, which reduces its temperature to the dew-point, and produces the peculiar chilly effect so familiar to the inhabitants of the Eastern States during the prevalence of a northeast storm: while on the Pacific coast the west winds from the ocean cross the comparatively cool current from the north and impart their mild and uniform temperature to the western slope of the Coast Range of mountains, giving rise to the remarkable fact of the summer temperature being the same for hundreds of miles in a north and south direction.

Were the whole of North America—from the Atlantic to the Pacific—a continuous plain, or were the surface diversified merely by eminences of comparatively small elevation, the moisture from the Pacific would be carried into the interior, and a much greater degree of fertility in the western portion of the Valley of the Mississippi would exist. In the actual condition of the continent however, the westerly wind which passes over the great mountain system extending from north to south along the western portion of the continent, deposits its moisture principally on the western slope of the Coast Range, and gives fertility and a mild climate to California, Oregon, Washington, and particularly to the regions farther north. The amount of rain which falls at Sitka, Russian America, amounts in some years to 60 inches. The remaining moisture which this westerly wind may contain is precipitated on the western slopes of the high ridges farther east, and when the current has passed over the whole Rocky Mountain system, it is almost entirely dessicated, and leaves the elevated plains east of the Rocky Mountains an arid region, so deficient in moisture as to be unfit for cultivation, unless by the aid of irrigation, with the exception of occasional oases, and along the borders of streams.

We have seen that two great systems of wind prevail over the United States, the upper from the northwest and the lower from the southwest. The latter carries the moisture

from the Gulf of Mexico and the Caribbean Sea over the whole of the Eastern States of the Union and the eastern part of the Valley of the Mississippi, and is therefore the principal fertilizing wind of the interior of the continent. Were the earth at rest this wind would flow directly northward, and would diffuse its vapor over the whole interior of the country to the base of the Rocky Mountains; but on account of the rotation of the earth it is thrown eastward, and bears its moisture in a northeasterly direction, leaving a large space, under the lee of the Rocky Mountains, (so to speak,) greatly deficient in this element of vegetable production.

These winds are shown on the accompanying map of the United States, which is copied in its principal features from a large map compiled by the Smithsonian Institution. In so small a sketch it is impossible to be accurate in the minute divisions; though it will serve to exhibit at a glance the relative proportions of the principal meteorological regions of the country. The northwest winds (those of the upper strata) are denoted by the heavier arrows with a circle on the end, and the lower ones—the surface or fertilizing winds—by the finer arrows. The dark portion of the map indicates the naturally woody regions of the country, well supplied as a whole with moisture from the fertilizing winds: the lighter shaded parts indicate rich arable prairie, along the streams of which, (where there is a local supply of vapor,) wood is found; but these districts as a whole have much less moisture than the naturally woody portions. The unshaded or white part of the map, within the boundary of the United States, indicates the regions so deficient in moisture that no dependence can be placed upon them for the purpose of agriculture. In some parts of them, where moisture is found, crops may be produced, but as a whole they are of little value in the way of affording the necessaries of human existence, and hence are incapable of sustaining other than a very sparse population. Portions of this unshaded part, on account of the nature of the soil, are barren and almost destitute of vegetation; while other parts, when occasionally watered by a fitful shower, yield patches of grass to which the buffalo by



his instinct is directed, but even these in the course of a few weeks are almost reduced to a powder by the drying influence of the unscreened rays of a powerful sun. What moisture rises from the evaporation of the rain which may fall on the regions indicated by the unshaded part of the map is constantly carried eastward instead of being precipitated again on the place whence it rose.

The direction of the several ridges of the Alleghany Mountains is parallel to that of the fertilizing wind, and hence these do not materially interrupt the southwestern currents, and are consequently sufficiently supplied with moisture, except in the more elevated valleys which are inclosed by a ridge at their southern extremities.

From the fact, abundantly proved by observation, that the vapor of the Pacific Ocean does not pass over the elevated crests of the Rocky Mountain system, it must be evident that the idea that the supply of the interior of the North American continent comes from the Southern Pacific by ascending to the cold regions of the top of the belt of rains is entirely untenable. The source from which the moisture of the interior is derived is principally the Gulf of Mexico. We shall endeavor to give in a subsequent Report an account of the climate of the several meteorological districts into which the United States may be divided; the remaining space allotted to this article will be devoted to a brief exposition of the storms of the Continent.

Storms of North America.—The two great systems of winds to which we have so frequently alluded as existing over the United States, present their meteorology in a simple form and on a very extended scale, while the general features of the phenomena of American storms are readily explicable on the principles of the theory propounded by Professor Espy. And first we may remark that on account of the height of the Rocky Mountain system, the storms or other commotions of the atmosphere which take place on its western side, are seldom if ever communicated to the air on the eastern; and this is a natural consequence of the principle which refers these commotions to the evolution of the

latent heat from portions of air charged with moisture. According to this view, an intervening region almost entirely without moisture will of necessity tend to intercept the progress of a storm, though it is not impossible that the drawing in of air on one side of a mountain of limited extent may cause a current across the mountain to supply the deficiency.

We think all the phenomena of the storms of the interior of this continent may be referred to disturbances in the equilibrium in the upper and lower strata of air. In the first place, all the disturbances of the atmosphere, however they may be produced, tend to move eastward over the United States, because this is the resultant motion of the great mass of current passing over the surface of this region. That the storms from the interior tend to move nearly east, with a velocity of from 20 to 30 miles an hour, is abundantly proved by the observations collected at the Smithsonian Institution, and the fact is interestingly and practically exhibited by means of the daily despatches gratuitously furnished this Institution by the Morse line of telegraph. These despatches are received every morning from the greater portion of the country east of the Mississippi River, and to render the information available in the way of predicting probable changes of the weather during the day or the following evening, a large map, containing merely the names of the places of observation, is attached to a wooden surface, into which, at each place, a projecting iron pin is driven. Small cards (previously provided) of about an inch in diameter, of different colors, to indicate rain, snow, clearness, and cloudiness, are attached to the map at the respective places of observation by means of the iron pins, and changed daily to correspond with the telegraphic despatches, so that an observer, at a glance, may see the condition of the weather at any portion of the country before mentioned. During the autumn, winter, and spring, if in the morning the visitor to the Institution observes a black patch indicating rain at Cincinnati, he may conclude that, in about twelve hours afterward, the same storm will reach Washington. Indeed so

uniformly has this been the case during the last year, that we have been enabled to decide whether it would be proper to advertise during the day the lecture to be given in the evening.

In summer it frequently happens that thunder storms commence their course at points intermediate between Cincinnati and Washington, and therefore it will not always follow that a clear sky in the morning at the former place will indicate a clear evening at the latter. But wherever the thunder storm commences it always moves eastward, or rather eastward inclining to the north, a direction which indicates that the direction of these circumscribed storms is principally governed by the motion of the lower stratum of air.

The extent of the interior storms, north and south, is exceedingly variable. In some cases a storm of not more than a hundred miles in width travels eastward along the lakes; and again at another time a storm of a similar width may commence at the south and move along the shore of the Gulf of Mexico. Again at other times the commotion appears to extend from some northern point in the British possessions, down to the Gulf of Mexico, and even farther south, and to move eastward, side foremost. In this motion the southern part of the storm first reaches the Atlantic Ocean, in the southeastern part of Georgia, and since the general trend of the coast is to the northeast, it is evident that the storm will appear to move from south to north along the coast, while in reality the whole system of disturbance is moving eastward, and will finally leave the continent at Newfoundland.

Another system of interior disturbances—which commence apparently at the south and confined principally to the eastern coast tends to draw in the air from the Gulf Stream along the surface, to be carried outward again by the upper current,—gives rise to our northeast storms. These are however in a great degree intercepted by the Alleghany Mountains, and do not extend very far into the interior. According to a suggestion of Dr. Hare, these storms are due to a

heating and rarefaction of the air in the Gulf of Mexico, as probably are also the "northers" which descend from the western plains.

Still another system of storms, originating in the Caribbean Sea and following the general direction of the Gulf Stream, sometimes sweep over the peninsula of Florida, and overlap somewhat upon the eastern coast of the United States. These are the great hurricanes,—(or cyclones as they are sometimes called,) the character and nature of which have given rise to so much discussion.

During the warm months of summer almost every part of the United States is occasionally visited with very violent though exceedingly circumscribed commotions of the atmosphere known, as tornadoes or water spouts. These generally move in nearly the same direction,—toward the northeast, except perhaps on the borders of the Gulf of Mexico, leaving their narrow path, sometimes only a few rods wide, marked with the evidence of energetic action of a most destructive intensity. The question naturally arises, is it possible in the present state of science to give a rational explanation of the various commotions (apparently fitful and complex and without an adequate cause) manifested in the light and invisible aerial covering of our globe? Can the question be answered? How is it possible that the soft and balmy air, which offers scarcely the least resistance to the motion of a lady's fan, can yet exert a power sufficient to level with the ground the largest trees of the forest in a single minute, to the number of 7,000 in the space of a square mile, and this devastating energy continue, as it has been known to do, for a distance of many miles?

The phenomena of these violent circumscribed storms, which appear peculiarly marked in America, have been investigated with much careful and laborious research by Franklin, Bache, Loomis, Olmsted, Hare, Redfield, Espy, and others. We owe to the lamented Professor Mitchell, of North Carolina, valuable suggestions in regard to the motions of the air in storms of this character. Professor Bache was the first to make an actual survey of the track of a tornado, and

to protract on a chart the relative position and direction of the prostrated trees and the lines described by bodies which had been moved by the force of the wind. Mr. Chappell-smith, of New Harmony, Indiana, has furnished the Smithsonian Institution with an account of a tornado and a map of its path, on which are delineated, from actual survey, the position and direction of several thousand trees. Professor Loomis has also minutely described the effects of a number of tornadoes, and has besides investigated with much care and extended research the phenomena of several large storms. He was the first to adopt the system of preparing a series of maps illustrating the phases of the storm at different periods.

The laborious observations of the lamented Mr. Redfield, particularly in regard to the hurricanes of the Atlantic Ocean, have intimately connected his name with the history of meteorology, while the theoretical expositions which have so long occupied the attention of Mr. Espy have done admirable service to the cause of the same branch of knowledge.

The controversial papers of Dr. Hare, bearing evidence of his great logical powers, served to give precision to the views of those engaged in these investigations, and thus to eliminate error as well as to advance the truth. In speaking of those who have given interesting expositions of the general facts of the meteorology of North America, we ought not to omit mentioning Mr. Robert Russell, of Scotland, who visited this country a few years ago, and who has since published a work on the agricultural resources of the United States and its meteorology, which is alike characterized by accuracy and sagacity of observation as well as by candor and justness of opinion.

The facts which have been gathered from the researches of those we have mentioned, as well as from other sources, ought to be sufficient to furnish an induction of the principles on which these phenomena depend; and although no theory at a given time in the history of a progressive science can be considered as perfect, yet we believe the general principles on which the disturbances we have mentioned depend have been successfully developed by Mr. Espy; and though

in subordinate particulars modifications will be required, yet we think the general propositions of his theory will stand the test of time.

As a general rule previous to the commencement of an extended storm (during winter), the surface current is from the southwest or some southerly direction, the temperature rises and the pressure of the air diminishes as indicated by the fall of the barometer. This state may continue for several days, and we think it is produced by the southerly current increasing in quantity, in velocity, and depth, thereby rendering the stratum of air next to the surface of the earth abnormally warm and moist, and consequently lighter, while the upper current remaining the same, the atmosphere above the surface of the earth gradually assumes a state of tottering equilibrium. This condition, according to Mr. Espy, is not brought about by the gradual diminution of the density of the lower stratum but by the increased density of the upper strata, due to the radiation into space of the latent heat which had been evolved during a previous storm. We think however that both causes are operative. This instability or tottering equilibrium will first take place at the far west, on the western plains east of the Rocky Mountains, since (as we have before said) the commotions on the western side can be but slowly propagated across the high mountain system. A storm then consists of the ascent of the lower current into the upper and the gradual transfer of the commotion of the air eastward. To take the simplest case, let us suppose the storm to be of circumscribed character, like that of a water spout or thunder storm. In this case after the unstable equilibrium has been produced, the slightest disturbance, such as the passage of the lower current over a slight elevation or over ground more highly heated than the adjoining will tend to establish an upward current. The light, warm and moist air below will be buoyed up with great rapidity and as it ascends will come under less pressure and will expand into a larger bulk. If it were perfectly dry it would again be in equilibrium, its bulk would be increased, its density would be diminished to that of the air to which it

had ascended, and its temperature would be the same as that of the surrounding stratum. But since it contains moisture and in expanding becomes colder, a portion of the vapor will be condensed, and in this condensation will give out its latent heat. Hence the air of the column will be warmer than that of the surrounding atmosphere; it will consequently rise to a greater height, again expand, again become colder; another portion of vapor will be condensed, and another amount of latent heat evolved, and thus the air will rush up with an accelerated velocity, and probably gather momentum sufficient to carry it to a height greater than that due to its buoyancy alone. The condensed vapor will fall in rain through the base of the cloud, the air on either side of the storm will be forced out from the uprising column into the surrounding air, and while the pressure at the base of the column will be diminished, that on each side will be increased, hence the barometer will be frequently found to rise slightly before the approach of a storm and to sink rapidly as the centre of the uprising column approaches the place of observation.

A series of observations has been made at the Smithsonian Institution to determine the variations of the barometer during the passage of thunder storms, and in every case in which observations of this kind have been obtained, a sudden fall has been observed in the barometer, and at the moment of the descent of the rain a slight elevation, followed again by a depression and then a rise, until the normal pressure of the day, or perhaps a little greater, has been obtained. The intermediate rise taking place at the moment of the fall of the rain may be properly attributed to the momentum of the drops as a sufficient cause.

Fig. 10 is intended to illustrate the conditions and phenomena of a commotion of this kind. The dotted space, *c d*, at the bottom represents the lighter atmosphere, consisting of the warm southwest current sur-charged with moisture; above this the parallel horizontal lines, *a b*, and the arrows, indicate the direction and position of the upper western current. The ascending column is represented by the upward turned arrows,

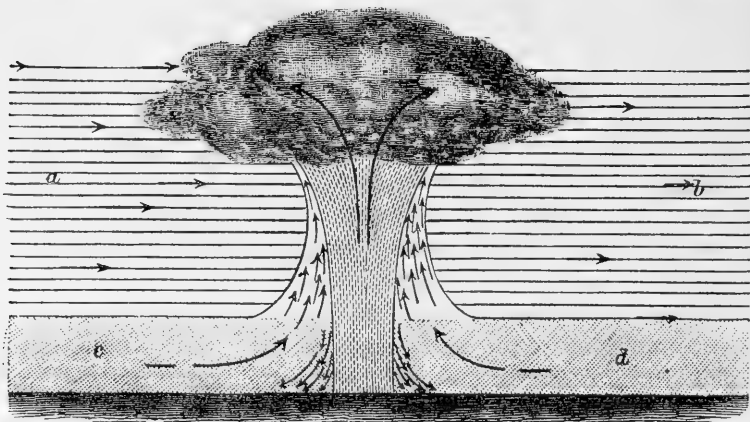


FIG. 10.

and the shaded portion above exhibits the cloud formed by the condensed vapor which is thrown outward on each side. The rain falling in the axis of the uprising column by its weight forces out the air in the direction of the arrows at the foot of the column.

When the air is saturated with moisture in warm weather, and especially when the sensation called closeness is observed, the rushing up of the column through a confined space may be so violent that drops of water may be carried up beyond the point of congelation and be converted into ice, and these will be thrown out on each side, exhibiting the phenomenon often observed in storms of this character, of two streaks of hail along the course of the tornado. In some cases these pieces of frozen water will be caught up by the inblowing air below and carried up again, perhaps several times in succession, each time receiving new accretions, and thus large hail stones will be formed exhibiting a concentric structure in which the centre will be of a light spongy consistency, and this succeeded by a stratum of transparent ice and this again by another stratum of snowy appearance, and so on, the outer surface being covered with large projecting crystals of solid ice. These facts are in strict accordance with what we might have predicted from the theory we have

adopted. If several large drops of water come in contact, and by their attraction rush into one larger drop, and if this be borne up so high that it begins to freeze, crystallization will commence at the surface, the air in the water will be driven inward as the solidification proceeds, and when the freezing is completed it will give a spongy appearance to the nucleus of the hail stone. As the hail stone is carried up a second time it will gather in its ascent another quantity of water which will again begin to freeze and produce the spongy envelope, inclosing the stratum between it and the coat of pure ice, surrounded by a stratum of solid ice, and so on. The number of concentric envelopes will indicate the number of times the hail stones have been carried up, and the collision of the stones in their ascent and descent will give rise to the peculiar noise which is heard during the passage of a storm of this kind.

The ascent of bodies in the centre of the up-moving column, and their being thrown out at the top, is not a mere matter of speculative inference, but rests upon direct observation. Bodies are seen to be carried up in the middle of the ascending column and thrown out as we have described; but above all Mr. Wise, the celebrated aeronaut, gives an account of what took place on the occasion of his balloon being drawn into the ascending column of a thunder storm. The balloon was carried up to a great height, thrown out on one side, sunk gradually down, was caught again by the in-blowing current which was rushing in to supply the column, again violently carried up, and again thrown out, and this several times in succession.

We have here, in accordance with the theory of Mr. Espy, a true, simple, and sufficient explanation of the production of hail, which takes place in the hottest and most sultry weather, when the air is most highly charged with moisture, and consequently when it contains the greatest amount of latent ascensional power. The vapor which ascends is derived from the moisture which a short time before existed at the surface of the earth, and since the ascending column usually carries up with it a quantity of fine dust, gravel,

pieces of leaves, &c., these are found in the nucleus of the hail stones.

In order that a storm of this kind may be attended with hail, it is necessary that it be of considerable violence, in order that the drops of water may be carried up to a sufficient height, and hence, as we have said before, this phenomenon occurs usually in the warmest and most sultry weather.

The writer is enabled to give the foregoing explanation of the nucleus and the alternate spongy layers of large hail stones from the effects he obtained by freezing water in a glass bulb. The freezing commenced at the exterior surface, to which the axes of the crystals were at right angles. The air contained in the water was forced in before the advancing crystallization, and formed at the centre of the globule a spongy mass precisely similar to that which formed the nucleus of the hail stone.

When the uprising column assumes the form of a tornado, it is more circumscribed, and is we think generally accompanied by a whirling motion. The power of the current however is in an upward direction. The gyration is an accidental circumstance, while the upward motion is an essential one; and the whole power of the tornado to produce mechanical effects is in this direction; hence as it passes along over the surface of the earth, the air flows in on every side to supply the up-moving column, trees are drawn in by the force of the centripetal current, and thrown with their tops towards the path of the tornado. The writer had an opportunity, on one occasion, of examining with Professor Bache the effects of a tornado after it had passed through an orchard. The trees were all prostrated in a strip of about four rods in width, with their tops inward toward the middle of the path. The whirling tends to contract the dimensions of the column, and to give it the peculiar appearance of an inverted cone descending from the clouds. The air which rushes into the revolving cylinder, charged with moisture, is immediately expanded, consequently cooled, and its vapor condensed into visible clouds, which gives rise to the peculiar appearance of the descending trunk.

The tremendous ascensional power which is exhibited in storms of this kind, although almost exceeding belief, is nevertheless in accordance with the established dynamical principle of the accumulation of momentum in cases of the continued action of a constant force. We are all familiar with the velocity given to an arrow by a simple propulsion of the breath along the interior of a blow-gun. In this case the air presses against the end of the arrow, at first with just sufficient force to move it; but the momentum it has thus acquired is retained, it receives another pressure from the air, retains the effect of this, and so on, until it leaves the other end of the tube with the accumulated momentum acquired during its whole passage through the interior of the gun. In the same way the air, as it approaches the uprising column below, commences its ascent with an amount of momentum which is constantly increased by continued pressure from behind. The ascensional momentum therefore becomes so great as to furnish a ready explanation for all the exhibition of mechanical power which is so frequently witnessed in storms of this character in our climate. On account of the rarefaction of the air in the centre of the storm in cases where it has passed directly over head, buildings are instantly unroofed, the sides are thrown outward, as if by the action of gunpowder, chests are broken open, and corks forced from empty bottles, in which they have been tightly fitted. In these cases the outward pressure being in part removed, the unbalanced repulsive energy of the atoms of the air within the edifice causes the outward explosion. The force of this outward tendency will not be surprising when we reflect upon the great pressure of the atmosphere in its normal state, which is equal to more than 2,000 pounds on every square foot of surface, and which frequently and suddenly experiences a reduction of a twentieth part of at least this amount, or in other words, of 100 pounds to the square foot—an unbalanced force abundantly sufficient to produce the effects we have mentioned.

Dr. Hare attributed the violent upward motion of the air in tornadoes to a peculiar electrical state of the atmosphere

in which, while the air was highly positive, the earth was negative, and the bodies carried up were repelled from the earth and attracted by the cloud, as in the case of the dancing figures between the two plates, one of which is connected with the prime conductor of an electrical machine and the other with the earth. We think however with Mr. Espy that electricity is altogether a collateral result,—an effect of the storm and not its cause; it is probable however that its presence tends to modify the appearance and produce phenomena of a subordinate character. It is well known that when a kite to which is attached a metallic string is sent up to a considerable height above the earth, the wire becomes highly charged with electricity, even in a clear day when not a cloud is visible; this effect is due to what is called induction. The positive electricity of the upper atmosphere drives the natural electricity of the wire from its top to its bottom, hence the upper end of the wire will be negative and the lower end positive; a similar effect must be produced on the cloud formed by the uprising column and on the column itself, the two form a continuous conductor of immense height, and hence like the wire must become charged at the lower end with positive electricity of great intensity, which will tend to elongate the trunk downwards by repulsion, and which will give occasional discharges to the earth as the tornado passes over good conducting substances.

The terrific and appalling grandeur of the tornado strikes the beholder with astonishment and awe, now pausing fitfully as if to select with malignant caprice the objects of its unsparing fury, now descending to the earth, and again drawing itself up, with its deep, loud, and sullen roar; its mysterious darkness; its apparent self-moving, resistless revolutions; carrying upwards branches of trees, beams of houses, and large objects of every description; its impetuous downward rush to the earth, and then again up to the sky, its sublime altitude, sometimes erect and at other times inclined; its reeling and sweeping movements; all these and more to be adequately conceived must be actually witnessed.

The thunder storm differs from the tornado in its less concentration, and consequently in the less intensity of its violence. It occurs usually in the United States in the after part of a sultry day, when the air has attained its maximum amount of vapor, and has therefore assumed a condition of unstable equilibrium. These storms are usually produced over a considerable extent of country on the same day, and occur nearly at the same hour for several days in succession, and probably serve to restore a more stable equilibrium to the air, and thus perform the office of the great winter storms which sometimes regularly succeed each other at given intervals. Their general course is eastward, but they sometimes deviate from this direction to a certain extent, apparently on account of the attraction of water courses; they partially exhaust, carry up and precipitate the moisture of the atmosphere, but sometimes leave the air immediately afterwards in a sultry condition. We hope to be able to give in another article an exposition of the electrical phenomena exhibited by thunder storms, but we may mention here the fact of the almost instantaneous fall of rain after each peal of thunder. It has been supposed that the drops of rain in this case were produced by the agitation of the discharge of lightning; but a little reflection will render it evident that the rain must have commenced its rapid descent before the discharge took place, since it follows the flash at so short an interval that we must suppose that it commenced to fall previous and not subsequent to the discharge. It is more probable that the fall of rain, on account of offering a conducting medium for the electricity, is the cause and not the consequence of the discharge in question.

The great interior storms we have mentioned usually commence at the Far West, even at the base of the Rocky Mountains, and generally occur in November, December, January, February and March. They are sometimes of great extent in a north and south direction. One of these storms, that of 1836, which was investigated with so much ability by Professor Loomis, reached from the Gulf of Mexico to unknown regions in the north. They are of varying breadth, some-

times several hundred miles across, and the cloudiness produced frequently overspreads simultaneously a considerable portion of the eastern part of the United States.

In common with nearly all the commotions of the atmosphere on the North American continent, they move eastward, at the rate sometimes of thirty-five miles an hour. In some rare instances the horizontal axis of the storm in a north and south direction is nearly a continuous straight line, and moves side foremost toward the east, in the form of an immense wave, or rather undulation. The pressure on the middle of this wave, on account of the uprising air, is less than the normal pressure of the atmosphere, while on either side, and particularly on the east, it is greater.

This pressure on the front and rear of the storm is due to the spreading out above of the air which has been carried up in the ascending current, and is greater on the east side of the storm on account of the action of the westerly current in which the whole commotion is carried forward. The approach of the storm is therefore generally indicated by a rise of the barometer, which is succeeded by a subsequent fall, and also by an increase of temperature due to the radiation from above of the latent heat evolved, and also by the increased pressure of the air forced out above. Sometimes the horizontal axis of the storm is curved, and again, which is of more frequent occurrence, broken up into a number of separate parts, forming altogether a system of which the several portions slightly vary in direction and velocity in their motion to the east.

These great storms, though of the same general nature as the thunder storm, are attended with an entire subversion of the upper and lower strata of the atmospheric ocean. After one of them has swept over the continent the commotion is immediately succeeded by a westerly wind, a great reduction of temperature, and a great increase in the degree of dryness of the air. We have endeavored to give an idea of the motions of the strata of the atmosphere accompanying these changes in Fig. 11, which exhibits an imaginary section of the currents in an east and west direction.

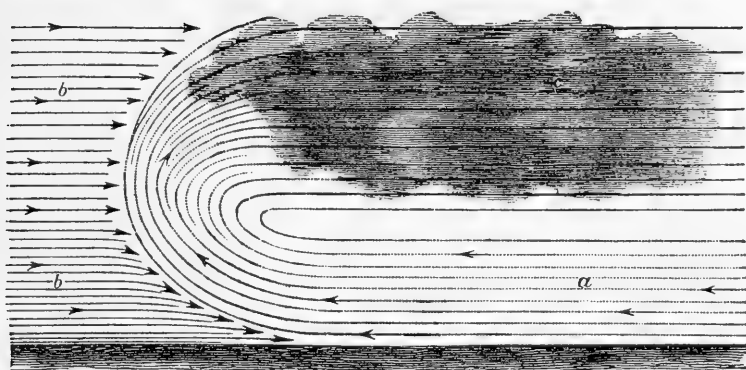


FIG. 11.

Previous to the commencement of the storm there exists over the surface of the United States a lower stratum of air moving from southern points of the horizon, and over this at an elevation of two or three miles the constant current from the west continues its habitual and un-interrupted course. The lower stratum, coming principally from the Gulf of Mexico, is abnormally warm, moist, and light, while the super-stratum is in its usual condition, and the whole is therefore in a state of unstable equilibrium. At the far west this lower stratum begins to be invaded by the denser air from the polar current, which coming from the northwest and mingling with the upper current, presses under, and turning the moist current upward produces an ascending column, or rather wall, which mingling with the upper current is carried rapidly to the east. The upper current is continued with varying energy, and by the condensation of the vapor from the lower, forms clouds and rain, which are carried in advance to the east, as the whole system of disturbance is borne in the same direction by the ordinary eastward flow of the upper current of the aerial ocean. The primary lower current is shown in the figure by the stratum *a*; the upper current, which has filled the whole space on the west down to the earth, by *bb*; the portion of the primary upper current into which the stratum *a* has ascended

and in which its vapor has been condensed into clouds and rain is represented by *c*.

As the storm advances eastward, it leaves the country behind it entirely covered with the westerly current, and in this way carries before it to the ocean the greater portion of the vapor with which the lower stratum, previous to the commencement of the storm, was saturated. The rain which falls at any given place is formed of the condensed moisture which a few hours previous existed at the surface of the earth in the same spot or its vicinity.

We have represented in Fig. 11 the whole cloudiness thrown eastward in advance of the storm, but in some cases, with a more energetic upward motion, a part of the ascending air will be thrown out to the west above; but this can scarcely ever take place to the same extent as on the eastern side. After the upward moving column has passed over a given place, the wind which was previously from the east, will suddenly change to the west, the sky will become clear and a great reduction of temperature follow. The whole effect then is due to the instable equilibrium produced in the air by the introduction of moisture and the accompanying elevation of temperature, together with the subsequent evolution of the latent heat. A similar condition of the atmosphere preparatory to the formation of another storm will gradually be re-produced. The westerly wind will again be buoyed up by the warm air from the south, it will therefore disappear at the surface of the earth, at which a calm will at first exist, the southerly wind will increase in velocity, the thermometer and hygrometer will indicate a higher temperature and increasing amount of vapor, the barometer will fall, and after a given interval another instable equilibrium will be produced, to be followed by another subversion of the strata of the aerial ocean and the repetition of all the previous phenomena. The intervals between two successive storms will also depend on the time of radiation into celestial space of the evolved heat, in order to reduce the upper stratum to its normal condition of temperature and density; but the time required to produce these effects is frequently

in winter very nearly the same for several successive periods. For example, most persons can remember the successive occurrence of a series of storms on Sundays. In one case we recollect this to have taken place six times in succession. There is nothing in this particular day to induce the occurrence of a storm, but merely it will be more likely to be remembered when it happens at this time; and although the interval between two storms may not be precisely seven days, yet it may differ so little from this that a part of the first and sixth Sundays may be included in the cycles of disturbance.

The wind as a general rule tends to flow towards the axis of the storm from each side, but at the surface of the earth, diversified with hills and valleys, the direction is far from being as regular as at first sight might be expected. Besides this, since the commotion of the atmosphere is usually divided into a number of separate groups—each having a separate ascending column or belt to which the in-blowing air is directed,—the arrows on the map indicating the direction of the winds generally present a very complex system of currents. On this account also, the rain does not simultaneously fall along an extended line from east to west but in separate places, the position of which is determined probably by the greater amount of moisture, and consequently the more intense action of the ascending current. As the storm approaches the eastern part of the United States however the in-blowing air to supply the up-moving current draws in the air from the ocean, charged with moisture; which being constantly supplied, the action may continue for several days, and the storm may perhaps become stationary, giving rise to prolonged easterly currents.

It would appear however from observations at the Smithsonian Institution, that the northeast storms are produced by the rarefaction of air on the east side of the Alleghany Mountains, being frequently independent of a previous interior storm from the west.

A considerable number of storms has been mapped in accordance with the plan first adopted by Professor Loomis,

exhibiting on the successive maps by colors the positions and movements of the lines of equal pressure and of equal temperature. We have not been able to find however (except in very rare cases) the advance of the storm side foremost in a continuous line. The conditions presented are similar to those we have described, namely a series of centres of commotion advancing eastward.

The storms next to be noticed are those spoken of as hurricanes, or cyclones, the true character or nature of which has given rise to much discussion between the advocates of the two rival theories, of an entirely horizontal gyratory motion of the wind on the one hand, and an in-blowing to a central area and upward motion of the air on the other.

Much of this discussion undoubtedly arose from the want of precision in the earlier conceptions of the motions of the air when referred to the surface of the earth, as in the case of a gyration, and in many cases to the ambiguity of the language in which these views were expressed. While Reid and Piddington supposed the motion of the wind to be in concentric continuous circles, and Mr. Espy at first in direct radial lines towards the centre, Mr. Redfield finally adopted an intermediate view, namely of a spiral inward motion. We are entirely convinced from the observations which have been collected at the Smithsonian Institution in regard to the large interior storms that they are not rotatory, and that when the gyrations do take place, (as they must in some cases on account of the in-blowing currents from all directions not exactly opposing each other,) the gyration is a secondary motion, the principal force being exerted in an upward direction. We are unable to conceive of any adequate cause of the great and continued velocity of the air in a circle of several hundred miles in diameter except that which is due to the heat evolved by the condensation of the vapor with which this portion of the atmosphere is saturated. This appears to be the true and sufficient source of the great motive power, and to afford (when connected with the rotation of the earth) a complete explanation of all the phenomena. These storms as we have said commence in

the Caribbean Sea, and describe a curve on the surface of the earth almost precisely the same as that which would be exhibited by the projection on a horizontal surface of the path described by an atom of air in its ascent at the equator, in its passage westward, and in gradually curving round toward the east. Mr. Redfield has shown that these curves in whatever longitude of the northern hemisphere the hurricanes have occurred, are of precisely the same character.

If it be admitted that the motive power of this violent commotion of the atmosphere is due to the evolved heat of the moisture of the air, it will follow that such storms will be most frequent and of greatest intensity in portions of the earth where the relative amount of moisture is greatest, and that they will therefore be found in the greatest number in the heated and moist air directly over the Gulf Stream. The atmosphere over this area must be in the highest degree in a state of tottering equilibrium, since the air rising from the heated surface along the axis of the stream must be much more highly charged with moisture than that on either side. Observation and theory are here in accord.

These storms sometimes overlap the eastern coast of the United States and produce great destruction of property along the seaboard, and frequently a loss of life and shipping in the region of the Gulf Stream.

Hurricanes of the same character are found in the southern hemisphere, describing similar curves, which turn south however from the equator round to the east, in an opposite direction to that of the curves described by the hurricanes of the northern hemisphere. The space to which we are limited in this article precludes a more minute discussion of the phenomena which have been observed, and the opinions which have been adopted, in regard to these storms. We may have an opportunity of resuming the subject on some other occasion.

In this paper we have endeavored to give an exposition of the general principles of the meteorology of the United States, reserving for a future report a more detailed account of the climatology of its different portions. We have especially

endeavored to exhibit our views of the theory of Professor Espy and to show its applicability to the explanation and in some cases to the prediction of the great commotions of the atmosphere. We think this theory has not received the attention from foreign meteorologists which its merits demand, and this perhaps has arisen from the fact that it has not been presented to the public in a form which would commend it to the immediate attention of scientists. It has been frequently coupled with propositions for the artificial and economical production of rain, which—however well based on scientific principles—would be too uncertain and too expensive to render them of any value in a practical point of view: and it must be confessed that the language of Mr. Espy in regard to the proofs of the truth of his theory, and of its great value as a scientific generalization, has occasionally been such as to awaken opposition to it rather than to secure its approval and final adoption.*

[* Fifty-six pages of Meteorological Tables following this part are omitted in the present re-print.]

METEOROLOGY IN ITS CONNECTION WITH AGRICULTURE.

PART V.—ATMOSPHERIC ELECTRICITY.

(Agricultural Report of Commissioner of Patents, for 1859, pp. 461-524.)

In this paper we intend to give a sketch of the general principles of atmospheric electricity;—a branch of meteorology which has attracted in all ages more attention, and has been regarded with more interest, than perhaps any other.

The vast accumulation of electricity in the thunder cloud, and the energy exhibited in its mechanical, chemical, and physical effects, have impressed the popular mind with the idea of the great efficiency of this agent in producing atmospheric changes, and have led to views of its character not warranted by cautious induction. It is frequently considered sufficient in the explanation of an unusual phenomenon to refer it simply to electricity. References of this kind however are by no means satisfactory, since the scientific explanation of a phenomenon consists in the logical reference of it to a general law; or in clearly exhibiting the steps by which it can be deduced from an established principle. Electricity is subject to laws as definite and invariable as those which govern the mechanical motions of the planetary system. In one respect indeed, there is a great similarity between them, and it will be seen in the discussion of electrical phenomena, that these are referable to forces similar in action to that of gravitation; and that the mathematical propositions which were demonstrated by Newton in regard to the latter, have been applied with admirable precision to represent those of the former.

In giving a general exposition of a subject of this kind, two plans may be adopted: either a series of facts may be stated, and from these a theory gradually developed by a careful induction, or we may begin with the general principles or laws which have been discovered, and from these deduce the facts in a series of logical consequences. The first method is called induction, the second, deduction; and they

are sometimes known by the more scholastic names of analysis and synthesis. The first method may perhaps be considered the more rigid, and where a systematic treatise on a subject is intended, and ample space allowed for its full discussion it might be preferred; but where the object is to give the greatest amount of information in the shortest time, to put the reader in possession of the means through which by his own reflection he can deduce from a single principle hundreds of phenomena, and declare—prior to experiment or observation, what will take place under given conditions, the latter method will be the proper one to be adopted.

It is impossible however to state a principle of very general application without employing an hypothesis or an assumption which though founded on strict analogy may possibly not be absolutely true. We adopt such an hypothesis temporarily, not as expressing an actual entity, but as a provisional truth which may be modified or even abandoned when we find it no longer capable of expressing all the phenomena. All we assert positively in regard to such an hypothesis is that the phenomena to which it relates and with which we are acquainted at the time exhibit themselves as if it were true.

When an assumed hypothesis of this kind furnishes an exact expression of a large number of phenomena, and enables us beforehand to calculate the time and form of their occurrence, it is then called a theory. The two terms—hypothesis and theory—though in a strict scientific sense of very different signification, are however often confounded and otherwise mis-applied. *Theory*, in common language, is frequently used in contradistinction to *fact*, and sometimes employed to express unscientific and indefinite speculations. The cause of truth would be subserved if these terms were used in a more definite and less general sense; for example, if the term *speculation* were restricted to those products of the imagination which may or may not have an existence in nature; the term *hypothesis* to suppositions founded on analogy and which serve to give more definite conceptions of laws; while the term *theory* is reserved for generalizations

which although presented in the language of hypothesis, yet really furnish the exact expression of a large class of facts.

Hypotheses—well conceived and properly conditioned by strict analogy, not only enable us, as above stated, to embrace at one view a wider range of phenomena, but also assist us in passing from the known to the unknown. When rightly used they are the great instruments of discovery, giving definite direction as to the experiments or observations desirable in a particular investigation, and thus marking out the line of research to be pursued in our endeavors to enlarge the bounds of the science of our day. We think that the tendency of some minds, instead of being too speculative is too positive; and while on the one hand there is too much of loose, indefinite, and consequently of useless speculation intruded upon science, on the other hand an evil of an opposite kind is frequently produced by attempting to express scientific generalizations of a complex character without the aid of proper hypotheses; and to this cause we would principally ascribe the looseness of conception which frequently exists in well-educated minds as to the connection and character of physical phenomena.

In accordance with the foregoing remarks we shall make use of a theory to express the well-established principles of electrical action, and from this endeavor to deduce such conclusions as are in strict conformity with the observed phenomena. The intelligent reader who attentively studies this theory, and exercises his reasoning faculties in drawing conclusions from it, will be able not only to explain many remarkable appearances which would otherwise be entirely isolated, but also to anticipate results, and to adopt means to prevent unpleasant occurrences or to ward off dangers.

The theory which we shall adopt is that invented by Franklin, and extended and improved by Epinus and Cavendish. It is sometimes called the theory of one fluid, in contradistinction to the theory of Dufay, of two fluids. The two theories however do not differ so much as at first sight might be supposed, and when expressed mathematically are essentially the same.

No part of the writings of Franklin exhibits his sagacity and his power of scientific generalization in a more conspicuous light than his theory of electricity. The talent to discover isolated facts in any branch of science, although possessed by few, is comparatively inferior to that characteristic of mind which leads to the invention of an hypothesis embracing in a few simple propositions whole classes of complete phenomena.

Theory of Electricity.

According to the theory of Franklin all the facts of ordinary electricity may be referred to the action of a subtle fluid, which perhaps fills all inter-planetary space, and may be the medium of light and heat. In order that the phenomena of electricity may be represented by the mechanical actions of this fluid, it is necessary to suppose that it is endowed with certain properties and relations which may be expressed in the following series of postulates:

1st. The electric fluid (or æther) consists of atoms so minute as to exist between the atoms of gross matter. •

2d. The atoms of the fluid repel each other with a force varying inversely as the square of the distance; that is, when the distances are 1, 2, 3, 4, 5, &c., the forces are 1, $\frac{1}{4}$, $\frac{1}{9}$, $\frac{1}{16}$, $\frac{1}{25}$, &c.

3d. The atoms of the fluid attract the atoms of ordinary matter with a force also varying inversely as the square of the distance.

4th. The atoms of gross matter devoid of electricity tend to repel each other also with a force inversely as the square of the distance.

5th. The atoms of the fluid can move freely through certain bodies of gross matter, such as metals, water, &c., which are hence called conductors, and cannot move, or but very imperfectly, through other bodies, such as glass, baked wood, dry air, &c., which are called non-conductors.

6th. When each equal portion of space has the same amount of electricity, and each body in it has so much of the same fluid as to neutralize the attractions and repulsions of the

matter, there are no indications of electrical action; and when the attractions and repulsions are thus neutralized a body is said to be in its natural condition.

7th. The electrical equilibrium may be disturbed by friction, chemical action, change of temperature, &c., or in other words (by these and other processes) the fluid may be accumulated in one portion of space, and rendered deficient in another, and in this case electrical action is exhibited.

8th. The phenomena are of two classes, namely statical, or those of attraction and repulsion, in which the electricity is at rest, and dynamical, or those in which the redundant electricity of one portion of space is precipitated into that of another in which there is a deficiency.

9th. When the electrical equilibrium has been disturbed and a body contains more than its share of electricity, it is said to be positively charged; and when it contains less, it is said to be negatively charged or electrified.

The fourth proposition of this theory was added by Cavendish, in England, and by Epinus, in Germany, and was found to be necessary in order to render the several parts of the theory (as given by Franklin) logically consistent with each other. At first sight it appears to be contrary to the general fact of the mutual attraction of all bodies, but it must be observed that when gross matter exhibits attraction it is in its normal condition, and that since the electrical force is infinitely more intense than that of gravitation the latter may be a residual phenomenon of the former.

According to this theory, there are two kinds of matter in the universe,—ætherial or electrical matter, and gross (or as it is frequently called by way of distinction,) “ponderable” matter. The two however may have the same essence, and differ from each other only in the aggregation of the atoms of the latter; or what we call gross matter may be (as suggested by Newton,) but a segregation or kind of crystallization of the ætherial matter in definite masses. Each kind of matter is in itself entirely inert, has no power of spontaneous change of place, and is equally subject to the laws of force and motion. A mass of ordinary “ponderable” matter,

when once at rest, tends to continue at rest until put in motion by some extraneous force; so also the electrical fluid, when at rest, tends to remain at rest, and only moves in obedience to some impulse from without. From this theoretical inference, which is in accordance with all observation it is an error to suppose that electricity is an ultimate power of nature, being in itself the cause of motion. Like the air, it is inert, and has no more tendency to spontaneous motion than this or any other fluid which may receive and transmit impulses, or which may have its equilibrium disturbed, and in the restoration of this equilibrium, give rise to motion and produce mechanical effects.

Perhaps some currency is given to the idea that electricity is not subject to the mechanical laws which govern the actions of gross matter, because it is called an "imponderable" agent, and has thus assigned to it a kind of semi-spiritual character. The term "imponderable," though convenient, is not properly applied, since it indicates a distinction which may possibly not exist. If electricity is in reality a fluid, it might exhibit weight, could it be so isolated and condensed as to become sensible to our balances. But whatever may be its nature, the phenomena which it exhibits can be referred to mechanical laws; and it is in order that such a reference may be definitely made, that the hypothesis of a fluid is adopted. For a similar reason the phenomena of light and radiant heat are referred to the vibrations of the ætherial medium, and it is in this way that the laws of motion which have been deduced from the study of gross matter have been so successfully applied to them, and it is only so far as the facts of what are called the "imponderable" agents are brought under the category of mechanical laws that they take the definite form which entitles them to the name of science.

Theoretical Deductions and Illustrations.—We do not intend to develop from the theory we have presented a complete system of electricity, but to give such deductions from it as will put the intelligent reader in possession of the principal known facts of atmospheric electricity, and particularly those which relate to thunder storms.

In the first place, if the ætherial medium in its ordinary state of diffusion fills all space, then it must be evident that when a body is charged with more than its natural share, a portion must be drawn from space around, and hence what one body gains other bodies in the vicinity must lose, or in other words there must always be as much negative excitement as positive. To exhibit this, as well as to illustrate some of the effects of the disturbance of the electrical equilibrium, provide two strips of glass an inch in width and twelve inches long, and on the end of one of these fasten with beeswax or sealing-wax a piece of woollen cloth about an inch

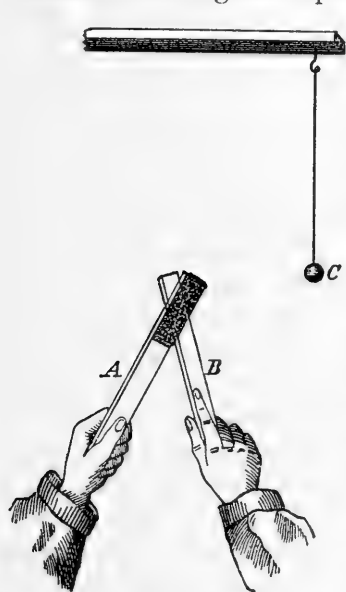


FIG. 1.

and a half long; if the glass slips are warmed and rubbed together, as shown in Figure 1, and afterwards separated, they will exhibit signs of electricity. If the strip of glass of which the end is naked be brought near a pith-ball *C*, suspended by a single fibre of non-conducting silk, so that the electricity which may be communicated to the ball cannot escape, the ball will be attracted, and immediately afterwards repelled. If now the end of the other glass having the woollen cloth on it be brought near to the same ball, attraction will take place at a considerable distance.

The one slip of glass will constantly attract, while the other will as constantly repel the ball. If however the two glasses be placed in contact as they were when first rubbed, and thus presented to the ball, neither attraction nor repulsion will be exhibited.

These results are in strict accordance with the theory we have adopted. By rubbing the glass and woollen cloth against each other the electrical equilibrium is disturbed—

a portion of the natural electricity of the cloth is transferred to the glass; the latter receives a positive charge of electricity, while the woollen cloth loses a portion of its natural share of the fluid, and assumes the negative state; and since the slips of glass, as well as the surrounding air, are non-conductors, the redundancy of the one cannot escape, nor the deficiency of the other be supplied, and therefore the charged condition of each will continue for a considerable time, particularly if the air be perfectly dry.

When the glass plate is made to touch the ball a portion of electricity accumulated on the surface of the former is transferred to the latter, which has then more than its natural share; and since atoms of free electricity repel each other, the ball will apparently be repelled from the glass; and also because there is an attraction between free electricity and un-saturated matter, the cloth which is in this condition will attract the same ball. When the two slips of glass are brought together and presented as a whole the attractions and repulsions may still be considered as existing, but since they are equal and opposed they entirely neutralize each other, and no external effect is perceptible.

The neutralization of the two opposite forces in this experiment affords an illustration of the condition of a body in its natural state. Although it contains a large amount of the fluid no action is produced on other bodies in their natural condition because the attractions and repulsions just balance each other.

For exhibiting the most important statical phenomena of electricity, and for verifying the deductions from the theory, we may employ a solid glass rod of about fifteen inches in length, and a rod of sealing-wax or of gum shellac of the same length. If these be well dried, held by one end and rubbed with a piece of woollen cloth at the other, electrical excitement will be produced. Instead of a solid glass rod a tube may be employed, provided the interior be perfectly dry, and well corked to prevent the access of moisture. If the end of the tube or rod be rubbed, and afterwards brought into contact with a small ball of pith, or of any light con-

ducting matter, suspended by a silk thread, the excitement will be communicated to the ball, and if the communication be from the glass rod the electricity will be that denominated positive; if from the rod of sealing-wax or shellac, it will be what is called negative. Since the phenomena exhibited by balls charged negatively and positively are very nearly the same, it is not of much consequence which we call the positive or which the negative, provided we always apply the same name to the same kind of excitement. In the early discovery of the two kinds of electrical excitement, that which was produced by rubbing glass with a woollen cloth was called *vitreous*, and that from the friction of the same substance on sealing-wax or gum shellac was denominated *resinous*, and these terms are still retained, particularly in foreign works on the subject.

The simplest instrument for exhibiting the attraction and repulsion of electrified bodies, and determining the intensity and character of the excitement, is the gold leaf electrometer, or electroscope, which any person with a little patience and some mechanical skill may construct for himself. Different forms of this instrument are exhibited in Figures 2, 3, and 8.

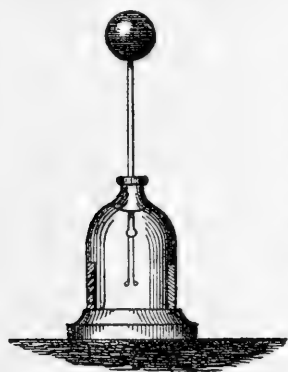


FIG. 2.

A brass wire, surmounted by a ball of the same metal, is passed through the cork of a small glass jar, or a large-sized vial, from which the bottom has been removed and its place supplied by a disc of wood; and to the lower end of the wire, which may be slightly flattened, is attached, by means of any adhering substance, two narrow strips of gold leaf so as to hang freely, and when un-excited parallel to each

other without touching. Two small pith balls suspended close together by threads a few inches in length may be employed, in place of the gold-leaf strips.

When we wish to ascertain if a body is electrified, or whether different parts of it are charged positively to the

same degree for example, we bring in contact with the part to be examined a small metallic ball suspended at the end of a very fine silk thread, (a fibre from a cocoon will serve for this purpose,) and afterwards bring the small ball, which may be called the carrier, in contact with the ball, or as it is called, the knob of the electroscope. The electricity of the carrier will distribute itself, on account of the repulsion of its atoms, throughout the knob, the stem, and the leaves of the electroscope. The leaves being the only movable part will diverge from each other, and will thus exhibit the electrical repulsion to the eye. We see from this experiment, as well as from that of the ball touched with the excited glass, that electricity may be transferred from one body to another, and that when it is applied to the end of an elongated metallic conductor it instantly diffuses itself over the whole mass. In the experiment we have just described, the body was supposed to have been *positively* electrified; but a similar effect would have been produced had it been negatively charged. In that case, a portion of the natural electricity of the carrying ball would have been drawn from it by the un-saturated matter of the electrified body, and the ball in turn, when brought in contact with the upper end of the electroscope, would draw from it a portion of its natural electricity—the deficiency extending to the leaves—which would therefore diverge, since according to the theory un-saturated matter repels un-saturated matter.

If we wish to ascertain whether a body is electrified negatively or positively, we transfer a portion of its charge to the electroscope by means of the carrying ball, and then, having rubbed a rod of glass with a piece of woollen cloth, we bring it near to the electroscope; if the leaves diverge farther when the rod of glass is brought near, the original charge is of positive or *plus* electricity; if on the contrary the leaves converge, we may consider the electricity as negative or *minus*; or the same conclusion may be arrived at by rubbing a stick of sealing-wax with the woollen cloth, which becoming negatively excited will cause the leaves in the case of a positive charge to converge, and in that of a negative charge to diverge.

Conduction and Insulation of Electricity.—By means of a simple electroscope of the kind we have just described we may at once determine whether a body is a conductor or non-conductor of electricity. If a slight charge be given to the electroscope, (which may be effected by touching the knob with a rod which has been rubbed by woollen cloth,) the charge will remain with but little diminution for several hours, provided the air is perfectly dry; while if the air is moist, the charge is soon dissipated. These facts show that the former is a non-conductor, and the latter a partial conductor. Dry air would be a perfect insulator of electricity, provided it were motionless; the atoms which impinge against a charged body however become electrified with the same kind of excitement, and are consequently repelled, their place being supplied by others and so on until the charge is gradually diminished and finally dissipated.

