A MEMOIR

OF

JOSEPH HENRY.

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A SKETCH OF HIS SCIENTIFIC WORK.

BY

WILLIAM B. TAYLOR.

Read before the Philosophical Society of Washington,
October 26th, 1878.

PHILADELPHIA:
COLLINS, PRINTER, 705 JAYNE STREET.
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SMITHSONIAN INSTITUTION

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[FROM THE BULLETIN OF THE SOCIETY. VOL. II.]

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A SKETCH OF HIS SCIENTIFIC WORK.*

To cherish with affectionate regard the memory of the venerated dead is not more grateful to the feelings, than to recall their excellences and to retrace the stages and occasions of their intellectual conquests is instructive to the reason. Few lives within the century are more worthy of admiration, more elevating in contemplation, or more entitled to commemoration, than that of our late most honored and beloved president—JOSEPH HENRY.

Distinguished by the extent of his varied and solid learning, possessing a wide range of mental activity, so great were his modesty and self-reserve, that only by the accidental call of occasion would even an intimate friend sometimes discover with surprise the fulness of his information and the soundness of his philosophy, in some quite unsuspected direction. Remarkable for his self-control, he was no less characterized by the absence of self-assertion. Ever warmly interested in the development and advancement of the young, he was a patient listener to the trials of the disappointed, and a faithful guide to the aspirations of the ambitious. Generous without ostentation, he was always ready to assist the deserving—by services—by counsel—by active exertions in their behalf.

In his own pursuits Truth was the supreme object of his regard,—the sole interest and incentive of his investigations; and in its prosecution he brought to bear in equable combination qualities of a high order; quickness and correctness of perception, inventive ingenuity in experimentation, logical precision in deduction, perseverance in exploration, sagacity in interpretation.†

* A large portion of the following discourse (including nearly the whole of the section on the "Administration of the Smithsonian Institution,") was necessarily omitted on the occasion of its delivery.

† Henry's tribute to Peltier, seems peculiarly applicable to himself. "He possessed in an eminent degree the mental characteristics necessary for a successful scientific discoverer; an imagination always active in suggesting hypotheses for the explanation of the phenomena under investigation, and a logical faculty never at fault in deducing consequences from the suggestions best calculated to bring them to the test of experi-

EARLY CAREER.

Of Henry's early struggles,—of the youthful traits which might afford us clue to his manhood's character and successes, we have but little preserved for the future biographer. Deprived of his father at an early age, he was the sole care and the sole comfort of his widowed mother. Carefully nurtured in the stringent principles of a devout religious faith, he adhered through life to the traditions and to the convictions derived from his honorable Scottish ancestry.

As a youth he was by no means precocious,—as seldom have been those who have left a permanent influence on their kind. He seems to have felt no fondness for his early schools, and to have shown no special aptitude for the instructions they afforded. Like many another unpromising lad, he followed pretty much his own devices, unconcerned as to the development of his latent capabilities. The books he craved were not the books his school-teachers set before him. The novel and the play interested and absorbed the active fancy naturally so exuberant in youth; and the indications from his impulsive temperament were that he would probably become a poet—a dramatist—or an actor.

He was however from his childhood's years a close observer—both of nature, and of the peculiarities of his fellows: and one characteristic early developed gave form and color to his mental disposition throughout later years,—an unflagging energy of purpose.

About the year 1814, while a boy of still indefinite aims and of almost as indefinite longings, having been confined to the house for a few days, in consequence of an accidental injury, his restless attention happened to be drawn to a small volume on Natural Philosophy, casually left lying on a table by a boarder in the house. Listlessly he opened it and read. Before he reached the third page, he became profoundly interested in the statement of some of the enigmas of the great sphinx—Nature. A new world seemed opening to his inquisitive eyes. Eagerly on he read,—intent to find the hidden meanings of phenomena which hitherto covered by the "veil of familiarity" had never excited a passing wonder or a doubting question. Was it possible ever to discover the real causes of things? Here was a new Ideal—if severer, yet grander than that of art. He no longer read with the languid enjoyment of a passive recipient; he felt the new necessity of reaching out with all the faculties of a thinker, with all the

ence; an invention ever fertile in devising apparatus and other means by which the test could be applied; and finally a moral constitution which sought only the discovery of truth, and could alone be satisfied with its attainment." (*Smithsonian Report* for 1867, p. 158.)

activity of a co-worker.* For the first time he realized (though with no conscious expression of the thought) that there is—so to speak,—an imagination of the intellect, as well as of the emotional soul;—that *Truth* has its palaces no less gorgeous—no less wonderful than those reared by fancy in homage to the *Beautiful.*

The owner of the book observing the close application of the boy, very kindly presented it to him;† and the thankful receiver many years afterward placed upon the inside of its front cover, the following memorandum:—

"This book although by no means a profound work, has under Providence exerted a remarkable influence on my life. It accidentally fell into my hands when I was about sixteen years old, and was the first book I ever read with attention. It opened to me a new world of thought and enjoyment; invested things before almost unnoticed, with the highest interest; fixed my mind on the study of nature; and caused me to resolve at the time of reading it, that I would immediately commence to devote my life to the acquisition of knowledge. J. H."

The new impulse was not a momentary fascination. Thenceforward the novel was thrown aside, and poesy neglected; though to his latest day a sterling poem never failed to strongly impress him. As it dawned upon his reason that the foundation of the coveted knowledge must be the studies he had thought so irksome, he at once determined to repair as far as possible his loss of time, (being then an apprentice to his cousin John F. Doty, a Watch-maker and Silver-smith in Albany,) by taking evening lessons from two of the Professors in the Albany Academy; applying himself diligently to geometry and mechanics. And here shone out that strength of will which enabled him to rise above the harassing obstacle of the *res angusta domi.* With the consent of his employer, so soon as he felt able, (although yet a mere boy,) he managed to procure a position as teacher in a country school, where for seven months successfully instructing boys not much younger than himself, in what he had acquired, he was enabled by rigid economy to take a regular course of instruction at the Albany Academy. Again returning to his school-teaching, he furnished himself with the means of completing his studies at the Academy; where learning that the most important key to the accurate knowledge of nature's laws is a

* "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." J. Henry. (*Agricultural Report* of the Patent Office, for 1857, p. 421.)

† The title of this book is "Lectures on Experimental Philosophy, Astronomy, and Chemistry: by G. Gregory, D.D., Vicar of West-ham." 12mo. London, 1808. The owner of the book was a young Scotchman named Robert Boyle; who was one of the boarders at his mother's house in Albany, N. Y.

familiarity with the logical processes of the higher mathematics, he resolutely set himself to work to master the intricacies of the differential calculus.

Having finished his academic course and passed with honor through his examinations, he then through the warm recommendation of Dr. T. Romeyn Beck—the distinguished Principal of the Academy, obtained a position as private tutor in the family of General Stephen Van Rensselaer.* As this duty did not exact more than about three hours a day of his attendance, he applied his ample leisure (having in view the medical profession)—partly to the assistance of Dr. Beck in his chemical experiments, and partly to the study of Anatomy and Physiology, under Doctors Tully and Marsh.