If, when the electrometer is charged in dry air, we touch the knob with a glass rod, the leaves will be but little affected; but if we breathe on the surface of the rod, the glass will become a partial conductor and the leaves will slowly converge. If the ball be touched with one end of a metallic wire, the electricity will instantly be conducted off. If we make a similar experiment with a piece of dry wood, the charge will be gradually dissipated, a fact which indicates that wood is a partial conductor. By increasing the length of an imperfect conductor we shall find that the time of drawing off the charge is increased, and in this way it may be shown that there are very few bodies which are perfect conductors or non-conductors; that every body offers some resistance to the passage of an electrical current, provided we increase the length sufficiently to make it perceptible. By experimenting on various bodies in the way we have described, we may form an approximate table of the degrees in which different substances are conductors or non-conductors of electricity. The human body is a very perfect conductor of ordinary electricity, since if we touch the knob of the electroscope with the finger, the leaves instantly collapse, provided we are standing on the ground at the time. If

however we place a non-conductor (for example a cake of bees-wax) under the feet, the whole of the charge will probably not be withdrawn but shared with the body, and the leaves will only partially converge. It may also be shown by the same instrument that in order to produce electrical excitement by friction, it is only necessary that two dissimilar substances be rubbed together, one at least of which must be a partial conductor. For example, if while a person is standing on a cake of bees-wax he place one finger on the knob of an electroscope and another person strike him on the back with a silk handkerchief, the leaves will instantly diverge, showing that the whole body has received a charge of electricity, which is prevented from escaping into the floor by the interposed non-conducting bees-wax.

After the introduction of furnaces for heating rooms by warm air, the public was surprised at exhibitions of electrical excitement which previously had not been generally observed. If our shoes be very dry and we move over the surface of the carpet with a shuffling motion on a very cold day, (particularly in a room heated by a furnace,) the friction will charge the body to such a degree that a spark may be drawn from the finger, and under favorable circumstances a jet of gas from a burner may be thus ignited. There is nothing new or wonderful in this experiment; it is simply an exhibition of the production of electricity by friction, which only requires the carpet, the shoes, and the air to be dry, conditions most perfectly fulfilled on a day in which the moisture of the air has been precipitated by external cold and its dryness increased by its passage through the flues of the furnace. In the ordinary state of the atmosphere, the electricity which is evolved by friction is dissipated as rapidly as it is developed, but in very cold weather the non-conducting or insulating power of the air is so much increased that the electricity which is excited by the almost constant rubbing of bodies on each other, is rendered perceptible. Every person is familiar with the fact that on removing clothes, or shaking garments in cold dry weather, the electricity evolved by the rubbing exhibits itself in sparks and flashes of light.

The popular idea in regard to this is that the atmosphere at such times contains more electricity than at others; but these appearances are not due to the variation of the electricity in the atmosphere, but simply to the less amount of vapor which is present. When the clothes are rubbed together one part becomes positive and the other negative, and in dry air the excitement increases to such an intensity that the restoration of the equilibrium takes place by a visible spark; but when the air is moist, the equilibrium is silently restored as soon as it is disturbed, and no excitation is perceptible.

Similar effects are observed on the dry plains of the western part of our continent: in rubbing the horses or mules, sparks of electricity may be drawn from every part of the body of the animal. Persons in delicate health, whose perspiration is feebly exhaled, sometimes exhibit electrical excitement in a degree sufficient to surprise those who are not familiar with the phenomena. But these exhibitions have no connection with animal electricity, and are merely simple illustrations of the electricity developed by friction in an atmosphere too dry to permit the usual immediate and silent restoration of the electrical equilibrium.

Distribution of Electricity.—The mutual repulsion of the atoms of electricity, varying inversely as the square of the distance, gives rise to the distribution of the fluid in regular geometrical arrangements, the form of which may be calculated with mathematical precision. As one of the simplest cases of distribution, suppose a conductor of the form of a cylinder, with hemispherical ends (for example, one of wood, covered with tin foil) to be suspended horizontally in dry air with silk threads, and thus insulated to be slightly electrified by touching the middle of it with a charged body; the atoms of the fluid, by their mutual repulsion, will separate as far as possible from each other, and be found at the two extremities. If the conductor were not surrounded with a non-conducting fluid, like the air, they would be driven off by the same repulsion into space, and thus indefinitely separated.

This inference from the theory can readily be proved to be in accordance with the actual condition of the excitement, by bringing into contact with the middle of the length of the conductor a small carrier ball, and afterwards applying it to the knob of the electroscope. If the charge given to the conductor be small, scarcely any electricity will be found at the middle; if however the carrier be brought into contact with either end of the conductor, it will receive a charge of such intensity as to cause the leaves to diverge widely from each other. If a charge of electricity be imparted to the centre of a conductor in the form of a thin circular disc the fluid will be found, by a similar examination, in the greatest intensity, at the outer rim.

If we electrify a solid globe of metal, the excitement will be confined to an indefinitely thin stratum just at the surface of the conductor; for if the electricity be imparted to the

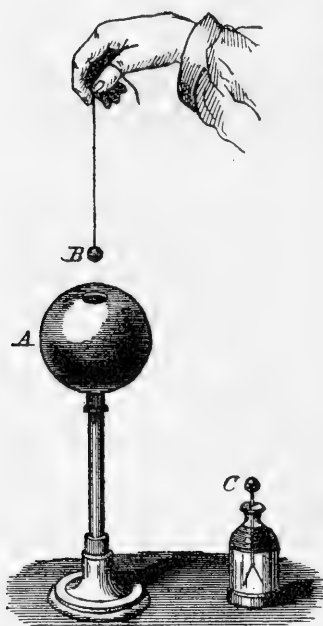


FIG. 3.

centre of the globe along a wire through a glass tube, the electrical atoms will evidently separate from each other as far as possible, on account of their mutual repulsion, and would continue to diverge even beyond the surface, were it not that they were stopped by the non-conducting air which surrounds and insulates the globe. That this inference is true may be shown by an arrangement which is exhibited in Fig. 3, in which *A* represents a hollow metallic globe insulated on a glass pillar and charged with electricity. If the carrier ball *B* be let down into the interior of the globe, so as to touch the inner surface and then withdrawn without touching the side of the hole it will be found

entirely free from electricity. If however it be made to

touch the outside of the globe, it will carry off with it a charge which will cause the leaves of the electroscope *C* to diverge in proportion to the original quantity imparted to the sphere. A similar effect will be exhibited if the ball *B* be lowered into an insulated cylinder of wire gauze *A*, Fig. 4, which has been charged with electricity. Not the least sign of excitement will be found on the inside, while a spark may perhaps be drawn from the exterior. The same result is produced, (as will be seen,) whether the globe be charged negatively or positively. On the hypothesis that the attraction and repulsion both observe the law of diminution with the square of the distance, this curious phenomenon is readily explained.

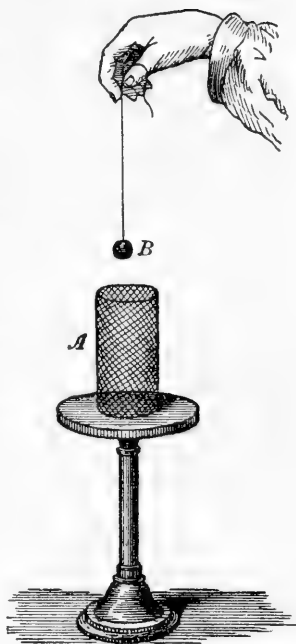


FIG. 4.

Newton has demonstrated the following propositions relative to the action of gravitation; and these principles are equally applicable to electrical attraction and repulsion, or to any other action which varies inversely as the square of the distance:

1. A particle of matter placed outside of a hollow sphere of attracting or repelling matter of uniform thickness, is acted upon as if all the matter were concentrated at the centre of the sphere.

2. A particle of matter (or of free electricity) placed at any point within a hollow sphere of uniform attracting or repelling matter, will be acted upon in every direction by an equal force, and will consequently be in equilibrium.

The form of the demonstration of the first of these propositions may be easily understood by a reference to Figure 5, and the accompanying considerations.

In this figure, a represents a particle of matter or of electricity attracted or repelled by the hollow sphere of which the centre is C . Let the two lines ad and ae represent the projection of a pyramid having its apex in a , and its base in de , then it will be evident that the attraction of the three sections of the cone, one through the centre, another coinciding with the upper part of the spherical shell, and the third with the lower part included within de , will be equal. For although the lower section is

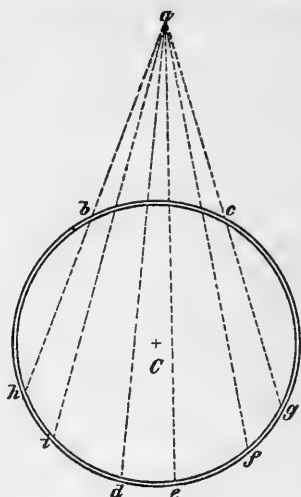


FIG. 5.

at a greater distance from a than the upper, yet its greater size just compensates for the greater distance, the surface increasing, as in the case of light, as the square of the distance, while the attraction and repulsion diminish in the same ratio. For the same reason, each of the two portions of the spherical shell are equal in action to a plate of equal thickness through the centre, included within the cone; and hence, the two together will be equal to a plate of double thickness at the centre.

If in the same way we suppose the whole spherical shell included in a series of pyramids or cones, having as a common apex the point a , and consider this series of cones made up of equi-angular pairs, the two members of which are on each side of the line through the centre as ha and fa , then it will be clear that the resultant action of each of these pairs of cones will be in a line through the centre, and all the action of the sphere made up of such cones the same as if it were at this point.

That a point at the centre of a hollow sphere would be equally acted upon in all directions is evident; but that the same should be the case when the point is at a , Fig. 6, for example, is not quite so clear. It may however be rendered evident by considering the actions of the opposite bases of

the two cones $b a c$ and $d a e$, or $f a g$ and $h a i$, which (for a reason similar to that given in the preceding proposition) are respectively equal to each other; and as we may consider the whole interior surface of the spherical shell made up of the opposite bases of a series of pairs of similar cones, it is clear that the particle at a will be equally attracted or repelled on all sides, or in other words will be apparently un-affected by the action of the excitement which may exist at the surface.

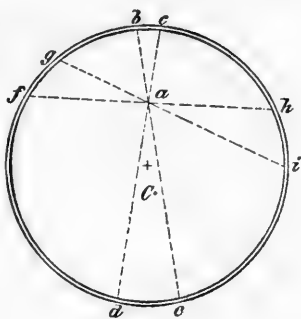


FIG. 6.

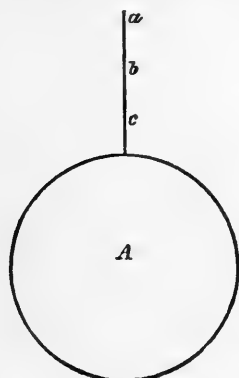


FIG. 7.

From the first of these propositions, it is easy to deduce the effect of a pointed rod in discharging the electricity from a globe. For if A , Fig. 7, be the centre of a charged sphere, from which the slender pointed conductor $a b c$ projects, then will the action of all the electricity of the sphere on the point a be the same as if directed from the centre; and if we suppose for example that the sphere is charged with positive electricity, then will the atoms of electricity of the point

a be repelled by all the atoms of the fluid of the globe, as if they were concentrated at A , and also the atoms of the fluid at the point b , below a , will be repelled by all the atoms of the electricity of the globe as if they were concentrated at the same point, and so on with the atoms at c , &c.; therefore the atoms at the point a will not only be directly repelled outward by the atoms of the fluid in the sphere, but they will also be pressed outward by the repulsion exerted on each of the atoms below, so that the whole force exerted to drive off the fluid from the point a will be in some relation to the number of atoms in the perpendicular column below this point; and hence the strong tendency to rupture the air and to escape, which must exist in a point projecting from a

charged surface; and for a similar reason, when the globe is charged negatively, to draw in electricity from surrounding bodies.

From the second proposition, we can readily deduce the fact of the distribution of the electricity at the surface; for if we communicate to the interior of a globe a quantity of electricity just sufficient to arrange itself in a stratum of the thickness of a single particle, it will so arrange itself on account of the mutual repulsion of the atoms, but if an additional quantity is thrown into the interior, it might not appear evident that this would also come to the surface, since the repulsion of the atoms already at the surface, (as it would seem at first sight,) would drive the additional atoms back towards the centre; but from the second proposition, the inner atoms are not affected by the outer, and consequently they would separate from each other by their mutual repulsion, as if the latter did not exist, and arrange themselves at the surface.

That this should take place when the sphere is charged with redundant electricity is not difficult to understand; but when a deficiency exists, the explanation has not been thought as easy. If however we suppose a quantity of the natural electricity drawn from the interior of a solid globe, then the un-saturated matter in the centre of the globe will act as a sphere, and draw into itself the electricity from around, and thus produce a hollow sphere of attracting matter, which will again draw into itself the natural electricity from around, and in this way, it must be evident, the deficiency will finally come to exist at the surface.

These propositions, which as we shall see are of great importance in the study of the theory of atmospheric electricity, can be readily demonstrated experimentally. If we coat a large hollow glass globe with tin foil, and insert through an opening into it a delicate electroscope, consisting of two slips of gold leaf suspended parallel to each other, (a small piece of the covering of tin foil being removed at two points on opposite sides to observe any effects produced within,) not

the slightest divergence will be seen in the gold leaves, when the globe outside is intensely charged with electricity. The same result will be obtained when a slip of gold leaf is suspended in the interior and electrified, either positively or negatively. It does not follow from these experiments that the electricity on the outside does not act on that of the inside. On the contrary, we must infer from the theory that every atom of electricity at the surface acts repulsively on every atom of electricity in the gold leaf; but these actions are equal in all directions, and therefore neutralize each other.

The second proposition may be demonstrated by means of a charged ball and the hollow globe, Fig. 3. If the charged ball, suspended by a silk thread, be placed at about eighteen inches above a gold leaf electroscope, and the divergence noted, and if then the ball be removed and its place occupied by the centre of the globe to which the electricity of the ball has been imparted, the divergence will be the same as before; or in other words, the action on the electroscope will be the same when a given quantity of electricity is concentrated on a ball at the centre of a sphere, or diffused throughout the surface of the same body. This experiment may be varied, with more striking results, by placing the hollow globe at a given distance from the electroscope, and then letting down a charged ball into its interior until it reaches the centre: the leaves will be seen to diverge to a definite degree; if the ball be now made to strike the interior surface of the globe, by moving the suspending thread of silk, the whole of the charge will pass to the surface of the latter, but the leaves will exhibit the same amount of divergence as before the transfer. The electricity which is distributed throughout the surface of the globe produces precisely the same effect as it did when confined to the ball at the centre.

The mathematical problem to be solved, for the purpose of calculating the distribution of a given charge of electricity in a body of any form, is to proportion the amount of the fluid in each part of the surface, so that the resultant action on the interior of a body will be completely neutralized. This problem, which is simple for the sphere, becomes

too complex, even for the highest powers of mathematics, for bodies of less regular forms than those generated by the revolution of simple curves.

Electrical Induction.—The attraction and repulsion of electricity, like those of magnetism, act at great distances, and produce phenomena which it is necessary clearly to understand in order properly to comprehend the explanation of many of the facts connected with atmospheric electricity.

For the exhibition of these phenomena, which are classified under the name of inductive effects, we may make use

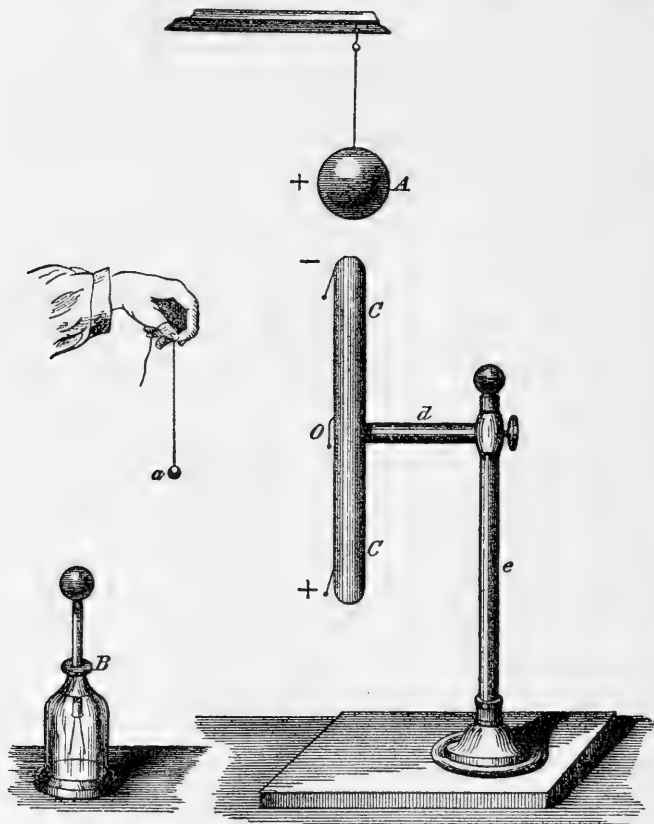


FIG. 8.

of the arrangement represented in Fig. 8, in which *A* is a metallic globe suspended in free air by a fine silk thread, and

thus insulated. *O* is a long cylindrical metallic conductor, supported by a rod of shellac or sealing-wax *d*, on stand *e*, having a glass stem.

Now each of these metallic bodies contains its natural share of electricity, and as long as this continues to be the same no electrical effects are exhibited; for although the natural electricity of *A* will repel the electricity of *O*, yet the matter of *A* will attract it with an equal force, and hence there will be no perceptible effect. Let us however suppose that there be imparted to the globe *A* a redundant quantity of electricity, then the equilibrium in the conductor *O* will be disturbed; the repulsion of the redundant fluid will be greater than the attraction of the un-saturated matter, and hence a portion of the natural electricity of *O* will be driven down to its lower end, and consequently the upper end will become negatively, while the lower is positively electrified. It must be evident therefore that between the two extremes there will be a point near the middle which will be in its ordinary condition.

These inferences may readily be shown to be true by observing three movable pith balls suspended by linen threads, one near the top, another at the middle, and the third at the lower end. Those at the extremities will diverge, exhibiting excitement, while the one at the middle will remain unmoved, indicating that this point is in a natural condition. To be assured that the upper end is negatively electrified, and the lower positively, it is only necessary to rub a stick of sealing-wax with woollen cloth, and bring it in succession near the two balls; the upper one will be repelled and the lower one attracted; or we may arrive at the same results by touching in succession the two extremities and the middle of the conductor with the small carrier ball *a*, and applying it to the knob of the electro-scope *B*.

If the conductor *O* be removed laterally to a distance from under the charged globe, the excitement will disappear, the atoms of natural electricity, by their mutual repulsion at the lower end, and attraction for un-saturated matter at

the upper end of the conductor, will distribute themselves uniformly, and assume their natural condition. In this experiment the fact is illustrated that all bodies are naturally charged with electricity, which exhibits itself when the equilibrium is disturbed by the action of some extraneous force. If the conductor *O* be restored to its former position the excitement will be renewed, provided the globe *A* has lost none of its charge, and the two pith balls will diverge as before. If the charge of electricity in the insulated globe be increased, the repulsive action or induction, as it is called, will also be increased; another portion of electricity will be impelled down into the lower end, increasing the repulsive action at that point, and also the amount of attraction at the upper end. The middle of the conductor however will still remain in a condition of neutrality. Again, if while the charge in the globe *A* remains the same, the space between it and the upper end of the conductor is diminished, a greater excitement will be exhibited by the increased divergence of the balls at the two extremities; for since the force increases with a diminution of distance, an additional quantity of the natural electricity of the upper end will be driven down into the lower end, and an equal amount of un-saturated matter will be left at the upper end.

We may still further vary the experiment by lengthening the conductor *O*, the charge of the globe and its distance from the upper end remaining the same, and for this purpose the conductor may be made to draw out like the tube of a telescope. We shall find that the greater the length, the greater will be the intensity of the effect at each end. To understand this we have only to recollect that the atoms of electricity constantly repel each other, and that in the case of a short conductor, but little comparatively can be driven from the upper end, because the self-repulsion of the electricity of the lower end and the attraction of the un-saturated matter of the upper end both conspire to restore the distribution, but when we give a greater length to the conductor for the free electricity of the lower part to expand into, and thereby lessen the intensity of the repulsion and

also remove the free electricity farther from the centre of attraction of the redundant matter, the tendency to restore the normal condition is much lessened, and a new quantity will be repelled into the lower end from the upper, and thus produce at that end a greater intensity of excitement. If we increase indefinitely the length of the conductor, (or what amounts to the same thing) if we connect the lower end of it by means of a metallic wire or other conductor with the earth or elongate it till it touches the earth, then we shall have the maximum of effect. The neutral point will descend to the earth, while the conductor, throughout its entire length, will be charged negatively.

The effects which we have described are those which would take place if we supposed the electricity in the globe suffered no change in its distribution on account of the induction; but this cannot be the case, since in the action of one body on another—an equal re-action must be produced, hence the un-saturated matter in *O* will re-act on the free electricity in the globe, and draw down into its lower side a portion of that which before existed in the upper side, and thus render the lower side more intensely redundant than before. This additional quantity of free electricity in the lower side will tend to increase the amount of un-saturated matter in the upper part of the conductor. The maximum effect will be produced, as we have before stated, when the lower end of the conductor is brought in contact with the earth, which may be considered as a conductor of infinite capacity. In this condition the self-repulsion of the atoms of the fluid in the lower part of the globe, and the attraction of the un-saturated matter in the upper end of the conductor, may become so great as to cause a rupture of the intervening air and a transfer of the redundant electricity in the form of a spark from the upper to the lower body.

If instead of the metallic conductor we substitute a rod of shellac or glass of the same length and diameter under the same conditions, no spark (or but a very feeble one) will be produced. The natural electricity cannot be driven down on account of the non-conducting character of the

material, and while it remains at the top it repels the free electricity of the globe as much as the matter of the globe attracts it. For a similar reason, if a small brass ball be placed on the top of a rod of glass and presented to the globe, but a feeble spark will be elicited; the inductive influence will act in this case under unfavorable conditions, a portion of the natural electricity, it is true, will be driven down into the lower surface of the ball, and an equal amount of un-saturated matter will exist at the upper surface; but the attractions and repulsions will be so nearly at the same distance that but a comparatively feeble effect will be produced. An attentive consideration of these facts is essential to a knowledge of atmospheric electricity, and necessary to understand and guard against the effects of the destructive discharges from the thunder-cloud.

The inductive action we have described takes place at a distance through an intervening stratum of air, but the same effect is produced, and with nearly the same intensity, when the intervening space is occupied with glass or any other non-conducting substance. If a disk of wood, which is a partial conductor, is interposed, the effect will be slightly modified, because an inductive action will take place in the substance of this which will tend to increase the effect in the conductor *O*, below.

As an illustration of the inductive influence of free electricity at a distance on the natural electricity of a conductor, we shall direct the attention of the reader to an arrangement exhibited in Figure 9, which is that of an experiment made by the author in Princeton, in 1842*. Two circular disks of wood, *a* and *b*, each about 4 feet in diameter, were entirely covered with tin foil; one was in connection with a large insulated conductor of an electrical machine in the upper story of a building, the other was supported on a glass foot in the lowest story, at the distance of about 25 feet below, with two floors and ceilings intervening. The upper disk being charged by the machine, the lower one was touched with the finger, so as to suffer the in-

* [Proceedings Am. Phil. Society, June 17, 1842. See *ante*, vol. 1, p. 203.]

duced electricity to escape into the ground. If when in this

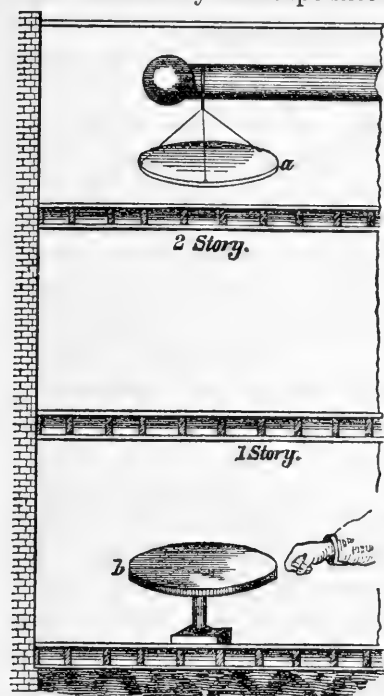


FIG. 9.

condition the knuckle was held near the lower disc and the upper one suddenly discharged by a spark received on a ball attached to the end of a wire connected with the earth, a spark was seen to pass between the knuckle and the lower disk. A similar effect was produced when the upper plate was suddenly charged by powerful sparks from the machine, though the intensity in this case was somewhat less.

In this experiment, the upper disk may represent a charged thunder-cloud, and the lower one the ground, or any conducting body within a house. While the

charged cloud is passing over the building, all conducting bodies in it, by this inductive action at a distance, have their natural electrical equilibrium disturbed; the upper part of each body becoming negatively electrified, and the lower part positively; and if the cloud continue in this position for a few minutes, the free electricity of the lower part of the conductor will be gradually driven into the earth, through the imperfect insulation of the floor. If in this case the lower part of the cloud is suddenly discharged, sparks of electricity may be perceived, and perhaps shocks experienced, by the inmates of the dwelling, produced by the sudden restoration of the equilibrium, due to the removal of the repulsive force of the cloud on the natural electricity of the bodies below.

The inductive action of the electrical discharge at a dis-

tance is still more surprisingly exhibited, by an arrangement shown in Figure 10, which the writer adopted about the same time during his electrical investigations at Princeton.

The roof of the house which he occupied in the college campus was covered with tinned iron, and this covering was therefore in the condition of an insulated plate, on account of the imperfect conduction of the wood and brick-work which intervened between it and the ground. To one of the lower edges of this covering was soldered a copper wire, which was continued downward to the first story, passed through a gimlet-hole in the window-frame into

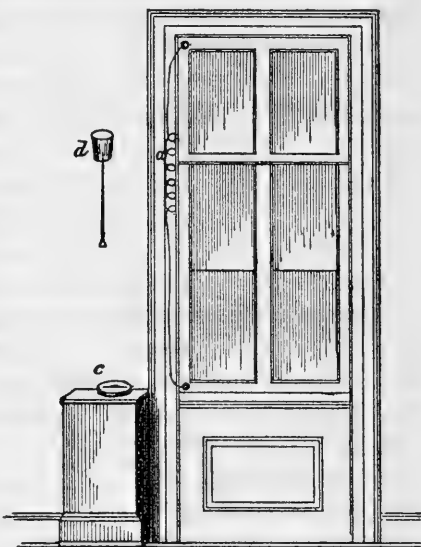


FIG. 10.

the interior of the author's study, and then passed out of the lower side of the same window, and thence into a well, in which it terminated in a metallic plate below the surface of the water. Within the study, the wire was cut and the two ends thus formed were joined by a spiral of finer wire *a* covered with silk thread. Into the axis of this spiral a large sized sewing-needle *d* was inserted, the point having been previously attached to a cork, which served as a handle for removing it. With this arrangement, the needle was found to become magnetic whenever a flash of lightning was perceived, though it might be at the distance of several miles. The intensity of magnetism and the direction of the current were ascertained by presenting the end of the needle to a small compass represented by *c*. In several instances the inductive action took place at such a distance, that after seeing the flash the needle was removed its magnetic con-

dition observed and another needle put in its place, before the noise of the thunder reached the ear. In this experiment the inductive action of the electrical discharge in the heavens was exerted on the natural electricity of the tinned roof, (a surface of 1,600 square feet,) and a considerable portion of this passed down through the wire into the well. The arrangement served to indicate an action which would otherwise have been too feeble to produce a sensible impression.

It must be observed that the effect here described was not produced by the actual transfer of any electricity from the cloud, but was simply the result of induction at a distance and would probably have been nearly the same had the intervening space been filled with glass or any other solid non-conducting substance. We say probably very nearly the same, because Professor Faraday has shown that the inductive effect at a distance is modified by a change in the intervening medium.

It is also proper to mention here, (although we cannot stop to give the full explanation of the means by which the result was obtained,) that the electricity passing along the wire was not that due to a single discharge into the well, but to a series of oscillations up and down in alternate directions until the equilibrium was restored.

Electricity in Motion.—The phenomena we have thus far described relate principally to electricity at rest. Those which relate to ordinary or frictional electricity in motion have not been so minutely investigated as the other class, and present much more difficulty in ascertaining the laws to which they are subjected. The discharge of electricity from the clouds or from an ordinary electrical machine is so instantaneous that we are principally confined in our investigations to the effects which remain along its path after its transfer.

The electricity however which is developed by chemical action in a galvanic battery is of sufficient quantity to produce a continuous stream, or at least a series of impulses in such rapid succession that they may be considered continuous. By employing electricity of this kind, it has been supposed

that we can study the fluid while it is actually in motion, and from the results deduce inferences as to the mode in which some of the effects are produced in the discharge of frictional electricity. The two classes of phenomena however, though referable to the same cause, are in many respects so different in character that considerable caution is required in drawing inferences from analogy. The phenomena of ordinary electricity are characterized by an intensity of action which indicates a repulsive force between the atoms of the hypothetical fluid, which is in some way—at least partially neutralized, in the case of galvanism.

Ordinary electricity in a state of equilibrium appears to produce but a very feeble effect upon bodies in which it is accumulated. However great may be the quantity present, no effect is perceived by a person when insulated on a glass stool, and charged either positively or negatively, so long as the electricity remains at rest. If however it is drawn from him in the form of a spark, then a disagreeable pricking sensation is experienced at the point of rupture. Dr. Faraday constructed a small metallic house or room, which he suspended by silk ropes in mid air, and charged it so strongly that long sparks could be drawn from the outside, yet not the least effect was perceived by the persons within: even when the air of the interior of the house was strongly electrified, the excitement was only perceptible on the outside.

It is fully established by the most satisfactory experiments that in all cases in which a discharge of electricity takes place by breaking through a stratum of non-conducting substance like air, there is an actual transfer of matter each way between the two ends or sides of the opening in the conductor along the path which the spark traverses. If two conducting rods be employed having the end of each terminated by a brass ball, one of which is covered with gold leaf, and the other with silver, a transfer in opposite directions of these two metals will be observed. A similar effect is produced in the discharge of lightning from the clouds, and there are several well authenticated cases on record, in

which a picture as it were of one body has been impressed on another between which the electrical discharge took place.

Another effect produced by the discharge, and having an important bearing upon the explanation of some of the mechanical results of electricity, is a sudden and violent repulsive energy given to the atoms of air and other substances through which it passes, and which causes them to separate with an explosive violence.

This may be shown by transmitting a discharge from an electrical battery between two brass balls projecting into the inside of a glass bulb, to the lower side of which is joined an air-tight tube containing a small quantity of water, and opening at the end into a cup of water, the arrangement with the exception of the balls being similar to that of an air thermometer. The moment the discharge takes place, the water will be driven down the tube, exhibiting a great enlargement of the volume of air in the bulb. This experiment was communicated by Mr. Kinnersley, of Philadelphia, to Dr. Franklin. The effect at first was attributed to heat produced by the discharge of electricity through the air in the bulb, but although there *is* heat evolved in this case, (as is proved by the fact that if a number of sparks be passed in succession the water does not return to its first altitude, and thus indicates an increase of temperature,) yet the principal cause is evidently the sudden repulsive energy given to the air at the moment of the passage of the discharge, as may readily be shown by inclosing a thermometer within the bulb. The increase of temperature which this indicates will be far too small to account for the great and sudden expansion produced. A similar exhibition of force is exhibited when a strong discharge of electricity is passed through a vessel (like the one we have described) filled with water. In this arrangement a thick glass bulb may be broken into pieces.

The mechanical effects produced by lightning must be attributed principally to this cause. When a powerful discharge from a cloud passes through a confined space filled with air, and surrounded by partial non-conductors, a tre-

mendous energy is exerted. In the case of a house examined by the writer, the discharge fell upon the top of a chimney at the west end of the building and passing through a stove-pipe hole traversed the space under the rafters, (called the cock-loft), to the chimney at the east end and thence down to the ground; the force exerted was sufficiently great to lift up the whole roof from the top of the walls on which it rested. In like manner, when the discharge takes place along the upright timbers of a house, the clap-boards are frequently blown off outward and the plaster inward as if by the explosion of gunpowder.

To a similar action we must ascribe the splintering of trees by lightning. At the moment of the passage of the discharge the sap or moisture is suddenly endowed with a repulsive energy which resembles in its effects the action of an explosive compound, separating the fibres longitudinally and projecting parts of the body of the tree to a distance. When a tree is struck by lightning the greatest effect is usually produced on the main stem just below the branches. A portion of the discharge appears to be received on each twig, leaf, and branch, and the whole concentrated by converging towards the trunk. The repulsion imparted to the atoms of a conductor is in some cases sufficiently great to at once dissipate in vapor fine metallic wires, and this so instantaneously that the silk covering by which they are surrounded for telegraphic purposes is not burned.

The repulsive energy is exerted not alone laterally, but perhaps in a greater degree in the line of direction of the conductor, tending to separate it as it were by transverse sections. Hence when electricity passes through a wall into the interior of a house, a pyramidal mass of plaster is thrown out. A similar effect is frequently produced when the discharge takes place between the cloud and the level earth: a large conical or pyramidal hole is formed, from which the earth is thrown out as if by the explosion of a quantity of powder beneath the surface. Such excavations are supposed by some to indicate a discharge of electricity from the earth to the cloud, but no conclusion of this kind can, with

certainly, be drawn from the phenomena. It simply indicates an intense repulsive energy exerted between the atoms of matter in the line of discharge. It sometimes happens when an old tree which has perhaps been moistened by the rain—is struck by lightning, instead of being rent laterally it is broken off transversely, the upper part being projected vertically upward. This effect however is not usually produced, since the force exerted by the tree to resist transverse breaking is much greater than that to prevent lateral tearing apart.

In the passage of electricity from a charged conductor, or from a cloud to the earth, it always follows the line of least resistance and by an antecedent induction determines the course it is to pursue. This is strikingly exhibited by an experiment devised by Sir W. S. Harris. A number of separate pieces of gold leaf are attached to a sheet of paper. If a discharge sufficiently strong to dissipate the gold and blacken the paper be passed through them, its course will be shown by the blackened parts; and it is especially worthy of remark, that not only are the pieces out of the line of least resistance untouched, but even portions of other pieces are left unchanged from the same cause. Now these separate pieces of gold leaf may be taken to represent detached conductors fortuitously placed in the construction of a building.

The apparently fitful course of a discharge in its passage through a building frequently excites surprise, leaping (as the electricity does) from one conductor to another, and sometimes descending to the earth in several streams; but that the discharge should leap from one conductor to another through a considerable intervening space of air is not surprising, since its original intensity was sufficient to enable it to break through a stratum of the atmosphere of perhaps a mile in thickness before it reached the house.

Whenever electricity passes through an interrupted conductor so as to exhibit the appearance of light, a great increase of intensity is always manifested at the point of disruption, as if the charge halted here for a moment until a

sufficient quantity of the fluid could accumulate to force its passage through the obstacle. An illustration of this action is presented in the fact, that at the point where the lightning leaves a conductor, and also where it is received by another conductor, signs of fusion or of more intense action are always exhibited. An effect of lightning described by Professor Olmsted, at a meeting of the American Association, in New Haven, may be explained on this principle. A row of five or six milk-pans, placed in the open air on a bench, was struck by a discharge from a cloud. The electricity passed through the whole series, making two holes in each pan, at opposite extremities of the diameter, or at the places where the electricity may be supposed to have entered and gone out.

There is another circumstance connected with the discharge of electricity—having an important bearing on the construction of lightning-rods, which may be mentioned in this place. When the repulsion of the atoms of electricity in a conductor or in a cloud and the attraction of the unsaturated matter below become so intense as to cause a rupture in the air, the electricity of the cloud is precipitated upon the conductor, and not only restores the natural quantity, but also gives it for a moment a redundancy of electricity, a fact which must be evident from the theory, when we consider the distance at which the induction is communicated. As this charge of free electricity passes down the rod to the earth, for example, it assumes the character of a wave, rendering the metal negative in advance; and thus in the transmission of free electricity through a rod of metal, the action consists of two waves, one of redundancy, immediately preceded by one of deficiency. Hence if a small ball connected with the earth by a wire be brought near a conductor (for example a lightning-rod) on the upper end of which, discharges of electricity are thrown from an electrical machine, sparks may be drawn from the rod, however intimately it may be connected with the earth below.

This effect was strikingly exhibited by an experiment

made by the author, which consisted in placing one end of a copper wire (a tenth of an inch in diameter) beneath the water of a well, its upper end being terminated by a small ball, and throwing on it sparks of electricity from a globe of a foot in diameter. Although in this case the conductor was as perfect as possible, yet sparks sufficiently intense to explode the oxy-hydrogen pistol were obtained from the wire throughout its whole length.

This effect was not due, as some have supposed, to the tendency of the electricity to seek another passage to the earth, as may be shown by catching the spark in a Leyden jar; but it was solely the effect of a transient charge of electricity passing along the surface of a conductor from one extremity to the other.

The phenomena may be expressed generally by the statement that when electricity is thrown explosively as it were, on the end of an insulated conductor, by a disruptive discharge through the air, it does not pass silently to the earth, but tends in part to be given off in sparks to all surrounding bodies. It is on this account that we object to the otherwise admirable arrangement of Sir W. Snow Harris for the protection of ships from lightning. Though the main portion of the discharge of electricity is transmitted innoxiously to the ocean by means of the slips of copper which are carried down along the mast and through the bottom of the vessel to the sheathing beneath, as proposed by him, yet we consider it safer to conduct it across the deck and over the sides of the vessel to the copper sheathing. It is true, the quantity which tends to fly off laterally from the rod is small, yet we have shown by direct experiment that it is sufficient even when produced by the electricity of a small machine, to set fire to combustible materials; and therefore it cannot be entirely free from danger in a ship, loaded for example with cotton.

The atoms of electricity, in their transfer from one body to another, still retain their repulsive energy; and if the discharge be not very large in proportion to the size of the conductor, it will be principally transmitted at the surface.

If the charge be very large, and the conductor small, it will probably pervade the whole capacity, and as we have seen, in some cases, will convert into an impalpable powder or vapor the solid particles. Because electricity in a state of rest is found distributed at the surface of a body, it was immediately assumed without examination, that electricity in motion passes along the surface; but this conclusion was supposed to be dis-proved by the fact that the conducting power of a wire for galvanic electricity is in proportion to the area of the cross-section, from which it follows that this kind of electricity pervades the whole mass of the conductor. But galvanic electricity differs from common electricity, apparently in the exertion of a much less energetic repulsion, and in a greater quantity developed in a given time. The deduction therefore from the experiments with galvanism can scarcely be considered as conclusive in regard to frictional electricity.

To settle this point, the writer devised a series of experiments which fully proved the tendency of electricity of high tension, (that is of great repulsive energy,) to pass along the surface. It will be sufficient to give as an illustration of this fact, the result obtained by the arrangement represented in Fig. 11, in which *C D* is a copper wire, (one of the best

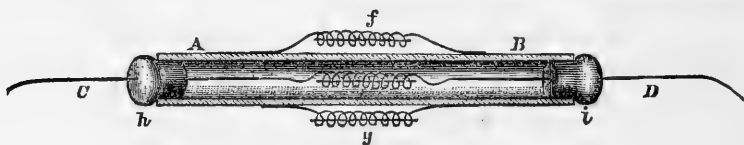


FIG. 11.

conductors of electricity,) of the size usually employed for ringing door-bells, passing through the axis of an iron tube, or a piece of gas-pipe, *A B*, about three feet long. The middle of this wire was surrounded with silk, and coiled into a magnetizing spiral, into which a large sewing-needle was inserted. The wire was supported in the middle of the tube by passing it through a cork (covered with tin-foil), at each end, *h i*, so as to form a good metallic connection between the copper and the iron. Two

other magnetizing spirals of iron wire, *f* and *y*, were arranged on opposite sides of the tube, the ends soldered to the iron. When these two spirals were also furnished with needles, and a discharge from a Leyden jar sent through the apparatus, as if to pass along the wire, the needle inside of the iron tube was found to exhibit no signs of magnetism, while those on the outside presented strong polarity. This result conclusively shows that notwithstanding the interior copper wire of this compound conductor was composed of a material which offered less resistance to the passage of the charge than the iron of which the outer portion was formed, yet when it arrived at the tin-foil covering of the cork, it diverged to the surface of the tube, and still further diverged into the iron wire forming the outer spirals. We must not however conclude from this experiment that the electricity actually passed on the outside of the tube. On the contrary, we must infer from the following fact that it passes just within the surface. If the iron be coated with a thin covering of sealing-wax, the latter will not be disturbed when a moderate discharge is passed through it, though with a large discharge in proportion to the conducting power of the rod, the outward pressure may become so great as to throw off the stratum of sealing-wax. This point is of some importance in regard to the question of painting lightning-rods. If the metal is of sufficient size to freely transmit an ordinary discharge from the clouds, the condition of the exterior surface can have but little effect, and we see no objection to coating it with black paint, the basis of which is carbon, a good conducting material.

It is also to the same repulsive energy that we may attribute the spreading of a discharge when it passes through partial conductors, as in the case in which a spark from an electrical machine is transmitted over a pane of glass on which particles of iron filings are sparsely scattered. It is probable that drops of rain and partially condensed vapor in the atmosphere are in some cases connected with a similar appearance of discharge of electricity in the heavens.

A much longer spark of electricity can be drawn through

rarified air than through that of ordinary density. The light which accompanies a discharge in this case assumes different colors, the violet predominating. This is a fact of interest in connection with the color exhibited by lightning, and we may infer that the discharges of a violet hue take place between clouds at a great elevation in the atmosphere.

The electric spark, when passed through a confined portion of atmospheric air, is found to produce a chemical combination of its component parts, namely nitrogen and oxygen, and to form nitric acid. The same result is produced on a grand scale in the heavens during thunderstorms; hence the rain water that falls, (in the summer season especially,) always contains a considerable quantity of nitric acid, which is considered by the chemist as furnishing a portion of the nitrogen essential to the growth and development of the plant: and to the same source is referred the nitric acid in the nitrate of lime and potash found in the form of efflorescence on damp ground and the walls of old buildings. Indeed, all the nitrate of potash from which gunpowder is manufactured is supposed to have its origin in this way, and the explosion from the thunder-cloud and that from the cannon, may be looked on as in one sense—the counterparts of each other.

Again, during the transmission of electricity from an ordinary electrical machine a pungent odor is perceived, something analogous to that produced by the slow combustion of phosphorous, which Professor Schönbein, by a long-continued series of researches, has shown to result from a change in the oxygen of the air. He supposes that this substance is composed of two atoms, which by their combination partly neutralize each other, but which are separated by the repulsion of the electric spark, and when thus set free—have a much greater tendency to combine with other substances than in their ordinary state of union. Oxygen thus changed or dissociated is called ozone, and as it would appear, performs an important part in many of the molecular and chemical phenomena of the atmosphere. To this increased combining power of oxygen may be attributed the

formation of the nitric acid we have mentioned, and without such an explanation, it would be difficult to conceive how particles of oxygen and nitrogen, which are rendered mutually repulsive by the electrical discharge, should enter into chemical combination.

We have seen that though metals are generally good conductors, yet when electricity falls upon a rod of iron or copper explosively, the energetic repulsion, which must always accompany these explosions, tends to throw the particles off on all sides, and when the discharge is sufficiently great the conductor itself is dissipated in vapor. Water is a much inferior conductor to iron, and though a large mass of it will silently discharge a conductor, yet it offers great resistance to the transmission of electricity explosively, and hence the electricity is sometimes seen to leave a conductor, and pass a considerable distance over the surface of water, rather than to force its passage through the interior of the mass. It is therefore highly important in arranging lightning rods that they should be connected at the lower end with a large surface of conducting matter, to prevent as far as possible the fluid from leaving the rod in the case of an explosive discharge.

Electricity of the Atmosphere.

Having given in the preceding sections a brief exposition of the general principles of electricity, we are now prepared to apply these to an exposition of the phenomena of atmospheric electricity.

The origin of the electricity of the atmosphere has long occupied the attention of physicists, and at different times they have apparently settled down on some plausible hypothesis which merely offered a probable explanation of the phenomena without leading to new facts or pointing out new lines of research.

The earth, as is now well known, is an excellent conductor for the most feeble currents of electricity, provided the contact with it of the electrified body be sufficiently broad. The aerial covering which surrounds it, is however

a non-conductor, and is capable of confining electricity in a condition of accumulation or of diminution, and of preventing the restoration of the equilibrium that without the existence of this insulator, would otherwise take place.

The hypothesis was at first advanced that the earth attracts the ætherial medium of celestial space and condenses it in a hollow stratum around the whole globe; that the electricity of the atmosphere is due to the action of this exterior envelope. Dr. Hare, our countryman, has presented this hypothesis with considerable distinctness. Without denying the possibility or even probability of such a distribution of electrical excitement, we may observe that if this electrical shell were of uniform thickness, and we see no reason to suppose it should vary in different parts in this respect, it would follow from the law of central forces, that it could have no effect in disturbing the equilibrium on the surface or in the interior of the earth; a particle of matter remaining, as we have seen, at rest or un-affected at any point within a hollow sphere. This fact appears to militate against the truth of this assumption.

Another hypothesis attributed the electricity of the atmosphere to the friction of the winds on each other and on the surface of the earth, but careful experiments have shown that the friction of dry air on air, or of air on solids or liquids does not develop electrical phenomena.

The next hypothesis—advanced by Pouillet, referred the electricity of the atmosphere to the evaporation of water, particularly that containing saline ingredients. But when pure water is carefully evaporated in a space not exposed to the sky, no electricity is produced except by the friction with the sides of the vessel in the act of rapid ebullition; and when the experiment is made with salt water the electrical effects observed are found to be produced by an analogous friction of the salt against the interior of the vessel. When pure water is evaporated under a clear sky the vapor produced is negatively electrified; but this state is contrary to that in which the atmosphere is habitually found.

Pouillet also supposed that the process of vegetation was a source of disturbance of the electrical equilibrium, but this has not been supported by critical experiments.

The discovery accidentally made a few years ago of the great amount of electricity evolved in blowing off steam from the boiler of a locomotive, seemed to afford a ready explanation of the electrical state of the atmosphere. It was then attributed to the condensation of the aerial vapor. Faraday proved however by one of his admirable series of model experiments, that this effect was due entirely to the friction of the water (which escaped in connection with the steam) on the side of the orifice through which the discharge took place. When dry steam, or that which is so heated as to contain no liquid water, was blown out, all electrical excitement disappeared; and when condensed air—even at elevated temperatures, was discharged from an insulated fountain, no electricity was produced.

The celebrated physicist of Geneva, Professor De la Rive, refers the electricity of the atmosphere to thermal action. It is well known that if the lower end of a bar of iron (or of any other metal not readily melted) be plunged into a source of heat while the upper end remains cool, a current of electricity will flow from the heated to the cooled end, the former becoming negative and the latter positive, and that these different states will continue as long as the difference of temperature is maintained. Now according to Professor De la Rive a column of the air is in the same condition as the bar of metal—its lower end is constantly heated by the earth and its upper cooled by the low temperature of celestial space. Unfortunately however for this ingenious hypothesis, a column of air is a non-conductor of electricity, while a bar of metal is a good conductor, and it still remains to be proved that such a distribution of electricity as that we have described relative to the bar of metal can be produced in a column of air.

The foregoing are the principal hypotheses which have been advanced to account for what has been considered the free electricity of the atmosphere. After an attentive study

of the whole subject, we have been obliged to reject them all as insufficient, and compelled in the present state of science to adopt the only conclusion which appears to offer a logical explanation of all the phenomena, namely that of Peltier, which refers them not to the excitement of the air, but to the inductive action of the earth primarily electrified.

The author of this theory we are sorry to say did not receive that attention which his merits demanded, nor his theory that consideration to which so logical and so fruitful a generalization was justly entitled. Arago, in his great work on the phenomena of atmospheric electricity, does not allude to the labors of Peltier; the reason of which may be that his work was not intended as a scientific exposition of the principles of the phenomena, but merely a collection and classification of observed facts.

Peltier commenced the cultivation of science late in life and since the untutored mind of the individual, like that of the race, passes through a series of obscure and complex imaginings before it arrives at clear and definite conceptions of truth, it is not surprising that his first publications were of a character to command little attention, or rather to excite prejudice on account of their apparently indefinite character and their want of conformity with established principles. His theory of atmospheric electricity requires to be translated into the ordinary language of science before it can be readily comprehended even by those best acquainted with the subject, and hence his want of appreciation may be attributed more to the peculiarities of the individual than to the fault of the directors of science in France.

According to the theory of Peltier, the electrical phenomena of the atmosphere are entirely due to the induction of the earth, which is constantly negative or what in the theory of Du Fay is called resinous. He offers no explanation (so far as we know) of this condition of the earth, which at first sight would appear startling, but on a little reflection is not found wanting in analogy to support it. The earth is a great magnet, and possesses magnetic polarity in some respects similar to that which is exhibited in the case of

an ordinary loadstone or artificial magnet. This magnetism is of an unstable character however, and is subjected to variations in the intensity and in the direction of its polar force. In like manner we may consider the earth as an immense prime conductor negatively charged with electricity, though its condition in this respect may—like that of its magnetical state—be subject to local variations of intensity, and perhaps to general as well as partial disturbance.

It may be said that this merely removes the difficulty of the origin of the electricity of the atmosphere to an un-explained cosmical condition of the earth; but even this must be considered an important step in the progress of scientific investigation. The hypothesis of Peltier has since his death been rendered still more probable by the labors of Sabine, Lloyd, Lamont, Bache, and others, in regard to certain perturbations of the magnetism of the earth, which are clearly referable to the sun and the moon. It must now be admitted that magnetism is not confined to our earth, but is common to other—and probably to all the bodies of our system; and from analogy we may also infer that electricity, a co-ordinate principle, is also cosmical in its presence and the extent of its operation. That the earth is negatively electrified was proved by Volta at the close of the last century. For this purpose he received the spray from a cascade on the balls of a sensitive electroscope; the leaves diverged with negative electricity.