His devotion to Natural Philosophy which had only grown and strengthened with his own growth in knowledge, led him constantly to repeat any unusual experiment as soon as reported in the foreign scientific journals; and to devise new modifications of the experiment for testing more fully the range and operation of its fundamental principles.

Communications to the Albany Institute.—The "Albany Institute" was organized May 5th 1824, by the union of two older Societies; with General Stephen Van Rensselaer as its President:† and young Henry became at once an active member: though with his modest estimate of his own attainments, he preferred the part of listener and acquirer, to that of seeming instructor, till urged by those who knew him best to add his contributions to the general garner.

Henry's first communication to the Institute was read October 30th 1824 (at the age of about twenty-six years) and was "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."‡ From the stopcock of a strongly made copper vessel in which steam could be safely generated under considerable pressure, he allowed an occasional escape; and he showed by holding the bulb of a thermometer in the jet of steam, at a fixed distance (say of four inches) from the orifice, that as the temperature and pressure increased within the boiler, the indications of the thermometer without grew lower;—the expansion and consequent cooling of the escap-

* Presiding Officer of the original Board of Trustees of the Albany Academy.

† The Albany Institute resulted from the fusion of "The Society for the Promotion of Usefnl Arts in the State of New York," organized Feb. 1791 (incorporated April 2nd 1804,) and the "Albany Lyceum of Natural History" formed and incorporated April 23rd 1823: of which latter society, Henry had been a member.

‡ *Trans. Albany Inst.* vol. i. part 2, p. 30.

ing steam under great pressure, increasing in a higher ratio than the increased temperature required for the pressure. And finally he exhibited the striking paradox, that the jet of saturated steam from a boiler will not scald the hand exposed to it, at a prescribed near distance from the try-cock, provided the steam be sufficiently hot.*

Prolific and skilful in devising experiments, Henry delighted in making evident to the senses the principles he wished to impress upon the mind. Extending the law of cooling by expansion, from steam at high temperatures, to air at ordinary temperatures, his next communication to the Institute (made March 2nd 1825,) was "On the Production of Cold by the Rarefaction of Air." As before, he accompanied his remarks by several characteristic exhibitions.

"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 caliber with a number of holes near the lower end, 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 temperatnre 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 interior atmosphere was so much reduced as to freeze the remaining water in the vessel."†

Although the principle on which this striking result was based was not at that time new, it must be borne in mind that this

* While it requires a heat of 250° F. to generate a steam-pressure of two atmospheres (*i. e.* one additional to the existing), 25° higher will produce a pressure of three atmospheres, and 100° higher, (or 355° F.) will produce a pressure of nine atmospheres: the curve (by rectangular co-ordinates of temperature and pressure) resembling a hyperbola. The increased velocity at high pressure produces a molecular *momentum* of expansion carrying the rarefaction beyond the limit of atmospheric pressure; and in the case of the exposed hand, the injected air current doubtless adds to the cooling impression.

† *Trans. Albany Inst.* vol. i. part 2, p. 36.

particular application, thus publicly exhibited, was long before any of the numerous patents were obtained for ice-making, not a few of which adopted substantially the same process.

State Appointment as a Civil Engineer.—Through the friendship and confidence of an influential judge, Henry received about this time an unexpected offer of an appointment as Engineer on the survey of a route for a road through the State of New York, from the Hudson river on the east, to lake Erie on the west. The proposal was too tempting to his natural proclivities to be refused; and being appointed, he embarked upon his new and arduous duties with the zeal and energy which were so prominent a feature of his character. He completed the survey with credit to himself, and to the entire satisfaction of the Commissioners of the work.*

So attractive appeared the profession of engineer to his enterprising disposition, that he was about to accept the directorship in the construction of a canal in Ohio, when he was informed that the Chair of Mathematics in the Albany Academy would soon become vacant, and that his own name had already been prominently brought forward in connection with the position. At the urgent solicitation of his old friend and former teacher Dr. T. Romeyn Beck, he consented with some hesitation to signify his willingness to accept the vacant chair if appointed thereto.

Election as Professor of Mathematics.—In the spring of 1826, Henry was duly elected by the Trustees of the Albany Academy to the Professorship of Mathematics and Natural Philosophy in that Institution. As the duties of his office did not commence till September of that year, he was allowed a practical vacation of about five months; which was partly occupied with a geological exploration in the adjoining counties, as assistant to Professor Eaton, of the Rensselaer School, and partly devoted to a conscientious preparation for his new position.

In a worldly point of view, this variety of occupation and versatility of adaptation might perhaps be regarded as unfavorable to success. As a method of culture, it was of unquestionable advantage to his intellectual powers. A hard student, with great capacity for close application, he accumulated large stores of information: and in addition to his constant thirst for acquirement in different directions, his leisure was occupied to a considerable extent with physical and chemical examinations. On the 21st of March 1827, he delivered before the Albany Institute a lecture on "Flame," accompanied with experiments.†

* In a popular journal ("The Eclectic Magazine") it is stated: "His labors in this work were exceedingly arduous and responsible. They extended far into the winter, and the operations were carried on in some instances amid deep snows in primeval forests."

† *Trans. Albany Inst.* vol. i. part 2, p. 59.

Meteorological Work.—The Regents of the University of the State of New York, endowed by the State Legislature with supervisory functions over the public educational institutions of the State, in 1825 established a system of meteorological observation for the State, by supplying to each of the Academies incorporated by them, a thermometer and a rain-gauge, and requiring them to keep a daily register of prescribed form, to entitle them to their portion of the literature fund of the State. In 1827, the Hon. Simeon De Witt, Chancellor of the Board of Regents, associated with himself Dr. T. Romeyn Beck and Professor Henry of the Albany Academy, to prepare and tabulate the results of these observations. The first Abstract of these collections (for the year 1828) comprised tabulations of the monthly and yearly means of temperature, wind, rain, etc., at all the stations, an account of meteorological incidents generally, and a table of "Miscellaneous Observations" on the dates of notable phases of organic phenomena connected with climatic conditions. These annual Abstracts, to which Henry devoted a considerable share of his attention, were continued through a series of years and were published in the "Annual Reports of the Regents of the University to the Legislature of the State of New York.* The third Abstract (for 1830) includes an accurate tabulation by Henry of the Latitudes, Longitudes, and Elevations of all the meteorological stations; over forty in number.

ELECTRICAL RESEARCHES AT ALBANY; FROM 1827–1835.

Of Henry's distinguished success as a lecturer and teacher, in imparting to his pupils a portion of his own zeal and earnestness in the pursuit of scientific knowledge, as well as in winning their affection and in inspiring their esteem, it is not designed here to discourse; but rather of his solitary labors outside of his professional occupation in communicating and diffusing knowledge. Very shortly after his occupation of the academic chair of mathematics and physics, he turned his attention to the experimental study of that mysterious agency—electricity. Professor Schweigger of Halle, had improved on Œrsted's galvanic indicator (of a single wire circuit) by giving the insulated wire a number of turns around an elongated frame longitudinally enclosing the compass needle, and by thus multiplying the effect of the galvanic circuits, had converted it into a real *measuring* instrument—a "galvanometer."† Ampère and Arago of Paris, develop-

* *Reports of Regents*, &c. Albany, vol. i. 1829–1835.

† The name of Galvani (as original discoverer of chemico-electricity) is usually retained to designate both the current and its generator; although the chemico-electric pile and battery were really first contrived by Volta in 1800. In the same manner Oersted is generally accounted the discoverer of electro-magnetism, although he never devised an electro-

ing Œrsted's announcement of the torsional or equatorial reaction between a galvanic conductor and a magnetic needle, had found that a circulating galvanic current was capable not only of deflecting a suspended magnet, but of *generating* magnetism—permanently in sewing needles, and temporarily in pieces of iron wire, when placed within a glass tube around which the conjunctive wire of the battery had been wound in a loose helix; and had thus created the "electro-magnet."* The scientific world was just aroused to the close interrogation of this new marvel, each questioner eager to ascertain its most efficient conditions, and to increase its manifestations. William Sturgeon of Woolwich, England, had extended the discoveries of Ampère and Arago, by dispensing with the glass tube, constructing a "horse-shoe" bar of soft iron (after the form of the usual permanent magnet) coated with a non-conducting substance, and winding the copper conjunctive wire directly upon the horse-shoe; and had thus produced the first *efficient* electro-magnet;—capable of sustaining several pounds by its armature, when duly excited by the galvanic current. He had also greatly improved lecture-room apparatus for illustrating the electro-magnetic reactions of rotations, etc., (where a permanent magnet is employed) by introducing stronger magnets, and thereby succeeding in exhibiting the phenomena on a larger scale, with a considerable reduction of the battery power.†

Faraday had not yet commenced the series of researches which in after years so illumined his name, when Henry published his first contribution to electrical science, in a communication read before the Albany Institute, October 10th, 1827, "On some Modifications of the Electro-Magnetic Apparatus." From his experimental investigations he was enabled to exhibit all the class illustrations attempted by Sturgeon, on even a still larger and more conspicuous scale, with the employment of very weak magnets (where required), and with a still further considerable reduction of the battery power. These quite striking and unexpected results were obtained by the simple expedient of adopting in every case where single circuits had previously been used, the manifold coil of fine wire which Schweigger had employed to increase the sensibility of the galvanometer. He remarks:—

magnet; and appears not to have been the first even to discover the directive influence of a current on a magnetic needle.

* *Annales de Chimie et de Physique*, 1820, vol. xv. pp. 93–100.

† *Trans. Soc. Encouragement Arts*, etc., 1825, vol. xliii. pp. 38–52. This battery (of a single element) consisted "of two fixed hollow concentric cylinders of thin copper, having a movable cylinder of zinc placed between them. Its superficial area is only 130 square inches, and it weighs no more than 1 lb. 5 ozs." Mr. Sturgeon was deservedly awarded the Silver Medal of the Society for the Encouragement of Arts, etc., "for his improved electro-magnetic apparatus." Described also in Thomson's *Annals of Philos.*, Nov. 1826, vol. xii., new series, pp. 357–361.

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

The coils employed in the various articles of apparatus thus improved, comprised usually about twenty turns of fine copper wire wound with silk to prevent metallic contact, the whole being closely bound together. To exhibit for example Ampère's ingenious and delicate experiment showing the directive action of the earth as a magnet on a galvanic current when its conductor is free to move, (usually a small wire frame with its extremities dipping either into mercury cups, or into mercury channels,) or its simpler modification, the "ring" of De La Rive (usually an inch or two in diameter and made to freely float with its galvanic element in its own bath,) the effect was strikingly enhanced by Henry's method of suspending by a silk thread a large circular coil twenty inches in diameter, of many wire circuits bound together with ribbon,—the extremities of the wire protruding at the lower part of the hoop, and soldered to a pair of small galvanic plates;—when by simply placing a tumbler of acidulated water beneath, the hoop at once assumed (with a few oscillations) its equatorial position transverse to the magnetic meridian. By a similar arrangement of two circular coils of different diameters, one suspended within the other, Ampère's fine discovery of the mutual action of two electric currents on each other, was as strikingly displayed. Such was the character of demonstration by which the new Professor was accustomed to make visible to his classes the principles of electro-magnetism: and it is safe to say that in simplicity, distinctness, and efficiency, such apparatus for the lecture-room was far superior to any of the kind then existing.

* *Trans. Albany Institute*, vol. i. pp. 22, 23.

Should any one be disposed to conclude that this simple extension of Schweigger's multiple coil was unimportant and unmeritorious, the ready answer occurs, that talented and skilful electricians, laboring to attain the result, had for six years failed to make such an extension. Nor was the result by any means antecedently assured by Schweigger's success with the galvanometer. If Sturgeon's improvement of economizing the battery size and consumption, by increasing the magnet factor (in those few cases where available), was well deserving of reward, surely Henry's improvement of a far greater economy, by increasing the circuit factor (entirely neglected by Sturgeon) deserved a still higher applause.

In a subsequent communication to Silliman's Journal, Henry remarks on the results announced in October, 1827:—"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 extensive than to my knowledge had been previously effected by a small galvanic element." The electro-magnet figured and described by Sturgeon, (in his communication of November, 1825,) consisted of a small bar or stout iron wire bent into a U or horse-shoe form, having a copper wire wound loosely around it in eighteen turns, with the ends of the wire dipping into mercury cups connected with the respective poles of a battery having 130 square inches of active surface. This was undoubtedly the most efficient electro-magnet then in existence.

In June of 1828, Henry exhibited to the Albany Institute a small-sized electro-magnet closely wound with silk-covered copper wire about one-thirtieth of an inch in diameter. By thus insulating the conducting wire instead of the magnetic bar or core, he was enabled to employ a compact coil in close juxtaposition from one end of the horse-shoe to the other, obtaining thereby a much larger number of circuits, and having each circuit more nearly at right angles with the magnetic axis. The lifting power of this magnet is not stated, though it must obviously have been much more powerful than the one described by Sturgeon.

In March of 1829, Henry exhibited to the Institute a somewhat larger magnet, of the same character. "A round piece of iron about one quarter 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." In this case the coil was wound upon itself.

Henry's "Quantity" Magnet compared with Moll's.—Shortly after this, Dr. G. Moll, (Professor of Natural Philosophy in the University of Utrecht,) having seen in England, in 1828, an electro-magnet of Sturgeon's which supported nine pounds from its armature, "determined to try the effect of a larger galvanic apparatus;" and in a paper published in 1830,* remarks: "I obtained results which appear astonishing, and are—as far as the intensity of magnetic force is concerned, altogether new. I have anxiously looked since that time into different scientific continental and English journals, without finding any further attempt to extend and improve Mr. Sturgeon's original experiment." Moll's horse-shoe formed of a round bar of iron about 1 inch thick, was about 8½ inches in height, and had a wrapped copper wire of about one-eighth inch diameter coiled 83 times around it. The weight of the horse-shoe and wire was about 5 pounds; of the armature, about 1¼ pound; and with a single galvanic pair whose acting zinc surface was about 11 square feet, the electro-magnet supported about 50 pounds. With cautious additions, the load could be increased to 75 pounds. An additional galvanic pair of about 6 square feet was applied without increasing the power of the magnet.†

As soon as the account of Moll's magnet reached this country, Henry who had obtained and had publicly exhibited nearly two years previously, considerably higher results, and who realized that there was at least one very important difference of construction between his own magnet and that of the Dutch savant, felt it a duty at once to publish the details of his own researches, in a more public form:—which he accordingly did in the January number of Silliman's American Journal of Science for 1831; (then published only quarterly;) causing a copy of Professor Moll's paper, taken from Brewster's Edinburgh Journal of Science for October 1830, to be inserted in the same number. At the conclusion of his own article he remarks: "The only effect Professor 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."