This experiment has been repeated in various parts of the globe, and always with the same result. That it indicates the negative condition of the earth is evident, when we reflect that the upper level from which the water falls must be considered as the exterior of the charged globe, and hence must be more intensely electrified than points nearer the centre. Since the earth is (as a whole) a good conductor of electricity, as shown by the operations of the telegraph, the electrical tension of it cannot differ much in different parts, and we are at present un-acquainted with any chemical, thermal, or mechanical action on land of sufficient magnitude to produce this constant electrical state. We are there-

fore induced to adopt the conclusion that the earth—in relation to space around it, is permanently electrical; that perhaps the ætherial medium, which has been assumed as the basis of electricity, as was supposed by Newton, becomes rarer in the vicinity of—and within bodies of ponderable matter. Be this as it may, all the phenomena observed in the atmosphere, and which have so long perplexed the physicist, can be apparently reduced to order, and their dependencies and associations readily understood, in accordance with the foregoing assumption. This is not a mere vague supposition, serving to explain in a loose way certain phenomena, but one that enables us not only to group at once a large class of facts, (which from any other point of view, would appear to have no connection with each other,) but also to devise means for estimating the relative intensity of action, and to predict both in mode and measure changes of atmospheric electricity before they occur. It follows, as a logical consequence from this theory, that salient points, such as the tops of mountains, trees, spires, and even vapors, if of conducting materials, will be more highly excited than the general surface of the globe, in a manner precisely similar to the more intense excitement of electricity at the summit of a point projecting from the surface of the prime conductor of an ordinary electrical machine.

It also follows from the same principle that if a long metallic conductor be insulated in the atmosphere, its lower end, next the earth, will be positive, and the upper end negative. The natural electricity will be drawn down by the unsaturated matter of the earth into the lower end of the wire, which will there become redundant, while the upper end will be rendered negative or under-saturated. That this condition really takes place in the atmosphere was proved in a striking manner by the experiment of Gay-Lussac and Biot in their celebrated aerial voyage, which consisted in lowering from the balloon an insulated copper wire, terminated at each end by a small ball. The upper end of this was found to be negative, and consequently the lower end must have been positive, since the whole apparatus—includ-

ing the balloon—was insulated. The experiments should be repeated at different elevations by some of our modern aeronauts, since the results obtained would have an important bearing on the theory of atmospheric electricity.

The same results may be shown in a simpler manner by the method invented by Saussure. This consists in attaching a leaden ball *f*, (Fig. 12,) to a long wire covered with silk or varnish, connected by means of a slight spring to the hook of

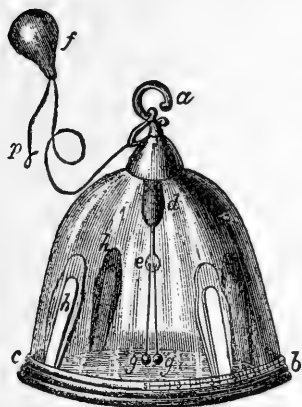


FIG. 12.

an electroscope. When this bulb is thrown upward by means of a string and handle *p*, so as to rise to a considerable height in the air, the pith balls *g g*, of the electroscope diverge with positive electricity, and the wire is dis-connected from the instrument. That this effect is not due to the friction of the bulb and the air is shown by whirling it in a horizontal circle round the head; not the least sign of electricity in this case being exhibited: and that it is not charged by absorbing free

electricity from the air, is proved by the fact that when the ball is thrown horizontally no excitement is manifest. The result is however just such as would be produced by the induction of the earth acting on the natural electricity of the wire and drawing it down to its lower extremity. A precisely similar effect would also be produced if the upper surface of the atmosphere were charged with this electricity. The intensity of the charge which the electroscope receives will depend upon the elevation to which the ball ascends, or in other words on the perpendicular component of the direction of the wire.

The method employed by Saussure in observing the variations of the electricity of the atmosphere illustrates the same principle. For this purpose he made use of one of his own electroscopes such as shown in Fig. 12. It consists of a bell-glass with a brass stem, *d e*, surrounded with sealing-wax, and two small pith balls, *g g*, suspended by very

fine wires: cb is a metallic foot, and $h h$ slips of tin-foil pasted on the inside and outside of the glass to discharge the pith balls when the electricity is so strong as to cause them to strike the glass. To measure the electrical intensity with this instrument the hook a was removed, and its place supplied with a pointed brass rod. The electroscope was first brought in contact



FIG. 13.



FIG. 14.

with the ground as exhibited in Fig. 13; then held vertically as shown in Fig. 14, and gradually elevated until the leaves began to diverge. Saussure found that the height to which the instrument was required to be elevated before the leaves showed signs of electricity varied at different times, and he estimated the intensity of the electricity of the atmosphere by the inverse ratio of this height.

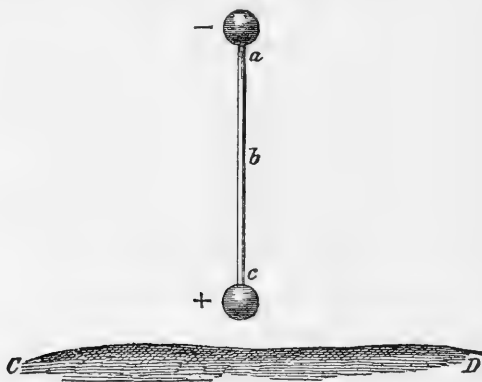


FIG. 15.

The explanation of this will be readily seen by a reference to Fig. 15, in which C, D , represents a portion of the surface of the earth negatively charged, and abc , a perpendicular conductor terminated above and below by a bulb. In this

condition the un-saturated matter in *C, D* will act upon each atom of the fluid in the conductor, and tend to draw the whole down into the lower bulb; the atoms at *a* will not only be attracted downward by the action of the earth on itself, but also pressed downward by the attraction of the earth on all the atoms above it, and hence the intensity of the electricity of the lower part of the conductor will be increased by an increase in the perpendicular length of the rod. Now, if we connect the lower bulb of the rod with the earth by means of a good conductor, the redundant electricity of the lower end will be drawn off into the earth and will no longer re-act by its repulsion on the electricity of the rod to drive it back into the upper bulb, and hence this will become intensely negative, and in this condition it will be a salient point on the surface of the earth. If while the apparatus is in this condition we could touch the upper ball with an electroscope it would exhibit a negative charge.

If a conductor 20 feet in length were made to revolve on a horizontal axis, passing through the middle of its length so that it could be immediately changed from a horizontal to a vertical position, any change in the apparent condition of the atmosphere would be shown by the greater or less intensity of the balls, as in succession they passed the lower point of their circuit; and an apparatus in the form of radiating conductors like the spokes of a wheel, if made to revolve, would furnish a constant source of electricity. An apparatus of this kind was constructed by M. Palmieri, of Italy, and might be used perhaps with success in studying the condition of the atmosphere in ascensions.

The most convenient apparatus however for exhibiting electricity by the induction of the earth is that invented by M. Dellman, and shown in Fig. 16; which consists of a large brass ball *a* supported on a thick brass stem—held insulated inside of a glass tube by passing through corks of gum shellac. The apparatus is fastened to a pole which is temporarily elevated into the air by a windlass or the hand, on the top of a house. When it reaches the height intended, the wire *k*, connected with the earth below, is pulled, the end

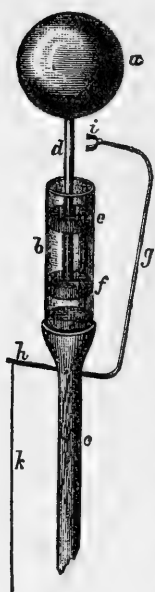


FIG. 16.

of the bent metallic lever $g h$, is depressed, and the fork i brought into contact with the stem of the globe, and thus a perfect metallic connection is formed between the latter and the ground. The wire k is then released, the lever falls back, the ball is insulated from the earth, brought down, and applied to an electroscope, and in all cases, when the sky is clear, is found to be negatively electrified. If the wire k be insulated through its entire length, and terminated in a bulb at a little distance from the earth, and a pull be given to it by means of a rod of glass, at the instant of contact of the point i with the stem d , the lower bulb will exhibit a positive charge of electricity. The arrangement will, in fact, be precisely the same as that exhibited in the previous figure, (Fig. 15), namely, a vertical conductor, the upper end of which is rendered *minus* and the lower end *plus* by the induction of the earth. This effect is entirely due to induction, and is independent of any free electricity which may exist in the air. The results are exhibited with the greatest intensity during perfectly clear and dry weather; and are not observed when the conductor is placed horizontally, but the indications increase as its upper end is gradually brought nearer the perpendicular.

That these effects are not due to the free electricity of the atmosphere is satisfactorily shown by the original experiments of Peltier. For measuring the intensity of the inductive influence of the earth he made use of an electrometer represented in Fig. 17; in which $a b$, is a glass cylinder furnished with a wooden foot and a glass cover: through the centre of this is cemented a brass tube carrying a ball c at the top, and an arched straddling wire at the bottom. At the level of the foot of the arched wire is suspended a fine magnetized needle g , the height of which is adjusted by the screw h . The intensity of the electricity is measured by the divisions pointed out by the deflected needle on the slip

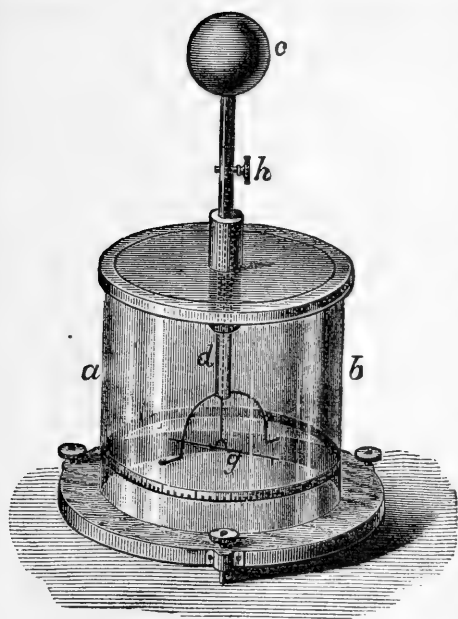


FIG. 17.

of paper surrounding the cylinder. This instrument, which is very sensitive, has been modified and improved by Dellman.

On the top of the flat roof of his house Peltier placed a flight of steps by which he could ascend holding in his hand an ordinary gold-leaf electroscope armed with a comparatively large sized polished ball. The ball of the electroscope was held at the height say of four feet above the roof of

the house, and in this position it was touched by the end of a wire connected with the earth below. It thus formed the termination of a perpendicular conductor, and was of course negatively electrified—the bulb more intensely than the leaves below, but the stratum of air in which it was placed being in the same state it exhibited no signs of electricity. It was then elevated by ascending the steps to the height of six feet above, and held by the lower plate. The leaves in this case diverged with negative electricity, because the ball was still farther removed from the earth, and the attraction being lessened, the part of the electricity in the leaves was set free and ascended to the bulb by repulsion, leaving a deficiency in the leaves. When the electroscope was brought down to its first position the leaves again collapsed since there was again an equilibrium; and when the electroscope was depressed below its normal position the leaves became positively electrified by the increased attraction of the earth, and in this way the electroscope was

made to diverge, to converge, and diverge again, by simply changing its elevation.

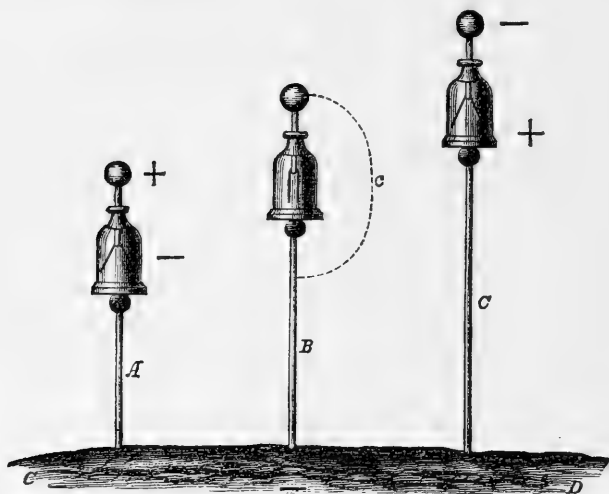


FIG. 18.

Fig 18 is intended to illustrate the condition of the electroscope in the three positions in which it is supposed to be supported on three metallic conductors of different heights. The electroscope brought into neutral condition by the ball is shown in the middle of the figure at *B*, in which the connection of the rod with the ball is indicated by the dotted line. When the electroscope is raised by the hand to a higher elevation its condition is exhibited by *C*, in which the greater height of the rod causes a greater amount of electricity to be drawn down, and the top of the rod and the bottom of the electroscope in connection with it to become more intensely negative, and hence to draw down into the leaves a portion of the natural electricity of the ball, and cause the former to diverge with positive excitement relative to the air around.

The condition of the electroscope when brought to a lower level is illustrated by *A*, in which the shortening of the conductor reduces the number of atoms on which the electricity of the earth acts, and hence those at the top are more pressed upward by their self-repulsion than in the former case, con-

sequently a portion of the natural electricity is driven into the upper ball and the leaves themselves diverge with a negative charge: the condition being opposite to that shown at *C*. The writer had the pleasure in 1837 of witnessing this interesting experiment as performed on a dry clear day by Peltier himself.

In order that the result may be shown with a slight change of elevation it is necessary that a large ball be employed, so that the effect may be multiplied by all the electricity of the greater surface. When the electroscope is terminated with the point of a fine needle, (though this is the best means of attracting electricity from the air at a distance,) no effect will be exhibited, provided the weather is dry and the sky cloudless.

From these experiments it appears evident that the positive electricity with which the air is apparently always charged in dry and clear weather, is not due to the free electricity of the atmosphere, but to the induction of the earth on the conducting materials of which the instruments are in whole or in part composed.

It is not difficult to deduce from the same general principles the apparent changes in the electrical state of the atmosphere at different times of the day and in different hygrometrical conditions of the air. Vapor of water mingled with the atmosphere renders the latter a positive conductor; and when the moisture of the air extends up as high as the upper part of the apparatus in Fig. 16, feeble negative electricity will by slow conduction be diffused through the adjacent strata, which acting upon the ball *a* will lessen the effect of the more intense action of the earth. While the latter tends to draw the natural electricity of the conductor down into its lower part and to render the upper end negative, the vapor around the ball will tend to draw it slightly upward and thus diminish the effect, and lead the casual observer to suppose that the air is less positively electrified. Peltier in this way has shown (as well as Quetelet and Dellman) that the variations of the electricity of the atmosphere observed from day to day, and at different times in the

twenty-four hours, correspond inversely with the variations in the amount of vapor.

The experiments we have thus far described are intended to establish the inductive character of the atmosphere in its condition of dryness and serenity, particularly during clear and cold weather.

We have employed movable conductors terminated by balls which have been of the most favorable form and relative dimensions to exhibit the effects of induction. The apparatus usually employed before the experiments of Peltier, were principally stationary insulated conductors terminated by points above, which as we have seen act powerfully in discharging electricity from a body, or in absorbing it from the surrounding medium.

If in the experiments with the apparatus, Fig. 16, the rod be terminated by a point instead of a ball, but feeble excitation will be observed during clear, cold weather, because the point exhibits so exceedingly small a surface that but very little electricity can be drawn down into the lower end before the intensity of attraction of un-saturated matter upwards comes into an equilibrium with the attraction of the earth downwards. With this instrument the observer would probably make a record to the effect that the electricity of the atmosphere was very feeble, whereas if the experiment were made with the apparatus previously described an opposite condition would be noted. But the result would be entirely different if the air were damp, and the insulated rod elevated to a considerable height: the negative intensity of the upper end would be sufficient to attract a portion of the natural electricity from the surrounding medium, even although this had become slightly negative by the previous induction of the earth. In this case the pointed conductor would indicate a large amount of electricity.

The intensity of the induction may even become so great as to absorb a portion of the natural electricity of the dry atmosphere as in the case of a very long wire, the upper end of which is furnished with a series of points, and raised to a great height by means of a kite. The points may attract

a portion of the natural electricity of the air, and thus produce at the lower end of the wire a series of sparks following each other, after the lapse of a certain time, at regular intervals.

From the foregoing it will be evident that in interpreting the indications of the two classes of instruments we have described, (which may be denominated those of induction and those of absorption,) we must keep constantly in view the principles that have been explained; and it is for want of a clear appreciation of these principles that so much complexity has been introduced in describing the otherwise comparatively simple effects of induction.

Electricity of the clouds.—The explanation of the thunder-storm and the tornado given by Peltier does not appear to us as satisfactory as could be desired. In common with most of the meteorologists of Europe, he fails to take into consideration the real character of the storm, which as we think has been fully established by theory and observation in this country, as consisting in the rushing up of the lighter air to restore the normal equilibrium of the atmosphere, disturbed or rendered unstable by the gradual introduction next to the ground of a stratum of warm and moist air. As an illustration of this disturbance, we may mention the fact pointed out to Arago, by Captain Hessard, as observed by him in the Alps, namely that during great heats there take place suddenly at the lowest stratum of clouds, upward rushings, extending vertically like rockets.

We shall endeavor to supply the deficiency we have mentioned in the exposition of Peltier, and to present on the principles of the induction of the earth in connection with the upward motion of the air, a logical explanation of the origin and continued supply of the great quantity of electricity developed in the meteors under consideration.

It follows from the principles of induction, that the upper end of all perpendicular insulated conductors must be electrified negatively, and the lower end positively, since the attraction of the un-saturated matter of the earth below will draw down the natural electricity of the conductor into its lower ex-

tremity, leaving a deficiency in the upper part. Now if we admit (agreeably to the theory of Mr. Espy,) that a cloud results from the upward motion of a mass of moist and heated air, the vapor of which is condensed as it ascends into the colder regions, thus forming a high perpendicular column of partially conducting material, it will be evident that by induction, the upper part of this cloud will become negatively electrified, and the lower part positively, as in the case of the conductor, Figure 15. The intensity of this excitement will depend upon the length of the vertical dimensions of the cloud, (which in many cases is exceedingly great,) and also upon the density, and consequently the conducting power of the vapor. The induction of the earth being very intense, a partial excitement of the atoms of vapor may take place even before the condensation of the whole mass has reached its maximum. If this be the case, a transparent mass of vapor, or that which is merely beginning to condense into cloud, will be electrified throughout its entire mass; and when the condensation of the vapor has gone so far as to render the interior a tolerably good conductor, the electricity of each atom will be repelled to the surface, as in the case of a globular conductor; the intensity will thus be highly increased, and while the rushing upward of moist air is going on, a series of discharges will take place between the upper and lower portions of the cloud.

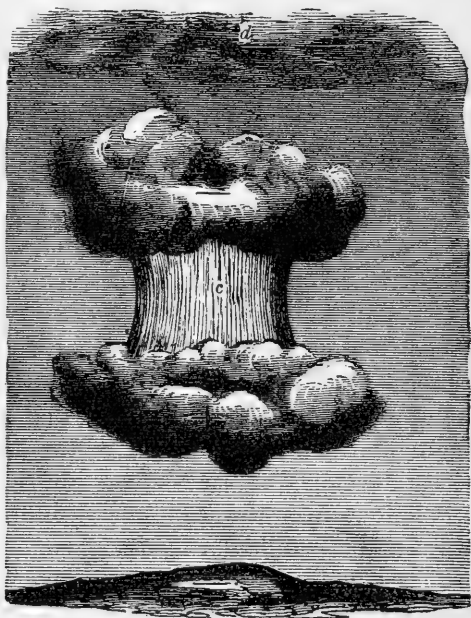


FIG. 19.

It is asserted by Mr. Wise that the thunder-cloud, when viewed on one side from a sufficient elevation, presents the appearance of an hour-glass, the upper and the lower ends spreading out almost into two distinct clouds, as seen in Figure 19.

We find that the same form of the thunder cloud has been described by other aerial voyagers, also by Volta; and we are inclined to consider it the usual one presented by this meteor, since it is precisely that which would be produced by the self-repulsion of the upper and lower parts of the cloud, each charged as it is throughout its mass with the same kind of electricity. The middle of the perpendicular dimensions of the cloud as illustrated by the perpendicular conductor, Figure 15, will be neutral, and hence no tendency to bulge out at this point will exist. Mr. Wise also states that flashes of sheet lightning are constantly seen at *c*, in the middle space; and sometimes intense discharges from the upper to the lower part of the cloud;—appearances in exact conformity with the views here presented.

The immense number of discharges of lightning from a single thunder-cloud in its passage over the earth, through a distance in some cases of more than 500 miles, indicates a constant supply of electricity; and this is found in the continued rushing up of new portions of moist air, and in the successive renewals of the perpendicular column with fresh materials, the electrical equilibrium of which is disturbed by induction.

In the case of a tornado or water-spout, the ascending current of air is confined to a very slender column, in which the action is exceedingly intense; and since it is scarcely possible that the rushing in from all directions of the air below to supply the upward spout can be directed to precisely the same central point, a whirling motion must be produced. This will tend to limit the diameter of the spout, and to create a partial vacuum at the axis of the column, in which the moist air will have its vapor condensed by the cold of the sudden expansion, and a conductor will thus be formed extending from the cloud to the earth. Through

this conductor a constant convective discharge of electricity will take place, and all the phenomena described by Dr. Hare will be exhibited.

In this view of the nature of the tornado or water-spout, although we adopt with Franklin and Espy, as the characteristic of the commotion of the atmosphere, the rushing upward in the form of a column (on the principles of hydrostatics) of a stratum of heated and moist air which had accumulated at the surface of the ground, yet the phenomena are modified and increased in number by the great amount of electricity which must be evolved by the simple action of the continued elevation of new portions of a constant stream of moist air. Since the conductor in the case of the tornado or water-spout, extends downward near to the earth, and the discharge is continually taking place, the cloud which is spread out immediately above will be negatively electrified, and the upper portion of the cloud, as exhibited in Figure 19, will be wanting. The greater or less degree of conduction of the depending spout will vary the phenomena and give rise to the different appearances which have been seen at the surface of the water. When the conductor does not quite reach to the earth visible discharges of electricity will be exhibited, and the surface of water will be attracted upward. When the conducting material of the spout touches the surface of the water, the liquid will be depressed.

That the rushing up of the air with intense violence does take place in the column of a land or water-spout is abundantly proved by direct observation, and that electricity cannot be the cause of this action, but is itself an effect, is proved by the fact, that since the column of moist air extends to the earth, discharges of the fluid must be made through it which would soon exhaust the cloud, were it not constantly renewed. In some instances the meteor has been known to continue its destructive violence along a narrow line of more than two hundred miles in length. To merely refer this prolonged action to a whirling motion of the air, without attempting to explain on known principles of science, the renewed energy of the rotation, is to rest satisfied with a very partial analysis of the phenomenon.

If by the action of an elevated horizontal current of air the upper part of a thunder-cloud be separated from the lower, we shall have a mass of vapor charged entirely with negative electricity, and from such a mass floating high in the atmosphere a new evaporation may take place by the heat absorbed directly from the sun. (Shown at *d*, Fig. 19.) The column of invisible vapor thus produced being a partial conductor elongated upward, the attraction of the earth will draw down a new portion of its natural electricity into the cloud from which the vapor was produced, and thus diminish its negative intensity. If now the upper end of this transparent column be condensed by the cold of the greater altitude into visible vapor, it will form a cloud of the second order of negative intensity. We shall thus have according to Peltier lower clouds intensely excited with positive electricity, clouds of medium elevation either neutral or slightly negative, and the highest cirrus clouds, which are formed by the secondary evaporation we have mentioned, strongly excited with negative electricity.

Since particles of ponderable matter similarly electrified repel each other, it is evident that the electrical state of the cloud must in some degree counteract the tendency to condensation which would result from the cold of the upper regions; and also the same action in the lower clouds will tend to prevent precipitation in the form of rain, even though the atoms of vapor are in a condition to coalesce into drops of water. It is evident also since the earth is negatively electrified, that the particles of vapor in the same state will be repelled farther from the surface, and those which are positively electrified will be drawn down. Hence, the negative clouds will tend to retain their elevated position, although they may be pressed downward by descending currents.

Negative clouds may also be formed near the surface of the earth by a detached portion of cloudy matter under a cloud more highly charged with positive electricity, which will cause the former by induction to discharge its positive electricity into the earth as well as a portion of its natural

electricity; and if the upper cloud be afterward driven away by the wind, the lower will be left highly negative.

Peltier states that he can determine from the appearance of a cloud whether it be positively or negatively charged. Clouds negatively electrified, (according to him,) are of a bluish gray color, while those which are positively charged are white and exhibit at the setting sun a red appearance.

From the foregoing considerations it must be evident that in addition to the disturbance which is produced in the atmosphere by the variations of heat and moisture we must take into account those that result from the changes in the electrical condition of the atoms of moisture. Though they may not be as important as the former, still they must modify the conditions of the general phenomena, and no theory of storms can be complete which does not include the effect of this agent.

On the principles we have developed, the discharges of lightning which are exhibited in volcanic eruptions are readily understood. The column of aqueous vapor, heated air, and other conducting materials, which sometimes rises to a great elevation from Vesuvius, must be subjected to the inductive action of the earth, and consequently the electricity of the upper end of the column, as soon as its elevation is sufficient to produce a condensation of the vapor, by the cold of the higher regions, must send down to the lower part of the column a large amount of electricity which when the length is great and the ascending stream rapid, will manifest itself in discharges of lightning.

In accordance with the same principles, thunder-storms have been artificially produced in a peculiar state of the atmosphere. About thirty years ago a farmer at Greenbush, near Albany, collected on a knoll in the middle of a field a large amount of brushwood, which was set on fire simultaneously at different points, and, burning, gave rise to an ascending column of heated air, extending to a great altitude. The air rushing in to supply the upward current assumed a rapid rotary motion, accompanied by a loud roaring and discharges of lightning of sufficient magnitude to

frighten the laborers from the field. The explanation in this case is too obvious to require a formal statement.

In the equatorial regions under a vertical sun masses of moist air are constantly rising during the daytime and producing electrical discharges to the earth. The vapor therefore which accompanies the reverse trade winds in the upper region must be negatively electrified, while the earth in the torrid zone must constantly be receiving electricity from the clouds. From this we may infer that there is a current of electricity through the earth, from the equator towards the poles and a neutralization by means of the air above, which may give rise to the aurora polaris.

Arago has described the different forms of lightning under three classes. The first class comprises the lightning which consists of a vivid luminous line or furrow, very narrow and sharply defined, the course of which is not a direct line, but is that denominated zig-zag. This peculiar form of lightning according to Moncel is referable to the effect of partial, interrupted conduction, and may be imitated by sprinkling iron filings on a plate of glass; the bifurcations of the discharge may also be referred to the same cause. The drops of rain distributed through the air perform the office of the particles of iron filings in the experiment, the repulsion of the electricity tending to separate it into different streams.

The next class consists of what is called "sheet lightning," which instead of being narrowed to bright sinuous lines, appears on the contrary to extend over immense surfaces. It not unfrequently has an intensely red tinge and sometimes a blue or violet color predominates. The color probably belongs to the flashes of lightning which take place at a great elevation, and seem to illuminate lower clouds, and thus to present the appearance of a broad flash.

We may also mention that flashes of lightning are sometimes observed in a summer evening without thunder, and known as "heat lightning." They are however merely the light from discharges of electricity from an ordinary thunder-cloud beneath the horizon of the observer, reflected from clouds, or perhaps from the air itself, as in the case of

twilight. Mr. Brooks, one of the directors of the telegraph line between Pittsburg and Philadelphia, informs us that on one occasion to satisfy himself on this point he asked for information from a distant operator during the appearance of flashes of this kind in the distant horizon, and learned that they proceeded from a thunder-storm then raging two hundred and fifty miles eastward of his place of observation.

The third class is called "globular lightning," which is remarkable (besides its peculiarity of form) for the slowness of its motion. The occurrence of this form of lightning is very rare, and were not the phenomenon well authenticated, we should be inclined to regard it as a delusion. But it does not comport with the cautious procedure of true science to deny the existence of all appearances which may not come within the prevision of what are considered as established principles. Although when facts of an extraordinary nature are related to us, they should not be received with that easy credence which might be due to less remarkable phenomena, yet after having fully satisfied ourselves of their reality, we must endeavor to collect all the facts connected with them, and to ascertain with accuracy the essential conditions on which they depend. Arago has given a number of instances of this remarkable form of the electrical discharge, the general appearance of which is that of a ball moving slowly through the air and sometimes when coming near a body, exploding with tremendous violence.

The only explanation which has been suggested for this remarkable meteor, and which at first sight appears to belong entirely to some other class of phenomena than those denominated electrical, is that which was in part suggested (I believe) by Sir W. Snow Harris. According to his hypothesis, the ball of light is the result of what is analogous to that which is known as a glow discharge, a phenomenon familiar to all who are in the habit of making electrical experiments. When a conductor connected with the earth is brought near a charged body, particularly when the air is damp, a partial silent discharge will take place, during which (although there may be no light perceptible in the space between the

two,) a glow of light will appear, attended with a hissing noise on the end of the conductor connected with the earth. Now, if we suppose that in the atmosphere between the cloud and the earth there exists a stratum or current of very dry air, while the remaining portions are in a very moist condition, and that the silent discharge from the cloud is taking place (for example) nearly perpendicularly to the earth, and passing through the dry stratum, then the partial interruption of conduction as the current of electricity passes through the dry stratum will give rise to the exhibition of light. Again if we suppose the cloud to be in motion, this appearance will travel with it, and the patch or glow of light will thus exhibit in mid-air a comparatively slow progressive motion, and disappear as if with an explosion, when a disruptive discharge takes place. This hypothesis can only be considered as an antecedent possibility, and is not presented as a full or satisfactory explanation; the phenomenon itself must be more frequently observed, and the associated condition of its appearance more minutely noted, before a definite hypothesis can be formed as to its cause.

Records of observations therefore with regard to this meteor are exceedingly desirable; they should however be made with scrupulous accuracy, and by persons accustomed to scientific investigations. We have found in examining testimony great difficulty in obtaining an accurate account of all the circumstances attending a peculiar occurrence of nature, from those who were present at the time and witnessed the phenomenon. It is astonishing how much the products of the imagination are mingled with the actual impressions made upon the senses, and how difficult it is to separate from the testimony of a witness what he actually saw and what he unconsciously infers from the previous crude conceptions of his mind, awakened at the instant by a powerful association of ideas. In the transit of the meteor which passed over a considerable portion of the United States, in November last, [1859,] a large number of persons declared that it fell in an adjoining field or in the water near by,

although it must have been at the time many miles in altitude above the surface of the earth.

Inductive action of the cloud.—A cloud formed as we have described must produce a great inductive effect on the earth beneath, and as it is borne along (from the west in this latitude) over the ground, the intensity of the electricity of the lower part must constantly vary, on account of the differing conductive capacity of the materials at or below the surface. For example, since water is a better conductor than dry earth, if the cloud is moving in a line that prolonged would cross a river, its course will frequently be changed, and in a similar way we can explain the fact that discharges of lightning more frequently fall on some places than others. Although the cloud may be impelled in the same direction by the wind, yet the attraction of the surface of the water (rendered more than naturally negative by induction,) will tend to draw it from its course. And since the induction acts at a distance through all substances, if a quantity of water or good conducting material exist below the surface of the earth, the cloud will be similarly affected. It frequently happens that when a heavy discharge of lightning passes near a house or descends along a rod, inductive effects are exhibited which are more startling than dangerous.

We have seen in the experiment described on page 693 (Fig. 9,) that an induced spark was exhibited at the edge of a large disc covered with tinfoil, in the lower story, by suddenly drawing the electricity from a similar disc in the upper part of a house. A precisely similar arrangement, but on a much more gigantic scale, is presented when a highly charged thunder-cloud is in the zenith of a building. Now if the intensity of this be suddenly diminished by a discharge to the earth, flashes of electricity and sparks from different objects within the house will be observed. The explanation of this is very easy. The free electricity of the cloud, which we may suppose to be positive, repels all the positive electricity of conductors and partial conductors into the ground, and renders them negative. They will be brought into this state very gradually however, either by the comparatively

slow approach of the cloud, or by its increase in intensity. The fluid therefore will escape into the ground without being perceptible in the form of sparks, but when the repulsion is suddenly relieved, at least in part, by a discharge of the cloud, the natural electricity rushes back and exhibits itself in flashes and sparks, and may even give shocks to persons in the vicinity. Although this sudden return of the electricity from the earth into which it has been driven, (in ordinary cases of conductors in a house supported by bad conducting materials,) is usually attended with but slight effects, yet it may under certain circumstances produce serious accidents, particularly when a person is in good conducting connection with the earth. A remarkable instance of this kind was described by Mr. Brydone, in a letter to the president of the Royal Society, in 1787.

Two laborers, each driving a cart loaded with coal, and sitting upon the front part, ascending a slight eminence, the one following the other at a distance of about twenty-four yards, as represented at *M* and *L*, Fig. 20, were conversing

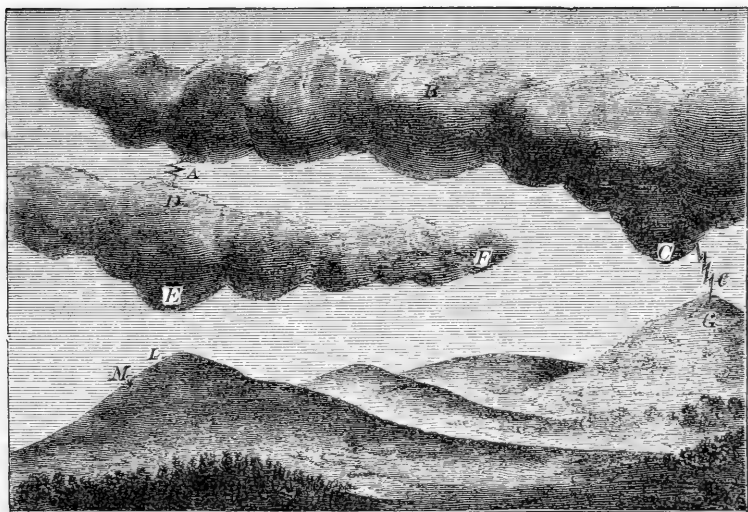


FIG. 20.

about the thunder which was heard at a distance, when in an instant the man in the hinder cart was astounded by a

loud report, and saw his companion and the two horses which he was driving fall to the ground. He immediately ran to his assistance, but found him quite dead. The horses were also killed, and appeared to have died without a struggle. The hinder cartman had the horses and driver of the forward cart full in view when they fell to the ground, but he saw no flash or appearance of fire, and was sensible of no shock or uncommon sensation. Each wheel was marked with a bluish spot on the tire, as if the iron had been subjected at that place to an intense heat, and directly under these spots were two holes in the ground, from which the earth was removed as if by an upward explosion. Flashes of lightning had been seen and thunder heard by Mr. Brydone also, who was in the vicinity at the time, but these were at the distance of five or six miles, as shown by the time elapsed between seeing the flash and hearing the thunder. There were no marks however of the exit of the discharge upwards from the body of the man or of the horses, or any effect which could be attributed to a discharge immediately from the cloud. The accident was seen by another person, from a greater distance, who was also astounded by the loud report, saw the horses and man fall to the ground, and perceived the dust arise at the place, although he observed no lightning or fire at the time. A shepherd in a neighboring field, during the same storm, observed a lamb drop down dead, and felt at the same time as if fire had passed over his face, although the lightning and clap of thunder were at a great distance from him. This happened a quarter of an hour before the accident to the cartman, and not over three hundred yards from the same spot. A woman making hay near the bank of the river close by, fell suddenly to the ground, and exclaimed to her companions that she had received a violent blow on her foot, and could not imagine whence it came.

A scientific analysis of these phenomena is given by Earl Stanhope, on principles similar to those of induction, which we shall translate into the precise language of that theory. Let us suppose a cloud eight or ten miles in length to be extended over the earth in the situation repre-

sented by $A B C$ in Fig. 20, and let another cloud $D E F$ be situated between the above-mentioned cloud and the earth. Let the two clouds be supposed to be charged with the same kind of electricity, and both positive. Let us further suppose that the lower cloud $D E F$ be only so far from the earth as to be just beyond the striking distance, and the man, cart, and horses to be at L , under the part E of the cloud which is nearest the earth. Now let the remote end C of the upper cloud approach the earth within striking distance, and suddenly discharge itself at G . The effect which would be produced by this arrangement, at the moment of the discharge C to G , will be understood by considering the condition of the electricity in the two clouds, and in the earth a moment previous to the discharge. Both clouds being positive, the two will act upon each other by repulsion, the free electricity of the lower cloud will be driven down into its lower surface, and will be accumulated particularly in the point E nearest to the earth. The ground underneath the lower cloud, and more especially at L , where the distance is least, will become highly negative. The natural electricity will be driven down into the ground by repulsion, and will be retained there as long as this condition remains, but when a discharge takes place at the point C to G , if the cloud B be a good conductor, the repulsion at A and D will be suddenly removed, and the natural electricity of the earth will return with a rush to the surface and pass beyond its point of natural equilibrium, as in this case into the man and horses. The loud report was caused by the discharge from D to A , which was invisible to the eye of the spectators on account of the density of the lower cloud.

An experimental illustration of the effects produced in this case may be readily furnished by charging two conductors, arranged in the relative position of the two clouds. At the moment a spark is drawn from the end C a discharge is observed at D to A . The death of the lamb and the shock felt in the foot of the woman were both produced according to this view by the sudden rushing up of the natural electricity of the ground, when the repulsion in the upper cloud was in part diminished by the distant discharge.

The inductive action at a distance which we have described affords a rational exposition of the effects which are perceived by persons of nervous sensibility on the approach of a thunder-storm, and may also be connected with the change which is said to take place suddenly in liquids in an unstable condition, such as the souring of milk and other substances near the point of fermentation. But whether the latter effects are due to the inductive action of the electricity or the tremor produced by the thunder, has not to our knowledge been definitely settled. If the effects are due to induction, it is probable that they would be greater in the case of milk in a metallic pan resting on the earth, than in one of glass, supported on glass legs or on a thick cake of bees-wax.

Precautions with regard to lightning.—Men have often been struck by lightning in open plains, and since the human body is a good conductor of electricity, from the principles above stated it must be evident that when standing it would be more likely to be struck than any point on the earth in the vicinity. There is less danger in a horizontal position, particularly if the person be resting on some non-conducting substance which would prevent the natural electricity from descending into the earth. Near the foot of a tall isolated tree is always considered a dangerous position, and this is in accordance not only with facts but well-established principles. The upper part of the tree being a partial conductor, particularly if covered with foliage, will become electrified by induction, will attract the discharge to itself, and in the passage of the lightning toward the earth it will act with energetic induction on all surrounding objects, and since the body of the man is a better conductor than the wood, the instantaneous inductive effect of the descending bolt will be greater on the head of a man than on the remaining part of the tree, and hence it will diverge from the line it was pursuing, break through the air, and pass through the body of the man. To attempt to explain this phenomenon by merely saying that the electricity leaves the tree because the human body is a better

conductor than the wood is to attribute to this agent pre-science and forethought, but by an application of the principles of induction, the whole is referred to the simple action of attraction and repulsion. In the interior of a house the safest position we can well imagine is that of being horizontally suspended in a hammock by silk cords in the middle of a room, and perhaps the next, that of lying on a mattress or feather bed on a wooden bedstead the materials of which are very imperfect conductors. It is scarcely necessary to say that if the bedstead be in the middle of the room, at a distance from the wall, the danger will be still less.

It may perhaps be well to dwell for a moment on the explanation of the foregoing statement. Let us suppose a man to be standing on a large piece of bees-wax, which is almost a perfect non-conductor, and exposed to a cloud highly charged with positive electricity. A portion of the natural electricity of his head would be drawn down into his feet; the former would become negatively electrified and attract the lightning of the cloud, while the latter would repel it; the tendency to be struck would be on account of the difference of these two actions. If the man stepped off the non-conducting wax on to the earth the redundant electricity which had collected in his feet would be discharged, his head would become still more negatively electrified, the repulsion which existed in the other case would disappear, while the attraction would be increased, and hence the tendency to be struck would be much greater.

Let us next consider what would take place if a man should be extended horizontally on a large disc of beeswax. In this case the upper part of the body, or that toward the sky, would become negative, and the lower part, or that in contact with the beeswax, would become positive, and the attractions and repulsions would be exhibited as in the first instance, but with less energy, because their foci would be much nearer each other, and consequently they would act with almost equal effect; while the repelled electricity not having space into which to descend, a less quantity of it would be repelled from each point of the upper surface. If

the disc of wax were placed above the man's head while in the standing position it would not screen the repulsive energy in the cloud, which like gravitation acts through all bodies; the induction would take place as before, the head would become highly negative, while the natural electricity which had been driven down would escape into the earth. The effect would therefore be the same as if the individual were standing on the earth without the intervention of the non-conducting material. A descending bolt would be attracted towards the head, and if the tenacity of the bees-wax were not sufficient to withstand a disruptive discharge, the body would be injured. From a mis-apprehension of these principles it has been supposed that the protection is increased by a slight covering over the body of silk or feathers, or by interposing a plate of glass between the sky and body; but it is well known that fowls and other large birds are struck, the slight covering of feathers affording no protection while the feet are in connection with the earth.

From the conducting capacity of the soot usually lining a chimney, and of the smoke and heated air which ascend from the flue, it will be clear that the vicinity of the fireplace during a thunder-storm is not the safest position that may be chosen in a house. A person leaning out of an open window may also not be in a very safe position, because the outside of the house, wetted with rain, will be rendered a partial conductor, and a descending charge along the wall may reach the body projecting beyond the surface. The induction is always greater where there is a large amount of conducting material, hence barns filled with damp hay will be more liable to be struck than when empty. Besides the action of induction in this case, it is generally supposed that the danger is increased by the ascent of vapor from the barn at the season mentioned; and this supposition, which is in accordance with scientific principles, is apparently borne out by observation.

On the principle of the increase of induction in the collection of a large number of conducting bodies in a given space, the assemblage of persons in churches, or other places

of public meetings, increases the tendency of lightning to fall on the edifice. The inductive action will be slightly increased when the audience assumes a standing position. For a similar reason sheep which are crowded together during a storm are frequently killed by lightning. The fact has several times been noticed that when a discharge passes through a number of animals arranged in a straight line, those which are at the extremities of the row suffer most; and this has been observed even when the animals were not in immediate contact with each other, as for example a number of horses in a series of stalls. It is probable that the heated air between the horses may have served as a conducting medium, and that the effect can be referred to the increase of intensity which always takes place in the electrical discharge at the points where the air is ruptured, or where the electricity enters and passes out.

The probability of injury from lightning is slight, even in this country where thunder-storms are comparatively frequent in the summer; and though it may be well to observe proper precautions, yet on account of the small risk to which we are subjected we should not deprive ourselves of the gratification of observing and studying one of the most sublime spectacles of nature; and indeed we know of no better way of overcoming the natural dread which many persons have of this meteorological phenomenon than by becoming interested in its scientific principles, and in studying, in connection with these, its appearance and effects.

Effects of the introduction of gas and water pipes.—Since the use of gas has become so general in our cities as to be considered almost one of the essentials of civilized life, a new source of danger has been introduced. Persons who repudiate the use of lightning-rods because they attract the electricity from the clouds should reject the introduction of gas—particularly in the upper stories of their dwellings, since the perpendicular pipes must act as the most efficient conductors between the cloud and the earth. We say the most efficient because they are connected below the ground with a plexus of pipes, in many cases of miles in extent, the whole of which

is rendered highly negative by the induction of a large cloud ; and since this action takes place with as much efficiency through the roof of a house and the chamber floors as it does through the open air, a gas-pipe within a house, (in proportion to its height) would powerfully attract any discharge from a cloud in its vicinity.

To obviate the danger from this source, the lightning-rod which rises above the top of the building should be placed in immediate metallic contact with the plexus of gas-pipes outside the house. If as is very frequently the case, the rod is made to terminate by simple insertion of a few feet in the dry earth, while the gas-pipe is connected with miles of metallic masses, rendered highly negative by induction, the path of least resistance, or of most intense induction from the cloud to the earth, will be down the rod to some point opposite the gas-pipe, then through the house and down the pipe into the great receiver below. This conclusion, from the theory, is fully borne out by observation. On Friday evening, May 14, 1858, a house in Georgetown, D. C., was struck by lightning, and on Saturday, the next evening, another house was struck in Washington, on Seventeenth street, north of Pennsylvania avenue. The writer carefully examined the conditions and effects in both cases, and found them almost identically the same. The houses were similarly situated, with gable ends north and south, and attached to the west side of each was a smaller back building. The lightning-rod of the house at Georgetown was placed on the southern gable. It terminated above in a single point, and its lower part was inserted into hard ground, through a brick pavement, to the depth of about five feet. The lightning fell upon the point, (which it melted,) passed down the rod until it came to the level of the eaves, thence leaving the conductor, it passed horizontally along the wet clapboards to the southwest eave or corner of the house, thence down a tinned iron spout to the tin gutter under the roof of the back building, and thence it pierced the wall of the house opposite the point on the outside of the back building corresponding to the position of a gas-pipe in the interior, after which no further effects of it could be

observed. A small portion of the charge however diverged to a second gas-pipe in an adjoining room. The back building was of wood, and the passage of the charge appeared to be facilitated by a large nail. The discharge was marked throughout its course by the effects it produced: 1st, the point of the rod was melted; 2d, a glass insulating cylinder through which the upper part of the rod passed was broken in pieces; 3d, the horizontal clapboard extending from the rod to the eave was splintered; 4th, the tin of the gutters and spout exhibited signs of fusion; 5th, the plaster was broken around the hole through which the charge entered the house.

The lightning-rod of the house which was struck in Washington was placed on the north gable; the electricity left the conductor at the apex of the roof, descended along the angle of the coping and the roof, which was lined with tin, to the northwest eave of the main building, thence southward along a tin gutter until it met a perpendicular tin spout, which conducted it to a point on the outside of the back building corresponding to a gas-pipe within; it then pierced a nine-inch brick wall and struck the gas-pipe, that which was embedded in the wall of the main building, at the distance of 15 inches horizontally north of the hole which it pierced in entering the interior. A lady was sitting with her back toward the point where the discharge entered the gas-pipe, at the distance of 18 inches, and though she was somewhat stunned at the time, and perceived a ringing sensation in her ears for some time after, she received no permanent injury.

At the last meeting of the American Association, Professor Benjamin Silliman, Jr., described two instances of a similar character, in which the discharge from the cloud struck twice, in different years, the lightning-rod of the steeple of a church in New Haven, left the conductor and entered the building, to precipitate itself on the gas-pipes of the interior. The remarkable fact was stated in connection with this occurrence, that the joinings of the gas-mains under the street on the outside of the building were loosened, apparently by the mechanical effect of the discharge, and

the company was obliged to take them up and repair the damage to prevent the loss of gas. An occurrence of this kind might perhaps lead the proprietors of gas-works to object to the proposition of connecting the end of the rod with their mains; but they should recollect that if means be not furnished to prevent the danger consequent upon the use of gas, a less amount of the article will be consumed; and furthermore that giving more efficiency to the inductive action of the rod on the cloud by the connection we have proposed, the tendency to a discharge will be lessened; and finally, that if the connection be not formed, the discharge from the cloud will itself find the main through the gas-pipes within the house.

There is another source of danger of a similar character in cities supplied with water from an aqueduct; the pipes in different stories of the buildings, connected with the water mains which under-lie the city, in most intimate connection with the earth, are subject to a powerful induction from the cloud above, and therefore will attract any discharge which may be passing in their vicinity, or even determine the point at which the rupture of the stratum of air between the cloud and the house shall take place. In this case the lightning-rod should also be connected with the pipes under ground, in order that the induction through the rod should be as perfect as possible, and that the consequent attraction may confine the charge and transmit it entirely to the large mains, and from them to the earth. Houses are sometimes supplied with water from the roof, collected in tanks in the loft, whence it is distributed by pipes to different parts of the building. This arrangement also tends to invite the lightning in proportion to the perpendicular elevation of this system of conductors. The lower ends of these are not usually in very intimate connection with the earth, and therefore a less powerful induction takes place than in the other instances we have mentioned. They should however be placed as in the preceding case in good metallic connection with the lightning-rod on the outside of the house. The same remark applies to steam and hot-water pipes used for heating large buildings.

The different sides of a building are not all equally exposed to accident from lightning. Thunder-clouds in this latitude approach us from the southwest, and hence the part of the house which faces this direction is not only more exposed to the fury of the storm, but also to the effects of the electrical discharge. The position then of the lightning-rod on this account is not to be neglected. The soot which lines a chimney is a good conductor, and hence the discharge not unfrequently passes into the house along the interior surface of this opening. But there is another circumstance which renders the chimney still more liable to be struck, namely, the column of heated air and smoke which ascends from it into the atmosphere when there is a fire burning below. These are tolerably good conductors of electricity, and as the latter may under some conditions extend to a considerable height in the atmosphere, they are sufficient to attract the descending discharge and determine its course to the chimney. A rod should therefore be placed on every chimney through which a column of heated air ascends during the season of the occurrence of thunder-storms.

Among the many novel propositions urged upon the attention of Congress there was one a few years ago with results having a bearing on this subject. For the purpose of lighting the public grounds an appropriation was made to erect a mast eighty feet in length on the top of the dome of the Capitol. This mast was surmounted by a lantern of about six feet in height and of corresponding diameter, containing a large number of gas-burners, and terminated above by a gilded copper ball of about a foot in diameter. After this gigantic apparatus had been erected in defiance of all the principles of architecture and illumination, the author of this report was called upon for his opinion as to the effect of lightning upon it. The answer given was that since the simplest method of obtaining electricity from the atmosphere is to elevate a piece of burning tinder on the end of a fishing-rod, the apparatus placed on the dome of the Capitol would be a collector of electricity on an immense scale, and therefore would probably be struck by lightning. As if to verify

this prediction, on the occurrence of the first thunder-storm the apparatus received a discharge from the cloud, which fused several holes in the upper part of the ball and indented the surface, but fortunately did no damage to the building. The apparatus was then removed, and the ball deposited in the museum of the Smithsonian Institution as an interesting illustration of the chemical and mechanical effects of a discharge of lightning.

Effects of telegraph wires.—In 1846, the Hon. S. D. Ingham, of Pennsylvania, requested the opinion of the American Philosophical Society as to whether security in regard to accidents from lightning is increased or lessened by the erection of telegraph wires, the poles of which are placed by the side of the roads along which persons with horses and carriages are constantly passing. The subject was referred to the writer, from whose report in regard to it the following facts and deductions are given.* The wires of a telegraph are liable to be struck by a direct charge from the clouds, and several instances of this kind have been observed. About the 20th of May, 1846, the lightning struck the elevated part of the wire which is supported on a high mast where the wire crosses the Hackensack river. The fluid passed along the wire each way from the point which received the discharge for several miles, striking off at regular intervals down the supporting poles. At each point where the discharge took place along a pole a number of sharp explosions were heard in succession, resembling the rapid reports of several rifles. During another storm the wire was struck in two places on the route between New York and Philadelphia. At one of these places twelve poles were struck and at the other eight. In some instances the lightning has been seen coursing along the wire like a stream of light, and in one case it is described as exploding from the wire in several places, though there were no bodies in the vicinity to attract it from the conductor.

That the wires of the telegraph should be frequently struck

* [Proceedings Am. Phil. Society, June 19, 1846, see *ante*, vol. I, p. 244.]

is not surprising when we consider the great length of the conductor, and consequently the many points through which it must pass along the surface of the earth peculiarly liable to receive the discharge from the heavens. Besides this, from the great length of the conductor, its natural electricity, driven to the farther end or ends of the wire, will be removed to a great distance from the point immediately under the cloud, and hence this will be rendered more intensely negative and its attractive power thereby highly increased. It is not probable however that the attraction, whatever may be its intensity, of so small a wire as that of the telegraph can of itself produce an electrical discharge from the heavens, although if the discharge were started from some other cause, (such as the attraction of a large mass of conducting matter in the vicinity,) the attraction of the wire might be sufficient to change the direction of the descending bolt and draw it, in whole or in part, to itself. It should be recollected also that on account of the perfect conductivity of the wire, a discharge on any one point of it must affect every other part of the connected line although the whole may be several hundred miles in length.

That the wire should give off a discharge to a number of poles in succession is a fact that might have been anticipated, since the electricity would by its self-repulsion tend to send a portion of itself down the partial conducting pole, while the remaining part, attracted by the wire in advance of itself, rendered negative by induction, would continue its passage along the metal until it met another pole, when a new division of the charge would take place, and so on. The several explosions in succession, heard at the same pole, are explained by the fact that the discharge from the cloud does not generally consist of a single wave of electricity, but of a number of discharges in the same path in rapid succession, so as in some cases to present the appearance of a continuous discharge of a very appreciable duration; and hence the wire of a telegraph is capable of transmitting an immense quantity of the fluid thus distributed in time, over a great length of the conductor.

From the foregoing in regard to the direct discharge, we think the danger to be apprehended from the electricity leaving the wire and striking a person on the road is small. Electricity of sufficient intensity to strike a person at the distance of twenty feet from a perfectly insulated wire would in preference be conducted down the nearest pole. It will however in all cases be most prudent to keep at a proper distance from the wire during the existence of a thunder storm, or even at any time when the sound of thunder is heard in the distance.

In case of wires passing through cities and attached to houses they should be provided at numerous points with electrical conductors to carry off the discharge to the earth. These consist of copper wires intimately connected with the earth by means of a plate of metal at the lower end, extending up the pole or side of the house, and terminating in a flat plate above, parallel to another plate of metal depending from the wire of the telegraph. The two plates are separated by a thin stratum of air, or some other non-conducting material, through which the intense discharge from the clouds will readily pass and be conducted to the earth, while the insulation of the wire for the purposes of the telegraph is un-impaired.

There are other electrical phenomena connected with the telegraph which, though frequently annoying to the operator, are not attended with the same degree of danger to his person. These are immediately referable to induction at a distance, and consist entirely in the disturbance of the natural electricity of the wire. Suppose a thunder cloud to be driven by the wind in such a direction as to cross at right angles, for example, the middle of a long line of telegraph wire. During the whole time the cloud is approaching the point of its path directly above the wire, the repulsion of the redundant electricity of the former will constantly drive the natural electricity of the latter farther and farther along the line, so that during the approach of the cloud a continuous current will exist in each half of the line. When the centre of action of the cloud arrives at the nearest point of the wire

the current will cease for a moment, and as the repulsion gradually diminishes by the receding of the cloud the natural electricity of the wire will return to its normal condition by a current opposite to that which was first manifested. Since the thunder clouds over the greater portion of the United States move from west to east, lines in a north and south direction are more liable to currents of this class, which may be denominated those of statical induction.

There is another class of currents which although they continue but for an instant are more intense than the preceding, giving rise to vivid sparks, and are due to the dynamic induction at a distance of a discharge from a cloud to a cloud, or from a cloud obliquely to the earth.

The greatest intensity is produced when the path of the lightning is parallel to the line of the telegraph, and in this case, under favorable circumstances, sparks and shocks may result from a discharge between two clouds at the distance of several miles. In these inductive actions there is no transfer of the electricity from the cloud to the wire, but simply the disturbance of the natural electricity of the conductor by the repulsive energy exerted at a distance. As already stated, nothing screens this induction; for like magnetism and gravitation, it acts as freely through the roof of a house, the air, and all other non-conducting materials as it probably would do through void space. A similar result is produced on long lines of railway, and sparks have been observed at the joining of the rails not in perfect metallic connection, particularly at the turn-tables.

The electrical telegraph is sometimes disturbed by other influences. It is evident from what we have said in reference to elevated bodies, that if a line of wire extends over a high hill the intensity of electricity will be greater at the high points than below, particularly during the occurrence of fogs; the wire will tend to absorb the electricity of the air, and transmit it from the higher to the lower portions; also during the fall of rain and snow on one portion of a long wire while clear weather exists at another, there would be a current of electricity observed in the intermediate portion.

During very warm weather a feeble current is observed at different periods of the day, which may be referred to thermoelectricity. It is well known that when one end of a long conductor is heated and the other cooled, a current of electricity will pass from the hotter to the colder extremity, and this will be continued as long as the difference of temperature exists. Extended lines in a north and south direction are most favorably situated for observing a current of this class. Currents of electricity of sufficient intensity to set fire to pieces of paper, have also been observed in connection with the appearance of the aurora borealis

Means of Protecting Buildings.

Although much has been written and said in disparagement of the admirable invention of our illustrious countryman, Franklin, yet an attentive consideration of all the facts, even independent of theory, fully establishes its great importance.

1st. It is well known, from general experience, that lightning directs itself to the most elevated portions of edifices. Cotton Mather declares that lightning is under the immediate direction of the "Prince of the powers of the air," because church steeples are more frequently struck than any other objects. It is therefore evident that the preservative means, whatever they may be, should be applied to the upper portions of a building.

2d. If other conditions be the same, lightning directs itself in preference—to metals. When therefore a mass of metal occupies the more elevated portion of a house we may be nearly certain that lightning, if it falls upon the building, will strike that point.

3d. Lightning when it enters a metallic mass does mischief only where it quits the metal, and in the vicinity of the point at which it issues. A house therefore entirely covered with metal would be safe, provided this covering were intimately connected with the ground by metallic conductors of sufficient size. When there are upon the roof or in any of the upper stories of an edifice several dis-

tinct metallic masses completely separated from each other, it will be difficult to tell which of them will be struck in preference. The safest practice is to unite all these masses by rods or bands of iron, copper, or other metal, so that each of them may be in metallic communication with a rod which may transmit the lightning to the damp earth.

"We thus deduce from facts established by observation alone without borrowing anything from theory," says Arago, "a simple, uniform, and rational means of protecting buildings from the effects of lightning. But when we refer, in addition to these facts, to the precise principles or laws of electrical action, as deduced from cautious and refined experiments in the laboratory, we are enabled to give rules for the protection of buildings which, when properly observed, reduce almost to insignificance the danger to be apprehended from the ordinary occurrences connected with the terrific exhibitions of thunder-storms."

From what has been said on the principles of induction, and also on the fact of the negative condition of the earth, it will be readily perceived that the upper end of an elevated conductor must become highly negative under the repulsive energy of a positive cloud, and though it may not be sufficient in itself to cause a rupture of the thick stratum of air intervening between the cloud and the earth, yet if a discharge does take place in the vicinity of this body, it will be drawn toward it, and if the conductor extends to the earth, and is in intimate connection with the damp ground, the discharge will pass innoxiously into this great reservoir. We further know from theory as well as experiment and observation, that the intensity of attraction is increased when the conductor is terminated above in a single sharp point. Although the attraction at a distance may be greater on a metallic globe of a few feet in diameter than on a metallic point, (since the former is able to receive a greater induced charge, which by the well-known law of attraction will act as if the whole were concentrated at the centre of the sphere,) yet the intensity of action of the point and its tendency to open a passage through the air is so great that it is preferred in protecting a given circumscribed space from lightning.

The question has been agitated whether one point or a number on the same stem is to be preferred? But this question may be readily settled, provided the reason for preferring a point to a ball or a globe is legitimate, since the surface of a ball itself may be considered as made up of an infinite number of points, and therefore a number of points close together must re-act upon each other, and thus approximate in result the effect of a continuous spherical surface. In the case of three points on the same stem, the whole amount of inductive effect produced in the rod is practically divided into three parts, and is therefore less concentrated than in the case of one point; and although at a distance the effect of the three may be equally energetic, yet the one point tends more effectually to rupture the air, and open (so to speak) a passage for the discharge from the cloud.

In reference to the subject of the termination of rods by balls or points, much discussion took place on the early introduction of the invention of Franklin, and the subject was elucidated by a very ingenious experiment made by Beccaria, in 1763, which is quoted by Arago. On the roof of a church at Turin this eminent electrician erected a rod of iron insulated on one of the flying buttresses. The upper part of this rod, which was terminated by a single metallic point, was hinged a few inches below the top, so that by merely pulling a string the point could be directed horizontally, upward, or downward. When the point was pulled downward during the presence of a thunder-cloud in the zenith, the lower end of the rod gave no sparks; but when the point was suddenly directed upward, in a few moments sparks appeared. When the point was downward, the rod presented a blunt termination toward the sky; when upward a sharp point. It might be well to repeat this experiment with some slight variation in the apparatus, in order to establish or dis-prove, by direct observation, the inference from theory that a single point acts more energetically than three or four points, terminating the same rod. The substance which terminates the conductor should be such as to preserve its form when subjected to the action of the weather, and be infusible by a stroke of lightning.

The first requisite is found in the tip of an iron rod gilded, to prevent its becoming blunted by rust; but a point of this kind, though it may protect a building from the first discharge which strikes it, will be melted, and the intensity of its action thereby diminished in the case of a subsequent explosion. At the upper termination of the lightning-rod, a small cone of platinum attached to a copper socket which fits on the top of the rod, made conical for that purpose, is now usually employed. Tips of this kind are now generally offered for sale in the large cities. The quantity of platinum on them however is generally too small, since we have known them in several instances to be fused by a discharge of lightning. The point itself should be the apex of a solid cone of platinum or of a thick plate of that metal, fastened by screwing or soldering to the copper socket.

We frequently see announcements in the papers of great improvements in lightning-rods, for which patents have been obtained, and among these boasted improvements have been the application of magnetized steel points to receive the lightning; but this invention, like most of the others which have been given to the public for the same purpose, is the result of some imaginary analogy, or of sheer charlatanism. It rests upon no foundation of observation, experiment, or theory. The magnetization of a bar, so far as it has any effect, tends to cause the electrical discharge to revolve around it, and to render the iron very slightly, if anything, a less perfect conductor.

The horizontal distance from a rod to which the protecting influence extends, is a question of considerable importance. It has generally been admitted that the point of a lightning conductor protects a horizontal circular space with a radius equal to twice its own height; that is, if the elevation of a rod above a flat roof be ten feet, it will protect a circular space of twenty feet radius, or forty feet diameter. But this rule cannot always be depended upon; for although it may be true in regard to buildings of stone or brick, with an ordinary sloping roof covered with tiles or slate, it would scarcely hold good if considerable masses of

metal formed part of the building or the roof. Observations have been recorded of parts of houses being struck within the limit just mentioned as that of protection; but scarcely any of them are satisfactory in determining the point, since it appears from the evidence that in several cases there were separate masses of metal which formed independent conductors, and in the other cases there was no evidence that the rod was in proper connection with the earth. In order to protect an extensive building, it will evidently be necessary to arm it with several lightning-conductors, and the less their height, the greater must be their number.

In the case of a tall steeple, it may be well to establish points at different elevations, by branches from the main rod; for if it be true that the rod merely attracts the lightning which has been determined by the earth itself, or some material under the ground, the discharge in its passage along the line of least resistance to the point at which it was aimed, may not be made to deviate from its direct course by the attraction of the distant elevated point, and may strike a lower portion of the building. Suppose for example a thunder-cloud is on the west side of a high steeple, and the point of attraction, which may be damp earth, a pool of water, or other conducting material on the surface or under the ground at the east end of the church: the discharge from the cloud, in its passage to the point of attraction, may strike a lower portion of the building, the action of the elevated point not being sufficient to deflect it from its course. This inference is in accordance with actual observation. Mr. Alexander Small wrote to Franklin, from London, in 1764, that he had seen in front of his window a very vivid and slender lightning discharge pass low down, without a zig-zag appearance, and strike a steeple below its summit.

It becomes a matter of interest to ascertain whether the action of an assemblage of conductors, such as is usually found in cities, produces any sensible effect in diminishing the electrical intensity of the cloud, or in other words whether their united influence produces any sensible diminution of the destructive effects of thunder-storms. Late researches

have shown that but a comparatively small amount of development of electricity is sufficient to produce great mechanical effects. Faraday has even asserted that the quantity of electricity necessary to de-compose a single grain of water, (and consequently the electricity which would be evolved by the re-composition of the same elements) would be sufficient to charge a thunder cloud, provided the fluid existed in the free state in which it is found at the surface of charged conductors. A similar inference may be drawn from the great amount of electricity developed by the friction of the small quantity of water existing in steam, as the latter issues through an orifice connected with the side of the boiler. We also find that an iron rod of three-fourths of an inch in diameter, is of sufficient size to transmit to the earth without any danger to surrounding objects a discharge from the clouds, which may be attended with a deafening explosion and with a jar of thunder powerful enough to shake the building to its foundation.

The intrepid physicist, De Raumer, sent a kite up into the air to the height of 400 or 500 feet, in the cord of which was inserted a fine wire of metal. During a thunder-storm he drew from the lower extremity of the cord not mere sparks but discharges nine or ten feet long and an inch broad.

Beccaria erected a lightning-rod which was separated in the middle by an opening, the upper part being entirely insulated. During thunder-storms intense discharges darted incessantly through the opening. So constant were these that neither the eye nor the ear could readily perceive the intermission.

"No physicist," says Arago, "will contradict me when I say that each spark taken singly would have given a shock attended with pain, that ten sparks would have numbed a man's arm, and a hundred would have proved fatal. Now a hundred sparks passed in less than ten seconds, and hence in every ten seconds there was drawn from the cloud a quantity of electrical energy sufficient to kill a man, and six times as much in every minute." Arago calculates in this way that all the lightning conductors of the building in

which the experiment was tried took from the clouds as much lightning as would have been sufficient in the short space of an hour to kill upwards of three thousand men. From the foregoing facts and conclusions we may infer that the lightning-rods of a city have considerable effect in silently discharging the clouds, and in preventing explosions which would otherwise take place; but we must recollect that on account of the upward rushing of the moist air, the electricity of the cloud is constantly renewed.

We cannot suppose that the sparks observed by Beccaria in his experiment, and the ringing of bells by Franklin, were due entirely to the electricity immediately received from the cloud. By the powerful induction of the redundant electricity of the latter, and the negative action of the earth beneath, the natural electricity of the top of the rod would be forced down into the earth, the point would become intensely negative, and in this condition would draw from the air around streams of electricity, and in this way a large volume of air around the top of the rod would become negatively electrified; and in case a discharge of lightning took place its first effect would be to neutralize or fill up, as it were, this void of electricity in the large mass of air surrounding and above the top of the rod, before the remainder of the discharge could pass to the earth. The peculiar sound which is heard when a discharge from a thunder-cloud is transmitted through a lightning-rod may possibly be attributed to this cause.

The Smithsonian building, with its high towers, situated in the middle of a plain, at a distance from all other edifices, is particularly exposed to discharges of lightning, and we have reason to believe that in as many as four instances within the last ten years the lightning has fallen upon the rods and been transmitted innoxiously to the ground.

In two of the instances the lightning was seen to strike the rod on one of the towers; in a third, a bright spark due to induction and attended with an explosion as loud as that of a pistol was perceived; and in the fourth instance, although the platinum top of the rod, which was one hundred and fifty

feet from the surface of the ground, was melted, the discharge was transmitted to the earth without any other effect than a slight inductive shock given to a number of persons standing at the foot of the tower. In three of these cases the peculiar sound we have mentioned was observed;—first, a slight hissing noise, and afterward the loud explosion, as if the former were produced by the effect of the discharge on the air in the immediate vicinity of the rod, and the loud noise from that on the air at a more distant point of its path.

The writer was led to reflect upon this effect of the rod by a remarkable exhibition he witnessed during a thunder-storm at night in 1856. He was in his office, which is in the second story of the main tower of the Smithsonian edifice, when a noise above, as if one of the windows of the tower had been blown in, attracted his attention: an assistant who was present was requested to take his lantern and ascertain what had happened. After an absence for some time he returned, saying he could discover nothing to account for the noise, but that he had heard a remarkable hissing sound. The writer then ascended to the top of the tower, and stood in the open trap-door with his head projecting above the flat roof within about twelve feet of the point of the lightning-rod. No rain was falling, though an intensely black cloud was immediately overhead and apparently at a small elevation; from different parts of this, lightning was continually flashing, indeed the air around the top of the tower itself appeared to be luminous. But the most remarkable appearance was a stream of light three or four feet long issuing with a loud hissing noise from the top of the lightning-rod. It varied in intensity with each flash, and was almost continuous during the observation. Although the whole appearance was highly interesting, and produced a considerable degree of excitement, yet the writer did not deem it prudent to expose himself to the direct or even inductive effect of a discharge under such conditions, thinking as he did with Arago, that however our vanity might prompt us to boast of the acquaintance of some great lords of creation, it is not always desirable to seek their presence or court

much familiarity with them. The effect of the rod in this case on the surrounding air and on the cloud itself by invisible induction must have been quite remarkable.

Action of lightning-rods.—The question as to whether the lightning-rod actually attracts the electricity from a distance has been frequently discussed. "It will be found," says Sir W. Snow Harris, "that the action of a pointed conductor is purely passive. It is rather the patient than the agent; and such conductors can no more be said to attract or invite a discharge of lightning than a water-course can be said to attract the water which flows through it at the time of heavy rain." This statement does not, as it appears to us, present a proper view of the case. From the established principles of induction, it must be evident that all things being equal a pointed rod, though elevated but a few feet above the ground, would be struck in preference to any point on the surface, and the propositions as to the space which can be protected from a discharge of lightning are founded on the supposition that the direction of the discharge can be changed by the action of the rod at a distance and the bolt drawn to itself. The true state of the case appears to us to be as follows:

1st. An elevated pointed rod, erected for example on a high steeple, by its powerful induction diminishes the intensity of the lower part of the cloud, and therefore may lessen the number of explosive discharges to the earth.

2d. If an explosive discharge takes place from the cloud due to any cause whatever, it will be attracted from a given distance around to the rod, and transmitted innoxiously to the earth.

A too exclusive attention to either one or the other of these actions has led to imperfect views as regards the office of the lightning-rod. On the one hand, some have considered that the whole effect of the rod is to lessen the number of discharges in the way described, and have considered it impossible that an explosive discharge could take place on a pointed conductor. But this is not the case, as was shown by Mr. Wilson many years ago by his experiments

in London. It is true, that when a needle is presented to a charged conductor, the electricity is drawn off silently without an explosion, and this is always the case if sufficient time be allowed for the electricity to escape in this way. But if the point be suddenly brought within striking distance of the conductor by a rapid motion, such as would be produced by the movement of a horizontal arm carrying the point immediately under the conductor in an instant, an explosive discharge will take place. In this case sufficient time is not given for the slower transmission of the electricity by what has been denominated the glowing discharge, and a rupture of the air is produced as in the action of a conductor terminated by a ball.

It would follow from this that in the case of a rapidly-moving cloud across the zenith of a rod, there would be a greater tendency to an explosive discharge on the point than when the cloud was nearly stationary. For a similar reason, if a point connected with the earth by a wire be directed toward an insulated conductor, and the latter be suddenly electrified by a discharge from a second conductor, an explosion will take place between the first conductor and the point. A similar effect would be produced if a lower cloud received a sudden discharge from one above it, a case which probably frequently occurs in nature. Mr. Wise informs us that when a discharge takes place from the base of a cloud to the earth, a discharge is seen to pass between the upper and lower part of the cloud. (A condition shown in Fig. 19.) We are warranted from the foregoing facts, as well as from the numerous examples in which lightning has actually been seen to fall upon pointed rods explosively, and the number of points which have been melted, to conclude that the rod under certain conditions does actually attract the lightning, though when properly constructed it transmits it without disturbance to the earth.

It has been denied by some that the point has any perceptible influence in lessening the number of strokes from a cloud, but this proposition can scarcely be doubted when we reflect upon the fact that it is not necessary to entirely

discharge a cloud in order to prevent a rupture of the air, it being only necessary to draw off a quantity of the fluid sufficient to reduce it just below that which is required to produce the explosion; and for this effect there may be required but a very slight diminution in the intensity of a cloud which is at about the striking distance, to prevent an explosion, particularly when we consider the prodigious number of sparks which during thunder-storms were silently withdrawn from the cloud by the pointed rod erected by Beccaria.

Arago has collected a large number of instances, from which it appears that the erection of a rod lessened the number of the explosive discharges.

The Campanile of St. Marks, at Venice, from the multitude of the pieces of iron in its construction, was in a high degree exposed to danger from lightning, and in fact prior to 1776, had been known to be struck nine times. In the beginning of that year a conductor was placed upon it, and since that time the edifice has been un-injured by lightning.

Previous to 1777, the tower of Sienna was frequently struck, and on every occasion much injured. In that year it was provided with a conductor, and has since received one discharge, but with no damage.

In the case of a church at Carinthia, on an average four or five strokes of lightning annually were discharged upon the steeple until a conductor was erected, after which one stroke was received in five years. At the Valentino palace the lightning conductors established by Beccaria, caused the entire disappearance of strokes of lightning which were previously of frequent occurrence.

The monument in London, although only accidentally provided with a virtual conductor, appears to have been exempt from damage by lightning for nearly one hundred and eighty years.

The action of the rod in diminishing the intensity of the cloud however, can only be of a very temporary character, and cannot, as some have supposed, affect its subsequent state, or disarm it of its fulminating power, since its elec-

tricity is constantly renewed; a fact sufficiently demonstrated by the observation that a thunder storm, through its whole course of several hundred miles in extent, continually gives discharges to the earth. Notwithstanding the instances given by Arago of the diminution of discharges of lightning after the erection of the rod, the fact is established by observation, experiment, and theory, that the rod does attract the lightning, and that it receives the discharge not alone silently, but explosively. The points of the conductors are frequently melted, and although in cases in which this occurs, the discharge passes harmlessly to the earth, yet in some instances the explosion might not have taken place had the rod not been present.

The following instructive illustration of the action of a very elevated conductor in transmitting a discharge from a thunder cloud is furnished us by Mr. Henry J. Rogers, telegraph engineer, who was himself an eye-witness of what he relates :

“In accordance with my promise I will endeavor to give you a brief description of the effect produced by atmospheric electricity at the House Telegraph mast, erected at the Palisades on the west side of the Hudson river, in the vicinity of Fort Lee, New Jersey, and distant about ten miles from the City Hall, New York, during a terrific thunder storm which occurred on Friday, June 17, 1853, between three and four o'clock p. m., while I was on an official visit.

“Before I proceed with the description it will be necessary to explain that the wires of the House and Morse telegraph lines cross the Hudson river between Fort Washington and the Palisades, inasmuch as this is the narrowest part of the river in the vicinity of New York, and the elevation of the land at the Palisades renders it a desirable place for suspending the wires from one shore to the other, so as to allow vessels of large size to pass under them free from interruption.

“The mast to support the wire was 266 feet in length, and was erected on the top of the columnar wall of the Palisades, which at this place is 298 feet above the river, as determined by trigonometrical measurement. The top of the mast was therefore 564 feet above the water, and was sufficiently elevated to allow for the unavoidable sagging of the telegraph wire, and to leave sufficient distance for vessels to pass beneath.

"It was composed of three pieces of heavy timber placed one above the other and fastened together by iron bands, to which were attached long iron braces or guys secured at the lower ends to the rock for the purpose of sustaining the mast in its perpendicular position. The braces or guys were formed of iron rods three-fourths of an inch in diameter, and painted black. The longer or outer ones, (those which were attached to the top of the mast and along which the electricity descended to the earth,) terminated about 32 paces from the lower end of the mast: they were composed of pieces of iron rod of thirteen feet in length, and each piece terminated in a bolt and shackle, thereby forming a series of links 30 in number.

"A lightning-rod six feet long, three-quarters of an inch diameter, painted white, sharpened to a point, but not tipped with platinum, and secured at its lower end to the iron band to which were attached the upper set of guys, projected about two or three feet above the truck of the mast. The point of the rod was at the time in the center of a cedar bush in full foliage which had been placed there by the riggers when they completed the mast.

"At 3 P. M., when the storm commenced, I placed myself in the railway house at Fort Washington, a point distant about three-quarters of a mile from the mast at Fort Lee, on the opposite side of the river. From my position I could distinctly observe the gust as it advanced from the southwest; and from the heat of the weather and appearance of the clouds I expected to witness heavy discharges of atmospheric electricity, and prepared my mind to observe the effects of the storm on the mast at Fort Lee, having frequently expressed a desire to witness a thunder-storm in the vicinity of the mast, as I felt assured the iron rod and guys would protect it from injury.

"As the gale increased the clouds advanced with a heavy atmosphere, and accompanied with frequent discharges of lightning and loud thunder. When it approached the mast the foremost cloud assumed the shape of an inverted cone, (similar to those I have witnessed in the Gulf of Mexico, forming a water-spout;) and I soon observed a terrific flash of lightning descend by the southern iron guy clearly defining its form and every link of the guy as though it were a rod of red-hot iron; and this appearance continued for at least four seconds, followed by three or four heavy peals of thunder in rapid succession, during which time the lightning appeared to flow in a continued stream of

fire along the iron guy, and giving off during its progress apparently as many snaps of electricity as there were links in the guy, and which I supposed to be caused by the resistance offered by each link to the free passage of the electricity.

"These discharges were succeeded by a heavy gush of rain which obstructed my view of the Palisades, but other discharges of atmospheric electricity followed as the cloud rushed on its course along the North river. The storm lasted about half an hour.

"Within 50 paces north of the mast described stood the Morse-line mast, which is about 40 feet less in height than the House mast; and during the storm there was no indication of any part of it being struck by lightning, although there is attached to it a conductor of atmospheric electricity. From this I infer that the discharge of lightning passed to the earth along the iron guys of the House mast, owing to its greater elevation, and to its being more south and thus toward the storm.

"Such was the vividness and intensity of the light which was emitted along the guy at the time of the discharge that I received the impression that the iron was melted, and expected every moment to see the mast prostrated by the wind, but was much surprised on examining the premises next day to find not the least evidence of fusion on the rod, or marks of any kind along its surface to indicate the passage of the electrical discharge.

"The Palisades in the vicinity of the mast are heavily timbered, and although the limbs of several trees are in contact with the iron guys running from the mast, not the slightest damage was done to any of these trees; but about one-fourth of a mile south of the mast a large tree was shattered by lightning during the same storm.

"The mast stood about five years, and during that time, as reported by those having charge of it, was struck at almost every violent thunder-storm that passed over the place. It was considered by persons living in the neighborhood as a protection against lightning.

"Indeed such was the confidence in it that the telegraph workmen did not hesitate to take shelter during a storm in a house 15 feet square which was built around the mast, and in which implements, windlasses, &c., were kept.

"Baltimore, November 30, 1853."

The facts presented in the foregoing narrative are highly

instructive. The descent of the visible vapor in the form of an inverted cone is a phenomenon which will be considered of special interest, particularly by those who ascribe the motive power of a tornado entirely to electricity.

The continuance of the discharge during four seconds is in accordance with other instances which have been frequently observed, and is to be attributed to a series of discharges in rapid succession through the same path.

The appearance of light along the whole course of the rods forming the guy may be attributed to the circumstance that the metal at the time of the discharge was covered with a thin stratum of water into which the electricity was projected by its self-repulsion, and on account of the imperfect conductivity of the liquid, gave rise to the phenomena observed.

This may be illustrated experimentally by discharging an electrical battery through a slip of tin foil wetted with a thin stratum of water. The discharge which would be insensible along the dry metal becomes luminous through its whole course.

While this account of Mr. Rogers clearly shows the attractive power of an elevated conductor under particular circumstances, it also proves the fact that an edifice may be protected from harm, provided it be furnished with a sufficient number of properly constructed rods.

Construction of lightning-rods.—Electricity (as we have seen—page 342,) tends to pass at the surface of a conductor of a sufficient size, but it does not follow from this that every increase of surface, the quantity of metal being the same, will tend to diminish the resistance of the conductor to the passage of a discharge. From an imperfect view of the subject, many persons have supposed that merely flattening the lightning-rod, and thus increasing the surface would tend to increase the conducting power, but it must be evident from the principle of repulsion, that in diminishing the distance between the two flat surfaces, we tend to increase the repulsion between the atoms, which would pass parallel to the axis along the middle of each flat side, and thus, though the

surface is increased by flattening a round bar, the conduction is diminished, and a greater intensity is given to the electricity at the edges, tending to increase the lateral escape of the fluid. The only proper way of diminishing the resistance to conduction in a rod of metal of a given capacity is to mold it into the form of a hollow cylinder; a gas-pipe for example will offer less resistance to conduction than the same weight of metal in the form of a solid cylinder; but we must not infer from this that a gas-pipe an inch in diameter will conduct better than a solid rod of iron of the same diameter. There is no known law of electricity which would lead us to suppose that by removing the metal from the interior of a rod, we increase its conducting capacity. On the contrary when the discharge is very great in proportion to the size of the conductor, it is probable that the discharge penetrates through the entire mass. The rod should be of sufficient size to transmit freely the largest discharge that experience has shown as likely to fall on a building. A rod of three-fourths of an inch of round iron is generally considered sufficient for this purpose, since a conductor of this capacity has in no case been found to have been fused by a discharge from the clouds. There is no objection on the score of electrical action to using a larger bar, or to the same weight of metal in the form of a hollow cylinder; indeed every increase of diameter lessens the resistance to conduction, and the tendency to give off lateral sparks.

Lightning-conductors are frequently constructed in this country with points projecting at intervals of two or three feet through their whole length; this plan has been adopted from some erroneous idea in regard to the action of the conductor, and of the proper application of points. The essential office of the conductor is to receive the discharge from the cloud, and to transmit it with the least resistance possible, silently and innoxiously to the great body of the earth below, and anything which militates against these requisites must be prejudicial. Now in the passage of the electricity through a conductor, it retains its repulsive energy, and hence each point along the rod in succession becomes highly charged, and tends to give off a spark to bodies in the neigh-

borhood. Besides this, the irregularity in the motion of the electricity which is thus produced, must on mechanical principles interfere with its free transmission. Points should therefore be omitted along the course of the rod, since they can do no possible good, and may produce injury.

We may conclude what we have said in regard to lightning-rods by the following summary of directions for constructing and erecting them :

1st. The rod should consist of round iron, of not less than three-fourths of an inch in diameter. A larger size is preferable to a smaller one. Iron is preferred, because it can be readily procured, is cheap, a sufficiently good conductor, and when of the size mentioned cannot be melted by a discharge from the clouds.

2d. It should be, through its whole length, in perfect metallic continuity ; as many pieces should be joined together by welding, as practicable, and when other joinings are unavoidable, they should be made by screwing the parts firmly together by a coupling ferule, care being taken to make the upper connection of the latter with the rod water-tight by cement, solder, or paint.

3d. To secure it from rust the rod should be covered with a coating of black paint.

4th. It should be terminated above with a single point, the cone of which should not be too acute, and to preserve it from the weather as well as to prevent melting it should be encased with platinum, formed by soldering a plate of this metal, not less than the twentieth of an inch in thickness, into the form of a hollow cone. Usually the cone of platinum, for convenience, is first attached to a brass socket which is secured on the top of the rod, and to this plan there is no objection. The platinum casing is frequently made so thin and the cone so slender, in order to save metal, that the point is melted by a powerful discharge.

5th. The shorter and more direct the rod is in its course to the earth the better. Acute angles made by bending in the rod and projecting points from it along its course should be avoided.

6th. It should be fastened to the house by iron eyes, and

may be insulated by cylinders of glass. We do not think the latter however of much importance since they soon become wet by water, and in case of a heavy discharge are burst asunder.

7th. The rod should be connected with the earth in the most perfect manner possible, and in cities nothing is better for this purpose than to unite it in good metallic contact with the gas mains or large water pipes in the streets; and such a connection is absolutely necessary if the gas or water pipes are in use within the house. This connection can be made by soldering to the end of the rod a strip of copper, which, after being wrapped several times around the pipe, is permanently attached to it. Where a connection with the ground cannot be formed in this way the rod should terminate, if possible, in a well always containing water, and where this arrangement is not practicable it should terminate in a plate of iron or some other metal buried in the moist ground. Before it descends into the earth, it should be bent so as to pass off nearly perpendicular to the side of the house, and it should be buried in a trench, surrounded with powdered charcoal.

8th. The rod should be placed, in preference, on the west side of the house, in this latitude, and especially on the chimney from which a current of heated air ascends during the summer season.

9th. In case of a small house a single rod may suffice, provided its point be sufficiently high above the roof, the rule being observed that its elevation should be at least half of the distance to which its protection is expected to extend. It is safer however, particularly in modern houses in which a large amount of iron enters into the construction, to make the distance between two rods less than this rule would indicate rather than more. Indeed we see no objection to an indefinite multiplication of rods to a house, provided they are all properly connected with the ground and with each other. A building entirely enclosed, as it were, in a case of iron rods so connected with the earth would be safe from the direct action of the lightning.

10th. When a house is covered by a metallic roof the latter

should be united in good metallic connection with the lightning-rods; and in this case the perpendicular pipes conveying the water from the gutters at the eaves may be made to act the part of rods by soldering strips of copper to the metal roof and pipes above, and connecting them with the earth by plates of metal united by similar strips of copper to their lower ends, or better with the gas or water pipes of the city. In this case however the chimneys would be unprotected, and copper lightning-rods soldered to the roof and rising a few feet above the chimneys would suffice to receive the discharge. We say soldered to the roof, because if the contact were not very perfect, a greater intensity of action would take place at this point, and the metal might be burnt through by the discharge, particularly if it were thin.

11th. As a general rule large masses of metal within the building, particularly those which have a perpendicular elevation, ought to be connected with the rod. The main portion of the great building erected for the world's exhibition at Paris is entirely surrounded by a rod of iron from which rises at intervals a series of lightning conductors, the whole system being connected with the earth by means of four wells, one at each corner of the edifice.

The foregoing rules may serve as general guides for the erection of lightning-rods on ordinary buildings, but for the protection of a large complex structure, consisting of several parts, a special survey should be made, and the best form of protection devised which the peculiar circumstances of the case will admit.

Numerous patents have been obtained in this country for improved lightning conductors, but as a general rule such improvements are of little importance.

Such assumed improvements on the form of the lightning-rod recommended by the French Academy in 1823 would pre-suppose some important discoveries in electricity having a bearing on the subject; but after the lapse of thirty years the same Academy being called upon to consider the protection of the new additions to the Louvre finds nothing material to change in the principles of the instructions at first given.

ON ACOUSTICS APPLIED TO PUBLIC BUILDINGS.

(Proceedings American Association Adv. of Science, vol. x, pp. 119-135.)

August 22, 1856.

At the meeting of the American Association in 1854, I gave a verbal account of the plan of a lecture-room adopted for the Smithsonian Institution, with some remarks on acoustics as applied to apartments intended for public speaking.* At that time the room was not finished, and experience had not proved the truth of the principles on which the plan had been designed. Since then the room has been employed two winters for courses of lectures to large audiences; and I believe it is the general opinion of those who have been present, that the arrangements for seeing and hearing, considering the size of the apartment, are entirely unexceptionable. The room has fully answered all the expectations which were formed in regard to it, previous to its construction. The origin of the plan was as follows:

Professor Bache and myself had directed our attention to the subject of acoustics as applied to buildings, and had studied the peculiarities in this respect of the hall of the House of Representatives, when the President of the United States referred to us for examination the plans proposed by Captain M. C. Meigs of the Engineer Corps, U. S. A., for the rooms about to be constructed under his direction in the new wings of the Capitol. After visiting with Captain Meigs the principal halls and churches of the cities of Philadelphia, New York, and Boston, we reported favorably on the general plans proposed by him, and which were subsequently adopted.

The facts we have collected on this subject may be referred to a few well-established principles of acoustics, which have

* ["On the Arrangement of Lecture Rooms, with reference to Sound and Sight."—Proceedings of the American Association for the Advancement of Science, May, 1854; vol. VIII, p. 106. Only the title published.]

been applied in the construction of the Smithsonian lecture-room. To apply them generally however in the construction of public halls, required a series of preliminary experiments.

In a small apartment it is an easy matter to be heard distinctly at every point; but in a large room, unless provision be made in the original plan of the building for a suitable arrangement, on acoustic principles, it will be difficult, and indeed in most cases impossible, to produce the desired effect. The same remark may be applied to the lighting, heating, and ventilation, and to all the special purposes to which a particular building is to be applied. I venture therefore to make some preliminary remarks on the architecture of buildings, bearing upon this point, which, though they may not meet with universal acceptance, will I trust commend themselves to the common sense of the public in general.

Architectural limitations.—In the erection of a building, the uses to which it is to be applied should be clearly understood, and provision definitely made, in the original design, for every desired object.

Modern architecture is not, like painting or sculpture, a fine art, *par excellence*. The object of these latter is to produce a moral emotion,—to awaken the feelings of the sublime and the beautiful; and we greatly err when we apply their productions to a merely utilitarian purpose. To make a fire-screen of Rubens' Madonna, or a candelabrum of the Apollo Belvidere, would be to debase those exquisite productions of genius, and do violence to the feelings of the cultivated lover of art. Modern buildings are made for other purposes than artistic effect, and in them the æsthetic must be subordinate to the useful; though the two may co-exist, and an intellectual pleasure be derived from a sense of adaptation and fitness, combined with a perception of harmony of parts, and the beauty of detail.

The buildings of a country and an age should be ethnological expressions of the wants, habits, arts, and feelings of the time in which they were erected. Those of Egypt,

Greece, and Rome were intended (at least in part) to transmit to posterity, without the art of printing, an impression of the character of the periods in which they were erected. It was by their monuments that these nations sought to convey to future ages an idea of their religious and political sentiments.

The Greek architect was untrammelled by any condition of utility. Architecture was with him in reality a fine art. The temple was formed to gratify the tutelary deity. Its minutest parts were exquisitely finished, since nothing but perfection on all sides and in the smallest particulars could satisfy an all-seeing and critical eye. It was intended for external worship, and not for internal use. It was without windows, entirely open to the sky, or if closed with a roof, the light was merely admitted through a large door. There were no arrangements for the heating or ventilation. The uses therefore to which buildings of this kind can be applied in modern times are exceedingly few; and though they were objects of great beauty, and fully realized the intention of the architect by whom they were constructed, yet they cannot be copied in our day without violating the principles which should govern architectural adaptation.

Every vestige of ancient architecture which now remains on the face of the earth should be preserved with religious care; but to servilely copy these, and to attempt to apply them to the uses of our day, is as preposterous as to endeavor to harmonize the refinement and civilization of the present age with the superstition and barbarity of the times of the Pharaohs. It is only when a building expresses the dominant sentiment of an age, when a perfect adaptation to its use is joined to harmony of proportions and an outward expression of its character, that it is entitled to our admiration. It has been aptly said, that it is one thing to *adopt* a particular style of architecture, but a very different one to *adapt* it to the purpose intended.

Architecture should change not only with the character of the people, and in some cases with the climate, but also with the material to be employed in construction. The use

of iron and of glass requires an entirely different style from that which sprung from the rocks of Egypt, the masses of marble with which the lintels of the Grecian temples were formed, or the introduction of brick by the Romans.

The great tenacity of iron, and its power of resistance to crushing, should suggest for it, as a building material, a far more slender and apparently lighter arrangement of parts. An entire building of iron, fashioned in imitation of stone, might be erected at small exercise of invention on the part of the architect, but would do little credit to his truthfulness or originality. The same may be said of our modern pasteboard edifices, in which, with their battlements, towers, pinnacles, "fretted roofs and long drawn aisles," cheap and transient magnificence is produced by painted wood or decorated plaster.

Lecture-room Acoustics.—To return to the subject of acoustics, as applied to apartments intended for public speaking: While sound, in connection with its analogies to light, and in its abstract principles, has been investigated within the last fifty years with a rich harvest of results, few attempts have been successfully made to apply these principles to practical purposes. Though we may have a clear conception of the simple operation of a law of nature, yet when the conditions are varied, and the actions multiplied, the results frequently transcend our powers of logic, and we are obliged to appeal to experiment and observation to assist in deducing new consequences, as well as to verify those which have been arrived at by mathematical deduction. Furthermore, though we may know the manner in which a cause acts to produce a given effect, yet in all cases we are obliged to resort to actual experiment to ascertain the measure of effect under given conditions.

The science of acoustics as applied to buildings, perhaps more than any other, requires this union of scientific principles with experimental deductions. While on the one hand, the application of simple deductions from the established principles of acoustics would be unsafe from a want of knowledge of the constants which enter into our formulæ,

on the other hand empirical data alone are in this case entirely at fault, and of this any person may be convinced who will examine the several works written on acoustics by those who are deemed practical men.

Sound is a motion of matter capable of affecting the ear with a sensation peculiar to that organ. It is not in all cases a motion simply of the air, for there are many sounds in which the air is not concerned; for example, the impulses which are conveyed along a rod of wood from a tuning-fork to the teeth. When a sound is produced by a single impulse, or an approximation to a single impulse, it is called a noise; when by a series of impulses, a continued sound, &c.; if the impulses are equal in duration among themselves, a musical sound. This has been illustrated by a quill striking against the teeth of a wheel in motion. A single impulse from one tooth is a noise, from a series of teeth in succession a continued sound; and if all the teeth are at equal distances, and the velocity of the wheel is uniform, then a musical note is the result. Each of these sounds is produced by the human voice, though they apparently run into each other. In speaking however a series of irregular sounds of short duration is usually emitted,—each syllable of a word constitutes a separate sound of appreciable duration, and each compound word and sentence an assemblage of such sounds. It is no little surprising that in listening to a discourse, the ear can receive so many impressions in the space of a second, and that the mind can take cognizance of and compare them.

That a certain force of impulse and a certain time for its continuance are necessary to produce an audible impression on the ear, is evident, but it may be doubted whether the impression of a sound on this organ is retained appreciably longer than the continuance of the impulse itself, certainly it is not retained the $\frac{1}{10}$ th of a second. If this were the case it is difficult to conceive why articulated discourse, which so pre-eminently distinguishes man from the lower animals, should not fill the ear with a monotonous hum; but whether the ear continues to vibrate, or whether the impression re-

mains a certain time on the sensorium, it is certain that no sound is ever entirely instantaneous, or the result of a single impression, particularly in enclosed spaces. The impulse is not only communicated to the ear but to all bodies around, which in turn become themselves centres of reflected impulses. Every impulse must give rise to a forward and afterward a backward motion of a small portion of the medium.

Sound from a single explosion in air equally elastic on all sides tends to expand equally in every direction; but when the impulse is given to the air in a single direction, though an expansion takes place on all sides, yet it is much more intense in the line of the impulse. For example, the impulse of a single explosion, like that of the detonation of a bubble of oxygen and hydrogen, is propagated equally in all directions, while the discharge of a cannon, though heard on every side, is much louder in the direction of the axis; so also a person speaking is heard much more distinctly directly in front than at an equal distance behind. Many experiments have been made on this point, and I may mention those repeated in the open space in front of the Smithsonian Institution. In a circle 100 feet in diameter, the speaker in the centre, and the hearer in succession at different points of the circumference, the voice was heard most distinctly directly in front, gradually less so on either side, until in the rear it was scarcely audible. The ratio of distance for distinct hearing directly in front, on the sides, and in the rear was about as 100, 75, and 30. These numbers may serve to determine the form in which an audience should be arranged in an open field in order that those on the periphery of the space may all have a like favorable opportunity of hearing, though such a disposition should not be recommended as the form of an apartment where a reflecting wall would be behind the speaker.

The impulse producing sound requires time for its propagation, and this depends upon the intensity of repulsion between the atoms, and secondly, on the specific gravity of the matter itself. If the medium were entirely rigid sound

would be propagated instantaneously; the weaker the repulsion between the atoms the greater will be the time required to transmit the motion from one to the other; and the heavier the atoms the greater will be the time required for the action of a given force to produce in them a given amount of motion. Sound also, in meeting an object, is reflected in accordance with the law of light, making the angle of incidence equal to the angle of reflection. The tendency however to divergency in a single beam of sound appears to be much greater than in the case of light. The law nevertheless appears to be definitely followed in the case of all beams that are reflected in a direction near the perpendicular. It is on the law of propagation and reflection of sound that the philosophy of an echo depends. Knowing the velocity of sound it is an easy matter to calculate the interval of time which must elapse between the original impulse and the return of the echo. Sound moves at the rate of 1125 feet in a second at the temperature of 60°.

If therefore we stand at half this distance before a wall, the echo will return to us in one second. It is however a fact known from universal experience that no echo is perceptible from a near wall, though in all cases one must be sent back to the ear. The reason of this is that the ear cannot distinguish the difference between similar sounds, as for example, that from the original impulse and its reflection if they follow each other at less than a given interval, which can only be determined by actual experiment, and as this is an important element in the construction of buildings the attempt was made to determine it with some considerable degree of accuracy. For this purpose the observer was placed immediately in front of the wall of the west end of the Smithsonian building at the distance of 100 feet; the hands were then clapped together. A distinct echo was perceived; the difference between the time of the passage of the impulse from the hand to the ear, and that from the hand to the wall and back to the ear, was sufficiently great to produce two entirely distinct impressions. The observer then gradually approached the building until no echo or perceptible pro-

longation of the sound was observed. By accurately measuring this distance and doubling it we find the interval of space within which two sounds may follow each other without appearing separately. But if two rays of sound reach the ear after having passed through distances the difference between which is greater than this, they produce the effect of separate sounds. This distance we have called the *limit of perceptibility* in terms of space. If we convert this distance into the velocity of sound, we ascertain the limit of perceptibility in time.

In the experiment first made with the wall a source of error was discovered in the fact that a portion of the sound returned was reflected from the cornice under the eaves, and as this was at a greater distance than the part of the wall immediately perpendicular to the observer the moment of cessation of the echo was less distinct. In subsequent experiments with a louder noise, the reflection was observed from a perpendicular surface of about 12 feet square, and from this more definite results were obtained. The limit of the distance in this case was about 30 feet, varying slightly perhaps with the intensity of the sound and the acuteness of different ears. This will give about the sixteenth part of a second as the limit of time necessary for the ear to separately distinguish two similar sounds. From this experiment we learn that the reflected sound may tend to strengthen the impression, or to confuse it, according as the difference of time between the two impressions is greater or less than the limit of perceptibility. An application of the same principle gives us the explanation of some phenomena of sound which have been considered mysterious. Thus, in the reflection of an impulse from the edge of a forest of trees each leaf properly situated within a range of 30 feet of the front plane of reflection will conspire to produce a distinct echo, and these would form the principal part of the reflecting surfaces of a dense forest, for the remainder would be screened; and being at a greater distance, any ray which might come from them would serve to produce merely a low continuation of the sound.

On the same principle we may at once assert that the panelling of a room, or even the introduction of reflecting surfaces at different distances will not prevent the echo, provided they are in parallel planes, and situated relatively to each other within the limit of perceptibility.

Important advantage may be taken of the principle of reflection of sound by a proper arrangement of the reflecting surfaces behind the speaker. We frequently see in churches, as if to diminish the effect of the voice of the preacher, a mass of drapery placed directly in the rear of the pulpit. However satisfactory this may be in an æsthetical point of view, it is certainly at variance with correct acoustic arrangements, the great object of which should be to husband every articulation of the voice, and to transmit it unmingled with other impulses and with as little loss as possible to the ears of the audience.

Another effect of the transmission and reflection of sound is that which is called reverberation, which consists of a prolonged musical sound, and is much more frequently the cause of indistinctness of perception of the articulations of the speaker than the simple echo.

Reverberation is produced by the repeated reflection of a sound from the walls of the apartment. If for example a single detonation takes place in the middle of a long hall with naked and perpendicular walls, an impulse will pass in each direction, will be reflected from the walls, cross each other again at the point of origin, be again reflected, and so on until the original impulse is entirely absorbed by the solid materials which confine it. The impression will be retained upon the ear during the interval of the transmission past it of two successive waves, and thus a continued sound will be kept up, particularly if the walls of any part of the room are within 30 feet of the ear. If a series of impulses, such as that produced by the rapid snaps of a quill against the teeth of a wheel be made in unison with the echoes, a continued musical sound will be the result. Suppose the wheel to be turned with such velocity as to cause a snap at the very instant the return echo passes the point

at which the apparatus is placed, the second sound will combine with the first, and thus a loud and sustained vibration will be produced. It will be evident from this that every room has a key-note, and that to an instrument of the proper pitch it will resound with great force. It must be apparent also, that the continuance of a single sound and the tendency to confusion in distinct perception, will depend on several conditions;—first, on the size of the apartment; secondly, on the strength of the sound or the intensity of the impulse; thirdly, on the position of the reflecting surfaces; and fourthly, on the nature of the material of the reflecting surfaces.

In regard to the first of these, the larger the room the longer time will be required for the impulse along the axis to reach the wall; and if we suppose that at each collision a portion of the original force is absorbed, it will require double the time to totally extinguish it in a room of double the size, because, the velocity of sound being the same, the number of collisions in a given time will be inversely as the distance through which the sound has to travel.

Again, that it must depend upon the loudness of the sound or the intensity of the impulse, must be evident, when we consider that the cessation of the reflections is due to the absorption by the walls, or to irregular reflection, and that consequently the greater the amount of original disturbance the longer will be the time required for its complete extinction. This principle was abundantly shown by our observations on different rooms.

Thirdly, the continuance of the resonance will depend upon the position of the reflecting surfaces. If these are not parallel to each other, but oblique, so as to reflect the sound not to the opposite but to the adjacent wall, without passing through the longer axis of the room, it will evidently be sooner absorbed. Any obstacle, also, that may tend to break up the wave and interfere with the reflection through the axis of the room will serve to lessen the resonance of the apartment. Hence, though the panelling, the ceiling, and the introduction of a variety of oblique surfaces, may not pre-

vent an isolated echo, provided the distance be sufficiently great and the sound sufficiently loud, yet that they do have an important effect in stopping the resonance is evident from theory and experiment. In a room 50 feet square in which the resonance of a single intense sound continued six seconds, when cases and other objects were placed around the wall its continuance was reduced to two seconds.

Fourthly, the duration of the resonance will depend upon the nature of the material of the wall. A reflection always takes place at the surface of a new medium, and the amount of this will depend upon the elastic force or power to resist compression and the density of the new medium. For example, a wall of nitrogen, if such could be found, would transmit nearly the whole of a wave of sound in air, and reflect but a very small portion; a partition of tissue-paper would produce nearly the same effect. A polished wall of steel however, of sufficient thickness to prevent yielding, would reflect for practical purposes all the impulses through the air which might fall upon it. The rebound of the wave is caused, not by the oscillation of the wall, but by the elasticity and mobility of the air. The striking of a single ray of sound against a yielding board would probably increase the loudness of the reverberation but not its continuance. On this point a series of experiments was made by the use of the tuning-fork. In this instrument the motion of the foot and of the two prongs gives a sonorous vibration to the air, which, if received upon another tuning-fork of precisely the same size and form, would re-produce the same vibrations.