The magnet which he had subsequently made, consisted of a cylindrical bar of iron one-half inch in diameter and about 10 inches long, bent into a horse-shoe and closely wound with several

* *Bibliothèque Universelle*, 1830, cah. 45, p. 19.

† Brewster's *Edinburgh Jour. Sci* Oct. 1830, vol. iii. n. s. pp. 209–218.

strands of fine silk-covered wire, each about 30 feet long; which arrangement when placed in the circuit of a single galvanic pair whose zinc surface was 6 inches by 4 inches (one-sixth of a square foot) sustained by its armature "39 pounds, or more than fifty times its own weight;" Moll's highest result (of which he justly felt proud) being only fifteen times the weight of the magnet, with 11 square feet of zinc surface. While Henry's magnet had the practical advantage of being about only one-half the size of Moll's—in each dimension, (and therefore about only one-eighth its weight without wrappings,) yet it supported more than half his load: (39 pounds to 75 pounds.) Moll had employed a single copper wire one-eighth inch thick and about 22 feet long: Henry, several strands each about one thirty-sixth of an inch thick, and 30 feet long;—the former making 83 turns around the iron core,—the latter, several hundred turns. But the most surprising contrast resulting from these differences was the enormous difference of battery-power applied; Moll pushing his up to 17 square feet,—Henry reducing his to one-sixth of one square foot. With a galvanic element reduced to two and a half square inches, his magnet sustained 28 pounds; or more than double the relative duty of Moll's at its highest power. The philosopher of Utrecht, though he evidently realized with him of Albany, the importance of close winding, employed but a single layer of coil. The latter, by means of well-considered trials had ascertained the great increase of magnetic force resulting from a succession of coils.

To Henry therefore belongs the exclusive credit of having first constructed the magnetic "spool" or "bobbin": that form of coil since universally employed for every application of electro-magnetism, of induction, or of magneto-electrics. This was his first great contribution to the science and to the art of galvanic magnetization. It may be very confidently affirmed that prior to 1829, no one on either hemisphere had ever thought of winding the legs of an electro-magnet on the principle of the "bobbin"; and that not till after the publication of Henry's method in January of 1831, was it ever employed by any European physicist.

"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 directed 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."* Moll's single conducting wire of one eighth inch diameter, while therefore *electrically* equivalent to about 20 of Henry's conducting wires (of the same length and weight) would be *magnetically* inferior thereto—for equal iron cores.

Notwithstanding that Henry's successes were thus both earlier and more brilliant than those of Moll, the two names are usually associated together by European writers in treating of the development of the magnet.†

Among the subsequent experiments on which Henry was engaged at the time of receiving the Edinburgh Journal of Science containing Moll's paper, was a series on a much larger magnet, consisting of a bar of soft iron two inches square (with the angles rounded) and twenty inches long, bent into a horse-shoe about nine inches high, and weighing 21 pounds. Its armature—a piece from the same bar ground to fit truly the ends of the horse-shoe, weighed 7 pounds. Nine coils of copper bell-wire each 60 feet in length (making 540 feet in all) were separately wound on different portions of the horse-shoe. "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 wire were left projecting and all numbered, so that the first and last end of each strand might be readily distinguished. In this manner was 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

* Sill. *Am. Jour. Sci.* January, 1831, vol. xix. p. 402. The three names—Arago, Sturgeon, and Henry, may well typify the infancy, the youth, and the mature manhood of the electro-magnet.

† Faraday in subsequently investigating the conditions of galvanic induction, referred with approbation to the magnets of Moll and Henry as best calculated to produce the effects sought. In constructing his duplex helices for observing the direction of the induced current, he however adopted Henry's method by winding 12 coils of copper wire each 27 feet long—one upon the other. (*Phil. Trans. Roy. Soc.* Nov. 24, 1831, vol. cxxii. (for 1832,) pp. 126, and 138. *Experimental Researches*, etc., vol. i. art. 6, p. 2; and art. 57, p. 15)

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, etc. The horse-shoe was suspended in a rectangular wooden frame 3 feet 9 inches high and 20 inches wide."

Two of the wires (one from each extremity of the legs) being joined together by soldering, so as to form a single circuit of 120 feet, with its extreme ends connected with the battery, produced a lifting power of 60 pounds. (*Ex.* 19.) The same two wires being separately connected with the same battery (forming a double circuit of 60 feet each) a lifting power of 200 pounds was obtained: (*Ex.* 10.) or more than three times the power of the former case with the same wire. Four wires (two from each extremity of the legs) being separately connected with the battery, (forming four circuits,) gave a lifting power of 500 pounds. (*Ex.* 12.) Six wires (three from each leg) united in three pairs, (forming three circuits of 120 feet each,) gave a lifting power of 290 pounds. (*Ex.* 18.) The same six wires being separately connected with the battery in six independent circuits, produced a lifting power of 570 pounds: (*Ex.* 13.) or very nearly double that of the same wires in double lengths. When all the nine wires were separately attached to the battery, a lifting power of 650 pounds was evoked. (*Ex.* 14.) In all these experiments "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 two-fifths of a square foot: the battery required only half a pint of dilute acid for its submersion."

"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 pounds." (*Ex.* 16.) This was certainly a very remarkable result; particularly when compared with Moll's 75 pounds with *eleven square feet* of zinc. In order to obtain the maximum attractive power of this magnet, with its nine coils, "a small battery formed with a plate of zinc 12 inches long and 6 wide, and surrounded by copper, was substituted for the galvanic element used in the former experiments: the weight lifted in this case was 750 pounds." (*Ex.* 15.) This is exactly ten times the maximum weight supported by Moll's magnet with a far greater battery power. In illustration of the feeble power of the magnetic poles when exerted separately, it was found that with precisely the same arrangements giving a holding power of 750 pounds to the double contact armature,—either pole alone was capable of sustaining only 5 or 6 pounds: "and in this case we never succeeded in making it lift the armature—weighing 7 pounds. We have never seen the circumstance noticed of so great a difference between a single pole and both."