It is a fact well established by observation that when two bodies are in perfect unison, and separated from each other by a space filled with air, vibrations of the one will be taken up by the other. From this consideration it is probable that relatively the same effect ought to be produced in transmitting immediately the vibration of a tuning-fork to a reflecting body as to duration and intensity as in the case of transmission through air. This conclusion is strengthened by floating a flat piece of wood on water in a vessel standing

upon a sounding-board; placing a tuning-fork on the wood the vibrations will be transmitted to the board through the water, and sounds will be produced of the same character as those emitted when the tuning-fork is placed directly upon the board.

A tuning-fork suspended from a fine cambric thread and vibrated in air was found, from the mean of a number of experiments, to continue in motion 252 seconds. In this experiment, had the tuning-fork been in a perfect vacuum suspended without the use of a string, and further, had there been no ætherial medium, the agitation of which would give rise to light, heat, electricity, or some other form of ætherial motion, the fork would have continued its vibration forever.

The fork was next placed upon a large, thin pine board—the top of a table. A loud sound in this case was produced which continued less than *ten* seconds. The whole table as a system was thrown into motion, and the sound produced was as loud on the under side as on the upper side. Had the tuning-fork been placed against a partition of this material a loud sound would have been heard in the adjoining room; and this was proved by sounding the tuning-fork against a door leading into a closed closet. The sound within was apparently as loud as that without.

The rapid decay of sound in this case was produced by so great an amount of the motive power of the fork being communicated to a large mass of wood. The increased sound was due to the increased surface. In other words the shortness of duration was compensated for by the greater intensity of effect produced.

The tuning-fork was next placed upon a circular slab of marble about three feet in diameter and three-quarters of an inch thick. The sound emitted was feeble, and the undulations continued *one hundred and fifteen* seconds, as deduced from the mean of six experiments.

In all these experiments, except the one in a vacuum, the time of the cessation of the motion of the tuning-fork was determined by bringing the mouth of a resounding cavity

near the end of the fork, this cavity having previously been adjusted to unison with the vibrations of the fork, gave an audible sound when none could be heard by the unaided ear.

The tuning-fork was next placed upon a cube of India-rubber, and this upon the marble slab. The sound emitted by this arrangement was scarcely greater than in the case of the tuning-fork suspended from the cambric thread, and from the analogy of the previous experiments, we might at first thought suppose the time of duration would be great, but this was not the case. The vibrations continued only about forty seconds. The question may here be asked, What became of the impulses lost by the tuning-fork? They were neither transmitted through the India-rubber nor given off to the air in the form of sound, but were probably expended in producing a change in the matter of the India-rubber, or were converted into heat, or both. Though the inquiry did not fall strictly within the line of this series of investigations, yet it was of so interesting a character in a physical point of view to determine whether heat was actually produced that the following experiment was made.

A cylindrical piece of India-rubber about an inch and a quarter in diameter was placed in a tubulated bottle with two openings, one near the bottom and the other at the top. A stuffing-box was attached to the upper opening, through which a metallic stem with a circular foot to press upon the India-rubber was made to pass air-tight. The lower opening was closed with a cork, in a perforation of which a fine glass tube was cemented. A small quantity of red ink was placed in the tube to serve as an index. The whole arrangement thus formed a kind of air-thermometer, which would indicate a certain amount of change of temperature in the enclosed air. On the top of the stem the tuning-fork was screwed, and consequently its vibrations were transmitted to the rubber within the bottle. The glass was surrounded with several coatings of flannel to prevent the influence of external temperature. The tuning-fork was then sounded, and the vibrations were kept up for some time. No reliable indications

of an increase of temperature were observed. A more delicate method of making the experiment next suggested itself. The tube containing the drop of red ink, with its cork, was removed, and the point of a compound wire formed of copper and iron was thrust into the substance of the rubber, while the other ends of the wire were connected with a delicate galvanometer. The needle was suffered to come to rest, the tuning-fork was then vibrated, and its impulses transmitted to the rubber. A very perceptible increase of temperature was the result. The needle moved through an arc of from one to two and a half degrees. The experiment was varied and many times repeated; the motions of the needle were always in the same direction, namely, in that which was produced when the point of the compound wire was heated by momentary contact with the fingers. The amount of heat generated in this way however is small, and indeed in all cases in which it is generated by mechanical means the amount evolved appears very small in comparison with the labor expended in producing it. Joule has shown that the mechanical energy generated in a pound weight by falling through a space of seven hundred and fifty feet elevates the temperature of a pound of water one degree.

It is evident that an object like India-rubber actually destroys a portion of the sound, and hence in cases in which entire non-conduction is required this substance can probably be employed with perfect success.

The tuning-fork was next pressed upon a solid brick wall, and the duration of the vibration from a number of trials was eighty-eight seconds. Against a wall of lath and plaster the sound was louder and continued only eighteen seconds.

From these experiments we may infer that if a room were lined with a wainscot of thin boards and a space left between the wall and the wood, the loudness of the echo of a single noise would be increased while the duration of the resonance would be diminished. If however the thin board were glued or cemented in solid connection to the wall, or embedded in the mortar, then the effect would be a feeble echo and a long continued resonance, similar to that from the

slab of marble. This was proved by first determining the length of continuance of the vibrations of a tuning-fork on a thin board, which was afterwards cemented to a flat piece of marble.

A series of experiments was next commenced with reference to the actual reflection of sound. For this purpose a parabolic mirror was employed, and the sound from a watch received on the mouth of a hearing-trumpet furnished with a tube for each ear. The focus was near the apex of the parabola, and when the watch was suspended at this point it was six inches within the plane of the outer circle of the mirror. In this case the sound was confined at its origin, and prevented from expanding. No conjugate focus was produced, but on the contrary the rays of light, when a candle was introduced, constantly diverged. The ticking of the watch could not be heard at all when the ear was applied to the outside of the mirror, while directly in front it was distinctly heard at the distance of thirty feet, and with the assistance of the ear trumpet at more than double that distance. When the watch was removed from the focus the sound ceased to be audible. This method of experimenting admits of considerable precision, and enables us to directly verify, by means of sound transmitted through air, the results anticipated in the previous experiments. A piece of tissue-paper placed within the mirror and surrounding the watch without touching it, slightly diminished the reflection. A single curtain of flannel produced a somewhat greater effect, though the reflecting power of the metallic parabola was not entirely masked by three thicknesses of flannel; and I presume very little change would have been perceived had the reflector been lined with flannel glued to the surface of the metal. The sound was also audible at the distance of ten feet when a large felt hat without stiffening was interposed between the watch and the mirror. Care was taken in these experiments so to surround the watch that no ray of sound could pass *directly* from it to the reflecting surface.

With a cylindrical mirror, having a parabolic base, very little increased reflection was perceived. The converging

beams in this case were merely in a single plane, perpendicular to the mirror, and passing through the ear, while to the focal point of the spherical mirror a solid cone of rays was sent.

The reflection from the cylindrical mirror forms what is called a *caustic* in optics, while that from a cylindrical mirror gives a true focus, or in other words collects the sounds from all parts of the surface and conveys them to one point of space. These facts furnish a ready explanation of the confusion experienced in the Hall of Representatives, which is surmounted by a dome, the under surface of which acts as an immense concave mirror, reflecting to a focus every sound which ascends to it, leaving other points of space deficient in sonorous impulses.

Water, and all liquids which offer great resistance to compression, are good reflectors of sound. This may be shown by the following experiment. When water is gradually poured into an upright cylindrical vessel, over the mouth of which a tuning-fork is vibrated, until it comes within a certain distance of the mouth, it will reflect an echo in unison with the vibration of the fork, and produce a loud resonance. This result explains the fact, which had been observed with some surprise, that the duration of the resonance of a newly plastered room was not perceptibly less than that of one which had been thoroughly dried.

There is another principle of acoustics which has a bearing on this subject. I allude to the refraction of sound. It is well known that when a ray of sound passes from one medium to another a change in velocity takes place, and consequently a change in the direction or a refraction must be produced. The amount of this can readily be calculated where the relative velocities are known. In rooms heated by furnaces, and in which streams of heated air pass up between the audience and speaker, a confusion has been supposed to be produced and distinct hearing interfered with by this cause. Since the velocity of sound in air at 32° of Fahrenheit has been found to be 1090 feet in a second, and since the velocity increases 1.14 feet for every degree of

Fahrenheit's scale, if we know the temperature of the room and that of the heated current the amount of angular refraction can be ascertained. But since the ear does not readily judge of the difference of direction of two sounds emanating from the same source, and since two rays do not confuse the impression which they produce upon the ear though they arrive by very different routes, provided they are within the limit of perceptibility, we may conclude that the indistinctness produced by refraction is comparatively little. Professor Bache and myself could perceive no difference in distinctness in hearing, from rays of sound passing over a chandelier of the largest size in which a large number of gas jets were in full combustion. The fact of disturbance from this cause however, (if any exist,) may best be determined by the experiment with a parabolic mirror and the hearing-trumpet before described.

These researches might be much extended; they open a field of investigation equally interesting to the lover of abstract science and to the practical builder; and I hope, on behalf of the committee, to give some further facts with regard to this subject at another meeting.

The Smithsonian Lecture-room.—I shall now briefly describe the lecture-room of the Smithsonian Institution, which has been constructed in accordance with the facts and principles previously stated, so far at least as they could be applied.

There was another object kept in view in the construction of this room besides the accurate hearing, namely the distinct seeing. It was desirable that every person should have an opportunity of seeing the experiments which might be performed, as well as of distinctly hearing the explanation of them.

By a fortunate co-incidence of principles, it happens that the arrangements for insuring unobstructed sight do not interfere with those necessary for distinct hearing.

The law of Congress authorizing the establishment of the Smithsonian Institution directed that a lecture-room should be provided; and accordingly in the first plan one-half of the first story of the main building was devoted to this pur-

pose. It was found impossible however to construct a room on acoustic principles in this part of the building, which was necessarily occupied by two rows of columns. The only suitable place which could be found was therefore on the second floor. The main building is two hundred feet long and fifty feet wide; but by placing the lecture room in the middle of the story a greater width was obtained by means of the projecting towers.

The main gallery is in the form of a horse-shoe occupying three sides of the room. The speaker's platform is placed between two oblique walls. The corners of the room which are cut off by these walls afford recesses for the stairs into the galleries. The opposite corners are also partitioned off so as to afford recesses for the same purpose.

The general appearance of the room is somewhat fan-shaped, and the speaker is placed in the mouth as it were of an immense trumpet. The sound directly from his voice and that from reflection immediately behind him is thrown forward upon the audience; and as the difference of distance travelled by the two rays is much within the limit of perceptibility no confusion is produced by direct and reflected sound.

Again, on account of the oblique walls behind the speaker and the multitude of surfaces, including the gallery, pillars, stair-screens, &c., as well as the audience, directly in front, all reverberation is stopped.

The walls behind the speaker are composed of lath and plaster, and therefore have a tendency to give a more intense though less prolonged sound than if of solid masonry. They are also intended for exhibiting drawings to the best advantage.

The seats are arranged in curves and were intended to rise in accordance with the *panoptic curve*, originally proposed by Professor Bache, which enables each individual to see over the head of the person immediately in front of him. The original form of the room however did not allow of this intention being fully realized, and therefore the rise is somewhat less than the curve would indicate.

The ceiling is twenty-five feet high, and therefore within the limit of perceptibility. It is perfectly smooth and unbroken with the exception of an oval opening nearly over the speaker's platform through which light is admitted.

No echo is given off from the ceiling, while this assists the hearing in the gallery by the reflection to that place of the oblique rays.

The architecture of this room is due to Captain B. S. Alexander, of the corps of Topographical Engineers. He fully appreciated all the principles of sound which I have given, and varied his plans until all the required conditions as far as possible were fulfilled.

ACCOUNT OF A LARGE SULPHURIC-ACID BAROMETER IN THE
HALL OF THE SMITHSONIAN INSTITUTION.

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August 23, 1856.

The opinion has been frequently advanced that a barometer in which the material used to balance the pressure of the atmosphere is of less specific gravity than mercury, and consequently of a wider range of fluctuation, might throw some new light on several important points of meteorology. The fluid usually proposed for this purpose has been oil or water, the viscid character of the former and its tendency to a change of condition has induced a preference for the latter. Several water-barometers have accordingly been constructed; but as far as I am informed, the indications of the instruments have not been reliable.

Mariotte used one of this character; also Otto von Guericke constructed a philosophical toy to which he gave the name of *aëroscope*, on the principle of a water-barometer. It consisted of a tube more than thirty feet high elevated on a long wall and terminated by a tall and rather wide glass cylinder hermetically sealed, in which was placed a toy in the shape of a man. All the tube except a portion of

the cylindrical part was concealed behind the wainscoting, and consequently the little image made its appearance only in fine weather.

A water-barometer was constructed by Professor Daniell, and placed in the hall of the Royal Society, of which a full account has been published in the Transactions of that institution. A minute account is given of the method of blowing the tube, and the details of permanently fastening it in the box which was to form the case. The tube was left open at both ends; to the upper one a stop-cock was attached, and the lower one was inserted in a small steam boiler, which served the purpose of boiling the water to expel all the air, of elevating it to the proper height by means of the elastic force of the steam, and also as a permanent cistern to the barometer. After the water was forced to the top and issued from the stop-cock in a jet, the latter was closed; the stop-cock in the boiler was opened, steam suffered to escape, and the water to settle in the tube until balanced by the pressure of air. The upper part of the glass under the stop-cock, (which had previously been drawn out into a fine tube,) was gradually heated by a blow-pipe, and as soon as it was sufficiently softened the pressure of the air effectually closed it. The part above the stop-cock was then removed with a file. This barometer was completed, after adjusting the scale, by pouring a quantity of castor-oil on the surface of the water to prevent contact with the air.

After a series of observations however it was found in the course of about three months that the column of water was gradually descending, and it was finally resolved to open the boiler and to examine the instrument. The oil upon the surface was found to have undergone a change, though the water below was perfectly bright and transparent. A portion of the water was taken out and placed under the receiver of an air-pump, and bubbles of air in abundance were extricated; the air was absorbed by the water, diffused through the whole mass to the top, where it was given off to the vacuum, and thus caused the gradual descent of the column. It was found however that it was not atmospheric air in the

vacuum, but nearly pure nitrogen: the oxygen had been absorbed in passing through the oil, producing rancidity and other changes in that liquid.

It was evident from this experiment that oil was not impervious to air. Another attempt to remedy this defect of the instrument was made by using a thin film of gutta-percha, to be left after the evaporation of the naphtha in which it had been dissolved.

An objection however to the use of water as the liquid for the barometer is the vapor which it always gives off, and of which the tension cannot readily be determined. In a glass vessel in which a cup of water is enclosed, Professor Espy informs me that he has found the dew-point always less than that which would be due to the temperature.

Desiring to fit up a barometer on a large scale as one of the objects of interest and use in the Smithsonian Institution, I consulted my friend Professor G. C. Schaeffer of the Patent Office, as to the best liquid to be employed. He advised the use of sulphuric acid, but I did not immediately adopt his advice on account of the apparently dangerous character of this substance. Happening however some time afterwards to be speaking on the subject of barometers with Mr. James Green, the instrument-maker, in the presence of Professor Ellet, of New York, the latter asked why I did not have a large one constructed with sulphuric acid. The suggestion having thus again been independently made, and Mr. Green expressing his willingness to undertake the work, I gave the order for the construction of the instrument, and requested Professor Ellet to give any suggestions as to the details which might be required.

The advantages of this liquid are: 1. That it gives off no appreciable vapor at any atmospheric temperature; and 2. That it does not absorb or transmit air. The objections to its use are: 1. The liability to accident from the corrosive nature of the liquid, either in the filling of the tube or in its subsequent breakage; and 2. Its affinity for moisture, which tends to produce a change in specific gravity. The filling however is a simple process and attended with but little if

any risk. The acid can gradually be poured into the tube while in its case, slightly inclined to the horizon. Any accident from breakage can be prevented by properly securing the whole instrument in an outer case, which will also serve to equalize the temperature. To prevent the absorption of moisture the air may be previously passed through a drying tube apparatus. The only point in which water would be preferable to sulphuric acid is the less specific gravity of the former, and consequently the greater range of its fluctuation, which is as 20 : 11, nearly.

The general appearance of the instrument and the several contrivances for adjustment and reading are in accordance with the reputation of the skillful and intelligent artizan who made it. The glass tube is two hundred and forty inches long and three-fourths of an inch in diameter, and is enclosed in a cylindrical brass case of the same length, and two and a half inches in diameter. The glass tube is secured in the axis of the brass case by a number of cork collars placed at intervals; which, while they prevent all lateral displacement of the tube, allow it to be moved upward and downward for the adjustment of the zero-point.

The reservoir consists of a cylindrical glass bottle of four inches in diameter with two openings at the top; one in the axis to admit the lower end of the long tube, which is tapered to about one-half of the general diameter, the other to transmit the varying pressure of the atmosphere.

To adjust the zero-point the whole glass part of the apparatus together with the contained acid is elevated or depressed by a screw placed under the bottom, until the level of the acid in the reservoir coincides with a fixed mark.

The scale for reading the elevation is divided into inches and tenths, and by means of a vernier, moved by a rack and pinion, the variations can be measured to the hundredth of an inch, and estimated to a still smaller division.

The vernier itself is not immediately attached to the cylindrical brass case, but to a sliding frame which can be moved along the whole opening through which the entire range of the column is observed. The motion of the frame

enables us to make the first rough adjustment, and that of the rack and pinion the minute one.

The drying apparatus, placed between the external air and the interior of the reservoir, consists of a tubulated bottle (with two openings) containing chloride of calcium, and connected with the reservoir by an India-rubber tube; by which arrangement the air is deprived of its moisture.

To ascertain the temperature of the column of the liquid two thermometers are attached, one at the top and the other near the bottom.

The whole apparatus is enclosed in an outer glazed case of twelve inches square, which serves (as mentioned before) as well for protection as for equalizing the temperature, which is ascertained with sufficient accuracy by taking the mean of the two thermometers.

A large correction is required in this barometer for the expansion and contraction by the changes of temperature. To determine the amount of this, the specific gravity of a quantity of the acid with which the barometer had been filled was taken at different temperatures. This process was performed with a very sensitive balance, by Dr. Easter, in the laboratory of the Institution.

STATEMENT IN RELATION TO THE HISTORY OF THE ELECTRO-MAGNETIC TELEGRAPH.*

(From the Smithsonian Annual Report for 1857, pp. 99-106.)

A series of controversies and law suits having arisen between rival claimants for telegraphic patents, I was repeatedly appealed to to act as *expert* and witness in such cases. This I uniformly declined to do, not wishing to be in any manner involved in these litigations; but I was finally compelled under legal process to return to Boston from Maine—whither I had gone on a visit, and to give evidence on the subject. My testimony was given with the statement that I was not a willing witness, and that I labored under the disadvantage of not having access to my notes and papers, which were in Washington.

In the beginning of my deposition I was requested to give a sketch of the history of electro-magnetism having a bearing on the telegraph, and the account I then gave from memory I have since critically examined, and find it fully corroborated by reference to the original authorities. My sketch, which was the substance of what I had been in the habit of giving in my lectures, was necessarily very concise, and almost exclusively confined to one class of facts, namely, those having a direct bearing on Mr. Morse's invention. In order therefore to set forth more clearly in what my own improvements consisted, it may be proper to give a few additional particulars respecting some points in the progress of discovery, illustrated by wood cuts.

There are several forms of the electrical telegraph; first, that in which frictional electricity has been proposed to produce sparks, and motion of pith balls at a distance.

Second, that in which galvanism has been employed to produce signals by means of bubbles of gas from the decomposition of water; or by other chemical re-action.

*[Presented to the Board of Regents of the Smithsonian Institution, on their investigation (by a special committee) of certain publications touching the origin of the electro-magnetic telegraph.]

Third, that in which electro-magnetism is the motive power to produce motion at a distance: and again, of the latter there are two kinds of telegraphs, those in which the intelligence is indicated by the motion of a magnetic needle and those in which sounds and permanent signs are made by the attraction of an electro-magnet. The latter is the class to which Mr. Morse's invention belongs. The following is a brief exposition of the several steps which led to this form of the telegraph:

The first essential fact (as I stated in my testimony) that rendered the electro-magnetic telegraph possible was discovered by Oersted, in the winter of 1819-20. It is illustrated by figure 1, in which the magnetic needle is deflected by the



FIG. 1.

action of a current of galvanism transmitted through the wire *A B*. (See *Annals of Philosophy*, Oct., 1820, vol. xvi, page 274.)

The second fact of importance, discovered in 1820 by Arago and Davy, is illustrated in figure 2. It consists in



FIG. 2.

this, that while a current of galvanism is passing through a copper wire *A B*, it is *quasi* magnetic, that is, it attracts iron filings in a cylindrical sheath around it, and not those of copper or brass, developing magnetism in soft iron. (See *Annales de Chimie et de Physique*, 1820, vol. xv, page 94.)

The next important discovery, also made in 1820, by Ampère, was that two wires through which galvanic currents are passing in the same direction attract—and in the opposite direction repel each other. On this fact Ampère founded

his celebrated theory that magnetism consists merely in the attraction of electrical currents revolving at right angles to the line joining the two poles of the magnet. The magnetization of a bar of steel or iron, according to this theory, consists in establishing within the metal by induction—a series of electrical currents, all revolving in the same direction at right angles to the axis or length of the bar.

It was this theory which led Arago, as he states, to adopt the method of magnetizing sewing needles and pieces of steel wire shown in figure 3. This method consists in trans-



FIG. 3.

mitting a current of electricity through a helix surrounding the needle or wire to be magnetized. For the purpose of insulation the needle was inclosed in a glass tube, and the several turns of the helix were at a distance from each other to insure the passage of electricity through the whole length of the wire, or in other words, to prevent it from seeking a shorter passage by cutting across from one spire to another. The helix employed by Arago obviously approximates the arrangement required by the theory of Ampère in order to develop by induction the magnetism of the iron. By an attentive perusal of the original account of the experiments of Arago (given in the *Annales de Chimie et Physique*, 1820, vol. xv, pages 93–95) it will be seen that properly speaking he made no electro-magnet, as has been often stated. His experiments were confined to the magnetizing of iron filings, sewing needles, and pieces of steel wire of the diameter of a millimetre, or of about the thickness of a small knitting needle.

Mr. Sturgeon, in 1825, made an important step in advance of the experiments of Arago, and produced what is properly known as the electro-magnet. He bent a piece of iron wire into the form of a horseshoe, covered it with varnish to insulate it, and surrounded it with a helix, of which the spires were at a distance. When a current of galvanism was

passed through the helix from a small battery of a single cup the iron wire became magnetic, and continued so during the passage of the current. When the current was interrupted the magnetism disappeared, and thus was produced the first temporary soft iron magnet.

The electro-magnet of Sturgeon is shown in figure 4, which is a copy from the drawing in the *Transactions of the Society for the Encouragement of Arts, &c.*, 1825, vol. XLIII, pp. 38-52. By comparing figures 3 and 4, it will be seen that the helix employed by Sturgeon was of the same kind as that used by Arago; instead of a straight steel wire inclosed in a tube of glass however, Sturgeon employed a bent wire of soft iron. The difference in the arrangement at first sight might appear to be small, but the difference in the results produced was important, since the temporary magnetism developed in the arrangement of Sturgeon was sufficient to support a weight of several pounds; and an instrument was thus produced of value in future research.

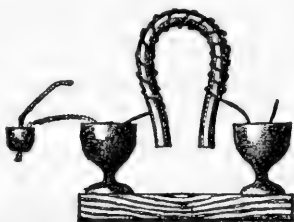


FIG. 4.

The next improvement was made by myself. After reading an account of the galvanometer of Schweigger, the idea occurred to me that a much nearer approximation to the requirements of the theory of Ampère could be attained by insulating the conducting wire itself, instead of the rod to be magnetized, and by covering the whole surface of the iron with a series of coils in close contact. This was effected by insulating a long wire with silk thread, and winding this around the rod of iron in close coils from one end to the other. The same principle was extended by employing a still longer insulated wire, and winding several strata of this over the first, care being taken to insure the insulation between each stratum by a covering of silk ribbon. By this arrangement the rod was surrounded by a compound helix formed of a long wire of many coils, instead of a single helix of a few coils. (Fig. 5.)



FIG. 5.

In the arrangement of Arago and Sturgeon the several turns of wire were not precisely at right angles to the axis of the rod, as they should be—to produce the effect required by the theory, but slightly oblique, and therefore each tended to develop a separate magnetism not coincident with the axis of the bar. But in winding the wire over itself, the obliquity of the several turns compensated each other, and the resultant action was at right angles to the bar. The arrangement then introduced by myself was superior to those of Arago and Sturgeon, first in the greater multiplicity of turns of wire, and second in the better application of these turns to the development of magnetism. The power of the instrument, with the same amount of galvanic force, was by this arrangement several times increased.

The maximum effect however with this arrangement and a single battery was not yet obtained. After a certain length of wire had been coiled upon the iron, the power diminished with a further increase of the number of turns. This was due to the increased resistance which the longer wire offered to the conduction of electricity. Two methods of improvement therefore suggested themselves. The first consisted—not in increasing the length of the coil, but in using a number of separate coils on the same piece of iron. By this arrangement the resistance to the conduction of the electricity was diminished, and a greater quantity made to circulate around the iron from the same battery. The second method of producing a similar result consisted in increasing the number of elements of the battery, or in other words the projectile force of the electricity, which enabled it to pass through an increased number of turns of wire, and thus by increasing the length of the wire, to develop the maximum power of the iron.

To test these principles on a larger scale, the experimental magnet was constructed, which is shown in figure 6. In this a number of compound helices was placed on the same bar, their ends left projecting, and so numbered that

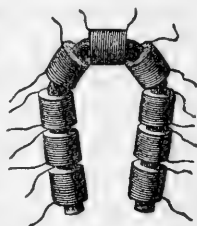


FIG. 6.

they could be all united into one long helix, or variously combined in sets of lesser length.

From a series of experiments with this and other magnets it was proved that in order to produce the greatest amount of magnetism from a battery of a single cup, a number of helices is required; but when a compound battery is used, then one long wire must be employed, making many turns around the iron, the length of wire and consequently the number of turns being commensurate with the projectile power of the battery.

In describing the results of my experiments, the terms "intensity" and "quantity" magnets were introduced to avoid circumlocution, and were intended to be used merely in a technical sense. By the *intensity* magnet I designated a piece of soft iron, so surrounded with wire that its magnetic power could be called into operation by an *intensity* battery, and by a *quantity* magnet, a piece of iron so surrounded by a number of separate coils, that its magnetism could be fully developed by a *quantity* battery.

I was the first to point out this connection of the two kinds of the battery with the two forms of the magnet, in my paper in Silliman's Journal, January, 1831, and clearly to state that when magnetism was to be developed by means of a compound battery, one long coil must be employed, and when the maximum effect was to be produced by a single battery, a number of single strands should be used.*

These steps in the advance of electro-magnetism, though small, were such as to interest and surprise the scientific world. With the same battery used by Mr. Sturgeon, at least a hundred times more magnetism was produced than could have been obtained by his experiment. The developments were considered at the time of much importance in a scientific point of view, and they subsequently furnished the means by which magneto-electricity, the phenomena of diamagnetism, and the magnetic effects on polarized light were discovered. They gave rise to the various forms of electro-

*[Silliman's American Journal of Science, Jan., 1831, vol. XIX, pp. 403, 404. See *ante*, vol. I, p. 42.]

magnetic machines which have since exercised the ingenuity of inventors in every part of the world, and were of immediate applicability in the introduction of the magnet to telegraphic purposes. Neither the electro-magnet of Sturgeon nor any electro-magnet ever made previous to my investigations was applicable to transmitting power to a distance.

The principles I have developed were properly appreciated by the scientific mind of Dr. Gale, and applied by him to operate Mr. Morse's machine at a distance.†

Previous to my investigations the means of developing magnetism in soft iron were imperfectly understood. The electro-magnet made by Sturgeon, and copied by Dana, of New York, was an imperfect quantity magnet, the feeble power of which was developed by a single battery. It was entirely inapplicable to a long circuit with an intensity battery, and no person possessing the requisite scientific knowledge, would have attempted to use it in that connection after reading my paper.

In sending a message to a distance, two circuits are employed, the first a long circuit through which the electricity is sent to the distant station to bring into action the second—a short one, in which is the local battery and magnet for working the machine. In order to give projectile force sufficient to send the power to a distance, it is necessary to use an intensity battery in the long circuit; and in connection with this at the distant station a magnet surrounded with many turns of one long wire must be employed to receive and multiply the effect of the current enfeebled by its transmission through the long conductor. In the local or short circuit either an intensity or quantity magnet may be employed. If the first be used, then with it a compound battery will be required; and therefore on account of the increased resistance due to the greater quantity of acid, a less amount of work will be performed by a given amount of material; and consequently though this arrangement is practicable it is by no means economical. In my original paper I state that the advantages of a greater conducting power, from using sev-

† [See Appendix A, at the end of this paper.]

eral wires in the quantity magnet may in a less degree be obtained by substituting for them one large wire; but in this case, on account of the greater obliquity of the spires and other causes, the magnetic effect would be less. In accordance with these principles, the receiving magnet, or that which is introduced into the long circuit, consists of a horse-shoe magnet surrounded with many hundred turns of a single long wire, and is operated with a battery of from 12 to 24 elements or more, while in the local circuit it is customary to employ a battery of one or two elements with a much thicker wire and fewer turns.

It will I think be evident to the impartial reader that these were improvements in the electro-magnet which first rendered it adequate to the transmission of mechanical power to a distance; and had I omitted all allusion to the telegraph in my paper, the conscientious historian of science would have awarded me some credit, however small might have been the advance that I had made. Arago, and Sturgeon, in the accounts of their experiments, make no mention of the telegraph, and yet their names always have been and will be associated with the invention. I briefly called attention however to the fact of the applicability of my experiments to the construction of the telegraph; but not being familiar with the history of the attempts made in regard to this invention, I called it "Barlow's project," while I ought to have stated that Mr. Barlow's investigation merely tended to disprove the possibility of a telegraph.

I did not refer exclusively to the needle telegraph when I stated in my paper that "the *magnetic* action of a current from a trough is at least not sensibly diminished by passing through a long wire." This is evident from the fact that the immediate experiment from which this deduction was made, was by means of an electro-magnet and not by means of a needle galvanometer.

At the conclusion of the series of experiments which I described in Silliman's Journal, there were two applications of the electro-magnet in my mind: one, the production of a machine to be moved by electro-magnetism, and the other,

the transmission of or calling into action power at a distance. The first was carried into execution in the construction of the machine described in Silliman's Journal in 1831;* and for the purpose of experimenting in regard to the second, I arranged around one of the upper rooms in the Albany

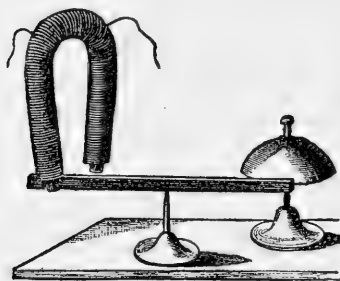


FIG. 7.

Academy a wire of more than a mile in length, through which I was enabled to make signals by sounding a bell. (Fig. 7.) The mechanical arrangement for effecting this object was simply a steel bar, permanently magnetized, of about ten inches in length, supported on a pivot, and placed with its north end

between the two arms of a horse-shoe magnet. When the latter was excited by the current, the end of the bar thus placed was attracted by one arm of the horse-shoe, and repelled by the other, and was thus caused to move in a horizontal plane and its further extremity to strike a bell suitably adjusted.

This arrangement is that which is alluded to in Professor Hall's letter† as having been exhibited to him in 1832. It was not however at that time connected with the long wire above-mentioned, but with a shorter one put up around the room for exhibition.

At the time of giving my testimony I was uncertain as to when I had first exhibited this contrivance, but have since definitely settled the fact by the testimony of Hall and others that it was before I left Albany, and abundant evidence can be brought to show that previous to my going to Princeton in November, 1832, my mind was much occupied with the subject of the telegraph, and that I introduced it in my course of instruction to the senior class in the Academy. I

*[Silliman's American Journal of Science, July, 1831, vol. xx, pp. 340-343. See *ante*, vol. i, p. 54.]

†[See Appendix B, at the end of this paper; and also Proceedings of the Albany Institute, January 13, 1858; vol. iv, pp. 244, 245.]

should state however that the arrangement I have described was merely a temporary one, and that I had no idea at the time of abandoning my researches for the practical application of the telegraph. Indeed, my experiments on the transmission of power to a distance were suspended by the investigation of the remarkable phenomena (which I had discovered in the course of these experiments) of the induction of a current in a long wire on itself, and of which I made the first mention in a paper in Silliman's Journal in 1832.*

I also devised a method of breaking a circuit and thereby causing a large weight to fall. It was intended to illustrate the practicability of calling into action at a distance a great power capable of producing mechanical effects; but as a description of this was not printed, I do not place it in the same category with the experiments of which I published an account, or the facts which could be immediately deduced from my papers in Silliman's Journal.

From a careful investigation of the history of electro-magnetism in its connection with the telegraph, the following facts may be established:

1. Previous to my investigations the means of developing magnetism in soft iron were imperfectly understood, and the electro-magnet which then existed was inapplicable to the transmission of power to a distance.

2. I was the first to prove by actual experiment that in order to develop magnetic power at a distance a galvanic battery of "intensity" must be employed to project the current through the long conductor, and that a magnet surrounded by many turns of one long wire must be used to receive this current.

3. I was the first to actually magnetize a piece of iron at a distance, and to call attention to the fact of the applicability of my experiments to the telegraph.

4. I was the first to actually sound a bell at a distance by means of the electro-magnet.

* [Silliman's American Journal of Science, July, 1832, vol. xxii, p. 408. See *ante*, vol. i, p. 79.]

5. The principles I had developed were applied by Dr. Gale to render Morse's machine effective at a distance.

The results here given were among my earliest experiments: in a scientific point of view I considered them of much less importance than what I subsequently accomplished; and had I not been called upon to give my testimony in regard to them, I would have suffered them to remain (without calling public attention to them) a part of the history of science to be judged of by scientific men who are the best qualified to pronounce upon their merits.

APPENDIX A.—*Letter from Dr. Gale.*

WASHINGTON, D. C., April 7, 1856.

SIR: In reply to your note of the 3d instant, respecting the Morse telegraph, asking me to state definitely the condition of the invention when I first saw the apparatus in the winter of 1836, I answer: This apparatus was Morse's original instrument, usually known as the type apparatus, in which the types, set up in a composing stick, were run through a circuit breaker, and in which the battery was the cylinder battery, with a single pair of plates. This arrangement also had another peculiarity, namely, it was the electro-magnet used by Sturgeon, and shown in drawings of the older works on that subject, having only a few turns of wire in the coil which surrounded the poles or arms of the magnet. The sparseness of the wires in the magnet coils and the use of the single cup battery were to me, on the first look at the instrument, obvious marks of defect, and I accordingly suggested to the professor, without giving my reasons for so doing, that a battery of many pairs should be substituted for that of a single pair, and that the coil on each arm of the magnet should be increased to many hundred turns each; which experiment, if I remember aright, was made on the same day with a battery and wire on hand, (furnished I believe by myself,) and it was found that while the original arrangement would only send the electric current through a few feet of wire, say 15 to 40, the modified arrangement would send it through as many hundred. Although I gave no reason at the time to Professor Morse for the suggestions I had proposed in modifying the arrangement of the machine, I did so afterwards, and referred in my explanations to the paper of Professor Henry, in the 19th volume of the American Journal of Science, page 400 and onward. It was to these suggestions of mine that Professor Morse alludes in his testimony before the Circuit Court for the eastern district of Pennsylvania, in the trial of B. B. French and others *vs.* Rogers and others. See printed copy of complainant's evidence, page 168, beginning with the words, "Early in 1836 I procured 40 feet of wire," &c., and page 169, where Professor Morse alludes to myself and compensation for services rendered to him, &c.

At the time I gave the suggestions above named, Professor Morse was

not familiar with the then existing state of the science of electro-magnetism. Had he been so, or had he read and appreciated the paper of Henry, the suggestions made by me would naturally have occurred to his mind as they did to my own. But the principal part of Morse's great invention lay in the mechanical adaptation of a power to produce motion, and to increase or relax at will. It was only necessary for him to know that such a power existed for him to adapt mechanism to direct and control it.

My suggestions were made to Professor Morse from inferences drawn by reading Professor Henry's paper above alluded to. Professor Morse professed great surprise at the contents of the paper when I showed it to him, but especially at the remarks on Dr. Barlow's results respecting telegraphing, which were new to him; and he stated at the time that he was not aware that any one had even conceived the idea of using the magnet for such purposes.

With sentiments of esteem, I remain, yours truly,

L. D. GALE.

Prof. JOSEPH HENRY.

APPENDIX B.—*Letter from Prof. Hall.*

ALBANY, N. Y., January 19, 1856.

DEAR SIR: While a student of the Rensselaer School, in Troy, New York, in August, 1832, I visited Albany with a friend, having a letter of introduction to you from Professor Eaton. Our principal object was to see your electro-magnetic apparatus, of which we had heard much, and at the same time the library and collections of the Albany Institute.

You showed us your laboratory in a lower story or basement of the building, and in a larger room in an upper story some electric and galvanic apparatus, with various philosophical instruments. In this room, and extending around the same, was a circuit of wire stretched along the wall, and at one termination of this, in the recess of a window, a bell was fixed, while the other extremity was connected with a galvanic apparatus.

You showed us the manner in which the bell could be made to ring by a current of electricity, transmitted through this wire, and you remarked that this method might be adopted for giving signals, by the ringing of a bell at the distance of many miles from the point of its connection with the galvanic apparatus.

All the circumstances attending this visit to Albany are fresh in my recollection, and during the past years, while so much has been said respecting the invention of electric telegraphs, I have often had occasion to mention the exhibition of your electric telegraph in the Albany Academy, in 1832.

If at any time or under any circumstances this statement can be of service to you in substantiating your claim to such a discovery at the period named, you are at liberty to use it in any manner you please, and I shall be ready at all times to repeat and sustain what I have here stated, with many other attendant circumstances, should they prove of any importance.

I remain, very sincerely and respectfully, yours,

JAMES HALL.

Professor JOSEPH HENRY.

ON THE APPLICATION OF THE TELEGRAPH TO THE PREMONITION OF WEATHER CHANGES.

(Proceedings American Academy of Arts and Sciences, vol. iv, pp. 271-275.)

August 9, 1859.

Professor Henry made a verbal communication relative to the application of the telegraph to the prediction of changes of the weather, particularly in the city of Boston and its vicinity.

It has been fully established by the observations which have been made under the direction of the Smithsonian Institution, and from other sources of information, that the principal disturbances of the atmosphere are not of a local character, but commence in certain regions, and are propagated in definite directions over the whole surface of the United States east of the Rocky Mountains.

From a careful study of all the phenomena of the winds of the temperate zones it is inferred that over the whole surface of the United States and Canada there are two great currents of air continually flowing eastward. These currents consist of an upper and a lower, the former returning the air to the south which was carried by the latter towards the north. The lower current, which is continually flowing over the surface of the United States, is about two miles in depth, and moves from the southwest to northeast. The upper or return current, which is probably of nearly equal magnitude, flows from northwest to southeast, or nearly at right angles to the other, and the resultant of the two is a current almost directly from the west. The reaction of these two currents appears to be the principal cause of the sudden changes of weather in our latitude. They give definite direction to our storms, accordingly as the latter are more influenced by the motion of the one or the other of these great aerial streams. The principal American storms may from our present knowledge, be divided into two classes, namely those which have their origin in the Caribbean Sea and

those which enter our territory from the north at the eastern base of the Rocky Mountains. Those of the first class, which have been studied with much success by the lamented Redfield and others, follow the general direction of the Gulf Stream, and overlapping the eastern portion of the United States, give rise to those violent commotions of the atmosphere which are in many instances so destructive to life and property along our eastern coast. These storms from the south are frequently two or three days in traversing the distance from Key West to Cape Race, and their approach and progress might generally be announced by telegraph in time to guard against their disastrous effects. Though the general direction of these storms appears to be made out with considerable certainty, much remains to be done in settling the theory of their character and formation.

The materials which have been collected at the Smithsonian Institution during the last seven years relative to the other class of storms have enabled us to establish general facts of much value, not only in a scientific point of view, but also in their application to the prediction of the weather. [This statement was verified by a series of maps, exhibited to the Academy by Professor Henry, on which were indicated the beginning and progress of some remarkable changes of weather.] From these maps it appears that the great disturbances of the atmosphere which spread over the surface of the United States enter our territory from the possessions of the Northwest or Hudson's Bay Company, about the sources of the Saskatchewan, at the base of the Rocky Mountains, and are thence propagated south and east, until in many instances they spread over the whole of the United States and probably a large portion of the British possessions.

For example, the great depression of temperature which occurred in January of the present year, and which will be remembered by every one as the most marked cold period of the season, entered the territory of the United States at the point before mentioned on the 5th of January, and on the 6th reached Utah, on the 7th Santa Fé, and on the 8th the

Gulf of Mexico, and passing onward it was felt in Guatemala on the 10th. While it was advancing southward it was spreading over the continent to the east; on the 7th it reached the Red River settlement and all places under the same meridian, down to the Gulf of Mexico. It reached the meridian of Chicago on the 8th, the western part of the State of New York on the 9th, New England on the 10th, and Cape Race on the 13th. It moved with about equal velocity over the Southern States and was observed at Bermuda on the 12th.

The remarkable frost of last June, so far as it has been traced, had the same origin and followed the same eastward course. The fact was also illustrated, (by the maps before mentioned,) that the warm periods which have occurred in past years have followed the same law of progression, and consequently their approach could have been announced to the inhabitants of the Eastern States several days in advance had a proper system of telegraphic despatches been established.

The value of the telegraph in regard to meteorology has been fully proved by the experience of the Smithsonian Institution. The Morse line of telegraph has kindly furnished the Institution during the last twelve months, free of cost, with a series of daily records of the weather from the principal stations over the whole country east of the Mississippi river and south of New York. In order to exhibit at one view the state of the weather over the portion of the United States just mentioned a large map is pasted on a wooden surface, into which, at each station of observation, a pin is inserted, to which a card can be temporarily attached. The observations are made at about seven o'clock in the morning, and as soon as the results are received at the Institution, an assistant attaches a card to each place from which intelligence has been obtained, indicating the kind of weather at the time; rain being indicated by a black card, cloudiness by a brown one, snow by a blue one, and clear sky by a white card.

This meteorological map is an object of great interest to

the many persons from a distance who visit the Institution daily; all appear to be specially interested in knowing the condition of weather to which their friends at home are subjected at the time. But the value of the map is not confined to the gratification of this desire. It enables us to study the progress of storms, and to predict what changes in the weather may be expected at the east, from the indications furnished by places farther west. For example, if a black card is seen in the morning on the station at Cincinnati, indicating rain at that city, a rain storm may confidently be expected at Washington at about seven o'clock in the evening. Indeed, so uniformly has this prediction been verified, that last winter the advertising in the afternoon papers of the lectures to be delivered at the Institution that evening was governed by the condition of the weather in the morning at Cincinnati; a rainy morning at the latter inducing a postponement of the lecture.

It must be evident, from the facts given, that if a system of telegraphing over the whole country east of the Rocky Mountains were established, information could be given to the Middle and Eastern States of the approach of disturbances of the atmosphere,—of much value to the agriculturist, the ship-owner, and to all others who transact business affected by changes of weather, as well as of importance to the invalid and the traveller. Indeed, with a proper combination of the lines now in operation, daily intelligence might be obtained in the city of Boston which would be of the highest interest to its inhabitants. [Professor Henry mentioned Boston in particular, because this city is so situated that the storms, both of the southern and western class, reach it after they have been felt in New York and in other places which are not so far east and north.] It is necessary to remark that the same use of the telegraph is in a measure inapplicable to the inhabitants of Western Europe, since they live on the eastern side of an ocean, and cannot be apprised of the approach of storms from the west. For the same reason the general laws of storms are more conveniently

studied by the meteorologists of this country than by those of Great Britain and France.

It should be distinctly understood that the remarks which have been made in this communication relate to the more violent changes of the weather which occur in autumn, winter, and spring. The thunder showers which occur almost daily during the warm weather in summer have somewhat of a local character, and commence at the same time and frequently at the same hour for several days in succession, at the same and different places; but wherever they commence they move eastward over the country until they are exhausted.

Professor Henry also spoke of the facts collected in regard to the nature of American storms, and their connection with the two great aerial currents continually flowing over the temperate zone. He considered that the great changes of the weather are principally due to the gradual production of an unstable equilibrium in the two currents by the accumulation of heat and moisture in the lower.

He spoke in high terms of the importance of the labors of Mr. Espy in developing the theory of the upper motion of air and the evolution of latent heat in the production of storms.

In reply to a question as to the possibility of crossing the Atlantic in a balloon, the Professor stated that he had little doubt, if the balloon could be made to retain the gas and to ascend into the upper current, it would be wafted across the ocean in the course of three or four days. If it descended into the lower current it would be carried to the north of east, and if it continued in the upper current it would reach Europe south of the same point. The course could be changed, within certain limits, by ascending and descending from one current to the other. The late balloon voyage from St. Louis to Jefferson county, New York, was of interest in confirming the theoretical direction of the great lower current of this latitude.

ON THE UTILIZATION OF ATMOSPHERIC CURRENTS IN AERONAUTICS.*

(From the Smithsonian Annual Report for 1860, pp. 118, 119.)

March 11, 1861.

DEAR SIR: In reply to your letter of February 25, 1861, requesting that I would give you my views in regard to the currents of the atmosphere and the possibility of an application of a knowledge of them to aerial navigation, I present you with the following statement to be used as you may think fit.

I have never had faith in any of the plans proposed for navigating the atmosphere by artificial propulsion, or for steering a balloon in a direction different from that of the current in which the vehicle is floating.

The resistance to a current of air offered by several thousand feet of surface is far too great to be overcome by any motive power at present known which can be applied by machinery of sufficient lightness.

The only method of aerial navigation which in the present state of knowledge appears to afford any possibility of practical application is that of sailing with the currents of the atmosphere. The question therefore occurs as to whether the aerial currents over the earth are of such a character that they can be rendered subservient to aerial locomotion.

In answering this question I think I hazard little in asserting that the great currents of the atmosphere have been sufficiently studied to enable us to say with certainty that they follow definite courses, and that they may be rendered subservient to aerial navigation provided the balloon itself can be so improved as to render it a safe means of locomotion.

It has been established by observations now extending over two hundred years, that at the surface of the earth

*[A letter addressed to Mr. T. S. C. Lowe, the Aeronaut, dated Washington, D. C., March 11, 1861.]

within the tropics, there is a belt along which the wind constantly blows from an easterly direction; and from the combined meteorological observations made in different parts of the world within the last few years, that north of this belt, between the latitudes of 30° and 60° around the whole earth, the resultant wind is from a westerly direction.

The primary motive power which gives rise to these currents is the constant heating of the air in the equatorial, and the cooling of it in and toward the polar regions; the eastern and western deflections of these currents being due to the rotation of the earth on its axis.

The easterly currents in the equatorial regions are always at the surface and have long been known as the trade winds, while the currents from the west are constantly flowing in the upper portion of the atmosphere, and only reach the surface of the earth at intervals,—generally after the occurrence of a storm.

Although the wind (at the surface) over the United States and around the whole earth between the same parallels, appears to be exceedingly fitful, yet when the average movement is accurately recorded for a number of years, it is found that there remains a large resultant of a westerly current. This is well established by the fact that on an average of many years packet ships sailing between New York and Great Britain occupy nearly double the time in returning that they do in going.

It has been fully established by continuous observations for ten years collected at this Institution from every part of the United States, that as a general rule all the meteorological phenomena advance from west to east, and that the higher clouds always move eastwardly. We are therefore from abundant observations as well as from theoretical considerations, enabled to state with confidence that on a given day, whatever may be the direction of the wind at the surface of the earth, a balloon elevated sufficiently high would be carried eastwardly by the prevailing current in the upper or rather middle region of the atmosphere.

I do not hesitate therefore to say that provided a bal-

loon can be constructed of sufficient size and of sufficient impermeability to gas to maintain a high elevation for a sufficient length of time, it would be wafted across the Atlantic. I would not however advise that the experiment of this character be made across the ocean, but that the feasibility of the project should be thoroughly tested and experience accumulated by voyages over the interior of our continent. It is true that more eclat might be given to the enterprise and more interest excited in the public mind generally by the immediate attempt of a passage to Europe; but I do not think the sober sense of the more intelligent part of the community would be in favor of this plan; on the contrary, it would be considered a premature and fool-hardy risk of life.

It is not in human sagacity to foresee prior to experience what simple occurrence, or what neglect in an arrangement, may interfere with the result of an experiment; and therefore I think it will be impossible for you to secure the full confidence of those who are best able to render you assistance, except by a practical demonstration in the form of successful voyages from some of the interior cities of the continent to the seaboard.

SYSTEMATIC METEOROLOGY IN THE UNITED STATES.

(From the Smithsonian Annual Report for 1865, pp. 50-59.)

It has been aptly said that man is a meteorologist by nature. He is placed in such a state of dependence upon the atmospheric elements, that to watch their vicissitudes and to endeavor to anticipate their changes become objects of paramount importance. Indeed the interest in this subject is so absolute that the common salutation among civilized nations is a meteorological wish, and the first introduction to conversation among strangers is a meteorological remark. Yet there is no circumstance which is remembered with so little exactness as the previous condition of the weather, even from week to week. In order that its fluctuations may be preserved as facts of experience, it is necessary that they should be continuously and accurately registered. Again, there is perhaps no branch of science relative to which so many observations have been made and so many records accumulated, and yet from which so few general principles have been deduced. This has arisen, first, from the real complexity of the phenomena, or in other words from the number of separate causes influencing the production of the ordinary results; second, from the improper methods which have been pursued in the investigation of the subject, and the amount of labor required in the reduction and discussion of the observations. Although the primary causes of the change of the weather are on the one hand, the alternating inclination of the surface of the earth to the rays of the sun, by which its different parts are unequally heated in summer and in winter, and on the other, the moisture which is elevated from the ocean in the warmer and precipitated upon the colder portions of the globe; yet the effects of these are so modified by the revolution of the earth on its axis, the condition and character of the different portions of its surface, and the topography of each country, that to strictly calculate the perturbations or predict the results of the simple laws of atmospheric equilibrium with that precision which

is attainable in astronomy, will probably ever transcend the sagacity of the wisest, even when assisted by the highest mathematical analysis. But although such precision cannot be looked for, approximations may still be obtained of great importance in their practical bearing on the every-day business of life.