Henry's "Intensity" Magnet.—But Henry's remarkable paper of January 1831 contains still another original contribution to the theory and practice of electro-magnetics; no less important than his invention of the magnetic spool. While Moll had endeavored to induce strong magnetism by the use of a powerful "quantity" battery, Henry had labored to derive from a minimum galvanic power its maximum magnetizing effect: and in his varied experiments on these two factors, he discovered very curious and unsuspected relations between them. A great majority of investigators—after having definitely ascertained the striking fact of the great inferiority in magnetizing power, of a single long continuous coil, to a proportionally shortened circuit of multiple coils,—would naturally have been led to abandon all further investigation of the feebler system. Henry however recognized in this a field of instructive inquiry: and for the first time showed that the coil of short and numerous circuits, least affected by a battery of many pairs, was on the contrary most responsive to a single galvanic element; while the single extended coil, least influenced by a single pair, was most excited by a battery of elements. He appears to have been the first to form a clear conception of the difference between "intensity" and "quantity" both in the battery and in the magnet: a difference which (as referred to the current), he was accustomed figuratively to illustrate by the mechanical difference between equal momentums of high and low velocity.*

The illustrious Laplace had suggested to Ampère in 1820,—immediately upon the discovery of the galvanometer, that by sending the galvanic current through long wires connecting two distant stations, the deflections of enclosed magnetic needles would constitute very simple and efficient signals for an instantaneous telegraph.† Peter Barlow the eminent English mathematician and magnetician taking up the suggestion, had endeavored more fully to test its practicability. He has thus

* "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." (*Smithsonian Report* for 1857, p. 103.) Although these terms are somewhat antiquated, and repudiated by recent writers, they will be retained in this Memoir, for their convenience.

† *Annales de Chimie et de Physique*, 1820, vol. xv. pp. 72, 73. Ampère made the experiment suggested by Laplace, through a long conducting wire "with perfect success." The length of the wire is not stated.

stated the result: "In a very early stage of electro-magnetic experiments it had been suggested that an instantaneous telegraph might be established by means of conducting wires and compasses. The details of this contrivance are so obvious, and the principle on which it is founded so well understood, that there was only one question which could render the result doubtful; and this was,—is there any diminution of effect by lengthening the conducting wire? It had been said that the electric fluid from a common [tin-foil] electrical battery had been transmitted through a wire four miles in length without any sensible diminution of effect, and to every appearance instantaneously; and if this should be found to be the case with the galvanic circuit, then no question could be entertained of the practicability and utility of the suggestion above adverted to. I was therefore induced to make the trial; but I found such a sensible diminution with only 200 feet of wire, as at once to convince me of the impracticability of the scheme. It led me however to an inquiry as to the cause of this diminution, and the laws by which it is governed."*

Henry in his researches just referred to, (assisted by his friend Dr. Ten-Eyck,) employed a small electro-magnet of one quarter inch iron "wound with about 8 feet of copper wire." Excited with a single pair "composed of a piece of zinc plate 4 inches by 7, surrounded with copper," (about 56 square inches of zinc surface,) the magnet sustained four pounds and a half. With about 500 feet of insulated copper wire (.045 of an inch in diameter) interposed between the battery and the magnet, its lifting power was reduced to two ounces;—or about 36 times. With double this length of wire, or a little over 1000 feet, interposed, the lifting power of the magnet was only half an ounce: thus fully confirming the results obtained by Barlow. With a small galvanic pair 2 inches square, acting through the same length of wire (over 1000 feet,) "the magnetism was scarcely observable in the horse-shoe." Employing next a trough battery of 25 pairs, having the same zinc surface as previously, the magnet in direct connection, (which before had supported four and a half pounds,) now lifted but seven ounces;—not quite half a pound. But with the 1060 feet of copper wire (a little more

* "On the Laws of Electro-magnetic Action." *Edinburgh Philosoph. Jour.* Jan. 1825, vol. xii. pp. 105–113. In explanation and justification of this discouraging judgment from so high an authority in magnetics, it must be remembered that both in the galvanometer and in the electro-magnet, the coil best calculated to produce large effects, was that of least resistance; which unfortunately was not that best adapted to a long circuit. On the other hand, the most efficient magnet or galvanometer was not found to be improved in result by increasing the number of galvanic elements. Barlow in his inquiry as to the "law of diminution" was led (erroneously) to regard the resistance of the conducting wire as increasing in the ratio of *the square root* of its length. (p. 111.)

than one-fifth of a mile) suspended several times across the large room of the Academy, and placed in the galvanic circuit, the same magnet sustained eight ounces: that is to say, the current from the galvanic trough produced greater magnetic effect after traversing this length of wire, than it did without it.

Speculating on this remarkable, and at the time, paradoxical result, Henry suggests in explanation, that "a current from a trough possesses more 'projectile' force (to use Professor 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 smaller 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." †

Here for the first time is presented to science the "intensity" coil,—a spool of a single fine wire closely wound again and again upon itself,—with its singular capabilities—not of power, but (what was never before suspected nor imagined) of subtile excitation from a distant source. Here for the first time is established the important principle, that there must be a proportion between the aggregate internal resistance of the battery, and the whole external resistance of the conjunctive wire or conducting circuit; that for a "quantity" magnet of multiple coils (or their equivalent a large wire of corresponding weight) a "quantity" battery of surface, or a single galvanic element is required; while for an "intensity" magnet of extended continuous fine coil, an "intensity" battery of many small pairs is requisite: ‡ with the further discovery that the electro-motive force of the latter form enables a very long conductor to be employed without sen-

* Really Laplace's project;—not Barlow's.

† Silliman's *Am. Jour. Sci.* Jan. 1831, vol. xix. pp. 403, 404.

‡ "For circuits of small resistance, galvanometers of small resistance must be used. For circuits of large resistance, galvanometers of large resistance must also be used; not that their resistance is any advantage, but because we cannot have a galvanometer adapted to indicate very small currents without having a very large number of turns in the coil, and this involves necessarily a large resistance." Prof. F. Jenkin, *Electricity and Magnetism*, 12mo. London, and N. Y. 1873, chap. iv. sect. 8, p. 89.

sible diminution of the effect.* Professor Moll, the foremost of Europeans in the chase, and close upon the heels of Henry in one portion of his researches, produced a powerful "quantity" magnet, but one hopelessly and radically incapacitated from any such application.

These memorable consequences of careful and judicious experiment, carried on in 1829, and 1830, formed truly a most pregnant epoch in the history of the infant science; and constituted a valuable addition to the world's capital. Their adoption underlies all subsequent applications of the intermittent magnet, and is the indispensable basis of every form of the electro-magnetic telegraph since invented. They settled satisfactorily (in Barlow's phrase)—the "only question which could render the result doubtful:" and though derived from the magnet, were obviously as applicable to the galvanometer needle.

It is idle to say in disparagement of these successes, that in the competitive race of numerous distinguished investigators in the field, diligently searching into the conditions of the new found agency, the same results would soon have been reached by others. For of what discovery or invention may not the same be said? Only those who have sought in the twilight of uncertainty, can appreciate the vast economy of effort, by prompt directions to the path from one who has gained an advance. Not for what might be, but for the actual bestowal, does he who first grasps a useful truth, merit the return of at least a grateful recognition.

If these results apparently so simple when announced by Henry, have never been justly appreciated either at home or abroad, no such complaint ever escaped their author. No such thought seems ever to have occurred to his artless nature. For him the one sufficient incentive and recompense was the advancement of himself and others in the knowledge of nature's laws. With the telegraph consciously within his grasp, he was well content to leave to others the glory and the emoluments of its realization.