The greater part of all the observations which have been recorded until within a few years past has been without system or co-ordination. It is true that the peculiar climate of a given place may be determined by a long series of isolated observations, but such observations, however long continued, or industriously and accurately made, can give no adequate idea of the climate of a wide region, of the progress of atmospheric changes, nor can they furnish an approximation to the general laws of the recurrence of phenomena. For this purpose a system of observation must be established over widely extended regions within which simultaneous records are made and periodically transmitted to a central position, where by proper reduction and discussion, such general conclusions may be reached as the materials are capable of yielding.

In discussing the records, the empirical method does not suffice. It is necessary that *a priori* assumptions should be provisionally adopted, not however at random, but chosen in strict accordance with well-established physical principles, and that these be finally adopted, rejected, or modified, as they are found to agree or disagree with the records. It is only by this method that the different causes which co-operate in the production of a series of complex phenomena can be discovered, as is illustrated in the history of astronomy, which previous to the investigations of Kepler consisted of an unintelligible mass of records of observations. But even with the application of the best possible process of discussion the labor necessary to be expended on such large masses of figures, in order to deduce simple results, is far beyond any individual effort, and can only be properly accomplished by governmental aid.

The importance of a combined system of meteorological

observations extending over a large area, and the peculiar advantages presented by our country for this object, were early appreciated, and such a system was commenced in 1819, under the direction of Dr. Lovell, Surgeon General of the Army. The stations embraced the principal military posts, from which reports were made at the end of each month as to the temperature, the pressure, and the moisture of the air, the amount of rain, the direction and force of the wind, the appearance of the sky, besides casual phenomena, such as the aurora, thunder-storms, shooting stars, &c. In 1825 a similar system, of more numerous stations in proportion to the area embraced, was established in the State of New York, the points of observation being the several academies under the direction of the board of regents of the University, an establishment having charge of the higher institutions of learning in that State.

In 1837 the Legislature of Pennsylvania made an appropriation of four thousand dollars for instruments, which were distributed to volunteer observers. This system was continued about ten years; that of New York has been kept up with more or less efficiency until the present time; while the army system was continued until the commencement of the war.

The lake system, established by the engineer department, under the superintendence of Captain (now General) Meade, consists of a line of stations, extending from the western part of Lake Superior to the eastern part of Lake Ontario, and has been efficiently continued for several years.

The Smithsonian meteorological system was commenced in 1849, and with occasional aid in defraying the expenses, has continued in operation until the present period. It was however much diminished in efficiency during the war, since from the southern States no records were received, and many of the observers at the north were called to abandon such pursuits for military service in the field. The efforts of the Institution in this line have been directed to supplementing and harmonizing all the other systems, preparing and distributing blank forms and instructions, calculating

and publishing extensive tables for the reduction of observations, introducing standard instruments, and collecting all public documents, printed matter, and manuscript records, bearing on the meteorology of the American continent, submitting these materials to scientific discussion, and publishing the results. In these labors the Institution has been in continued harmonious co-operation with all the other efforts made in this country to advance meteorology, except those formerly conducted by the Navy Department under Lieutenant Maury. These were confined exclusively to the sea, and had no reference to those made at the same time on land. Without desiring to disparage the labors of Lieutenant Maury, I may say that his results would have lost nothing of their value by the adoption of a less exclusive policy on his part. The meteorology of the sea and that of the land pertain to a connected series of phenomena which can be properly studied only by a combined system of observations relating to both. The method pursued by Lieutenant Maury consisted in dividing the surface of a map of the ocean into squares of ten degrees on a side, and in recording within each of these the direction of the winds obtained from the log-books of the vessels which had traversed the several regions. In this way he accumulated a large amount of data, which though published in connection with many crude hypotheses, are of great value in the study of the meteorology of the globe.

In 1853 a meteorological system was commenced in Canada, the senior grammar school in each county being provided with instruments; and the observations have been continued to the present time. In regard to this system, Mr. Hodgins of the educational department remarks: "We have never lost sight of the great practical importance to a new and partially settled country, of establishing early in its history, before its physical condition is materially changed, a complete and comprehensive system of meteorological observations, by which may be tested theories of science which are yet unsettled, and which may be solved, relating to natural phenomena which have long remained among the sealed mysteries of nature."

The observations thus far have been taken without remuneration, but the importance of the system has become so well recognized, that the Canadian government has decided to establish ten permanent stations, in addition to the observatories at Toronto and Kingston, distributed so as to afford the most complete information relative to the climatic features of the whole province. The points selected are Windsor, Goderich, Stratford, Simcoe, Barrie, Hamilton, Peterborough, Belleville, Pembroke, and Cornwall; that is, two stations on Lake Erie, one on Lake Huron, three on Lake Ontario, one on Lake Simcoe, one on the Ottawa river, one on the bay of Quinté, one on the St. Lawrence, near the eastern extremity of the province, and two in the interior of the country. The records made at the public schools of Canada have been furnished to the Smithsonian Institution, as well as to the committee on immigration of the New York House of Assembly, for the purpose of furnishing facts relative to the climate—of importance to settlers; and recently the department of royal engineers has applied for the returns, with a view to the consideration of their bearing on questions of defence. To secure a greater degree of responsibility, and to promote the efficiency of the system, the government has provided for the payment of fifty cents a day to the teachers of the grammar schools at the stations before enumerated, as remuneration for the service rendered.

Under the direction of the distinguished academician Kupfer, there is established over the vast Russian territory a network of thirty meteorological stations, where are noted the various changes of the atmosphere as to temperature, pressure, moisture, &c. The most northern of these stations is at Hammerfest, in $70^{\circ} 41'$ north latitude, $21^{\circ} 26'$ east longitude from Paris, and the most southern is at Tiflis, in $41^{\circ} 42'$ north latitude, and $42^{\circ} 30'$ east longitude.

A like system of simultaneous observations has been for several years in operation in Great Britain and Ireland, in connection with the Board of Trade, and under the direction of the late Admiral Fitzroy.

Other and similar systems of meteorology have been

established in France, Italy, and Holland. From these different organizations, as well as from insulated observatories, telegrams of the weather are sent every morning, at seven o'clock, from the principal cities of Europe to Paris, where under the superintendence of the celebrated Leverrier, they are discussed, and the results transmitted by mail to all parts of the world in the successive numbers of the daily *International Bulletin*. A similar publication is periodically made in Italy, under the direction of M. Matteucci, so well and favorably known by his discoveries in physics. The British Government has also established a system of observations for the sea, and furnished its navy with accurate instruments, carefully compared with the standards of the Kew observatory. It is estimated in a report to Parliament that through an annual appropriation of about fifty thousand dollars, statistics may be collected in fifteen years sufficient, with what has already been obtained, to determine the average movement of the winds on every part of the ocean.

From the great interest which has been awakened in regard to meteorology throughout the world, and the improved methods which have been adopted in its study, it can scarcely be doubted that in a few years the laws of the general movements of the atmosphere will be ascertained, and the causes of many phenomena of the weather, which have heretofore been regarded as little else than the capricious and abnormal impulses of nature, will become adequately known; although, from the number of these causes, and the complexity of the resultant effect, it may never be possible to deduce accurate predictions as to the time and particular mode of their occurrence.

Indeed, the results which have been already derived from the series of combined observations in this country, fully justify the wisdom and forethought of those who were instrumental in establishing them. Although their organization was imperfect, the observers in most cases untrained, and the instruments of an inferior character, yet they have furnished data which through the labors of Redfield, Espy, and

Hare, whose memories are preserved in the history of science, have led to the establishment of principles of high theoretical interest, as well as of great practical value. Among these I need here mention only the fact now fully proved that all the meteorological phenomena of at least the middle and more northern portions of the temperate zone are transmitted from west to east. The passage of storms from one part of the country to the other was noticed by Dr. Franklin on the occasion of observing an eclipse of the moon. He showed that our south-west storms are felt successively later and later as the point of observation is farther to the north-east; that they arrive last at the extreme north-eastern portions of our continent. We now know however that the successive appearance of the storm at points farther along the coast is due to the easterly movement—sideways as it were, of an atmospheric disturbance, greatly elongated north and south, and reaching sometimes from Canada to the Gulf of Mexico. Hence to persons residing along the seaboard the phenomenon would appear to have a northwardly progression, on account of the north-easterly trend of the coast; yet the storm not unfrequently reaches simultaneously Bermuda and Nova Scotia.

Few persons can have failed to observe the continued motion of the higher clouds from the west, or to have recognized the just meteoroscopy of Shakspeare in a well-known passage:

“The weary sun hath made a golden set,
And by the bright track of his fiery car
Gives token of a goodly day to-morrow.”

The breaking forth of the sun just before his setting shows that the rear of the cloud which has obscured his beams has in its easterly course reached our horizon, and will soon give place to an unobscured sky.

It must be observed however that all the storms which visit our coast are not of this nature; those denominated cyclones, and which seldom extend far into the interior, are probably of a rotatory character. These usually commence in the Caribbean sea, move first toward the northwest, and

gradually curving round before they reach our latitude, take an easterly direction, as has been shown by Redfield and others.

The first practical application which was attempted of the principle we have mentioned was made by this Institution in 1856; the information conveyed by telegraphic despatches in regard to the weather was daily exhibited by means of differently colored tokens, on a map of the United States, so as to show at one view the meteorological condition of the atmosphere over the whole country. At the same time publication of telegraphic despatches was made in the newspapers. The system however was necessarily discontinued at the beginning of the war, and has not yet been resumed. Similar applications have since been made in other countries, particularly in England, under the late Amiral Fitzroy; in France, under Leverrier; and still later, in Italy. In the last-mentioned country tabular statements are to be published annually, comparing the predictions with the weather actually experienced.

The British Government has also recently introduced the system of telegraphic meteorological predictions into India. The cyclone of October, 1864, which did such damage to the shipping in Calcutta and destroyed the lives of sixty thousand persons, called special attention to the subject. The Asiatic Society of Bengal estimated the cost of such a system at 67,000 rupees (about \$30,000), a sum which the government hesitated to appropriate, though it decided to furnish the necessary instruments and an allowance of fifty rupees a month to the assistant at the telegraph station at Saugor, on the seaboard to the southward of Calcutta, in the direction from which the most severe storms approach that port.

It must be evident from what we have said in regard to the movement of storms, that a system of telegraphic meteorological predictions would be at once more reliable and of more benefit to the eastern coast of the United States, than those made in England and France, on the western coast of Europe, could possibly be to those countries, since the dis-

turbances of the atmosphere which reach them advance from the ocean, while the majority of those of a similar nature which visit especially the middle and eastern portions of our coast, come overland from a westerly or south-westerly direction, and their approach may be telegraphed in some cases many hours before their actual arrival.

But the expense of the proper establishment of a system of this kind can only be defrayed by the general government or some organization in possession of more ample means than can be applied by the Smithsonian Institution to such a purpose. This will be evident from the fact which we have mentioned of the cost of the establishment of a similar system in India, and from a report of a committee of the two houses of Parliament appointed to consider certain questions relating to the meteorological department of the board of trade. From this it appears that the amount expended during the eleven years ending with 1865 was 45,000 pounds sterling, or an average of about \$20,000 a year. The same committee recommend that meteorological observations at sea be continued under the direction of the hydrographic office of the admiralty, and an appropriation of £1,500 annually be made for instruments, and £1,700 for discussion and publication of results; making a total of £3,200. For weather statistics on land, the annual sum of £4,250, including instruments, discussion, and publications, is recommended, and for telegram storm warnings, £3,000; making a total annual expenditure of £7,450 for the land, and a grand annual total for land and sea of £10,450, or \$52,250.

The present would appear to be a favorable time to urge upon Congress the importance of making provision for re-organizing all the meteorological observations of the United States under one combined plan, in which the records should be sent to a central depot for discussion and final publication. An appropriation of \$50,000 annually for this purpose would tend not only to advance the material interests of the country but also to increase its scientific reputation. It would show that although the administration of our Government is the expression of the popular will, it is not

limited in its operation merely to objects of instant or immediate utility, but that with a wise prevision of the future it withholds its assistance from no enterprise, however remote the results, which has for its end to advance the well-being of humanity.

It is scarcely necessary at this day to dwell on the advantages which result from such systems of combined observations as those which the principal governments of Europe have established and are now constantly extending. I may however in passing, briefly allude to some facts which may not at once occur to the mind of the general reader. They enable the mariner to shorten the time and diminish the danger of the passage from one port to another by indicating to him the route along which prevail at a particular season of the year the most favorable winds for his purpose. They also furnish the means by which the sailor is taught the important lesson which has saved thousands of lives and millions of property, namely, that of finding the direction of the centre of the cyclone, and of determining the course in which he must steer in order to extricate himself from the destructive violence of this fearful scourge of the ocean. To the agriculturist they indicate the character of the climate of the country, and enable him with certainty to select the articles of culture best adapted to the temperature and moisture of the region, and which in the course of a number of years will insure him the most profitable returns for his labor. They furnish the statistics of the occurrence of sterile years and of devastating storms, which may serve as the basis on which to found insurance institutions for protection against the failure of crops, and thus give to the husbandman the same certainty in his pursuits as that possessed by the merchant or the ship-owner. They may also afford warning of the approach of severe frosts and violent storms, in time to guard at least in some degree against their injurious effects. To the physician a knowledge of such results as can be obtained from an extended system of observations is of great importance, not only in regard to the immediate practice of his art, but also to the improvement of his science.

The peculiar diseases of a region are principally dependent on its climate; an extreme variation of temperature in a large city is invariably attended with an increase of the number of deaths. The degree and variation of the moisture at different times and in different places have also a great influence on diseases, and the more the means of studying the connection of these elements and the corresponding condition of the human body are multiplied the more will the art and the science of medicine be improved. I may mention that scarcely a week passes at the Institution in which application is not made for meteorological information relative to different parts of this country, with the hope to improve the condition, if not restore the health, of some patient. The knowledge which at present exists however as to the connection of climate and disease, particularly in our own country, is—in comparison to what might be obtained—of little significance.

No other part of the world can at all compare with this country in the conditions most favorable to the advancement of meteorology, by means of a well-organized and properly sustained system of combined observations. Such a system extending from east to west more than two thousand miles would embrace in its investigation all the phenomena of the great upper current of the return trade wind, which continually flowing over us at a high elevation carries most of the disturbances of the atmosphere eastward. It would also include the effects produced by the polar and equatorial currents as they contend for the mastery along the broad valley which stretches without interruption from the arctic circle to the Gulf of Mexico, and would settle with precision the influence of the great fresh-water lakes in ameliorating the climate of the adjacent regions. But above all, in a popular view, it would furnish the means more effectually than any other system—of predicting the approach of storms and of giving the ships of our Atlantic coast due warning of the probability of danger.

REMARKS ON "VITALITY."*

(From the Smithsonian Annual Report for 1866, pp. 386-388.)

In the early study of mechanical and physiological phenomena, the energy which was exhibited by animals, or in other words, their power to perform what is technically called work, that is to overcome the inertia and change the form of matter, was referred to the vital force. A more critical study of these phenomena has however shown that this energy results from the mechanical power stored away in the food and material which the body consumes; that the body is a machine for applying and modifying power, precisely similar to those machines invented by man for a similar purpose. Indeed, it has been shown by accurate experiments that the amount of energy developed in animal exertion is just in proportion to the material consumed. To give a more definite idea of this, we may state the general fact that matter may be considered under two aspects, namely matter in a condition of power, and matter in a state of entire inertness. For example, the weight of a clock or the spring of a watch when wound up is in a state of power, and in its running down gives out, tick by tick, an amount of power precisely equal to the muscular energy expended in winding it up. When the weight or the spring has run down, it is then in a condition of inertness, and will continue in this state, incapable of producing motion, unless it be again put in a condition of power by the application of an extraneous force. Again, coal and other combustible bodies consist of matter in a condition of power, and in their running down into carbonic acid and water, during their combustion, evolve the energy exhibited in the operations of the steam engine. The combustible material may be considered the food of the steam engine, and experiments have been made to ascertain

*[Remarks on a communication "On Vitality," by the Rev. H. H. Higgins, in the Proceedings of the Literary and Philosophical Society of Liverpool, (England,) 1864. Re-printed in the Smithsonian Report for 1866, pp. 379-386.]

the relative economy in the expenditure of a definite amount of food in the natural machine and the artificial engine. The former has been found to waste less of the motive power than the latter.

In pursuing this train of investigation the question is asked, "Whence does the coal or food derive its power?" The answer is, that these substances are derived from the air by the decomposing agency of the impulses from the sun, and that when burned in the engine or consumed in the body they are again resolved into air, giving out in this resolution an amount of energy equivalent to that received from the sun during the process of their growth. All the materials of the crust of the earth, with the exception of coal and organic matter, are in a state of inertness, and like the burnt slag of the furnace, have expended their energy, and in this condition of inertness they would forever remain, were it not for extraneous influences, principally that from the sun.

From this point of view the phenomena we have been considering consist merely in the transfer of power from one body to another, and from a wide generalization from all the facts, the conclusion has been arrived at that energy is neither lost nor gained in the transfer; and pursuing the same train of reflection, we are finally led to the result that all power is derived from the primordial, unbalanced attraction and repulsion of the atoms of matter.

In the gradual development of the principles we have given there has been a tendency to extend the views we have presented too far, and to refer all the phenomena of life to the mechanical or chemical forces of nature. Although it has been, as we think, conclusively proved that from food, and food alone, come all the different kinds of physical force which are manifest in animal life, yet as the author of the preceding paper has shown, there is something else necessary to life, and this something, though it cannot properly be called a force, may be denominated the vital principle. Without the influence of this principle the undirected physical powers produce mechanical arrangements and assume a

state of permanent equilibrium by bringing matter into crystalline forms or into a condition of simple aggregation, while under *its* mysterious influence the particles of matter are built up into an unstable condition in the form of organic molecules. While therefore we may refer the changes which are here produced, or in other words the work performed, to the expenditure of the physical powers of heat, chemical action, &c., we must admit the necessity of something beyond these which from the analogy with mental phenomena, we may denominate the directing principle. Although we cannot perhaps positively say in the present state of science that this directing principle will not manifest itself when all the necessary conditions are present, yet in the ordinary phenomena of life which are everywhere exhibited around us, organization is derived from vitality, and not vitality from organization. That the vital or directing principle is not a physical power which performs work, or that it cannot be classed with heat or chemical action, is evident from the fact that it may be indefinitely extended—from a single acorn a whole forest of oaks may result.

The principles of which we have here endeavored to give an exposition are strikingly illustrated in the transformation of the egg when subjected to a slightly elevated temperature. The egg of a bird for example consists, as we know, of a congeries of organized molecules or vesicles, enclosed in a calcareous shell, thickly punctured with minute holes, through which the oxygen of the air can enter, and vapors and gases escape. Let us observe the difference of changes which take place in two newly-laid eggs, one of which is not possessed with vitality, and the other is endowed with this mysterious principle. Both of these eggs are in a condition of power, the carbon, hydrogen, nitrogen, sulphur, &c., of which their organized molecules are composed, are in a state of unstable equilibrium and ready, when set in motion by a slight increase of temperature, to rush into the more stable compounds of carbonic acid, vapor of water, &c., by chemical attraction. While the eggs are in an unchanged condition they possess the same amount of what is called potential energy, which

in both cases will be expended in the transformation of the materials; but how different will be the effects produced. In the case of the egg deprived of vitality, all the organized molecules will be converted into gases and vapors, with the development of heat and an elastic energy, in some cases sufficient to burst the shell, the power originally stored away in the egg being thus dissipated in the production of chemical and mechanical changes. In the case of the egg possessed of vitality, a portion of the organized molecules will also run down into vapors and gases, which will gradually escape through the perforations of the shell, and will thus, as in the previous case, evolve an equivalent amount of power; but this, instead of being dissipated in mere mechanical or chemical effects, will be expended, under the directing principle of vitality, in elevating to a higher degree of organization the molecules of the remainder, and in transforming them into organs of sensation, perception, and locomotion; in short, in the production of a machine precisely similar to those constructed by the intellectual operations of man when guiding or directing the powers of nature. If we examine the transformation as it goes on from day to day, we shall see that it does not consist in a simple aggregation of particles in the production of the organs we have mentioned, but in preliminary arrangements, such as canals, and provisional parts, afterwards to be obliterated, and the adoption of means for a more remote end, the whole indicating an intention realized in the sentient, living, moving animal.

This vital principle, from strict analogy, cannot be considered as an essential property of matter, since it is only continued by transmission from one living being to another. It is true that it ceases to manifest itself when a slight derangement takes place in the organized material with which it is connected, and death ensues; but this is precisely analogous to the manifestation of the thinking, willing principle within us, the existence of which is revealed to us by our own consciousness as a primordial truth, beyond which nothing can be more certain.

SUGGESTIONS AS TO THE ESTABLISHMENT OF A PHYSICAL OBSERVATORY.*

(From the Smithsonian Annual Report for 1870, pp. 141-144.)

December 29, 1870.

MY DEAR SIR: Yours of the 28th of November was duly received, but I delayed answering it until the pressure of business which accumulated during my absence should have somewhat subsided, and also that I might receive the plans which you mention. I am now gratified in being able to inform you that my visit to Europe was both pleasant and profitable, and that I have returned much improved in health, and with enlarged views as to the present state of science in the Old World.

While abroad I gave special attention to physical observatories, of which there are several in England and on the continent, although no one of them fully realizes my idea of what such an establishment ought to be.

A physical observatory is one the primary object of which is to investigate the physical phenomena of the earth and the heavenly bodies in contradistinction to an ordinary astronomical observatory, which is principally devoted to the observation and discussion of the motions of the planets, and the determination of the relative positions of the fixed stars. Of the latter kind but one or two are needed in any country, and as these require a numerous corps of observers and computers they can only be supported by appropriations annually from a national government. The United States Observatory at Washington is of this character, and including all expenses requires an annual appropriation of at least \$50,000. The labors of such an observatory are indispensable to the advancement of the science of theoretical astronomy, and its application to geodesy and geography.

The establishment I would advise you to found is of the character of the one first mentioned, namely a physical observatory, the principal object of which would be, as I have

*[A letter addressed to Mr. Leander McCormick, dated Washington, December 29, 1870.]

indicated, to investigate the nature and changes of the constitution of the heavenly bodies ; to study the various emanations from these in comparison with the results of experiments, and to record and investigate the different phenomena which are included under the general term of terrestrial physics.

A wide field has been opened for the study of the nature of the sun and other heavenly bodies by the application of the spectroscope, different modifications of the telescope, and other lately invented appliances. We now know that the sun is undergoing remarkable changes, the character of which can only be ascertained by the results of accurate observations compared with those of experimental investigation. The observer should divide his attention between the phenomena revealed by a critical and continued examination of the sun and the production of similar phenomena in the laboratory. In this way European investigators have arrived at most interesting results.

Again, we know that the emanations from the sun, and probably from the stars, differ essentially in character. There is first, the emanation known as light, which of itself consists of various rays (generally indicating the incandescence of substances,) that give the sensation of different colors, some of which though in their ordinary condition imperceptible to the eye, may be perceived by that organ after they have passed through certain liquids; next, the heat emanation, which is also of different kinds; then the chemical emanation, by which photographic impressions are produced; and lastly, the phosphorogenic emanation (abounding also in the electric discharge), that produces the temporary glow of the diamond and the luminosity of the compounds of lime, barium, and other substances with sulphur, when taken into the dark. To study these or other emanations as they may appear in the fixed stars, or are reflected from the moon and planets, or as they may be found in the aurora borealis, the zodiacal light, and in shooting-stars or larger meteors, requires peculiar instruments, and such as are not found, at

present, in ordinary astronomical observatories. For example, the celestial phenomena which address themselves to the sense of sight are studied by means of refracting telescopes, as are also those of the photographic ray, although this requires a peculiar form of lens, while the heat-ray of lower intensity and the phosphorogenic ray are not transmitted by glass; the former is readily converged to a focus by a lens of rock-salt, and the latter by one of quartz. They may all however as in the case of light, be concentrated into foci by metallic reflectors.

In regard to terrestrial physics, the phenomena are also various, and the forces by which they are produced are constantly changing both in intensity and in some cases in direction. We now know that the magnetism of the earth scarcely remains the same from one moment to another, and that these changes are connected with the appearance of the aurora borealis and electrical discharges in the atmosphere. They also in all probability may ultimately be referred to disturbances produced by external influences, such as those from the sun, moon, and planets. Furthermore, we may now consider the whole earth as an immense conductor charged with negative electricity, of which the intensity is in a continued state of change, and of the laws of which, as well as those of the changes of magnetism, a knowledge is highly desirable. For the proper study of these, continuous self-recording instruments are necessary.

There is also an important field of observation in regard to ordinary meteorology, such as the changes of the pressure of the atmosphere, and its connection with other phenomena; of the normal and abnormal winds; isolated currents of the atmosphere, and especially those of a vertical direction; the radiation of heat from clouds and different terrestrial surfaces; the variation of its intensity in ascending above and penetrating below the surface of the earth, &c. In short, the field is almost boundless, and every year reveals new facts in terrestrial and celestial physics, which never fail to furnish new points for investigation to those who are qualified by education and endowed by nature for their proper appreciation.

The conductor of an observatory such as I have mentioned to be successful must have peculiar characteristics. He must possess a minute knowledge of all the latest discoveries in physics, a keen eye to detect new appearances, imagination to suggest hypothetical causes, logical power to deduce consequences from these, to be tested by observation or experiment, and ingenuity to devise apparatus for verifying or disproving his deductions. When *such* a man is found he should be consecrated to science and fully furnished with all the implements necessary for the prosecution of his researches, those of physics as well as of astronomy, and himself and family placed beyond all anxiety as to the supply of their necessary wants. It may not be amiss to combine with his studies and duties, in the way of research, a small amount of lecturing,—just enough by sympathetic communication with admiring pupils to fan as it were his enthusiasm, and to impart a portion of it to others. He should also have at his command a skillful workman who under his direction could construct the temporary apparatus which are constantly required in original research. It is also important that he be associated with the faculty of a well-endowed college or university, to which he will become an important acquisition both in regard to the reputation which he will give to the institution and the effect he will have on the other members of the faculty in the way of stimulating them to higher efforts. In such an association he can call for the co-operation of the professors, and especially that of the physicist, the chemist, and the mathematician.

One of the most important points perhaps to which I should call your attention, is that of the building to be erected, since from the tendency to error in this line more injury has resulted to public institutions in this country than from any other cause. It should be recollected that “money is power;” that every dollar possesses a definite amount of potential energy as it were which can always command intellectual or physical labor. But money as a power is unlike all other kinds of power in that it is by judicious investment capable of yielding a constant supply

of energy in the way of interest without diminishing the original amount. It is therefore in the highest degree injudicious in the founding of an establishment to exhaust the source of its power by architectural displays not absolutely required and which may forever involve a continual expense from the remaining funds to keep them in repair. As a general rule the buildings of educational or scientific institutions should be gradually evolved from the experience and wants of the establishment, and not as is too frequently the case from *a priori* misconceptions of those who have no adequate idea of the uses to which the structure is to be applied. It should be impressed upon the public that *buildings* do not constitute an institution, and that reputation and usefulness in science do not flow from visible and tangible manifestations, but are the immaterial fruit produced by the spirit of an organization. I trust that millions of human beings yet unborn will be familiar with the intellectual results of your observatory, although a single inquiry may never be made as to the style of the building in which these results have been produced.

My advice then would be, first, if possible that the right man be procured for director; secondly, that the principal instruments be constructed under his supervision; and thirdly, that the operations be commenced in an inexpensive wooden building, which will be found better in many respects for physical and astronomical observations than one of stone and brick. The instruments could be insured, I should think, at a small premium, and in that case if destroyed by fire might be replaced by others embracing the improvements which may have been suggested in the meantime.

As an illustration of what I have just said in regard to the building, I may mention that on a visit to Mr. Lockyer I found him carrying on a series of observations which have challenged the admiration of the world in a temporary structure made of rough boards, unplastered, and scarcely including a space of fifteen feet square.

As to the location of your observatory, you will infer from what I have said that I think it important to connect it with some well-endowed and well-established college or university.

EFFECT OF THE MOON ON THE WEATHER.*

(From the Smithsonian Annual Report for 1871, pp. 460, 461.)

Since the form of the orbit of the earth is affected by the attraction of Venus and the other planets, as well as by its satellite the moon, they must in some degree also affect the form of the atmospheric covering of the globe, and tend to produce tides which are of greatest magnitude when they are in opposition to or in conjunction with the sun. But whether these disturbances of the atmosphere or those produced by the moon are of such a character as to give rise to the violent atmospheric commotions denominated storms, is a question which has long agitated the scientific world.

The times and peculiarities of the meteorological occurrences are more varied and less definitely remembered than almost any other natural phenomena, and hence the large number of different rules for predicting the changes of the weather. The only way of accurately ascertaining the truth of any hypothesis in regard to atmospheric changes, is that of having recourse to trustworthy records of the weather through a long series of years, and it is one of our objects in collecting meteorological statistics at the Smithsonian Institution to obtain the means of proving or disproving propositions of the character you have advanced.

The moon, being the body nearest to the earth, produces the highest tide in the waters of the ocean, and must also produce the greatest effect on the aerial covering of the earth. It has not been satisfactorily proved however that the occurrence of the lunar tides is connected with appreciable changes in the barometrical or thermometrical condition of the atmosphere. The less pressure of the air at a given place on account of the action of the moon, is just balanced by the increased height of the aerial column.

The principal causes of the violent changes of the atmos-

*[Letter to a correspondent in reply to inquiries and suggestions on the subject.]

phere are due I think to its instability produced by the formation and condensation of vapor. It is not impossible that when the air is in a very unstable condition on account of the heat and moisture of the lower strata, the aerial tide may induce an overturning of the tottering equilibrium at some one place in the northern or southern hemisphere more unstable than the others, and thus commence a storm which, but for this extraneous cause, would not have happened. To detect any such influence of the moon however, it will be necessary to compare simultaneously the records of the weather from day to day throughout all the northern and southern temperate zones, and to ascertain whether the maximum of these changes have any fixed relation in time to the changes of the moon.

The changes of the moon take place at a given moment on every part of the earth: the greatest effect of a lunar tide ought therefore to be felt in succession entirely around the earth in the course of about twenty-four and one-half hours.

The problem cannot be determined however by such casual observations as those which you narrate. I have not the least idea that the attraction of Venus produces any appreciable effect. It is too small to produce a result which would be indicated by any of our meteorological instruments.

I am far from subscribing to the justice of your remarks in regard to Mr. Espy, since I have a great respect for his scientific character, notwithstanding his aberration, in a practical point of view, as to the economical production of rain. The fact has been abundantly proved by observation that a large fire sometimes produces an overturn in the unstable equilibrium of the atmosphere and gives rise to the beginning of a violent storm, but it was not wise in him to insist on the possibility of turning this principle to an economical use.

ON THE ORGANIZATION OF A SCIENTIFIC SOCIETY.*

(Bulletin of the Philosophical Society of Washington, vol. I, pp. v-xiv.)

Delivered November 18, 1871.

GENTLEMEN: I have been requested to make some remarks on the character and object of this Society which may serve to introduce it to the world through the pages of a Bulletin of its proceedings, or the public journals of the day, and in compliance with this request, I beg leave to submit the following reflections on the importance, as well as on the proper conduct of such an association.

This Society was formed by the call for a meeting of a number of gentlemen impressed with the importance of an association of a strictly scientific character, in the city of Washington. At the meeting which resulted from this call, a name and a constitution were adopted for the Society, and without delay, in a series of subsequent meetings, the objects of the association were prosecuted with such marked success, as to fully realize the anticipations which had been entertained with regard to the enterprise. This is manifest from the number, character, and variety of the communications presented and discussed.

In regard to the name which has been chosen, "THE PHILOSOPHICAL SOCIETY OF WASHINGTON," it is proper to remark that it was adopted not without considerable deliberation. The term "Philosophical" was chosen not to denote, as it generally does in the present day, the unbounded field of speculative thought, which embraces the possible as well as the actual of existence, but to be used in its restricted sense to indicate those branches of knowledge that relate to the positive facts and laws of the physical and moral universe. The second term, "Washington," was selected to denote the fact that the Society is a *local* establishment; that it arrogates to itself nothing on account of its position at the

* [Anniversary Address of the President of the Philosophical Society of Washington.]

national capital; makes no claim to any connection with the government, nor to being in any respect a special representative of the science of the country.

The importance of such a society must be evident to all who are acquainted with the history of science. It is mainly through the influence exerted and the assistance rendered by such associations, that science is advanced and its results given to the world. Man is a sympathetic being, and no incentive to mental exertion is more powerful than that which springs from a desire for the approbation of his fellow men; besides this, frequent interchange of ideas and appreciative encouragement are almost essential to the successful prosecution of labors requiring profound thought and continued mental exertion. Hence it is important that those engaged in similar pursuits should have opportunities for frequent meetings at stated periods. This is more particularly the case with the cultivators of abstract science who find comparatively few fully capable of appreciating the value of their labors, even in a community how much soever enlightened it may be on general subjects. The students of history, of literature, of politics, and of art, find everywhere men who can enter in some degree into their pursuits, and who can appreciate their merits and derive pleasure from their writings or conversation; while the mathematician, the astronomer, the physicist, the chemist, the biologist, and the student of descriptive natural history, meet with relatively few who can sympathize with them in their pursuits, or who have a sufficient knowledge of their particular subjects to be able to award them that intelligent appreciation and encouragement essential to their sustained and laborious efforts. To them, the world consists of a few individuals to whom they are to look for that critical judgment of their merits which is to be finally adopted by the general public, and with these it is of the first importance that they should have more frequent intercourse than that which arises from casual meetings.

Furthermore, a society of this kind becomes a means of instruction to all its members, the knowledge of each be-

coming as it were the knowledge of the whole. Again, there is a common bond of union between all branches of science, since they all relate to the existence and laws of the same universe in which the more we extend our knowledge the more we find of "unity in the midst of infinite diversity."

This connection is obvious in the relations of astronomy, mathematics, and physics, as well as in those of geology, chemistry, and biology, which are so closely related in many cases as to be separable only by conventional limits. In a society therefore like the one in question, embracing in its objects as it does—all branches of science, each investigator may find others cultivating fields separated from his own by insensible degrees, from whom he can have not only full sympathy and adequate appreciation, but also in many instances, important suggestions and essential aid.

The governing body of such a society, in order that the organization may produce the desired effect, must be largely composed of men who by education and experience in the processes of investigation, are justly entitled to the appellation of "scientific," and who from their positive contributions to the science of the day, are acknowledged by the scientific world as worthy of this distinction. It is true that useful societies are formed for the self-improvement of their members by the production of essays on various subjects, or by cultivation of branches of natural history requiring no previous special training; the city of Washington however needs something of a higher order, namely a society for the *advancement* of science, since in no other city in the Union are there so many men, in proportion to the population, connected with scientific pursuits, or so many facilities afforded for scientific investigation.

The Philosophical Society of Washington, though of a local and unostentatious character, if true to itself and its mission, may accomplish much towards increasing the reputation of the country and influencing public opinion with regard to questions of a scientific character. However wide the diffusion of general knowledge, public opinion in regard to scientific questions must eventually be determined by the

authority of societies, journals, and individuals, of established scientific reputation. It is therefore of the first importance that the operations of this Society be conducted with great care, and that nothing be given to the world under its sanction which is not based upon thorough investigation or established scientific principles. We should be warned by the fate of a society established in this city some thirty years ago, which although it included among its members a few men of true science, was under the control principally of amateurs and politicians, and therefore was unfit to discharge the duty which it claimed as one of its functions, to decide questions of a strictly scientific character. It should have been borne in mind by this association that votes on questions in science should be *weighed*, not counted! Had the proposition of the motion of the earth been decided in the days of Galileo by the popular voice, this philosopher and his friends would have been vastly in the minority. The society to which I allude, after achieving an unenviable notoriety, by assuming to be the arbiter of the science of the country, gradually sunk into oblivion, from which its memory should not be recalled except as a warning to those who would adventure in the same line.

It is an essential feature of a scientific society that every communication presented to it should be subject to free critical discussion. Such discussion not only enlivens the proceedings but is generally instructive, frequently eliciting facts which though insignificant when isolated, when brought together mutually illustrate each other and lead ultimately to important conclusions. The extent to which discussions may be allowed evidently depends on the candor and temper of those who engage in them. Among the things to be avoided are merely verbal criticism, undue harshness on the one hand and unmerited praise on the other, regard being had to truth rather than to victory or mutual adulation. There is nothing perhaps that marks more distinctly one of the characteristics of a true scientist than the manner in which he receives and appropriates to his use the critical remarks that may be made upon his

communications. He can (in many cases at least) derive from them the indication that he has failed to present on some points a clear statement of his investigations; or that in some other points his conclusions are not fully sustained by the premises. Unfortunately, it frequently happens that persons of a sensitive disposition are apt to consider criticisms of the kind we have mentioned as personal attacks, and feel that it is as offensive to doubt the accuracy of their experiments or conclusions, as it is to doubt their word. It should be recollected however that the most gifted are liable to err, and that these criticisms are *prior* to publication, and therefore of value to the permanent reputation of both the individual and the society.

Another important matter in regard to such a society is the publication of its proceedings. If its object were merely the intellectual and moral improvement of its members it might dispense with any publication whatever,—even with the announcement of its existence. If however it aspires to the more important office of *advancing* science or of enlarging the bounds of thought and assisting to diffuse a knowledge of new truths, it should then publish—if not quarto volumes of transactions—at least a bulletin of its proceedings. This publication should present an exposition of the organization of the society, its constitution and by-laws, give a list of the members, a synopsis of the contents of all communications submitted for consideration, and an account of important facts which may be elicited during discussions or recalled to memory at the moment by association of ideas.

Such a bulletin will enable the members of the society to publish without delay through a proper channel a synopsis of their investigations, and also minor facts and inferences not considered in themselves of sufficient importance to form a communication to a scientific journal or to occupy a place in philosophical transactions. Such facts are nevertheless frequently found to be valuable contributions to the general stock of knowledge. Were it possessed of the requisite funds the society might establish a higher reputation by the publication of independent transactions. Inasmuch as this is

not the case however, the next best plan should be adopted, namely, that of publishing papers in full through other channels, such for instance as the Smithsonian Institution, the reports of government bureaus, and scientific journals. In such cases the bulletin should contain references as to where the articles in full are to appear, and in this respect it would do good service in assisting to make more generally known the valuable contributions to science which are diffused through voluminous executive and congressional documents not readily accessible to the scientific world.

The editing of the bulletin should be under the direction of the secretaries and a committee appointed for the purpose, and a number should be issued as often as material of the proper character and of sufficient quantity is accumulated. It should be distributed to the principal learned societies of this and other countries, and may also be presented to leading journals in this and other cities. Without at least such a publication, the society cannot have a recognized existence.

I have stated that there is no city in the United States where, in proportion to the number of its inhabitants, there are so many men of education actively engaged in pursuits connected with science, as in Washington. In illustration of this remark I may refer to those who are engaged in the Coast Survey, the Office of Weights and Measures, the National Observatory, the Nautical Almanac Office, Patent Office, Engineer Department, Hydrographic Office, Ordnance Department, Medical Departments of the Army and Navy, Light-house Board, Signal Corps, Agricultural Department, Bureau of Statistics, Census Office, Bureaus of Navigation and Steam Engineering, the Smithsonian Institution, etc., etc. In addition to this, no city in the Union possesses more ample facilities, in the way of books and implements, for the prosecution of scientific research. The library of Congress, enriched by the Smithsonian Deposit with the transactions of all the principal learned societies of the world, is almost unrivalled in scientific works. If to this extensive collection we add the special libraries of the Patent Office, the Agricultural Department, the Coast Survey, the National

Observatory, and the Surgeon-General's Office, we have a collection of modern books on science, accessible to the members of the society, scarcely surpassed by the collections of the most favored cities of the old world. Nor are the articles of apparatus—necessary for any line of investigation—beyond the reach of any member of the Society who may possess the knowledge and skill requisite to their proper use. There is great liberality on the part of the heads of departments in regard to furnishing apparatus that may in any degree facilitate the special investigations under their direction.

Among those connected with the various organizations just mentioned, a considerable number is engaged in *original* investigations, the results of which are of interest to the scientific world, and which will be facilitated and improved by the discussions of this Society. Furthermore, in the daily operations of the different establishments, facts of scientific importance are continually becoming evident that would be lost if not preserved in the records of the Society. It is not however alone to facilitate operations now going on, or to preserve facts that may have been casually discovered, but also to suggest new investigations and to encourage others to enter the field of research who have not yet essayed their hand in this direction. In the great domain of science, there is abundant room for an indefinite number of laborers of different grades of attainment and original powers of mind. A series of careful observations made with proper instruments, with regularity and precision, (requiring little more than the exercise of the senses and a conscientious regard for truth,) is frequently a valuable contribution to science. A series of analyses in which prescribed formulas are observed, and in the application of which no more talent is required than that possessed by the majority of persons of ordinary ability and education, may give results of scientific value. For the production of results of the kind mentioned, and those which are effected by the scientist who is capable of detecting hitherto undiscovered facts and developing new laws, there is room for all grades of talent and of powers of original investigation. It is remarkable how

much may be done by the association of minds determined on a common pursuit ; how much (under such conditions as exist in the city of Washington), may be effected in the way of directing attention to special lines of investigation, in suggesting questions to be asked of nature, and in pointing out the ready means by which the answers may be elicited, by arousing into activity talents that without such stimulus and suggestion would ever remain dormant.

The bane of many societies is the time consumed in details of business and in the discussion of non-essential points relative to their government. Happily, the organization adopted by this Society obviates this evil and secures the devotion of almost every evening exclusively to its legitimate purposes. For the government of men whose object is the advance of *truth*, but few rules are necessary, and these (unlike the laws of the Medes and Persians—expressed in inexorable codes) must consist of simple principles, readily adaptable to all contingencies.

In conclusion, I would say that with so many facilities as exist in the city of Washington for the pursuit of science, this Society would be derelict of duty did it fail to materially aid—through communion of thought and concert of action, the advancement of the great cause of human improvement. I am happy in cherishing the opinion however that the success of “The Philosophical Society of Washington” is scarcely any longer problematical, and in this I am sustained by the record of its transactions.

ON THE EMPLOYMENT OF MINERAL OIL FOR LIGHT-HOUSE
ILLUMINATION.

(Report of the United States Light-House Board for 1875; pp. 5, 6.)

- - - During the year, the Board—under the personal direction of its chairman (assisted from time to time by other members of the Board), has made an extensive and careful series of experiments with regard to the merits of the mineral oils of this country for the purposes of light-house illumination. In order to obtain a great variety of oils, the Board—on November 24, 1874—advertised in various newspapers published in different parts of the United States, inviting manufacturers and dealers to furnish it with specimens of domestic mineral-oil for test as to their fitness for light-house purposes. As soon as a sufficient quantity had been received, the investigation was begun, and it has been continued, with results which lead to the belief that there can be had in this country an oil of suitable quality for light-house use, and perhaps at a considerable reduction in expense.

For the purpose of comparing our mineral-oils with those now coming into use abroad, the Trinity House authorities have been requested to send to the Board a specimen of that used in lights under their control: and when this is obtained, which is expected to be soon, further experiments will be made. While with its present knowledge of the qualities of these oils, the Board proposes to put them into use at light-stations on the mainland, it would hesitate to endanger valuable property, and the lives of its employés, by placing them on board of light-ships, in structures standing in the water, or at other points from which the keepers could not escape in case of accident.

INVESTIGATIONS RELATIVE TO ILLUMINATING MATERIALS.

(Report of the United States Light-House Board for 1875; pp. 86-103.)

Preliminary Remarks.

It has been the policy of the Light-House Board since its first establishment not only to adopt the latest improvements that have been made in other countries, but also to add by original investigations to the sum of knowledge respecting aids to navigation. In accordance with this policy, the Board has endeavored to keep itself informed as to the progress of the light-house systems of other countries, and in the erection of new towers and the supply of new apparatus, to adopt those improvements which have from actual experience been preferred; and furthermore, the committee on experiments has devoted a portion of every year to investigations which might develop new facts tending to greater economy or efficiency in the various appliances by which the dangers of navigation are diminished.

At the commencement of the operations of the Light-House Board, in 1852, sperm oil was generally employed for the purpose of illumination. This was an excellent illuminant, but as its price continued to advance from year to year, it was thought proper to attempt the introduction of some other material. The first attempt of this kind was that of the introduction of colza oil, which was generally used in the light-houses of Europe:—an oil extracted from the seed of a species of wild cabbage, known in this country as rape, and in France as colza. For this purpose a quantity of rapeseed was imported from France and distributed—through the agricultural department of the Patent Office, to different parts of the country, with the hope that our farmers might be induced to attempt its cultivation. Although the climate of the country appeared favorable to its growth, and special instructions were prepared and distributed by the Light-House Board—for its culture and the means of producing oil from it, yet the enterprise was not undertaken with any ap-

proximation to success, except in Wisconsin, where a manufactory of rape-seed oil was established by Col. C. S. Hamilton, formerly of the United States Army. To this manufactory the Light-House Board gave special encouragement, and purchased at a liberal price—all the oil that could be supplied: the quantity however which could be procured was but a small part of the illuminating material required by the annual consumption of the Light-House establishment.

The price of sperm oil still continuing to increase, the Board employed Prof. J. H. Alexander, a chemist of Baltimore, to make a series of investigations on different oils, to ascertain a method of detecting adulterations in them, and to determine the relative economical value of different kinds of oil which might serve for use in light-houses. In his report Prof. Alexander recommended—as a means of detecting adulterations in oil, a thermal test based upon the amount of heat evolved by mixing a given quantity of the oil with sulphuric acid of a given specific gravity, and noting the rise of temperature as indicated by a standard thermometer in a unit of time. For using this method, it was proposed to ascertain by actual experiment—the heat evolved by mixing pure oils with a given quantity of acid, and afterward oils adulterated with given quantities of lard or inferior oils. This ingenious suggestion was however never reduced to practice. The method was too refined; the difference of heat evolved was scarcely sufficient to be noted unless great precautions were taken to prevent loss by radiation and conduction, and consequently it could not be employed by ordinary inspectors. In regard to lard oil, Prof. Alexander—not having ascertained the best conditions for burning it, consequently rated it very low in the scale of economical value as a light-house illuminant.

In this stage of the history of the subject, the chairman of the committee on experiments commenced himself to investigate the qualities of different kinds of oil, and was soon led to direct his attention to the comparative value of sperm and lard oils. The experiments made by Prof. Alexander

were with small lamps, and the comparison in this case, as will be shown, was much against the lard oil.

Experiments on Lard Oil.

The first experiment of the new series consisted in charging two small conical lamps of the capacity of about a half pint, one with pure sperm oil and the other with lard oil. These lamps had single-rope wicks, each containing the same number of strands; they were lighted at the same time, and the photometrical power ascertained by the method of shadows. At first the two were nearly equal in brilliancy, but after burning about three hours the flame of the lard oil had declined in photometric power to about one-fifth of that of the sperm oil. The question then occurred as to the cause of this decline, and it was suggested that it might be due—first, to a greater specific gravity in the lard oil, which would retard the ascent of it in the wick, after the level of the oil had been reduced by burning in the lamp; or second, to a want of sufficient attraction between the oil and the wick to furnish the requisite supply as the oil descended in the lamp; or third, it might be due in part to the imperfect liquidity of the oil, which would also militate against its use in mechanical lamps.

The lard oil was subjected to experiments in regard to each of these points. It was found—by the usual method of weighing equal quantities of the two fluids, that the specific gravity of the lard oil was greater than that of the sperm oil.

It was also found—by dipping two portions of the same wick into the two liquids and noting the height to which each ascended in a given time, that the surface attraction of the sperm oil was greater than that of the lard oil, or in other words the ascensional power of sperm oil was much greater than that of lard oil at ordinary temperatures. This method was also employed in obtaining the relative surface attraction of various other liquids. I say surface attraction instead of capillarity, because it was found in the course of these investigations that substances which had less capillarity (that

is less elevating power in a fine tube) had sometimes greater power in ascending in the meshes of a wick.

The relative fluidity of the different oils was obtained by filling with them in succession—a pear-shaped vessel of about the capacity of a pint, with a narrow neck, and having a hole in the lowest part of the bottom of about a tenth of an inch in diameter. Such a vessel filled with any number of perfect liquids would be emptied in the same time, whatever their specific gravity. As at any given horizon inertia is directly proportional to gravity, the heavier the liquid the greater would be the power required to move it; but the motive power would be in proportion to the pressure, or in other words to the weight, and therefore all perfect liquids should issue from the same orifice with the same velocity. To test this proposition, eight fluid ounces of clean mercury, and then the same bulk of distilled water, were allowed to run out of the vessel above mentioned; the time observed was the same within the nearest second. It was found in repeating this experiment with sperm and lard oils that the rapidity of the flow of the former exceeded considerably that of the latter, the ratio of time being 100 to 167.

The results thus far in these investigations were apparently against the use of lard oil; it was observed however in the experiments on the flow of the two oils on different occasions that a variation in the time occurred, which could be attributed only to a variation in the temperatures at which the experiments were made. In relation to this point, the effect of an increase of the temperature above that of the atmosphere—on the flowing of the two oils, was observed. By this means the important fact was elicited that as the temperature was increased, the liquidity of the lard oil increased in a more rapid degree than that of the sperm oil, and that at the temperature of about 250° F. the liquidity of the former exceeded that of the latter.

A similar series of experiments was made in regard to the rapidity of ascent of the oil in the wick, and with a similar result. At about the temperature just above mentioned, the ascensional power of the lard oil was greater than that of

the sperm oil. These results were recognized as having an important bearing on the question of the application of lard oil as a light-house illuminant. It only required to be burned at a high temperature, and as this could be readily obtained in the case of larger lamps, there appeared to be no difficulty in its application.

The previous trials had been with small lamps with single solid wicks, instead of the Fresnel lamp with hollow burners. After these preliminary experiments two light-houses of the first order at Cape Ann, Massachusetts, (separated by a distance of only 900 feet,) were selected as affording excellent facilities for trying—in actual burning, the correctness of the conclusions which had been arrived at. One of these light-houses was supplied with sperm oil and the other with lard oil, each lamp being so trimmed as to exhibit its greatest capacity. It was found by photometrical trial that the lamp supplied with lard oil exceeded in intensity of light that of the one furnished with sperm oil. The experiment was continued for several months, and the relative volume of the two materials carefully observed. The quantity of sperm oil burned during the continuance of the experiment was to that of lard oil as 100 is to 104.

The freezing temperature of lard oil depends upon the temperature at which it was expelled by pressure from the animal tissues in which it was contained. It is higher however than the freezing temperature of sperm oil, on an average of from 3° to 4° F., but this is a matter of no practical objection to the substitution of lard oil for sperm oil, since the heat evolved from an Argand lamp is—in cases where the draught passes through the reservoir, sufficient to keep the lard oil liquid even during the lowest external temperature. Indeed, the small difference in temperature in freezing of the two oils is a matter of little moment in cases which frequently happen when the temperature of the atmosphere is below zero on the Fahrenheit scale. At such a temperature both oils would alike become solid unless some means were afforded for preventing the freezing.