In the year 1831, Henry had suspended around the walls of one of the upper rooms in the Albany Academy, a mile of copper bell-wire interposed in a circuit between a small Cruickshank battery and an "intensity" magnet of continuous fine coil. A narrow steel rod—a permanent magnet—pivoted to swing horizontally like the compass needle, was arranged so that normally when pointing north, this end remained in contact with one leg of the soft iron core, while near the opposite end of the compass needle, a small stationary office-bell was placed. At each exci-

* Beyond a certain maximum length, there is of course a decrease of power proportioned to the increased resistance of a long conductor: but the magnetizing effect has not been found to be diminished in the ratio of its length.

tation of the electro-magnet, the compass needle was repelled from one leg (by its similar magnetism) and attracted by the other leg, so that its free end tapped the bell. This simple device the Professor was accustomed to exhibit to his classes, in illustration of the facility of transmitting signals to a distance by the swift action of electro-magnetism.

Henry regarded his "quantity" magnet as being *scientifically* more important than his "intensity" magnet; and his success in constructing such, of almost incredible power, caused numerous requisitions on his skill. In April, 1831, Professor Silliman published in his Journal "An Account of a large Electro-Magnet made for the Laboratory of Yale College," under his charge. The iron horse-shoe about one foot high was made from a three inch octagonal bar 30 inches long; and was wrapped with 26 strands of copper wire each about 28 feet long. When duly excited by a single galvanic element consisting of concentric cylinders of copper and zinc, presenting about five square feet of active surface, the magnet lifted more than a ton weight. For reversing the polarity of the magnet, a duplicate battery was oppositely connected with extensions of the ends of the coils, so that either battery could be alternately dipped. With a load of 56 pounds suspended from the armature, the poles of the magnet could be so rapidly reversed, that the weight would not fall. Professor Silliman remarks of the maker: "He has the honor of having constructed by far the most powerful magnets that have been known; and his last, weighing (armature and all) but 82½ pounds, sustains over a ton;—which is eight times more powerful than any magnet hitherto known in Europe." * And Sturgeon (the true foster-father of the magnet) thus heralds the Yale College triumph: "By dividing about 800 feet of conducting wire into 26 strands and forming it into as many separate coils around a bar of soft iron about 60 pounds in weight and properly bent into a horse-shoe form, Professor Henry has been enabled to produce a magnetic force which completely eclipses every other in the whole annals of magnetism; and no parallel is to be found since the miraculous suspension of the celebrated oriental imposter in his iron coffin."†

The first Electro-magnetic Engine.—Among his ingenious applications of the new power, Henry's invention of the Electro-magnetic Engine should here be noticed. In a letter to

* Silliman's *Am. Jour. Sci.* April, 1831, vol. xx. p. 201. *Relatively,* some of Henry's smaller magnets were many times more powerful than this. A miniature one made by Dr. Ten-Eyck under his direction, sustained 200 times its own weight; and one still smaller, sustained more than 400 times its own weight! (Sill. *Am. Jour. Sci.* vol. xix. p. 407.)

† *Philosoph. Magazine; and Annals,* 1832, vol. xi. p. 199.

his friend Professor Silliman, he says: "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." The device consisted of a horizontal soft iron bar, about seven inches long, pivoted at its middle to oscillate vertically, and closely wrapped with three strands of insulated copper wire, whose ends were made by suitable extensions to project and bend downward at either end of the beam in reversed pairs, so as conveniently to dip into mercury thimbles in connection with the plates of the battery. Two upright permanent magnets having the same polarity, were secured immediately under the two ends of the oscillating bar, but separated from them by about an inch. So soon as the circuit was completed by the depression of one end of the oscillating electro-magnetic bar, a repulsion at this end co-operating with an attraction at the opposite end, caused immediately a contrary dip of the bar, which by reversing the polarity of this magnetic beam, thus produced a constant reciprocating action and movement. The engine beam oscillated at the rate of 75 vibrations per minute for more than an hour, or as long as the battery current was maintained.* This simple but original device comprised the first automatic pole-changer or commutator ever applied to the galvanic battery,—an essential element not merely in every variety of the electro-magnetic machine, but in every variety of the magneto-electric apparatus, and in every variety of the highly useful induction apparatus.

In an interesting "Historical Sketch of the rise and progress of Electro-magnetic Engines for propelling machinery," by the distinguished philosopher James P. Joule, he remarks: "Mr. Sturgeon's discovery of magnetizing bars of soft iron to a considerable power, and rapidly changing their polarity by miniature voltaic batteries, and the subsequent improved plan by Professor Henry of *raising* the magnetic action of soft iron,—developed new and inexhaustible sources of force which appeared easily and extensively available as a mechanical agent; and it is to the ingenious American philosopher above named, that we are indebted for the first form of a working model of an engine upon the principle of reciprocating polarity of soft iron by electro-dynamic agency "†

In Henry's deliberate contemplation of his own achievement, his remarkable sagacity and sobriety of judgment were conspicuously displayed. Unperturbed by the enthusiasm so natu-

* Silliman's *Am. Jour. Sci.* July, 1831, vol. xx. pp. 340–343.

† Sturgeon's *Annals of Electricity* etc., March, 1839, vol. iii. p. 430. Sturgeon himself the first to devise a *rotary* electro-magnetic engine, deserves honorable mention for correcting the statement of an American writer, and declining his mistaken award by frankly recognizing Henry's right to priority. (*Annals of Electricity*, April, 1839, vol. iii. p. 554)

ral to the successful inventor, he carefully scanned the capabilities of this new dynamic agent. Considering the source of the power, he arrived at the conclusion that the deoxidation of metal necessary for the battery, would require the expenditure of at least as much power as its combustion in the battery could refund; and that the coal consumed in such deoxidation could be much more economically employed directly in the work to be done.* As the battery consumption moreover was found to increase more rapidly than the magnetic power produced, he was at once convinced that it could never supersede or compete with steam.† He believed however that the engine had a useful future in many minor applications where economy was not the most important consideration.

When sometime afterward, a friend urged him to secure patents on his inventions,—the "intensity" electro-magnet with its combinations, and the magnetic engine with its automatic pole-changer, earnestly assuring him that either one with proper management would secure an ample fortune to its owner, he firmly resisted every importunity; declaring that he would feel humiliated by any attempt at monopolizing the fruits of science, which he thought belonged to the world. And this aversion to self-aggrandizement by researches undertaken for truth, was carried with him through life.‡

While such disinterestedness cannot fail to excite our admiration, it may perhaps be questioned whether in these cases it did not from a practical point of view, amount to an over-fastidiousness:—whether such legal establishment of ownership, shielding the possessor from the occasional depreciations of the envious,

* These considerations have been more than justified by later comparative investigations. Rankine estimates that the consumption of one pound of zinc will not produce more than one-tenth the energy that one pound of coal will; and that though in the efficient utilization of this energy it is four times superior, its useful work is therefore less than half that of coal; while its cost is from forty to fifty times greater. (*The Steam Engine and other Prime Movers.* By W. J. M. Rankine, London and Glasgow, 1859, part iv. art. 395, p. 541.)

† James P. Joule (himself an inventor of an electro-magnetic engine) in a letter dated May 28, 1839, said: "I can scarcely doubt that electro-magnetism will eventually be substituted for steam in propelling machinery." (Sturgeon's *Annals of Electricity*, vol. iv. p. 135.) This was some years before he commenced his investigations on the mechanical equivalent of heat and other motors. He subsequently estimated that the consumption of a grain of zinc though forty times more costly than a grain of coal, produces only about one-eighth of the same mechanical effect.

‡ This trait calls to mind Faraday's avowal made nearly thirty years later, when in a letter to Messrs. Smith and Bentley dated January 3, 1859, (declining the publication of his "Juvenile Lectures") he said: "In fact I have always loved science more than money; and because my occupation is almost entirely personal, I cannot afford to get rich." (Bence Jones' *Life of Faraday*, vol. ii. p. 423.)

and securing by its more tangible remunerations the leisure and the means for more extended researches, would not have been to science more than a compensation for the supposed sacrifice of dignity by the philosopher.*

Nor did this repugnance to patenting arise (as it sometimes does) from any theoretical disapproval of the system. On the contrary, he frequently expressed his strong conviction that a judicious code of patent laws—if faithfully administered—furnishes the most equitable method of recompensing meritorious inventors. The institution was a good one—for others.

The discovery of Magneto-electricity.—From the magnetizing influence of the galvanic current, physicists were almost inevitably led to expect the converse reaction; and this anticipation appears to have been coeval with electro-magnetism. As early as 1820, the illustrious Augustin Fresnel remarked: "It is natural to try whether a magnetic bar will not produce a galvanic current in a helical wire surrounding it;" and he made various experiments to determine a question which was supposed to involve the soundness of Ampère's theory. In November 1820, he announced that though he at first supposed his attempt at the magneto-electric decomposition of water was partially successful, he was finally satisfied that no decisive result was obtained.†

Five years later, Faraday attempted the same experimental inquiry; and among his earliest publications gave an account of his unsuccessful trials. After describing his arrangements he says: "The magnet was then put in various positions and to different extents into the helix, and the needle of the galvanometer noticed: no effect however upon it could be observed. The circuit was made very long, very short, of wires of different metals and different diameters, down to extreme fineness, but the results were always the same. Magnets more and less powerful were used. . . . Hence it appears that however powerful the action of an electric current may be upon a magnet, the latter has no tendency by re-action to diminish or increase the intensity of the former."‡

Nor were American physicists discouraged by the records of repeated failures: and when the great Henry magnet was received at Yale College, Professor C. U. Shepard (Chemical Assistant to Professor Silliman) at once attacked the problem with this new equipment. He remarks: "As its magnetic flow

* Several hundred patents have since been granted in this country for ingenious modifications of—or improvements upon the electro-magnetic telegraph; and probably a hundred for equally ingenious varieties of the electro-magnetic engine; all of which would have been tributary to Henry as an original patentee.

† *Annales de Chimie et de Physique*, 1820, vol. xv. pp. 219–222.

‡ *Quarterly Journal of Science*, July 1825, vol. xix. p. 338.

was so powerful, I had strong hopes of being able to accomplish the decomposition of water by its means. My experiment however proved unsuccessful. . . . I hope however to resume the research hereafter, under more favorable circumstances."*

Henry, unsatisfied with past efforts, determined to pursue the subject in an exhaustive series of experiments; and had reached some momentary indications of the galvanometer, when his experiments were temporarily interrupted. Meanwhile it was announced in May, 1832, that Faraday had secured the long sought prize; though the announcement was brief, and to those eager for particulars, somewhat disappointing. Henry was accordingly induced to publish in the following number of Silliman's Journal (that for July) a sketch of his own trials both before and after the announced discovery. With reference to Faraday's discovery he remarks: "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 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. But if the magnet be withdrawn from or introduced into such a helix, a current of electricity is produced *while 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.'†

"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

* Silliman's *Am. Jour. Sci.* April, 1831, vol. xx. p. 201, foot-note.

† *Philosoph. Mag. and Annals of Phil.* April, 1832, vol. xi. pp. 300, 301. Although Faraday's first communication on galvanic induction, and on magneto-electricity, was read before the Royal Society November 24, 1831, the published Transactions for 1832, containing this memoir did not reach this country till more than a year later: so that the meager abstract of the Royal Institution Proceedings above given, was the only notice of this important discovery,—here accessible for many months.

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 these 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 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. . . . 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." No increase of effect was however observable. On comparing the two methods separately, it was found that while the sudden introduction of the end of a magnetized bar within the helix connected with the galvanometer deflected the needle seven degrees, the sudden magnetization of the bar when within the helix, deflected the needle thirty degrees. A cylindrical iron bar was made to rotate rapidly on its axis within a stationary helix, by means of a turning lathe, but no result followed.

In the following month (June) by employing a horse-shoe armature (admitting longer coils), Henry succeeded in obtaining vivid sparks from the magnet. "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. . . . It appears from the May number 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."

Henry's gratification at the acquisition of the new insight into natural law, quite absorbed all sentiment of personal pride in its independent attainment; and his appreciation and congratulation of Faraday as the first discoverer of magneto-electricity, were hearty and unreserved. He was also particular always to assign to Faraday the first observation of the curious phenomena of momentary galvanic induction; although himself an independent discoverer of the fact.

In the course of these experiments he made a very important original observation on a peculiar case of self-induction, whereby he was enabled to convert a galvanic current of "quantity" into one of "intensity." This entirely new result seemed to contradict all previous experience. He thus concludes his 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 above described. It is this:—when a small battery is moderately excited by diluted acid and its poles

* *Philosoph. Mag. and Annals*, May, 1832, vol. xi. pp. 359, 360.

(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. . . . 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 reaction on itself projects a spark when the connection is broken."* This is the earliest notice of the curious phenomenon of self-induction in an electric discharge.

Election as Professor at Princeton.—The Trustees of the College of New Jersey at Princeton, were about this time in search of a Professor to fill the chair of Natural Philosophy in that College, made vacant by the resignation of Professor Henry Vethake, who had accepted a Professorship of Natural Philosophy in the recently established University of the City of New York. Professor Henry had already won considerable reputation as a lecturer and teacher, no less than as an experimental physicist. Professor Silliman of Yale College, urging his appointment, wrote: "Henry has no superior among the scientific men of the country." And Professor Renwick of Columbia College (New York) still more emphatically added: "He has no equal."

Professor Henry was unanimously elected by the Trustees;† and he accepted the appointment: although strongly attached to his first Academy, endeared to him by early memories, by six years of successful labors, and by the warm regard of all his associates. May it not be added that his residence at the capital of the State of New York, was further endeared to him by life's romance,—a most congenial and happy marriage contracted in 1830.

ELECTRICAL RESEARCHES AT PRINCETON: FROM 1833 TO 1842.

In November 1832, Henry left the scene of his early scientific triumphs, the Albany Academy, and removed to Princeton with his family. For a year or two he gave his whole attention and

* Silliman's *Am. Jour. Sci.*, July, 1832, vol. xxii. p. 408.