The next step toward the introduction of lard oil was the

devising of a system by which it could be inspected, and the Board assured—before it should be too late to remedy the evil, that the lard oil purchased was of a good quality. This was a matter of great importance, and involved no small degree of responsibility, since the contractor was entitled to his pay immediately after the acceptance of the oil; and the quantity purchased amounted annually to nearly 100,000 gallons.

The conclusion was arrived at that it was impossible—from any single test that could be applied to small samples, to determine the quality of the oil as applicable to light-house purposes, and that in the present state of our knowledge as to its character, the following tests are required to fully insure in all cases the required quality of the article:

1. Specific gravity at 60° F.
2. Liquidity at different temperatures.
3. Freedom from acids or alkalies.
4. Resistance to freezing.
5. Actual burning in fifth-order lamps for at least ten hours.
6. Photometric power after burning one hour, and again after burning ten hours.
7. The condition of the wick at the end of the burning.

These tests are of very unequal value, and several of them might be dispensed with, were others reduced to an absolute standard—determined by the actual experience of burning in the light-houses.

The specific gravity of impure lard oil and of that which has been carefully refined—differ but little, and hence unless the experiment be made by means of a delicate balance, the indications will be of comparatively little value. Still, as a given sample might contain some foreign substance which is not usually mixed with this oil, the test with the hydrometer should not be omitted.

In making this test, a cylindrical vessel containing the oil—of sufficient diameter to permit the hydrometer to float freely without hindrance from the sides, should be immersed in a vessel containing several gallons of water, which when

once reduced to 60° by the addition of ice-cold water, can (on account of the great specific heat of water,) be readily kept at that temperature by a slight addition of cold water from time to time, the whole being continually stirred. It is scarcely necessary to state that the vessel containing the oil must be so weighted at the bottom that it will stand erect in the cold bath in which the experiment is made.

Liquidity at different temperatures is a test similar in character to that of specific gravity; although the difference in degree of liquidity of different kinds of oil, such as sperm, whale, and lard, is very considerable, the difference between different samples of lard oil is small. Still this test (for a similar reason to that given for the specific gravity,) should be applied.

The test for free acids and alkalies is easily made, and should in no case be omitted. A portion is put into beaker-glasses, with a slip of litmus-paper in one and a slip of turmeric paper in the other, and suffered to remain immersed perhaps twenty-four hours; and at the end of that time, if one of these papers exhibits no redness and the other no brownness, the oil may be considered void of free acid and of alkali,—either of which would lessen its value, the former tending to corrode the lamp, and the latter interfering with the burning quality.

Resistance to freezing is an important test, but not as easily applied in the case of lard-oil as might at first be imagined. Lard-oil possesses the remarkable property of resisting the influence of a low temperature if suddenly applied, while it will freeze at a much higher temperature if the cold be continued for several hours.

For example if a small portion of lard oil be placed in a test-tube and submitted to a rapid diminution of temperature by being plunged in a freezing-mixture, it will remain liquid for some time at a temperature of 19° or 20° , whereas it will congeal at a temperature of 40° if suffered to remain at that temperature for several hours.

The plan adopted for determining the freezing-point of different samples of oil, at one operation, consisted in making a series of small openings or windows, closed with

glass, in the side of a cylindrical wooden tub about $2\frac{1}{2}$ feet in diameter. Concentric within this tub was placed another cylindrical vessel (of smaller diameter) of zinc, filled with a freezing-mixture of salt and pounded ice. A series of small beaker-glasses, filled with the several samples of oil, was placed opposite the windows in the space between the two cylinders, each containing a thermometer which could be read through the window. The whole was then inclosed by a tightly-fitting cover, through which projected the handle of a crank, by which the freezing-mixture could be stirred. The samples of the oil subjected to this cold-air bath gradually passed through the several stages of diminution in limpidity and clearness—to opacity and solidity, the time of each being noted.

The most reliable test is that of actual burning in a lamp of the fifth order and the measurement of the photometrical power. The objection to the application of this test to the oil of every barrel is the large quantity of oil required and the amount of labor involved in the proper execution of the process. Thus in testing 60,000 gallons contained in casks of forty gallons each, at least 500 gallons would be required. It is therefore evident that this test can only be applied to samples selected from a given lot, while the single barrels are proved to be of a similar character by the more simple tests.

Another method of insuring that all the casks of a given lot contain oil of the same quality consists in taking a small equal portion from each of several casks and mingling them together, the quality of the compound being ascertained by the application of burning or the other tests.

The determination of the photometrical power is in the present state of science (unless great precaution is observed), a problem of some uncertainty. The difficulties are of two kinds, the first to find a photometer which shall give the ratio of the two lights, and second, to find an invariable standard to which oil of the proper quality may always be referred.

These difficulties can I think be sufficiently overcome for the practical purposes of the Light-House Board. The greater difficulty is that of obtaining a standard of reference.

For this a sample of lard oil manufactured by Mr. Alden, of Boston, was at first employed, but this itself was found to be variable, and hence we were obliged to adopt some other standard. The one which has been finally adopted is the English sperm candle, which burns with considerable uniformity at the rate of 120 grains per hour, or two grains per minute.

In regard to the investigation, the experiments were carried on under many difficulties. They were made at first in the engineer's office of the second light-house district in Boston, with such appliances as could be procured at the moment, with the assistance of Mr. William Goodwin, the acting light-house engineer, who took much interest in the subject and rendered efficient service.

In the erection of a new lamp-shop at the Staten Island depot, care was taken to make provision for a dark room in which the photometrical examinations could be made with more precision than had been obtained in the temporary apartments previously used. This room extends the whole length of the building, is about 80 feet long by 12 wide; the windows are closed by iron shutters to exclude the light; and the walls, floor, and all other parts are painted black, after being sanded to remove any glare which might exist.

In the first experiments on lard oil the photometrical process employed was that of Rumford, which consists in ascertaining the relative intensity of two lights from their distances from a screen on which shadows of equal darkness are thrown by an intermediate body. In this case the relative intensities sought are indicated by the square of the distances in inches and parts of inches of each light from the screen on which the shadows are cast. But this method, which is used by the French manufacturers of apparatus, and is very simple in theory, does not admit of much accuracy.

The arrangement therefore known as Bunsen's photometer was introduced in its stead; and this (with some peculiar modifications) leaves nothing to be desired. This arrangement consists in placing two lights at the extremity of a scale so divided into distances that the relative intensity of

the two flames may be immediately read off in terms of candle-power, when a small intermediate movable screen is equally illuminated on both sides. This screen is usually formed of a piece of white pasteboard of about four inches square, fixed perpendicularly at right angles to the length of the scale in a sliding frame, by which it can be brought nearer to or farther from one of the lights. In the centre of this square is a circular hole of about half an inch in diameter which is closed by a piece of thin paper, rendered translucent by a solution of spermaceti in oil of turpentine. This forms a spot which is darker than the other parts of the white screen, and is equally dark on both surfaces when the screen is receiving an equal quantity of light from each flame; the screen is moved backward and forward until this effect is produced, and the index will then point on the graduated scale to the number of the relative power of one of the lights in the terms of the other.

The screen may also be made of thin paper, the whole of which is rendered translucent except a round spot in the centre of half an inch in diameter. If a light is placed before the screen on one side, the whole of the greased part will appear dark, on account of part of the light going through the translucent portion. If now another light be placed on the opposite side an equal portion will be transmitted through the pellucid part, and the two surfaces will appear of like intensity when the two lights are equal, or when from their respective distances they throw equal amounts of light on the two faces of the screen.

In order that both sides may be seen at the same moment without moving the head from one edge of the screen, two mirrors making with each other an angle of 90° are placed so that the screen itself will bisect the angle.

For dividing the scale into parts related to each other as the square of their distances from a centre, the following formula and table will furnish the means. Let a be the length of the scale, and x the distance from the candle end to the movable screen; then $a-x$ is the distance between the lamp end and the screen. Denote the degree of illumination on the candle and lamp sides of the screen

by L and L' respectively. Let the intensity of the candle end equal one candle, while that of the lamp is n candles. Then, since the illumination of the screen varies directly as the intensity and inversely as the square of the distance, we have the following proportion:

$$L : L' :: \frac{1}{x^2} : \frac{n}{(a-x)^2}, \text{ and when } L = L' \text{ we have } (a-x)^2 = nx^2$$

whence $x = \frac{a}{1 + \sqrt{n}}$ For convenience of using this formula

it is best to change its form into $x = a \frac{\sqrt{n}-1}{n-1}$.

The following table has been computed by calling the length of the scale 100 and assigning successive integral values to n , from 1 to 100. The column A shows the value of $a-x$ for each assumed value of n :

Table of distances and candle-powers.

Number of can- dles.	A	Number of can- dles.	A	Number of can- dles.	A	Number of can- dles.	A
1	50.00	26	16.40	51	12.28	76	10.29
2	41.42	27	16.14	52	12.18	77	10.23
3	36.60	28	15.89	53	12.08	78	10.17
4	33.33	29	15.66	54	11.98	79	10.11
5	30.90	30	15.44	55	11.88	80	10.05
6	28.99	31	15.23	56	11.79	81	10.00
7	27.43	32	15.02	57	11.70	82	9.94
8	26.12	33	14.89	58	11.61	83	9.89
9	25.00	34	14.64	59	11.52	84	9.84
10	24.03	35	14.46	60	11.43	85	9.79
11	23.17	36	14.29	61	11.35	86	9.73
12	22.40	37	14.12	62	11.27	87	9.68
13	21.71	38	13.96	63	11.19	88	9.63
14	21.08	39	13.80	64	11.11	89	9.58
15	20.52	40	13.65	65	11.04	90	9.54
16	20.00	41	13.51	66	10.96	91	9.49
17	19.52	42	13.37	67	10.89	92	9.44
18	19.07	43	13.23	68	10.82	93	9.40
19	18.66	44	13.10	69	10.75	94	9.35
20	18.27	45	12.97	70	10.68	95	9.31
21	17.91	46	12.85	71	10.61	96	9.26
22	17.58	47	12.73	72	10.54	97	9.22
23	17.25	48	12.61	73	10.48	98	9.17
24	16.95	49	12.50	74	10.41	99	9.13
25	16.67	50	12.39	75	10.35	100	9.09

The standard adopted with which to compare all other lights is (as we have said) that of the London sperm candle, which (under ordinary conditions) burns 120 grains of sperm per hour. If it burns more or less than this amount during the trial, a correction of a proportional amount is made in the results.

This standard however is too small for determining the power of large lamps, and for this purpose an intermediate standard is provisionally adopted. For example—in determining the power of a lamp of the first order, the power of a lamp of the fourth order is first obtained, and this is used as a comparison with the larger lamp.

In the case of the arrangement—at the Staten Island depot, for photometrical measurements, three scales are employed, diverging from a centre at which the lamp to be measured is temporarily placed; at the farther end of each scale is placed a sperm candle to serve as the standard of comparison. These scales are of different lengths, one being 100 inches in length, another 150 inches, and the third 200 inches; besides these, one of the scales is occasionally replaced by one of 700 inches in length, which is put up in sections.

As the semi-diameter of the burner of the lamp and that of the candle must be included in the length of the scale, a portion of the latter at each end is cut off. In adjusting the scales therefore to their places, the measurement must be taken from the middle of each scale; thus in the case of the one of 200 inches in length, the middle of it must be just 100 inches from the centre of the lamp on one side and 100 inches from the centre of the candle on the other. In making the examination, three observers (one at each scale) simultaneously take the photometric readings, and the mean of the three results is adopted as the candle-power of the light under examination.

In the examination of oil previous to purchase, (as before stated,) a lamp of the fifth order is charged with the oil in question, and when in a state of equilibrium of combustion—is subjected to the trial. For greater precision ten read-

ings are taken on one side of the scale, and then the photometer is reversed and as many taken from the opposite side. In this way the mean of sixty readings, twenty on each scale, furnishes the data on which the character of the oil principally rests. As a means of simultaneously weighing the candles for checking the effects of their irregular burning, three balances are provided, each of which bears one of the candles in a socket supported by a metallic link, through which the scale-beam passes and is attached to the hook of the scale-pan below.

On the opposite scale-pan a series of grain weights are placed, which can be taken off by a pair of pincers without disturbing the equilibrium of the scale; the interval of time during which a given grain weight is burned is marked by a watch. If the interval is equal to two grains for each minute, the candle is burning at its normal rate; if not, a correction is made by simple proportion, which is applied to the measurement previously obtained.

The lamps containing the oil for trial are lighted and trimmed in an adjoining apartment. They are introduced into the dark room through a window closed with a sliding shutter. In order to prevent an overflow of oil at the burner by the oscillation of the liquid in the reservoir by the agitation of transfer, each lamp is placed on a small carriage moving on a railway passing through the window, which enables the lamp to be placed in its position with rapidity, and without the slightest disturbance of the equilibrium of the oil.

The temperature of the room is also noted, and as far as possible it is kept at a heat of not far from 70°. For this purpose—during warm weather, the inspection may be made at night.

For reading the divisions on the scales in the dark room, a mirror is employed to throw the light of the lamp under inspection on the graduation.

To exclude all extraneous light, the three candles and the lamp to be tested—are each surrounded by a cylindrical sheet-iron screen painted black, through which a hole (a little larger than the flame) allows the light to pass along the scale

to the photometer. The trial-lamps are those of the fifth order. Each (after it has been lighted) is allowed to burn an hour before being submitted to the photometrical measurement. If it gives a power less than 8 candles, the oil is rejected. If it passes that test, it is allowed to burn undisturbed without being trimmed—for 8 or 9 hours longer, and if it is found at the end of that time to exhibit no diminution in the brilliancy of the light, it is considered worthy of adoption, especially if after this it continues to burn 4 or 5 hours additionally with no perceptible diminution that can be detected with the naked eye. The best lard oil will burn 16 hours without trimming.

Each candle before the measurement commences is suffered to burn until it has assumed a perfect and uniform rate of consumption: it should be prevented from guttering by removing a portion of the melted spermaceti which may accumulate in the cup at the top of the candle beyond the power of the feeble incipient flame to consume,—by absorbing it with one end of a strand of candle-wick cautiously introduced. If any portion of the spermaceti is suffered to run down the side of the candle and drop off below, the correction for variation in burning will be worthless.

All materials for the use of the Light-House Establishment are purchased by contract in accordance with published specifications as regards quality and certain conditions. The award is given to the lowest bidder, provided he can offer trustworthy surety as to his ability to fulfill the contract. When bids are equal, or nearly so, preference however is given to the bidder who is a manufacturer of the oil, and not a mere vender of the article. During the inspection, permission is granted to the contractor to be present at the operation, in order that he may be assured that full justice is done him in the examination. After seeing the precision with which the photometric and other processes are conducted, he is generally fully satisfied as to the results obtained, even though his oil may have been rejected.

The oil is delivered in iron-bound casks, varying from 38 to 50 gallons. These are placed (previous to inspection) under a shed and arranged in different lots, each containing

oil of the same quality. From different casks samples are taken in tin canisters of a capacity of about half a gallon, each canister being marked with the number of the lot and the cask from which the oil was taken. Before the sample is drawn from the cask the oil within is thoroughly mixed by rolling the cask or by stirring. The object of this is to obtain in the sample an average amount of solid matter which may be contained in the oil.

The purest lard oil is that which is manufactured by submitting the solid leaf lard to great pressure during the coldest period of winter. Oil of this quality is used for burning in small mechanical lamps; it gives a bright flame and does not incrust the wick. The light-house lamps however—being of a much larger size and evolving a much greater amount of heat, can consume oil of a coarser character; and indeed it has been found that oil containing a certain amount of solid matter, provided the latter is not too much in quantity to be consumed by the lamp, gives a higher illuminating power. On this account—before this fact was generally known in the trade, complaints were made of the Light-House Board giving the preference to oil which in the market would not be considered of the first quality.

The quantity of oil is estimated by weight, allowing 7·6 pounds per gallon. It is weighed in gross and afterwards emptied into large tanks in an under-ground vault. The empty barrels are next weighed; the weight of these deducted gives the net weight of the oil.

Previous to the establishment of the general light-house depot at Staten Island, from which all the supplies are now distributed, and where the lamps and other light-house appliances are prepared for immediate use, the oil was received at various ports along the coast in accordance with terms of the contract, and was stored until wanted for use, in cellars hired for that purpose.

After the introduction of lard oil however, the board constructed a spacious under-ground receptacle capable of containing 50,000 gallons of oil, and retaining it during the whole year at a temperature not to exceed 65° Fahrenheit.

The under-ground vault contains five tanks, each of the

capacity of ten thousand gallons. On each tank is a register, consisting of a glass tube so divided as to give the contents in hundreds of gallons. The oil is delivered in three installments: The first on the 1st of May, the second on the 10th of June, and the third on the 22d of July. The vault and tanks were constructed under the direction of General Poe while engineer secretary of the board, who took a lively interest in the introduction of lard oil and in the preliminary experiments for determining its quality.

A photometer room was afterwards fitted up in the Smithsonian Institution, in which several series of investigations were made in regard to the illuminating power of different oils. At the same time a series of experiments was undertaken relative to their chemical characters and conditions, in which experiments the chairman was assisted by Prof. C. M. Wetherill whose untimely death the science of this country has been called to mourn. Among the investigations in the laboratory, are those given in the following table, relative to the expansions of different oils—intended to facilitate the purchase, the measurements being made at different temperatures. To obviate the necessity of the correction for temperature, the oil is now purchased by weight. The following results may however be of value in the application of different oils to light-house purposes:

Experiments upon Light-House Oils.

[Density and volume of oils (and water) at different temperatures.]

Temperature, C.	Sperm oil.		Whale oil (unrefined).		Lard oil (refined).		Lard oil (unrefined).	
	Volume.	Density.	Volume.	Density.	Volume.	Density.	Volume.	Density.
4°	1.0000	0.89256	1.0000	0.92825	1.0000	0.92488		
10°	1.0053	0.88788	1.0049	0.92370	1.0042	0.92103	1.0000	0.92086
15°	1.0095	0.88418	1.0095	0.91952	1.0093	0.91632	1.0051	0.91614
20°	1.0134	0.88072	1.0145	0.91498	1.0124	0.91356	1.0109	0.91090
25°	1.0168	0.87778	1.0166	0.91311	1.0164	0.90992	1.0146	0.90760
30°	1.0208	0.87432	1.0200	0.90999	1.0204	0.90641	1.1169	0.90556
35°	1.0243	0.87139	1.0236	0.90688	1.0237	0.90351	1.0204	0.90247
40°	1.0286	0.86721	1.0297	0.90146	1.0278	0.89986	1.0244	0.89897

Experiments upon Light-House Oils—Continued.

[Density and volume of oils (and water) at different temperatures.]

Temperature, C.	Kerosene.		Water (C. M. W.)		Water (Kopp).		Alcohol (Pierre), vol. at 0° C. = 1 vol.	
	Volume.	Density.	Volume.	Density.	Volume.	Density.	C.	Density.
4°	1.0000	0.81199	1.00000	1.00000	1.0000	1.00000	0°	1.0000
10°	1.0050	0.80799	1.00048	0.99952	1.0003	0.99975	10°	1.0107
15°	1.0106	0.80347	1.00086	0.99915	1.0008	0.99918		
20°	1.0152	0.79984	1.00176	0.99824	1.0017	0.99831	20°	1.0217
25°	1.0187	0.79709	1.00303	0.99698	1.0028	0.99717		
30°	1.0234	0.79346	1.00447	0.99555	1.0042	0.99579	30°	1.0331
35°	1.0276	0.79020	1.00619	0.99384				
40°	1.0321	0.78674	1.00774	0.99232	-----	-----	40°	1.0448

*Chemical Analysis of Light-House Oils.*No. 1.—*Refined winter-pressed lard oil.*

	First ex- periment.	Second ex- periment.	Mean.	By calcula- tion.
Carbon -----	76.87	76.53	76.75	C ₄₄ 76.74
Hydrogen -----	11.58	11.63	11.61	H ₄₀ 11.63
Oxygen -----			11.64	O ₅ 11.63
			100.00	100.00

No. 2.—*Crude lard oil.*

Carbon -----	77.07	76.70	76.88	
Hydrogen -----	11.72	11.69	11.71	
Oxygen -----			11.41	
			100.00	

No. 3.—*Sperm oil.*

Carbon -----	79.52	79.41	79.46	C ₅₃ —79.70
Hydrogen -----	12.28	12.28	12.28	H ₄₉ —12.28
Oxygen -----			8.26	O ₄ — 8.02
				100.00

*Experiments of Mixing Oils with Oil of Vitriol of 66° Beaumé,
at 62° F.*

[Of oil, 2 fluid ounces; of sulphuric acid, 1 fluid ounce.]

First experiment.—Winter-pressed lard oil.

Temperature of oil before mixing	70° F.
Temperature of oil after slow mixing	130°
Difference	60°

At the expiration of 3 minutes, temperature..... 134°

At the expiration of 4 minutes, temperature..... 134°

Second experiment.—Winter-pressed lard oil.

Temperature before mixing	70° F.
Temperature after mixing rapidly	169°
Difference	99°

Third experiment.—Winter-pressed lard oil.

Temperature before mixing	70° F.
Temperature after mixing	165°
Difference	95°

Fourth experiment.—Crude lard oil.

Temperature before mixing	66° F.
Temperature after mixing	164°
Difference	98°

Refrigeration of Oils.

Those experimented upon were whale, sperm, refined lard, and crude lard oils.

First experiment.—At 30·2° F. they were all sirupy; in the crude lard oil a yellowish solid began to separate.

At 26·6° the sperm oil began to solidify.

At 24·8° the refined lard oil began to yield a white precipitate.

At 17·6° the whale oil was a thick sirup, without deposit. The crude lard oil was quite hard. The pure lard oil was not as hard as the crude lard oil. The sperm oil was not as hard as the pure lard oil. These experiments performed in test tubes.

Second experiment.—Upon pure winter-pressed lard oil, in a test tube.

At 17·6° F., it begins to deposit flakes of solid matter.

At 14° it is quite thick.

At 10·4° it is perfectly solid.

If now the temperature rises, a small portion of the oil remains solid until the temperature reaches 44·6°.

Third experiment.—The oils were placed in large cylinders and exposed to a temperature of 24·8° F., with the following results:

1. Crude lard oil, much sediment.
2. Sperm oil, ditto.
3. Pure refined lard oil, a little sediment.
4. Winter-strained lard oil, very little sediment.
5. Whale oil, no sediment.

In the use of sperm oil, it was found that the purer it could be obtained the better, and hence it was the custom to strain the oil (and also the drippings) through clean white sand previous to using it. In the case of lard oil however, (as before stated,) it was found that removing all the solid matter diminished its photometric power.

All fatty oils absorb oxygen, which unites with them to form oxides of their combustible ingredients; accordingly oil freely exposed to the air must in time gradually diminish in its power of combustion. It should therefore not be open to the atmosphere when the oil is to be stored, but covered with a thin wooden plane which floats upon the surface of the oil and thus in a great measure excludes the air. The freezing of lard oil does not appear to affect its quality.

Considerable difficulty was experienced in the introduction of lard oil on account of the objection to it on the part of the keepers; in some cases from the want of experience in using it, and in others from the interference of venders of sperm oil. This difficulty however was obviated by a resolution of the board, by which any keeper who declared his inability to burn lard oil should be requested to resign, since it had been abundantly proved that this oil with proper management could be made to compete favorably with sperm oil. Its introduction was a matter of great importance in an economical point of view; it saved the Government \$100,000 annually for several years.

Another important step in the introduction of lard oil was that of furnishing a lamp which would burn it with the greatest perfection. This was effected by the invention of Mr. Joseph Funck, foreman of the lamp-shop. In order to burn

lard oil, it is necessary (as has been said) that it should be kept at a high temperature, and for this purpose the heat of the draught of the lamp was passed through the centre of the reservoir.

Previous to the change in the illuminating material there had been used in the light-house establishment three classes of lamps, viz, the mechanical lamp for the first, second, and third orders, and the moderator and fountain lamps for the fourth, fifth, and sixth orders.

In the mechanical lamp the oil was placed in a reservoir below the burner and pumped up by means of clock-work. This apparatus is of a complicated character, and is subject to derangement. The valves must be renewed from time to time and the clock-work cleaned. The proper performance of these operations is beyond the skill of an ordinary keeper, and requires the frequent service of a trained and expert attendant.

The moderator lamp is less complicated, and was invented to obviate the difficulties just mentioned. In this the oil is elevated by the descent of a heavy piston, and forced up through a small conical hole, the flow being regulated by the conical end of a wire, which is gradually withdrawn as the weight descends, so as to give a less obstructed flow as the hydrostatic pressure of the oil increases. From this arrangement it takes its name of moderator lamp. This apparatus however is liable to irregularity on account of derangement of the supplying apparatus, the varying friction of the packing of the piston, as well as the change in the flow of the oil, owing to its diminished liquidity on a reduction of its temperature.

The reservoir of the fountain lamp consists in an air-tight vessel, (usually cylindrical,) from the bottom of which descends a tube, terminating at the open end in a small cup, from which the burner is directly supplied with oil on the well-known principle of the bird fountain, this vessel being filled with oil by inverting it and pouring in the liquid through the open end of the tube. It is then re-inverted and the end of the tube inserted in the small cup below the level of the

oil which it contains. The oil in the reservoir in this condition is supported by the pressure of the atmosphere on the surface of the oil in the cup. When this surface is lowered by burning, the end of the tube is opened, and a bubble of air passes up and an equal bulk of oil descends, and in this way a nearly constant level of oil is maintained. I say nearly constant because the air which goes up is of some volume and in the act of passing up produces an oscillation which in some degree affects the steadiness of the flame.

There is however a greater defect in this lamp from the oscillations in the level when the reservoir has been exhausted of a considerable portion of its charge of oil. In this case the arrangement is one similar to an air thermometer with a large bulb, and is affected by a sudden draught produced by the opening and shutting of a door or the ordinary ventilation of the lantern. This was partly remedied by bending the tube, and thereby increasing the resistance to a sudden change in the level of the oil.

The improvement of Mr. Funck consisted in substituting for these lamps one of constant level, in which the oil is placed above the burner, and the flow of oil necessary for perfect combustion is regulated by a small floating piston, placed in an enlarged portion of the supply-tube, and carrying on its upper surface a conical projection which increases or diminishes the size of the supplying orifice in accordance with the rapidity of combustion. This lamp is not only free from the objections pertaining to the other lamps, but is less expensive and better adapted to the burning of lard oil. It affords a freer combustion, and consequently a more intense light, though at the cost of a larger amount of the burning material.

In this lamp the heated air and products of combustion pass through a cylindrical opening in the reservoir, which is placed directly above the lamp, the opening in it forming as it were a prolongation of the chimney, thus not only preventing the oil from freezing in the coldest weather, but supplying it to the burner at the temperature best adapted for perfect combustion.

Established Superiority of Lard Oil.—In regard to the comparative character of lard and colza oils we may be allowed to print the following letter from Colonel Hamilton, the manufacturer of the latter oil, who was present at the trial to which he alludes:

FOND DU LAC, WIS., May 16, 1868.

DEAR COMMODORE: I must confess my great disappointment at the result of the experiments at Staten Island. It is however not really so much the failure of rape-seed oil as the undeniable excellence of lard oil as a burner. I fully believe that our rape-seed oil of this year is as good as any that was ever made in Europe, and I know it is far better than any we have ever before made. I am satisfied now that for self-heating lamps there is no oil that will bear comparison with lard, but I am equally satisfied that no colza oil will yield a better result than ours under exactly the same tests. We have but one more experiment to make with colza; it is its extraction by chemical displacement. If this fails we shall abandon the whole business.

If all things are put together, I think the following statement will be allowed, to wit: Our colza oil is equal to any foreign colza. It is better than any we have heretofore made. It is better than sperm oil or any other burner, excepting only lard oil. Our failure then is owing to the superior excellence of lard oil, which, under the persistent investigation of the board, has been shown to be the best and cheapest safe illuminator available.

The board are entitled to great credit in producing this result. It will be remembered that but a few years since, lard oil was pronounced unsuitable for light-house purposes, but the perseverance of the board has brought out the fact that it is much the best and cheapest oil, and that the expenses of lighting the coast and harbors have been thereby greatly reduced. Surely the country at large should acknowledge this, and give due credit to the board. We have endeavored to do with colza what the board has effected with lard oil, and we have been unsuccessful both for ourselves and the light-house interest. The undertaking has been no source of profit to us, and had the capital and time that have been devoted to colza been used in our other branch of manufacture (linseed oil), it would at least have re-im-bursed us with a fair remunerative return. As regards the oil we have offered, we have hoped the board would take it. I do not think we can improve upon the quality, and it is the last we shall venture to offer to the acceptance of the board, for we shall henceforth abandon the manufacture, except for local wants.

We are grateful to each member of the board for the interest they have always shown in our undertaking, and for their uniform kindness and courtesy. Accept, my dear Commodore, for yourself and your associates in the board, my warmest thanks for your many kind expressions of interest, and believe me, truly and gratefully, yours,

C. S. HAMILTON.

Com. A. A. HARWOOD, U. S. N.,
Secretary Light-House Board, Washington, D. C.

From the date of the introduction of lard oil in 1865, '66, and '67, until the end of 1873, when the attention of the board was again directed to the study of mineral oil, continual improvements were made in the processes of its preservation and inspection, and also in the lamps and other appliances for its employment, and nothing further as a

light-house illuminant was required. It is therefore with regret that we are urged, on account of the increased price of the article, due in some degree to the reputation as a burning material given it by the board itself, to substitute for it a less reliable but a much more economical material.

Experiments on Mineral Oils.—At the time lard oil was introduced, a series of experiments was made on the comparative value of the different petroleum oils used in this country. They were all considered too dangerous to be intrusted to the ordinary keepers of the light-stations of our coast. Since the date of these investigations however, improvements have been made in the manufacture of these oils, by which a much greater range has been obtained in the temperature at which they give off a noticeable vapor. During the last two years, a new series of investigations therefore has been made relative to these illuminating agents, of which we propose in the succeeding pages to give a brief account.

The crude petroleums of the Pennsylvania oil region are of a greenish or yellowish appearance, and have a specific gravity of 45° to 49° Beaumé at a temperature of 60° Fahrenheit. Some are so volatile as to evaporate rapidly at the ordinary temperature of the air, rendering it dangerous to approach an open cask of crude petroleum with a flame; others are much less volatile, requiring a temperature of from 200° to 300° F. to vaporize them. The volatility of the hydro-carbons is intimately connected with their specific gravity. They become heavier as the volatile ingredients are driven off by heat. The inflammability of the oils is also connected with their volatility and specific gravity. The light volatile oils ignite at ordinary temperatures, as we have said, on the approach of a burning match, while the heavier require a higher temperature for ignition. The process of manufacturing these oils consists in separating them from each other as they occur in the crude oil of the springs by what is called fractional distillation; for this purpose the crude oil is placed in an iron still provided with a worm of the same metal submerged in a tank of water

for cooling it; the still is then gradually heated; the first product that passes over is gaseous at ordinary temperatures, and can only be condensed into a liquid form by cooling the worm with ice, or by compressing the gas with an air-pump into a strong receiver. After all the vapor is given off at the temperature, say of 90° F., the temperature of the liquid in the still is raised, and it then exhales a vapor at a higher temperature and of greater density; and thus on successively, a series of liquids is produced, each of which requires to be heated to a higher degree before taking fire on the approach of a lighted match. The more volatile vapors are heavier than atmospheric air, and when suffered to escape from the cask containing them will flow along the surface of the floor of a room, and reaching a distant fire-place will ignite, and burning backward to the reservoir will set fire to the oil from which they emanated.

Many serious accidents have occurred in this way, by the firing of a canister containing petroleum oil which has been left open, although at a distance in some cases of from 20 to 30 feet from a lighted fire. Another source of danger from the lighted oils from which the more volatile vapors arise—results from the fact that these vapors when mixed with a certain portion of atmospheric air explode on the approach of a flame with extreme violence. When the proportions of vapor and air are equal no explosion takes place; but when they are in the ratio of 10 parts of the vapor in volume to 100 parts of air the explosion is most violent; when the quantity of air or of petroleum vapor is increased or diminished, the explosion is less violent until one or other becomes excessive, and when the vapor is in excess, it kindles without explosion, as is the case with ordinary street gas when issuing from the burner.

A notable case of the explosive quality of a mixture of petroleum vapor and air occurred in connection with the light-house service in 1864, on Lake Michigan. The keeper in one of the light-houses of this district substituted on his own responsibility an ordinary kerosene lamp of tinned iron for the usual lard oil lamp. This gave a good light

and required no trimming during the night; it burned well for several nights, and the keeper congratulated himself on the success of what he considered a very important experiment. Unfortunately however on the last morning that the lamp was used he attempted to put it out in the usual way by blowing the air from his lungs down the chimney, when an explosion took place, which scattered the oil in a burning state over the deck of the tower and also on his clothes. In his fright he ran down the stairs of the tower, and had scarcely reached the ground when a violent explosion was heard above, which blew off the whole lantern and broke the lenticular apparatus.

The explanation of these two explosions is not difficult. The burning of the oil during the night left a space void of the liquid in the reservoir of the lamp which was filled with air and vapor, which happened on this occasion to be near the explosive proportions; on blowing air down the chimney it mingled with the vapor, furnishing the quantity necessary for the violent combination, and consequently the explosion occurred which broke the lamp. The second explosion was caused by the ascent of the vapor from the burning oil on the deck, and took place when the quantity exhaled amounted to a tenth part of the volume of air present. The two then suddenly rushed into combination, producing the effects that we have mentioned.

Under favorable circumstances, this lamp lighted with kerosene might have burned silently for several weeks, but in accordance with the doctrine of chances, time enough being given, an explosion was inevitable. Facts of this kind in connection with the difficulty experienced in burning mineral oil in light-house lamps, induced the Light-House Board to adopt lard oil.

Various experiments have been made from time to time by the Light-House Board with a view to the introduction of petroleum as an illuminating material, as soon as oil could be obtained in this country of a suitable character, lard oil having advanced in price to such a degree as

to render this change desirable in an economical point of view. In the meantime experiments had also been made in France and England for the purpose of introducing mineral oil as a light-house illuminant, but it was not until 1873 or 1874 that the result was entirely satisfactory.

The process of manufacturing the oil has been very much improved in this country of late years, and there are now several companies which profess to produce oil entirely safe, and otherwise suitable for light-house purposes. In view of further experiments with mineral oil, an advertisement was inserted in the papers in 1874 requesting manufacturers to send samples of their oils to be tested at the Light-House depot at Staten Island, and in accordance with this numerous specimens were received and submitted to examination.

The first test to which the oils thus furnished were subjected was that of flashing; that is, the determination of the temperature at which the oil gives off a vapor which will flash into a flame on the approach of a small taper, or in other words which indicates the rise of a vapor which mixed with atmospheric air will tend to produce an explosion. The flashing temperature differs however from that at which the liquid takes fire as a whole. This will be understood if we suppose that two liquids have been mixed together, a light and a heavy one; the flash in this case will be due to the vapor from the lighter mixture, while the burning is due to the temperature at which the compound is fired. To make this flashing test requires considerable precaution. The oil to be tried is gradually heated by a spirit-lamp in a water-bath, a sensitive thermometer being suspended in the oil with the bulb slightly below the surface; the heat of the water is very slowly increased by moving from time to time the spirit-lamp from under the basin of the water-bath which contains the oil, and the point of flashing is obtained by passing over the surface of the oil a small flame until the first indication of flash is observed. The flame should not be so large as to heat the surface, and is best pro-

duced by a very small jet of gas from a glass tube drawn nearly to a point and connected with the gas pipe of the house by a tube of india-rubber, the quantity of gas being regulated by a stop-cock, so that the flame is a mere pencil of light about a quarter of an inch in length and a twentieth in diameter. The basin which contains the oil is about four inches in diameter, and is sometimes covered with a plate of thin glass, the thermometer passing through an aperture in this cover, and a larger hole being left open in the same for inserting the pencil of the flame. The basin containing the oil is sometimes left entirely open, the cover being discarded, but we do not think this as safe a method as the other. Great caution must be taken in raising the temperature very gradually, so that every part of the liquid may have the same heat and the thermometer thus truly indicate the temperature. If the rise of the temperature be very sudden, the thermometer will not respond, and the real flashing temperature will be higher than that which is indicated.

The next test was that of the firing of the mass of the liquid, which is sometimes 10 or 12 degrees higher than that of the flashing temperature; but generally the two are very near each other.

The next test was the determination of the specific gravity. This was obtained by weighing, in a glass flask with a narrow neck, an equal quantity of distilled water and of the oil in question; the ratio of the two, reduced to water as unity, gave the specific gravity required. To facilitate the operation, a flask containing just 1,000 grains of distilled water, was balanced by a permanent weight. The scales were tested by double weighing. The first series of weighings was made at the temperature of 74° F., that of the apartment in which the experiment was conducted; but oil and other substances change their bulk, and consequently their specific gravity, with a change of temperature. It is therefore necessary, in order that results may be compared, that the experiments be all made at the same temperature, or reduced to a standard temperature. The temperature

formerly adopted in England for specific gravity is 62° F.; but in the case of petroleum the temperature of 60° has been adopted in this country and England. In the first series of experiments made with the oils in question, the weighing was conducted at a temperature of 74° , as we have said, namely that of the atmosphere at the time. A series of experiments at a lower temperature was afterward made, in order to obtain a correction by which to reduce the specific gravity first obtained, to that of a temperature of 60° ; but as each oil exhibits a different rate of expansion by heat, the process became very laborious. Experiments were therefore made to determine the correctness of indication of the specific gravity of the oils by means of a hydrometer. This was found to differ from that obtained by weighing within one per cent., and was therefore concluded to be sufficiently accurate for practical purposes.

To obtain the specific gravity of the oils by means of a hydrometer, a vessel of a depth of about 14 inches (containing say 10 gallons of water,) is provided; into this are introduced several glass cylinders containing the oil, and into these cylinders the hydrometers are plunged, the level of the oil being so far above the water that the under contact of the surface of the liquid with the scale may be observed. Before inserting the glass cylinders containing the oils into this water-bath, the liquid is brought to the temperature of 60° by mixing ice-water with it, at which temperature it may be kept for a long time, on account of the large quantity of the liquid and the great specific heat of the water. A change of temperature may be prevented by occasionally adding a small quantity of ice-cold water, care being taken to mingle the mixture by stirring. By this process the specific gravity at 60° of a large number of samples may be obtained in a comparatively short time. In this country and England the density or relative weight of petroleum oils is generally expressed in terms of the arbitrary scale of Beaumé, instead of that of the specific gravity. The following table gives the equivalent of the Beaumé scale in terms of specific gravity:

Beaumé's hydrometer for liquids lighter than water.

Degrees, Beaumé.	Specific gravity.	Degrees, Beaumé.	Specific gravity.	Degrees, Beaumé.	Specific gravity.	Degrees, Beaumé.	Specific gravity.
10	1.000	23	.918	36	.849	49	.789
11	0.993	24	.913	37	.844	50	.785
12	.986	25	.907	38	.839	51	.781
13	.980	26	.901	39	.834	52	.777
14	.973	27	.896	40	.830	53	.773
15	.967	28	.890	41	.825	54	.768
16	.960	29	.885	42	.820	55	.764
17	.954	30	.880	43	.816	56	.760
18	.948	31	.874	44	.811	57	.757
19	.942	32	.869	45	.807	58	.753
20	.936	33	.864	46	.802	59	.749
21	.930	34	.859	47	.798	60	.745
22	.924	35	.854	48	.794		

Another test to which the mineral oil was subjected was that of a reduction of temperature. For this purpose the samples were placed in an air-bath reduced to the temperature of 25° F. At this temperature several of the oils exhibited a thickened condition, especially those of the higher fire-test. The apparatus used for this purpose was the same as that previously described as employed in the case of lard oil.

The next test to which the oil was subjected was that of its liquidity. This test is of some importance in regard to lamps in which the oil is pumped up by machinery, and also as to the solid matter in the oil. It therefore gives a characteristic of the oil which with others serves to determine its degree of impurity. For this purpose the same method was employed as that described for determining the liquidity of lard oil. The liquidity exhibited by this process varied greatly in different oils.

All the experiments on the flowing of the oils were made at the temperature of the air, which was from 72° to 74°. In this case, as with lard oil, a marked difference was found in the time of flowing at different temperatures, and hence for comparison the experiments should be made at a standard temperature.

Another experiment was made to ascertain whether oils of

higher flashing test gave off a vapor at the ordinary temperature of the atmosphere; for example, at about 70°. For this purpose a barometer tube of about 33 inches in length, and an interior diameter of one-half of an inch, was filled with warm mercury, and inverted in a basin of the same metal. The finger was then placed under the open mouth of the tube in the basin and the tube slowly inverted so as gradually to pass the vacuum through the whole length of the column, and thus to gather up any particles of air that might adhere to the side of the tube; this left a space, when the inverted tube was held vertically, of about three inches of the open end of the tube unfilled with mercury; this being re-filled, the finger applied to the open end, and the tube again replaced with the open end downward in the basin, the vacuum produced by this process was nearly as perfect as if the mercury had been boiled in the tube, or the latter filled with the metal in a vacuum. After this, a small quantity of oil to be tested was drawn into a small glass syringe, the curved point of which being introduced beneath the open mouth of the tube under the surface of the mercury, a small quantity of the liquid was injected into the column; this rapidly rose by its levity to the top, and there a portion of it flashed into vapor, as was evident by the depression of the mercurial column.

From this experiment it is evident that kerosene—even of a high flashing temperature, does give off vapor at ordinary temperature. It is however of so feeble tension that it does not appear capable of producing explosion unless considerable time be allowed for its accumulation. It might not be apparent that although vapor was given off in a vacuum, it would be given off under the full pressure of the atmosphere; but it has been shown by the experiments of Dalton and others that vapors diffuse themselves in a space filled with atmospheric air with the same elasticity and quantity as in a vacuum, time only being required to produce the effect in the atmosphere.

The oils were also examined as to the remains of any free acid which they might contain, by simply immersing in

each sample a slip of litmus paper, which was suffered to remain in the liquid for 24 hours; under this test several of the samples produced a redness, denoting the presence of an acid which might corrode the metal of the lamps, also indicating the want of a thorough washing of the oil by an alkaline water.

Another experiment, which was exhibited to us by one of the proprietors of the oil which has a flashing test of about 140° F., consisted in lighting a lamp-wick charged with the oil and plunging it into a vessel filled with the same. The oil did not take fire, although the combustion of the wick was vigorous, and indeed the flame was put out when the wick was plunged beneath the surface of the oil. This experiment—which is frequently exhibited to the public, tends to give a sense of safety in the use of mineral oil which is at least in some degree fallacious. To illustrate this, the following experiments were made: First a slip of cotton cloth, about 6 inches wide and 2 feet long, was saturated with oil, having a flashing test of 140° , and suspended vertically from a ring-stand; a lighted match was then applied to the middle of the length of the slip, when it instantly took fire, and burned with a fierceness quite appalling.

After this, two pieces of cloth—one of cotton and the other of wool, were saturated with petroleum and placed flat on two pieces of tinned iron to protect the floor. On each of these was then dropped an ordinary friction match in the state of ignition. They both broke instantly into flames which soon entirely consumed the cloth, although but little air could obtain access to its under side, and notwithstanding the good conducting power of the tinned iron.

In a similar experiment made with the same kind of cloth saturated with lard oil, the cloth did not take fire when a lighted match was dropped upon it. Two cotton cloths of the same size were saturated—one with lard oil, the other with petroleum, and lighted at the same time. The petroleum cloth was consumed in 1 minute 23 seconds; the lard cloth in 5 minutes.

To render these experiments more strikingly applicable to cases of accident which might occur in a light-house, a

piece of cotton cloth about 2 feet square, which had been used to wipe the table on which kerosene had been spilled, was crumpled up into the condition of an ordinary dish-cloth, and thrown into a corner of the room. When a lighted match was dropped on this, it instantly burst forth into a most violent combustion.

These experiments are important in establishing the fact that oils which are commonly sold as entirely free from danger are not really so. They may be safe from explosions at ordinary temperatures, and in this respect are to be preferred to the lighter oils; but when spread over a large surface they burn with greater intensity, even (as I have seen,) on a surface of ice. Indeed, the results are so striking, that it might be well to repeat them in the presence of every light-house keeper, in order to impress him with an idea of the danger to be apprehended in spilling the oil over his clothes, or in carelessly dropping his matches on cloths which which had been used in cleaning the apparatus.

Among the peculiar properties of mineral oil is its great surface-attraction or power of adhering and spreading on other surfaces, as well as ascending wicks to a much greater height than other oils. This property is recognized by the house-keeper who finds the exterior of the lamp covered with a film of oil shortly after it has been subjected to a thorough cleansing. It rises along the interior surface of the lamp and spreads over the outside. On account of this property it can be freely burned in lamps of which the fountain is at a considerable distance below the flame, and in which no overflow is required to produce a brilliant combustion.

A series of experiments was next made with regard to the burning qualities of mineral oils of different densities, from which it was inferred that the lighter oils in lamps of the fourth order gave a greater amount of illumination than the heavier oils, and furthermore that the latter charge the wick more than the former, from which it would appear that in using mineral oil, while safety should be the prominent consideration on the one hand, in the choice of the material, regard must be had on the other, to the illuminating power.

In regard to the relative photometric power of lamps of the same order charged with mineral and with lard oil, all the experiments we have yet made on this point tend to the conclusion that in smaller lamps with the more volatile oils a greater photometric power is obtained than with the same lamp when charged with lard oil; but with the larger lamps the reverse is the case, the lard oil burned in these lamps giving greater power than the mineral oil.

An unexpected difficulty arose in the course of the investigations for the introduction of mineral oils, on account of the form of the flame. While a lamp with a constricted chimney, like that used in the German student-lamp, gave the greatest photometrical power, it was found that the shape of the flame did not correspond with the arrangement of the lens apparatus, a large portion of the light being thrown upward toward the sky and another toward the earth. It was only after a series of trials with chimneys of different forms and button-deflectors that a flame of the best shape was obtained. To compare these flames in actual use, they were placed in succession in a light-house, with a lens of the fourth order, and the photometrical power determined at different distances, from a mile to ten miles in extent, by interposing between the eye and the light a series of thin colored glasses, until the light was totally extinguished. It was found in these experiments that some of the flames which had an appearance of greater brilliancy near by, failed to produce comparatively the same effect at a greater distance. Having settled upon the form of the flame to be used in lamps of the lower orders, arrangements have been made for the introduction of mineral oils into all the stations in the third district, at which lights of the fourth and smaller orders are at present in use. The substitution of mineral for lard oil however is a matter of no small difficulty, and requires to be made with great precaution. An entire change in all the lamps is required; the several parts of the apparatus which in the case of lard-oil lamps were united by soft solder must now be joined with spelter.

The importance of this was evinced by an accident which

happened in the photometric room in the case of a lamp of the fourth order under trial; the heat unsoldered an air-tube and let down the oil from the reservoir on the flame, which produced so fierce a combustion that it would have set fire to the building had it not been of fire-proof materials.

The gradual introduction of mineral oil will be made as rapidly as experience indicates the best and safest mode of employing it. It has already been adopted in the smaller lamps for lighting the Mississippi and its principal tributaries. The substitution however is not on account of the superior quality of this oil in comparison with lard oil,—since we think the latter as an illuminating material is inferior to no other at present in use,—but simply on account of the comparative cost of the two materials. This relative expense will be definitely ascertained after we have determined the best form of lamps to be used. Experiments thus far have been principally confined to the lower orders of lamps.

ON THE ORGANIZATION OF LOCAL SCIENTIFIC SOCIETIES.*

(From the Smithsonian Annual Report for 1875; pp. 217-219.)

DEAR SIR: - - - - - In answer to your question, as to the plan of organization and operation of a scientific association, I submit the following:

The object of your society being—as you inform me, to cultivate “scientific taste and knowledge among its members,” this object should be kept constantly in view, and care taken that it be not interfered with by a tendency to waste the time of the meetings in the discussion of irrelevant matters, especially those which relate to the government and organization of the establishment. I have been a member of several societies which failed to effect their object, by endless discussions on points of order or propositions as to the constitution and by-laws. There is in this country a tendency to express little thought in many words, to cultivate a talent for debate, or the art of making the worse appear the better cause—which is by no means favorable to either the increase or the diffusion of knowledge. The object of your society is not that of a mere debating club, but that of an association for the real improvement of its members in knowledge and wisdom.

It has been from the first, the policy of the Smithsonian Institution to encourage the establishment of such societies, on account of the great advantage they are to their members in the way of intellectual and moral improvement, as well as in the way of positive contributions to science.

Such an organization is an important institution for the advancement of adult education and the diffusion of interesting and useful knowledge throughout a neighborhood. The society must however be under the care of a few enthusiastic and industrious persons; it should adopt the policy of awakening and sustaining the interest of the greatest possible number of persons in its operations, and for this purpose the meetings must be rendered attractive. Care should be taken

*[Extract from a letter in reply to inquiries from a correspondent.]

to provide a series of short communications on various subjects, on which remarks should be invited after they have been read. Clergymen, lawyers, physicians, farmers, mechanics, and others should all be pressed into the service, and each solicited to contribute something, the object being to make the special knowledge of *each* the knowledge of *all*. I once belonged to a society conducted on this plan, which is still in existence, and a meeting of which I had the pleasure of attending about ten years ago; and by way of illustrating what I have said, permit me to mention the proceedings on the occasion in question. First a number of mineralogical specimens were presented and described, next a short paper was given on the local geology of the vicinity, and then a brief lecture on astrology, in which the process of casting nativities was described. This last subject—which on first thought might appear beyond the capacity of the majority of an ordinary audience, proved to be a source of interesting remarks, in which nearly all participated. This arose from the fact that astrological ideas and usages survive in modern civilization, and each one was enabled to give an example of beliefs and practices still existing in different parts of the country, as to the influence of the moon in various processes of agriculture, on disease, and even in relation to the survival of astrology in our language and general superstitions.

The farmer should be encouraged to bring to the meeting specimens of the various botanical productions which he meets with in agricultural operations, as well as specimens of the different soils of which his farm is composed. These should be referred to a committee, and their names and peculiarities given at a subsequent meeting. If a plant or a mineral or an animal is unknown to any member of the association, a specimen of it may be sent to this Institution, where it will be examined and after being properly labelled—returned.

The mechanic should be encouraged to give accounts of the processes which he employs, or of any facts of special interest which he may have observed in the course of his operations.

In short, all the members should be induced to observe, and also be instructed as to the method of observation. It is of vast importance to an individual that he be awakened to the consciousness of living in a universe of most interesting phenomena, and that one very great difference between individuals is that of *eyes* and *no eyes*.

What I have said relates to the uses of a local society in the improvement of its members; but the importance of an establishment of this kind should not be confined to the mere *diffusion* of knowledge. It should endeavor to *advance* science by co-operating with other societies in the institution and encouragement of original research. Thus it can make collections of the flora and fauna, of the fossils, rocks, minerals, &c., of a given region, of which the location of the society is the centre, and thereby contribute essentially to the knowledge of the general natural history of the continent. It can also make explorations of ancient remains, and collect and preserve the specimens of the stone-age—which still exist in many parts of our country, and to which so much interest is at present attached. Further, it can induce its members to make records of meteorological phenomena, many of which—of great interest, can be made without instruments, such as the times of the beginning and ending of storms, the direction of the wind, the first and last frost, the time of sowing and harvesting, the appearance and disappearance of birds of certain kinds, the time of the blossoming and ripening of various fruits, &c.; and as soon as the means of the establishment will afford, a series of meteorological observations should be entered upon with a perfect set of instruments.

In order however to give still greater interest to the society it should make arrangements in due time for the publication of its proceedings, to be exchanged for the transactions of other societies at home and abroad, the foreign exchange (if desired) to be made through this Institution.

I beg leave to assure you that the Smithsonian Institution will be happy to co-operate with your society in every way in its power.

THE METHOD OF SCIENTIFIC INVESTIGATION, AND ITS APPLICATION TO SOME ABNORMAL PHENOMENA OF SOUND.*

(Bulletin of the Philosophical Society of Washington; vol. II, pp. 162-174.)

Delivered November 24, 1877.

GENTLEMEN: I beg leave to tender you my sincere thanks for the honor you have conferred upon me, and the good feeling you have manifested toward me, by my re-election as president of this society. I say the good feeling which you have manifested toward me, because I know that there are many of your members who can much more efficiently discharge the duties of the office than I can. I may perhaps be allowed to say—without the charge of undue egotism, that I have never occupied any position for which I have been voluntarily a candidate. The several offices of honor and responsibility which I now hold—no less than nine in number, have all been pressed upon me without solicitation on my part, and I now begin to feel—in view of that peculiarity of human nature so admirably exhibited in the character of the Archbishop of Granada, that I ought to diminish the number of my responsibilities, gradually leaving to others the honor and the toil of office. It is therefore with no feigned hesitation that I again accept the re-election to the position to which your kindness has called me.

I have however taken from the first a deep interest in the society, knowing that it is intimately connected with the intellectual development of the city of Washington, and that it has a reflex influence upon every part of the United States. It tends to keep alive an active spirit of scientific advancement, not only to diffuse a knowledge of the progress of discovery among its members, but also to stimulate—by friendly criticism and cordial sympathy, to new efforts in the way of explorations into the unknown.

While but comparatively few qualifications are necessary for admittance, yet no person is elected who is not supposed

*[Anniversary address of the president of the Philosophical Society of Washington.]

to have at least a high appreciation of science, some familiarity with its principles, and capability of doing something in the way of promoting the objects of the association.

The general mental qualification necessary for scientific advancement is that which is usually denominated "common sense;" though added to this, imagination, invention, and trained logic—either of common language or of mathematics, are important adjuncts. Nor are objects of scientific culture difficult of attainment. It has been truly said that the "seeds of great discoveries are constantly floating around us, but that they only take root and germinate in minds well prepared to receive them."

The preparation however is not difficult, and many possess the requisites in an eminent degree who are not aware of the fact. Genius itself has been defined as a mind of general powers, determined—enthusiastically it may be, on one pursuit.

The method of discovery or of scientific observation is not difficult. There is a story in a work entitled "Evenings at Home" which produced an indelible impression on my mind. It is entitled "Eyes and No Eyes," and related to two boys who started on a walk during a warm summer afternoon. On their return one was fatigued, dissatisfied, having seen nothing, encountered only dust and heat; while the other was charmed with his walk, which had been over the same ground, and gave a glowing account of the objects with which he had met and of the reflections which were awakened by them. On this story De la Bêche has founded a work, entitled "How to Observe in Geology," which I would commend to the attention of every member of this society, while I suggest that good service would be done to the advance of knowledge were a similar work published relative to all branches of science.

Method of Scientific Investigation.—The first requisite for an observer is that his mind should be actively awakened to the phenomena of nature with which he is surrounded. Thousands of persons of excellent mental capacity pass through the world without giving the slightest attention to the ever

varying exhibitions which are presented to them. The sun rises and sets, the seasons change, the heavens every night present new aspects, but these to them are matters of course; they excite no interest, and it is only when some extraordinary phenomenon occurs, such as the blazing comet or the startling earthquake, that their attention is arrested. Another requisite is the power of the perception of truth, which enables the observer to recognize and define with unerring accuracy what he has seen without any tinge of color from *à priori* conceptions. Still another is the faculty of eliminating accidental conditions from those which are essential; and further, the characteristic of perseverance is indispensable.

The fields of scientific labor may be divided into two classes, viz., those which relate to the empirical observation of facts and those which relate to the systematic series of investigations as to the law or cause of special phenomena. As illustrations of the first class, may be specified the facts of the phenomena of the physics of the globe, those of ordinary meteorology and natural history; while as examples of the second, we have the phenomena of chemistry, physics, and astronomy.

The remarks I have previously made refer principally to the former. In order to elucidate the method of investigation, in the latter case, I will suppose the existence of a new phenomenon which is unconnected with any of the present generalizations of science, but of which it is desired to discover the law, or the facts with which it is associated. Such facts standing alone form no part of science; they are usually discovered in the course of investigations, and are of great importance in pointing out fields of new research which promise an abundant harvest.

The first step in the investigation is to re-produce the phenomenon; the next is to form in the mind a provisional hypothesis as to its cause; and in the choice of this we are governed by analogy. For example, if it appears to resemble some of the phenomena of electricity, we *assume* that it is produced by electricity; we next endeavor to ascertain by

what known action of electricity such an effect could possibly be produced : for this purpose we invent an hypothesis, or imagine some peculiar action of electricity sufficient to produce the effect in question ; we then say to ourselves, if this be true, it will logically follow that a specific result will take place if we make a certain experiment ; the experiment is devised and tried, but no positive result is obtained. In order to this negative result, the logical deductions must have been inconclusive, or the experiment must have been defective, or the hypothesis itself erroneous.

We examine each of the two former steps, and finding nothing amiss in them, we conclude that the hypothesis was not true. Another hypothesis is then invented, another deduction inferred, and another experiment made ; still no result is obtained. At this stage of the research the inexperienced investigator is prone to abandon the pursuit : not so he who has successfully attempted to penetrate the secrets of nature. Undeterred by failure, he changes from time to time his hypotheses, makes new guesses, and again repeats the question as to their truth by means of experiment ; until at length nature—as if wearied by his solicitations, grants him a new and positive result. He has now two facts, and an hypothesis to explain them ; from this hypothesis he makes a new deduction, which is also tested by a new experiment ; but now perhaps he obtains a result which although of a positive character, is not what he expected. He has however made an advance ; he has three facts and an hypothesis to explain two of them. In this case he does not usually abandon his preconceived idea, but modifies it until it includes the new fact. With the hypothesis thus improved, he deduces—it may be in rapid succession, a number of new conclusions, the truth of all of which is borne out by the results of the experiments. The investigator now feels that he is on the right track ; that the thread of Dædalus is in his hand, and that he will soon be in the full light of day : but usually the escape from the labyrinth is not so easy. In the height of his successful career it not unfrequently happens that a result is obtained diametrically op-

posed to his previous generalization, which conclusively forces upon his mind the conviction that he is still far from attaining his end; that he has not yet seized upon the fundamental principle of the phenomena, which have grown into a class under his hands.

At this stage of the inquiry his self-esteem is much depressed; he throws aside for a while his apparatus, refers to his library for new suggestions: the subject however is not discharged from his mind; it still goes with him, and is perpetually recurring; it is mingled with his dreams, and is seen associated with the every-day occurrences of life, until at length, in some happy moment of inspiration, it may be after refreshing sleep, the truth flashes upon him; he catches a more extended conception of the relations of the phenomena; a more comprehensive hypothesis is suggested, from which he is enabled to deduce in succession a large number of new conclusions to be submitted to the test of experiments. These are all found to yield the expected results, and the generalization which has thus been obtained is more than an hypothesis; it is entitled to the name of a verified theory. The investigator now feels amply rewarded for all his toil, and is conscious of the pleasure of the self-appreciation which flows from having been initiated into the secrets of nature, and allowed the place not merely of an humble worshipper in the vestibule of the temple of science, but an officiating priest at the altar.

In this sketch of a successful investigation which I have given, it will be observed that several faculties of the mind are called into operation. First, the imagination—which calls forth the forms of things unseen and gives them a local habitation, must be active in presenting to the mind's eye a definite conception of the modes of operation of the forces in nature sufficient to produce the phenomena in question: second, the logical power must be trained, in order to deduce from the assumed premises the conclusions necessary to test the truth of the assumption in the form of an experiment: and lastly the ingenuity must be taxed to invent the experiment or to bring about the arrangement of apparatus adapted to test the conclusions.

These faculties of the mind may all be much improved and strengthened by practice. The most important requisite however to scientific investigations of this character, is a mind well stored with clear conceptions of scientific generalizations, and possessed of sagacity in tracing analogies and devising hypotheses.

Without the use of hypotheses or antecedent probabilities, as a general rule—no extended series of investigations can be made as to the approximate cause of casual phenomena. They require to be used however with great care, lest they become false guides which lead to error rather than to truth.

It is not enough for a physical investigation that we have the simple idea, which may be embodied in a mathematical equation; we must see clearly with the mind's eye the operations in nature, and how the phenomena are produced in accordance with the well-known laws of force and motion.

An Investigation of an Acoustic Phenomenon.—As an illustration of what I have said, as well as an original scientific communication, I may be allowed to present in this connection an account of some observations on the phenomena of sound in its application to fog-signals, in which I have been engaged during the past summer, and which are an extension of the investigation of whose progress I have given an account at different times to the society.

This year my attention was again directed to the peculiar effect observed for several years past on the coast of Maine, which has been classed among those to which the term "abnormal phenomena of sound" is applied. In August, 1873, this was partially examined, and the result published in the Light-House Report for 1874. In order to investigate it further, I associated myself with General J. C. Duane, engineer of the first Light-House district; Commander H. F. Picking, inspector of the same district; Mr. Edward L. Woodruff, assistant engineer of the third district, and Mr. Charles Edwards, assistant engineer of the first district.

The phenomenon to be investigated was exhibited in connection with the fog-signal at a station called Whitehead, on

the coast of Maine, at the entrance of Penobscot Bay. It was reported as having been frequently experienced by the captains of the steamers plying between Boston and New Brunswick, and it had also been observed on two different occasions by officers of the light-house establishment.

The phenomenon, as reported by these authorities, consisted in hearing the sound of a ten-inch whistle distinctly as the station is approached till within the distance of from four to six miles, then losing it through a space of about three miles, and not hearing it again until within about a quarter of a mile from the instrument, when it suddenly becomes audible almost in its full power.

This phenomenon—according to the statement of the keeper of the light-house station, is noticed whenever the vessel is approaching the station from the south-west, and the wind is in the same direction. It is especially observed during a fog (when the warning of the signal is most wanted), and this is here always accompanied by a wind from the south or southwest.

Our first object was to verify the phenomenon, and for this purpose we steamed to the south-west, directly against the wind, which was blowing at the time with a velocity of about ten miles per hour; this fortunately happened to be the direction of the wind during which the phenomenon was most frequently observed. The whistle was sounded every minute by an automatic arrangement, and the time at which the several blasts were given could be noted from the vessel by the puffs of steam emitted by the whistle. As we increased our distance from the signal the sound very slightly diminished in loudness until the distance was about a half mile, when it suddenly ceased to be heard, and continued inaudible for about a mile farther, when it was faintly heard, and continued to increase in loudness until we reached the distance of four miles; at this point it was heard with such clearness that the position of the station could be readily located in the densest fog, but on proceeding still farther in the same direction it gradually diminished and was finally again lost.

As a second experiment we re-traced the same line back to the station, and observed the same effects in a reversed order. The sound was heard the loudest at a point about four miles from the station, and after that it diminished and became inaudible through a space of about two miles, and then suddenly burst forth nearly in full intensity at a distance of a quarter of a mile, and continued loud until the station was reached.

For the investigation of this phenomenon, we may assume provisionally that it is due to a peculiar condition of the atmosphere, either as to heat, pressure, or moisture, or a combination of all of them, which existed at the time in that part of the track of the steamer which may be denominated "the region of silence." But if this were true, such a condition of the atmosphere ought to be indicated by ordinary meteorological instruments. To test this, the temperature of the air was noted through the whole space by an ordinary thermometer, and also its pressure by means of an aneroid barometer, but no variation was observed in these instruments in passing through the air along the path of the vessel.

To complete this series of observations however the indications of a delicate hygroscope should have been noted. Unfortunately we were not provided with an instrument of this kind; the fact however that the phenomenon was frequently observed during a fog, or while the air is uniformly saturated with moisture, indicates that the phenomenon is not due to a difference of moisture in the region of silence. Indeed, it is sufficient to remember that a wind was blowing at the rate of ten miles an hour to be convinced that an isolated portion of air could not remain in a fixed position, even for an instant.

Another hypothesis might be assumed,—that the apparent silence was caused by the transverse reflection, in some way, of sound from the shore, but there was nothing in the configuration of the land which favored such an hypothesis.

The only explanation which presented itself was that of the upward refraction of sound, an hypothesis which has been

found fertile in new results in previous investigations of the same subject. To test this and to ascertain the dependence of the phenomenon on the wind, the position of the focus or the origin of the sound was changed. For this purpose the whistle of the steamer was sounded while a portion of the observing force was placed at the station; by this arrangement it was found that while the vessel, in reference to the sound of the signal at the station, passed through a region of silence, the observers at the station who gave attention to the sound from the steamer heard no interruption of the signal. This experiment was repeated each way, going to and coming from the station.

From this result it appears that the sound going *with* the wind was heard at every point on its course, while the sound moving against the wind was suddenly lost at a given point and not recovered again until a distance of more than a mile had been traversed by the vessel. This result was in strict conformity with the theory of refraction; in the case of the sounds travelling against the wind, the upper part of the wave would usually be more retarded than the lower, and consequently the sound wave would be thrown upward above the head of the observer. At a given altitude this difference of velocity would cease, and by the general tendency of sound to spread, the sound wave would again reach the earth.

But to test this still further, and to show that the locality was not an essential condition of the existence of the interval of silence, the experiment was repeated on the opposite side of the station, so that the sound from the fog-signal would move in the direction of the wind. Some of the observers were placed at the station and the others remained on board the vessel; both instruments were sounded, the one in the intervals of the sounding of the other.

In this case the sound from the fog-signal was continuous to those on board of the vessel through a distance of over four miles, and could probably have been heard many miles farther, but the progress of the steamer in that direction was stopped by the land.

From the report of the observers at the station it appeared that as the vessel passed into the distance but one blast was heard during its whole course. In this case (as in the preceding experiment of sailing to the south west) the sound moving against the wind was refracted upward, and as the whistle was but six inches in diameter it did not give sufficient volume to again reach the earth by spreading.

In experiments of previous years the fact has been shown that the sound is heard under certain conditions better when moving against the wind than in the opposite direction. This was notably the case in the experiments made at Sandy Hook in September, 1874, during which a sound from the west was heard at first *with* the wind about three times as far as a sound from a similar source was heard from the east, or *against* the wind; then the same sound was heard from the west three times as far as from the east after the wind had settled to a calm; and in a third observation the same phenomenon was observed after the wind had changed to a direction *opposite* to that of the sound, and had increased to a velocity of ten miles an hour from the east. These effects were afterwards shown to be connected with the fact that the upper wind during the whole day was blowing strongly from the west, and that the apparent changes of the wind were due to currents at the surface, and thus a sufficient explanation was given to the phenomena observed.

It would appear however from the investigations of last summer that the wave of sound which has been refracted upward may descend at a greater distance from its origin than even that at which sound moving with the wind can be heard, probably involving a peculiar case of undulating or compound refraction; but this requires further investigation.

Each series of observations gives rise to new questions, and indicates that the subject is one which is rich in new results. Unfortunately however the observations can only be made by the aid of steamers; and these—in the Light-House service, can only occasionally be employed in the rare intervals of more imperative duties.

In order to collect data for further use in the explication of the phenomena, the light-keepers at Block Island and Montauk Point (the eastern portion of Long Island) have been directed to blow the fog-signals for an hour on every Monday morning, each noting whether he can hear the sound from the other station; observing at the same time the direction of the wind and the apparent motion of the clouds.

From the result of these observations during the year it appears that the clouds give frequent indications of adverse wind currents, and that the number of times the sound has been heard against the wind is greater than the number of times it has been heard with the wind; a result which though unexpected—is not in discordance with previous assumptions.

It will be recollected by the Society that I have in previous years mentioned a remarkable phenomenon, which I have denominated the "*ocean echo*." This has also been observed by the distinguished scientific adviser of the Trinity Board, and is considered by him as the key of all the abnormal phenomena of sound observed, and as a special illustration of the truth of his hypothesis that such abnormal phenomena are produced by invisible clouds of flocculent atmosphere. The phenomenon in question consists in a reverberation in the form of an echo from a point in the verge of the horizon to which the axis of a fog-trumpet is directed.

In regard to this, I first adopted the provisional hypothesis that this was produced by a reverberation from the crests of the waves of the ocean; but it having been stated that the same phenomenon is exhibited while the sea is smooth, this assumption must be abandoned, or in some way modified to suit the observed facts. To test the hypothesis of the reverberation being due to a reflection from an invisible cloud on the verge of the horizon, the trumpet of the large siren on Block Island was gradually elevated from a horizontal to a vertical position, and while in this position it was sounded at intervals for several days; but in no case was an echo heard from the zenith, but in every instance an

echo was returned from the horizon around its whole circumference.

In another experiment with a vertical trumpet at Little Gull Island, a small cloud, from which a few drops of rain fell on the area of the base of the light-house, passed directly across the zenith, and during this passage no echo was observed from the cloud, although the trumpet directed toward it was sounded several times in succession.

Again, in order to obtain additional facts in regard to the nature of this echo, observation was made from a vessel, by steaming out directly as if into the region of the echo—*i. e.*, in the direction of that point in the horizon from which the echo appeared to emanate.

In this case the loudness of the echo appeared to gradually diminish as we advanced, and to spread itself through a much longer arc of the horizon, while the duration of the echo increased in time.

It would follow from this experiment that the echo is not a reflection from a definite surface, since it would then increase in loudness as the surface is approached, but a series of rebounds from points at various distances.

Another fact of great importance in determining the nature of the echo is that derived from the observations of the keeper at Block Island. He has recorded every Monday during a year, the observations of the length of the continuance of the echo, the state of the weather, the direction of the wind, and the other meteorological data. Whence it is found that the echo from the sound of the siren is always heard during a wind in any direction, and of all intensities, but during the occurrence of a very high wind, with less duration after the original blast, than in calmer weather; and above all, that it is heard equally well during a dense fog, when evidently the air must be homogeneous and saturated in every part with vapor.

From these facts it appears to me conclusive that the reverberation, constituting the *ocean echo*, cannot be due to invisible clouds. The only hypothesis suggesting itself to my mind as a basis for further investigation of this subject

—is that in the spread or divergency of the sound, the direction of the impulse turns through an angle of a little more than 90° , so as to meet the surface even of the smooth ocean in a direction by which it would be reflected to the ear of the observer, making the angle of reflection equal to the angle of incidence; although from the gradual dispersion of sound-beams, the precise equality of these angles is obviously not very important to the result.

On returning from this excursion by the N. Y. Western railway to the Hudson river at Troy, opportunity was taken to make some observations on the action of sound in the Hoosac tunnel, through which I passed, on the afternoon of September 7th, accompanied by Mr. E. L. Woodruff. Resting at East Windsor, near the western outlet, I spent a considerable part of the following day in making an examination of the work. Mr. W. P. Granger, the chief engineer, and Mr. A. W. Locke, his principal assistant, very courteously furnished a hand-car, and cordially proffered every facility for making any desired investigations. This tunnel (as is known to most of those present) is nearly five miles long, rising by an easy grade of 26.4 feet to the mile from either mouth to about the middle of the tunnel, where it opens into a vertical ventilating shaft through the rock—of upwards of a thousand feet in height. The top of this shaft opens between two ridges of the Hoosac Mountain, which rise respectively some 400 and 700 feet higher. From the middle of the tunnel when entirely clear of smoke, the distant opening at either end appears as a faint star. The darkness seems oppressive; and when a train is passing through, the air becomes so thickly clouded that the glare of torches cannot be seen at a distance of more than a dozen feet.

It had been constantly observed by those employed in the tunnel, that during the approach of a locomotive at no great distance, and a few minutes afterward, the sound of the engine was very much deadened and obstructed; so much so indeed as to imperil the workmen engaged in lining the top of the tunnel with a brick arch, who frequently failed to hear the locomotive until it was close upon them. This ob-

scuration of sound was not unnaturally attributed to the dense clouds of smoke constantly emitted by the locomotive; but this explanation can hardly be accepted as the true one, nor the condition noted as constituting even an appreciable cause of such acoustic opacity. When we reflect that a puff of exhaust-steam at high temperature is ejected at about every four feet of rail traversed by the driving wheels, it is not difficult to realize that in an atmosphere so systematically made heterogeneous there must be a very great amount of dispersion and absorption of sound waves struggling through such a medium. This has been well illustrated by the striking experiments of the distinguished physicist of the Royal Institution. A very simple method of confirming this explanation, and of eliminating entirely the effect of the smoke, would be the employment of locomotive engines driven by the combustion of coke or of charcoal. This experimental determination of the question did not occur to me till after we had left the tunnel; but on suggesting it to Mr. A. W. Locke, the assistant engineer in charge, he very obligingly undertook the conduct of such an experiment at the earliest convenient opportunity. The result has not yet been ascertained.

When the tunnel was entirely clear, and a gentle current of air flowing down the central ventilating shaft and out at the two ends (as is usual in the summer season, when the external temperature is higher than the internal), it was observed that a prolonged but irregular echo followed any loud noise, such as the sudden shutting down of the lid of a tool-chest. The unequal or somewhat intermittent character of the echo appeared to result from the irregular surface of the rock forming the walls of the tube. A somewhat similar echo is sometimes returned from the dense foliage of trees. It is proper to add that a very perceptible echo was heard from the portion of the tunnel lined with brick. The effect could in neither case be ascribed to any invisible "flocculence," as the air must have been in a very homogeneous condition.

Inasmuch as in such observations the waves of sound are

reflected back to the ear from points at a considerable distance from their origin, (this being especially true of the ocean-echoes,) we are liable to be seriously misled if we rely too confidently on the experiments of the laboratory, and form hasty generalizations from apparent analogies, without carefully considering *all* the meteorological conditions by which the rays of sound may be deflected, distorted, and diverged. It is now well established by numerous observations and experiments—made independently on both sides of the Atlantic, that the lines of acoustic propagation (conveniently called sound-beams) which are sensibly very rectilinear for the distance of a hundred or two hundred feet, and which are thus obedient to the katoptric and dioptric laws of precise focal convergence, by means of solid mirrors and of gaseous lenses, are yet at the distance of a few miles so strangely contorted and aberrant as seemingly to contradict all the analogies suggested by our experience with the rays of light. It is the accumulation of comparatively slight divergencies continued through many thousands of yards, whether under the influence of constant conditions or of changing and reversed conditions, which produces such marked anomalies at the distance of five or of ten miles, and which makes their investigation as laborious as it is instructive and important. And not until we have mastered all the conditions affecting the transmission of sound throughout its entire sensible range, and have thus become enabled to predict its true course, and to announce its varying limits of audibility at the earth's surface, under given circumstances, can we be said to have perfected the theory of this most interesting and indispensable agent of communication.

OBSERVATIONS IN REGARD TO THUNDER-STORMS.*

(Journal of the American Electrical Society; 1878; vol II, pp. 1-8.)

DEAR SIR: I highly approve the object of your society; and I beg leave to express the opinion that much valuable information may be collected and preserved through its organization, especially in regard to the phenomena of thunder-storms.

For this purpose it might be well to prepare a series of questions to direct attention to especial points of inquiry, and I take the liberty of suggesting the following as a contribution toward this end:

A.—*Particulars of the Storm.*—1. Give the number and time of occurrence of thunder-storms, so as to show their distribution through the months, days, and hours of the year.

2. Note the point of the horizon in which the storm generally arises in any given locality, and the point to which it tends.

3. Observe whether it usually divides into two storms at any point. If so, what is the topography of the surface below?

4. Determine the width of the storm from the extent of the surface covered by rain, and also (if possible by means of the telegraph) the length of its path.

5. Note the condition of the air before and after the storm as to temperature, moisture, and pressure; also the temperature of the water which falls.

6. Give the direction of the wind previous to the beginning of the storm, during its continuance, and after its ending.

7. Observe whether a calm precedes the violent part of the storm, and whether in front of it a curtain of dust is raised to a considerable height in the air.

*[A letter addressed to the corresponding secretary of the American Electrical Society, dated Washington, D. C., October 13, 1877.]

8. Note the number of seconds the sound of a discharge continues, which will give approximately the minimum length of its path.*

9. Note the time between the appearance of the flash and the sound of the thunder, and also the angle of elevation; these will give approximately the height of the cloud.

10. Ascertain whether any hail accompanies the storm; if it does, note whether it falls along in two tracks or in one. In the former case, give the distance between the tracks. Give the size and character of the hail, whether it consists of an agglomeration of crystals, or of rounded masses stratified with clear ice and snowy concretions, and whether it contains in some cases small particles of dust or sand.

11. Note the number of discharges between the different parts of the same or different clouds, and also between the cloud and the earth. The former will probably be more frequent than the latter.

12. Note the color of the lightning, particularly if it be violet or purplish, which will probably indicate a cloud of great elevation.

B.—Effects of the Electric Discharge.—1. State what kind of trees are struck, and on what parts of the tree the effects are most apparent—on the branches, or the trunk.

2. What are the mechanical effects observed in the tree; is it torn asunder laterally, or is it broken transversely to the axis, or both?

3. Was the tree green or dry?

4. When a house is struck, state if it had a lightning-rod, and, if so, give its character, and especially its connection with the ground.

5. Mention the part of the house struck. If the chimney, was there fire in it at the time? Give the path of the discharge through the house, and its relation to conducting metals.

6. Note the inductive effects of the discharge in produc-

*The velocity of sound in open air at the temperature of 62° Fahr. is 1,125 feet a second, or nearly a mile in four and seven-tenths seconds.

ing sparks and flashes between different objects within the premises.

7. If the discharge passes through metallic conductors and disintegrates them, note any appearance which might tend to indicate whether the effect is produced by heat or by repulsive energy imparted to the atoms at the moment of the discharge.

8. If the discharge takes place between two surfaces, note if there is any apparent transfer of material from one to the other.

9. Note any peculiarity of odor that may be observed. All mechanical effects produced by the discharge should be mentioned, and special notice taken as to whether they are not in most cases produced by a violent repulsive energy given to the air in the path of the discharge, and whether the effects are greater in the direction of the axis of the discharge than in that at right angles to the same.

10. Note the effect upon man and animals; whether a part of the discharge passed through the body, or whether inductive shocks were felt.

A Notice of Two Thunder-Storms.—The following account of two thunder-storms which occurred last summer may perhaps be thought worthy the attention of your society, and of a permanent record in a scientific publication.

The first one I shall describe, and of which I had an opportunity to examine the effects, occurred at New London, Conn., where a violent discharge of electricity took place on the premises of Mrs. Alger, of that city. Mrs. Alger's house is situated on an elevation overlooking the river and the surrounding country, on one side of a lawn on which, at a distance of 150 feet from the dwelling, stood a tall flagstaff ninety feet high. Across this, at about twenty feet from the top, was a spar like the yard-arm of a ship, which was braced by two iron rods, joining the top of the mast with the two ends of the cross-piece. The lightning struck the mast, and brought the whole down to the ground. The upper part, including the cross-piece, came down unbroken, it being probably protected by the iron rods. The remaining seventy

feet of the mast was broken into larger and smaller fragments, principally at right angles to the axis, and scattered over the lawn in every direction. One piece—consisting of an entire portion of the mast six feet long and nearly a foot in diameter, was thrown ninety feet to the north, and another piece of about the same dimensions was projected ninety-six feet in an opposite direction. From the foot of the mast to different points of the compass a number of furrows were plowed in the earth. One of these was in the direction of the house, along the side of which—marks of the discharge were visible at intervals. These indicated the passage of a part of the electrical discharge to a sewer on the farther side of the house. Marks of the discharge were also observed on the inner walls.

The house was of wood, and the walls consisted of clapboards on the exterior, and lath and plaster within. The effects on the inside were especially noticed at two points, one of which was opposite an iron safe, and the other was a portrait, the gilding of the frame of which was deflagrated.

But perhaps the most singular effect was exhibited in an interior room of about twelve feet square, around the cornice of which, under the ceiling, was a narrow strip of gilt beading. Though this room was insulated from the outer wall, the gilt throughout the whole circuit of the room was entirely burned off, while the wall in the corner farthest from the mast presented a blackened appearance, indicating that the electricity had passed upward from the earth at this point, and probably down again through the same channel.

These effects are note-worthy on account of the immense energy displayed in disintegrating the mast, and in the projection of the two large pieces of it in opposite directions. It would appear from this, as well as in the case of trees struck by lightning, that the greatest intensity of the force is in the line of the direction of the discharge, the tenacity of the wood being greatest in this line.

I have heard of several cases in which the trunks of trees of considerable size have been separated into two parts, as if by violent repulsion in the line of the axis, and of one in-

stance in which the whole body of the tree was separated from the trunk, and falling into soft earth, was left standing in a vertical position.

The other phenomenon of the burned gilding in the inner room appears to me to be due to the action of what is called the return stroke. The house and all conducting material within it, before the moment of the discharge, supposing the cloud to be positive, were electrified negatively. When the discharge took place the tension was suddenly relieved and the natural equilibrium restored with such intensity that the effect described was produced.

The other storm occurred at Carysfort Reef light-house, Key West, Fla., about seven miles from the nearest land, on the 12th of April, 1877. This light-house is built in the water on a submerged reef, and consists of a framing of stout iron pillars or piles, each about 100 feet in length, arranged in the form of an octagon, with one pillar in the centre, interlaced at various points regularly with smaller iron rods. It is 122 feet in height to the top of the lantern. At about forty feet above the water is the keeper's dwelling, some forty feet in diameter, the roof and sides of which are of iron-plate, while the floor is of wood. From the roof of this dwelling, extending to the lantern directly above, is an iron cylinder thirteen feet in diameter, containing the spiral stairway.

The following account of the storm and its effects, is given in a letter to Capt. W. H. Heuer, light-house engineer, from C. D. Hawkins, lampist of the seventh light-house district:

"On the afternoon of the 12th of the present month (April, 1877) I noticed indications of a squall, and on consulting the barometer found it was falling. At sunset the wind was blowing rather fresh from the southward,—the sky being dark and wearing a thickened aspect. About 9 p. m. a very severe squall struck the light-house, the wind having increased to a perfect gale, which was accompanied by thunder and lightning. The thunder was of the most terrific character, resembling in sound an artillery duel. There was not exceeding one-fourth to one-half a second between the flashes of lightning. About 10:30 p. m. the light-house was struck. The report caused thereby resembled

the explosion of a shell under water at the distance of about 300 yards. Within thirty minutes the light-house was struck seventeen times: each stroke was most perceptibly felt, and could be heard in the keeper's closed dwelling, —resembling in sound the discharge of a shot-gun. Each time the light-house was struck it trembled violently from top to bottom. The whole air seemed pervaded with electricity, and on arising from bed, my bare feet coming in contact with the floor (which was wet from the rain beating in at the window), I received a severe electrical shock which caused my hair to stand straight, and I was compelled to jump into bed and put on shoes before I could venture again onto the floor. One of the men assisting me was so violently affected as to vomit; the other men were similarly affected. The storm continued furiously till midnight, at which time it abated a little until daylight, when the wind hauled to the westward, and at eight o'clock it was blowing furiously again. The storm continued unabated until sunset, when it subsided. On Saturday the weather was fine, and we continued our repairs. The storm was one of the most violent I ever experienced or heard of."

An additional fact to those given in this letter was obtained through Capt. Heuer from the same source, viz: each time the light-house was struck, the piles which formed the stable part of the structure seemed to become luminous. This appearance may have been an optical deception, produced by the reflection of the light of the discharge from the various points of the building. This appearance however is not without a parallel in the history of discharges of atmospheric electricity, vertical rods of iron at the moment of a discharge having presented a luminous appearance throughout their whole length.

The physiological effects mentioned were probably due to induction, since during the continuance of the cloud above the light-house, all parts of the building must have been in a highly negative condition, supposing the cloud to be positive. This condition in itself, without variation, would scarcely produce any perceptible effect on the bodies of the inmates. It would however be in a state of continual variation with the constant changes in the intensity of the action of the cloud above, especially at the moment of the discharge,

when a neutralization would suddenly take place, sufficient to produce through nervous influence—the phenomena witnessed. The shock received by the person in his foot on getting out of bed is in direct accordance with the well-established principles of induction. So long as the body was in a horizontal position in the bed, which is only a partial conductor, the inductive effect would be much less on it than on the wet floor and iron pillars. When the foot therefore approached the floor, a positive charge would pass from the latter to the former.

That any physiological effects should have been observed under the conditions in which the observers were placed could scarcely, from *a priori* considerations, have been anticipated. They were in a space entirely enclosed by metallic conductors, with the exception of the floor, and in such a condition it might be thought that they would have been completely protected, since the interior of a hollow vessel, (like that of a quart measure,) when insulated and charged, gives no indication of electricity. If a metal ball is suspended by a silk thread, and made to touch the interior of such a vessel charged exteriorly, and afterward transferred to a delicate electrometer, no sign of electricity is observed.

It should be remembered however that in the case of the experiment just mentioned the statical charge of electricity is nearly uniformly distributed over the sides and bottom of the vessel; whereas, in the case of a dynamic charge, the electricity may pass in greater quantity on one side than on another, and the equilibrium of the interior may not be preserved; or furthermore, in this case the diameter of the house being more than twice as great as its vertical height, it would be represented in the preceding experiment by a very shallow vessel in which a complete neutralization could not take place. But whatever may be the explanation of the phenomena, the facts stated are of importance in the theoretical consideration of the action of lightning protection.

With thanks for the honor conferred upon me by my election as a member of the American Electrical Society,

I remain, very truly, &c., &c.

OPENING ADDRESS TO THE NATIONAL ACADEMY OF SCIENCES.*

(Proceedings of the National Academy of Sciences; vol. I, pp. 131, 132.)

Read April 16, 1878.

GENTLEMEN: It gives me great pleasure to welcome you to another anniversary meeting of the National Academy of Sciences. We have only to regret that the room we offer for your use is not better adapted for the purpose; but we expect—with considerable confidence, that Congress will make the appropriation for a new museum building which has been asked for; and if that expectation is fulfilled, we can promise you with certainty that an apartment expressly adapted for the purposes of the Academy will be provided.

During the past year the departments of Government have applied to the Academy for information on two questions relative to the tariff on sugar, and to a series of changes proposed to be made in the material of the Nautical Almanac. A detailed account of these questions and the answers to them will be given by Prof. Hilgard, the home secretary, in his annual report.

Another matter, of which a full account will be given you by Prof. Fairman Rogers, the treasurer of the Academy, relates to a fund which has been established by a number of my personal friends, the income of which is to be devoted during the lives of myself and family toward our maintenance, and afterward, as in the case of the A. Dallas Bache fund, under the direction of the Academy, to the advance of physical science.

This entirely unexpected token of affectionate regard was made at a time when it was doubly grateful. After an almost uninterrupted period of excellent health for fifty years I awoke on the 5th of December, [1877,] at my office in the Light-House depot on Staten Island, finding my right hand

*[Address by the president of the National Academy of Sciences, at the opening of its session held in Washington April 16-19, 1878; read by the home secretary of the Academy.]

in a paralytic condition. This was at first referred by the medical adviser to an affection of the brain, but as the paralysis subsided in a considerable degree in the course of two days, this conclusion was doubted, and on a thorough examination through the eye and by means of auscultation and chemical analysis Dr. S. Weir Mitchell and Dr. J. J. Woodward pronounced the disease an affection of the kidneys. The paralysis of the hand was accompanied with paroxysms of pain through the region of the heart and with oppression of breathing.

Under the judicious and generous direction of Drs. Mitchell and Woodward and the constant supervision of my family physicians, Drs. N. S. Lincoln and G. Tyler, the paroxysms have subsided and I am slowly improving, and now enjoy the prospect of being restored in a measure to my former condition of health.

But I am warned that I must devote my energies with caution, and expend no more power, physical or mental, than is commensurate with my present condition; and in consideration of this I think it advisable to curtail as much as possible the responsibilities which devolve upon me in connection with the various offices which have been pressed upon me in consideration of my residence in the city of Washington and my connection with the Smithsonian Institution.

It will be recollected that five or six years ago I asked leave to resign the presidency of the National Academy, believing that there were other members who had more leisure, and were better qualified to discharge the duties than myself. I received however a circular letter, signed by a number of the principal members of the Academy, requesting me to continue to hold the office.

I must think that the idea of my special fitness for the position was founded on the supposition that it was necessary for the president of the Academy to be a citizen of Washington; but this idea has been found incorrect by experience, and it is proved to be sufficient for carrying on the business of the Academy with the departments of Government, that the home secretary should reside in this city.

I therefore ask leave to renew my request to be allowed to resign the presidency of the Academy, the resignation to take effect at the next meeting. I retain the office six months longer in the hope that I may be restored to such a condition of health as to be able to prepare some suggestions which may be of importance for the future of the Academy.*

CLOSING ADDRESS TO THE NATIONAL ACADEMY OF SCIENCES.†

(Proceedings of the National Academy of Sciences; vol. I, pp. 132, 133.)

Read April 19, 1878.

GENTLEMEN: I have been much interested in the proceedings of the present meeting of the National Academy. Although I have been unable to be present, except during a small part of the session, yet I have been made acquainted with everything that has occurred.

Whatever might have been thought as to the success of the Academy, when first proposed by the late Prof. Louis Agassiz, the present meeting conclusively proves that it has become a power of great efficiency in the promotion of science in this country. To sustain this effect however much caution is required to maintain the purity of its character and the propriety of its decisions.

For this purpose great care must be exercised in the selection of its members. It must not be forgotten for a moment that the basis of selection is actual scientific labor in the way of original research, (that is in making positive additions to the sum of human knowledge,) connected with unimpeachable moral character.

* [Responses were made by Messrs. F. A. P. Barnard, and W. B. Rogers, and it was unanimously—

Resolved, That with every sentiment of sympathy and regard for Professor Henry, the Academy most respectfully declines to entertain any proposition looking to his retirement from the office of President.]

† [Address by the president of the National Academy of Sciences, at the close of its session in April, 1878; read by the home secretary of the Academy.]

It is not social position, popularity, extended authorship, or success as an instructor in science, which entitles to membership, but actual new discoveries; nor are these sufficient if the reputation of the candidate is in the slightest degree tainted with injustice or want of truth. Indeed, I think that immorality and great mental power actually exercised in the discovery of scientific truths are incompatible with each other, and that more error is introduced from defect in moral sense than from want of intellectual capacity.

Please accept my warmest thanks for the kind expressions of sympathy you have extended to me during this period of my illness, and for your personal partiality in refusing to accept my resignation as president of the Academy. I shall be thankful if a beneficent Providence extends my life during another year and grants me the privilege of greeting you again in a twelvemonth from this time—as successful laborers in the fields of science.

I can truly say that I entertain for each member of the Academy a fraternal sympathy, and rejoice at every step he makes in the development of new truths.

With my best wishes for your safe return to your homes, and for a rich harvest of scientific results in the ensuing year, I now bid you an affectionate farewell.

END.



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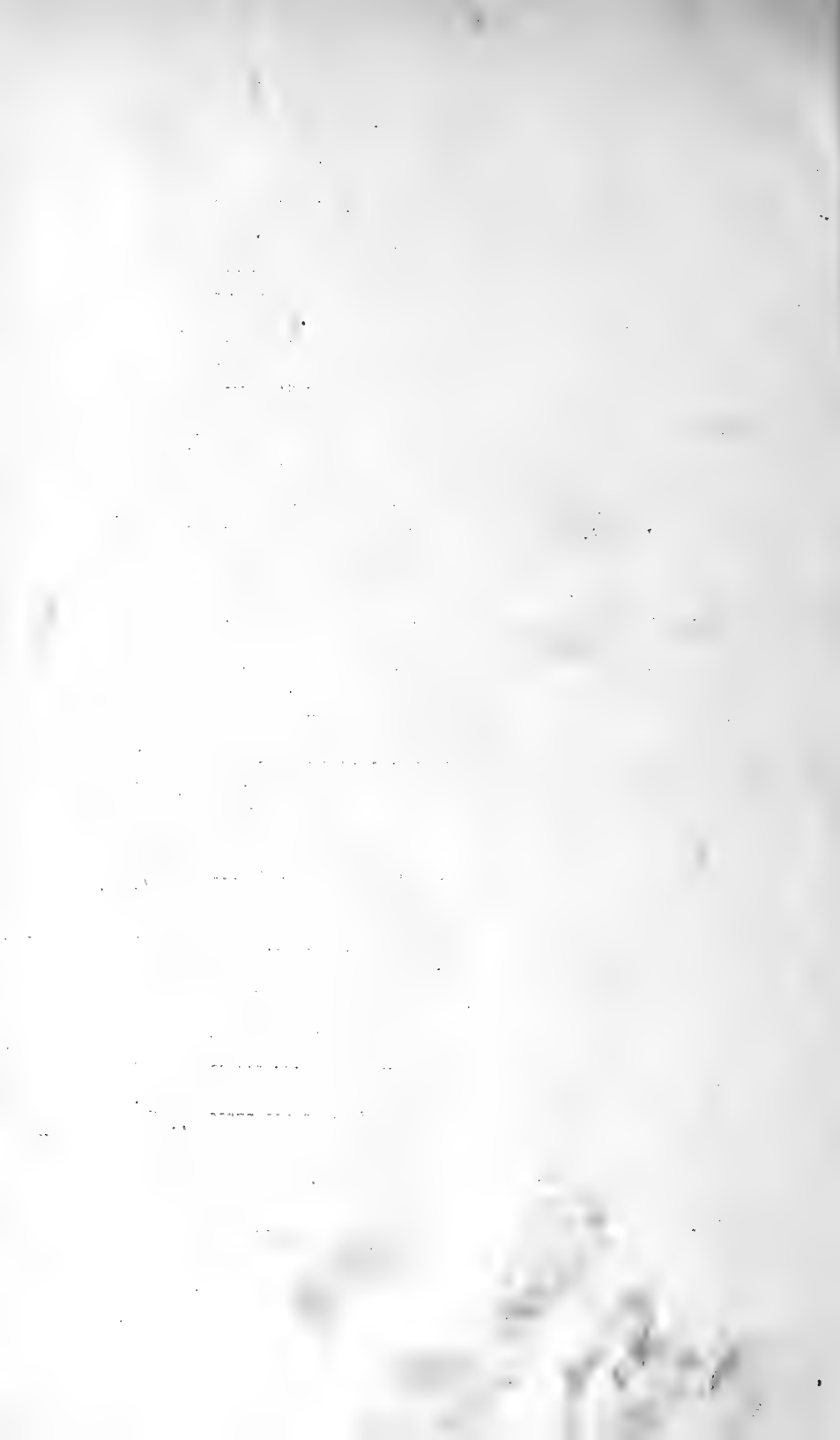
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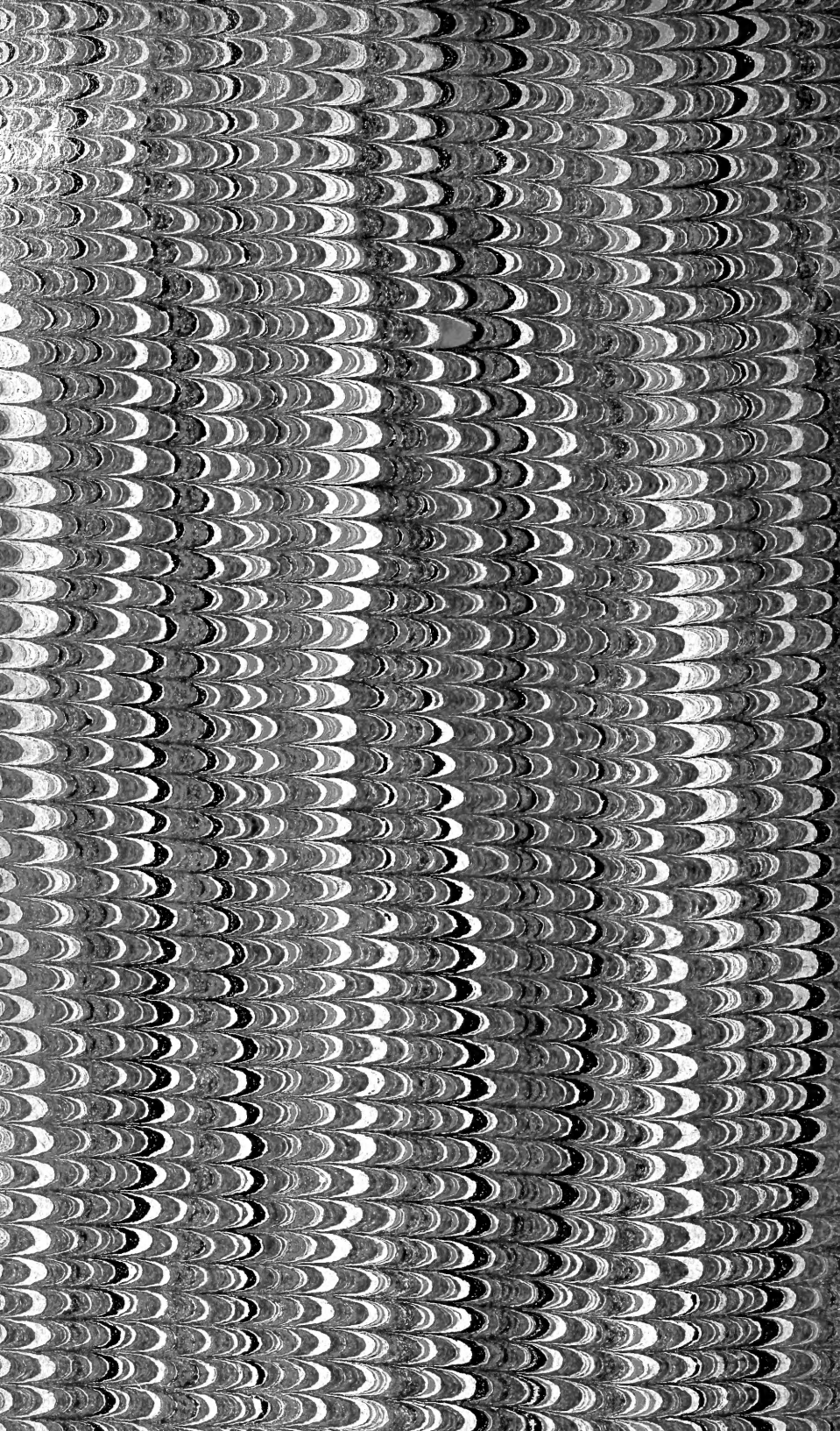
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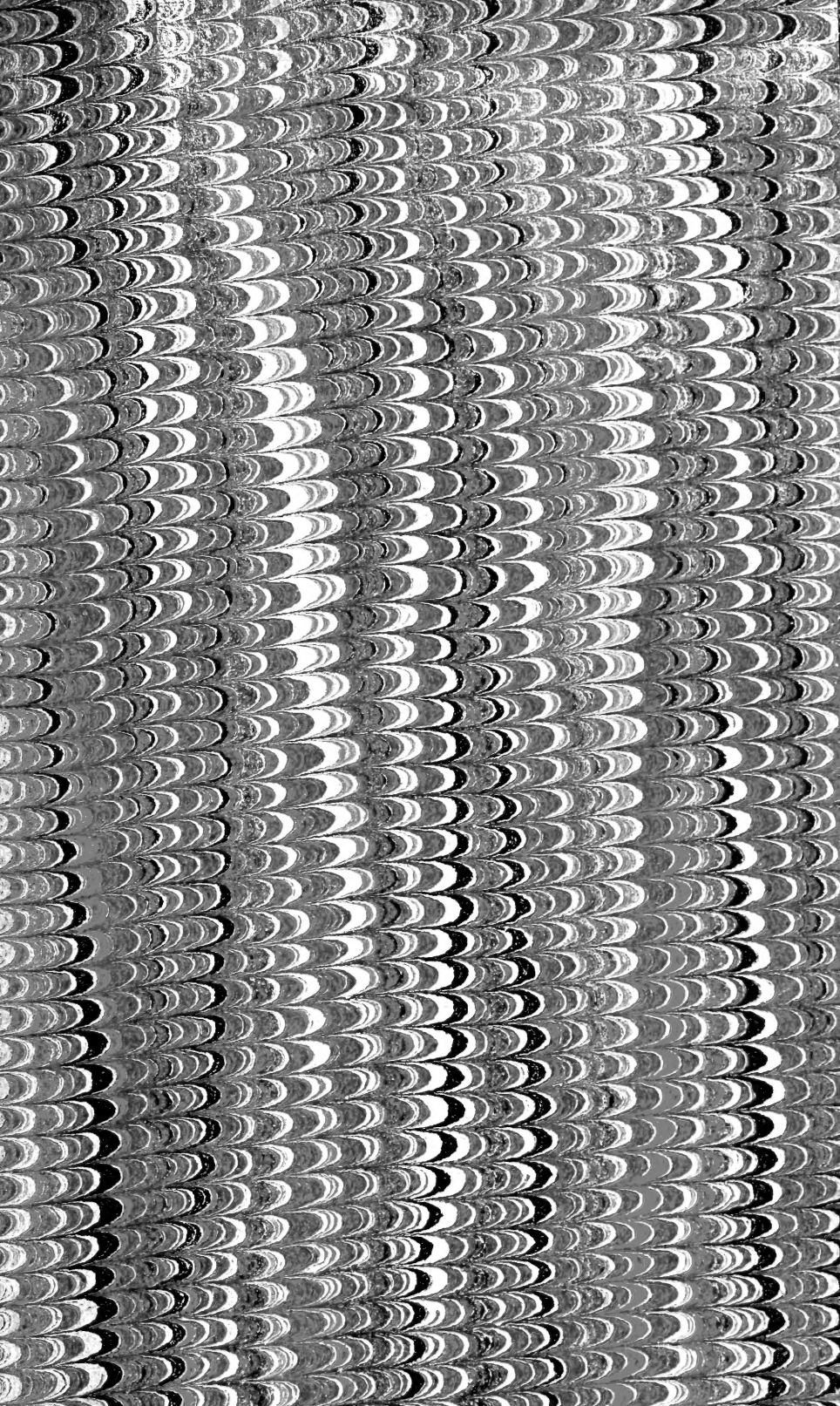
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