† Dr. Maclean connected with the Faculty of the College of New Jersey at Princeton for fifty years, and for fourteen years its venerable president, in his History of the College (2 vols. 8vo. Philadelphia, 1877,) gives a very interesting account of the appointment and election of Joseph Henry as Professor of Natural Philosophy in 1832, vol. ii. pp. 288–291.

exertions to the duties of exposition and instruction; and during Dr. Torrey's visit to Europe in 1833, at the Doctor's request Professor Henry filled *ad interim* his chair of Chemistry, Mineralogy, and Geology. These occupations left him no leisure for the pursuit of original research. He subsequently gave lectures on Astronomy, and also on Architecture.

In 1834, Henry constructed for the Laboratory of his College an original form of Galvanic Battery; so arranged as to bring into action any desired number of elements, from a single pair to eighty-eight. Each zinc plate 9 inches wide and 12 inches deep was surrounded by a copper case open at top and bottom, and giving thus one and a half square feet of efficient surface. Eleven of these in eleven separate cells, formed a sub-battery; and eight of these were grouped together by means of adjustable conductors, so as to form from the whole a single battery. By means of a crank and windlass shaft in proper connection, any one or more of the eight sub-batteries could be immersed or disengaged, and if desired, a single cell could alone be charged. By another arrangement of adjustable conductors, all the zinc plates could be directly connected together, and all the copper plates together, after the plan of Dr. Hare's "calorimotor" battery; thus giving the "quantity" effect due to a single element of 132 square feet of zinc surface, or of any smaller area desired. As the author remarks concerning its various arrangements, "they have been adopted in most cases after several experiments and much personal labor." A detailed account of this battery was given in a communication read January 16th 1835, before the American Philosophical Society (of which he had recently been elected a member), and was published in its Transactions.*

Meanwhile he had been engaged in his brief intervals of relaxation from his exacting professional cares during the past year, in repeating and extending his interesting observations (commenced at Albany in 1832), on the remarkable intensifying influence of a long conductor, and especially of a spiral one, when interposed in a galvanic circuit of a single pair, or a battery of low "intensity." A verbal communication on this curious form of "induction," was made to the Society on the same occasion as the description of his battery, and was illustrated by experiments exhibited before the Society.

Faraday in his "eighth series of Researches" (read before the Royal Society June 5th 1834), pointed out very fully the differing actions of a single galvanic element giving a "quantity" current, and of a series of elements giving an "intensity" current:† thus entirely confirming the results obtained by Henry more than three years previously.

* *Trans. Am. Philos. Soc.* vol. v. new series, art. iv. pp. 217–222.

† *Phil. Trans. Royal Soc.* June 5, 1834, vol. cxxiv. art. 990–994, pp. 445, 446. *Experimental Researches in Electricity*, vol. i. pp. 301, 302.

In the Philosophical Magazine for November, 1834, appeared a paper by Faraday, "On a peculiar condition of electric and magneto-electric Induction:" in which he notices as a remarkable fact, that while a short circuit wire from a single galvanic element, gives little or no visible spark, a long conductor gives a very sensible spark. "It is however very interesting thus to observe an original current of electricity having a very low intensity producing ultimately a counter current having an intensity probably a hundred fold greater than its own, and the experiment constitutes one of the very few modes we have at command of converting quantity into intensity as respects electricity in currents." And he remarks: "If the connecting wire be much lengthened, then the spark is much increased."*

In this interesting research, Faraday appears to have entirely overlooked Henry's earlier labors in the same field;—as contrary to his usual custom, he makes no allusion to the same results having been obtained, and published in Silliman's Journal two years and a half before.† These observations were made by Faraday the subject of his "ninth series of Researches," in a communication "On the influence by induction of an electric current on itself:" read before the Royal Society January 29th 1835. In this paper he repeats, that with a single galvanic pair, a short wire will not give a shock; while the wire surrounding an electro-magnet will give a shock at each breaking of the circuit. He found a similar result with the wire helix alone,—without its magnetic core. "The power of producing these phenomena exists therefore in the simple helix, as well as in the electro-magnet, although by no means in the same high degree." With continuous straight wire of the same length, he obtained a similar effect,—"yet not so bright as that from the helix." When a short wire is used, "all these effects disappear;" although there is undoubtedly a greater "quantity" of electric current in the shorter wire; thus giving "the strange result of a diminished spark and shock from the strong current, and increased effects from the weak one."‡

While Henry derived only satisfaction from these extended verifications of his own observations, by one whom he had accustomed himself to look up to with admiration and regard, Dr. A. Dallas Bache, his attached friend, then Professor of Natural Philosophy in the University of Pennsylvania,—more jealous than himself of his scientific fame, strongly urged and insisted that he should immediately publish an account of his later researches. Henry accordingly sent to the American Philosophical

* *Philosoph. Mag.* Nov. 1834, vol. v. pp. 351, 352.

† Sill. *Am. Jour. Sci.* July, 1832, vol. xxii. p. 408, above quoted.

‡ *Phil. Trans. Royal Soc.* Jan. 29, 1835, vol. cxxv. art. 1061–1067. and 1073, pp. 43–45. *Experimental Researches in Electricity*, vol. i. pp. 325–328. This memoir did not reach this country of course till a year later.

Society a memoir (comprising the details of his recent verbal communication) "On the Influence of a Spiral Conductor in increasing the Intensity of Electricity from a galvanic arrangement of a Single pair, etc.," which was read before the Society, February 6th, 1835.

After citing his former paper of July, 1832, the writer remarks that he had been able during the past year to extend his experiments on the curious phenomenon. "These though not so 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." He then relates that employing a single pair of his battery (comprising one and a half square feet of zinc surface), he found as in his earlier experiment in 1832, that the poles being connected by a piece of copper bell-wire five inches long, no spark was given on making or breaking contact. Fifteen feet of interposed wire gave a very feeble spark; and with successive additions of fifteen feet, the effect increased until with 120 feet the maximum spark appeared to be reached, and beyond this there was no perceptible increase; while with double this length (or 240 feet) there seemed to be a diminution of intensity. From various trials the inference was drawn that the length required for maximum effect varied with the size of the galvanic element. Thicker wires of the same length produced greater effect, depending in some degree on the size of the battery. A wire of forty feet when coiled into a cylindrical helix "gave a more intense spark than the same wire uncoiled." A ribbon of sheet copper about an inch wide and twenty-eight feet long, being covered with silk and coiled into a flat spiral—like a watch spring—(after the plan of Dr. Richie) gave a vivid spark with a loud snap. When uncoiled, it produced a much feebler spark. With the insulated copper ribbon folded in its middle, and the double thickness coiled into a flat spiral, there was no spark whatever, although the same ribbon unrolled gave a feeble spark: thus showing that the induction of the current upon itself was neutralized by flowing equally in opposite directions in the double spiral. With a larger copper ribbon one inch and a half wide, and 96 feet long, spirally coiled (weighing 15 pounds) the snap of the spark could be heard in an adjoining room with the door closed. Want of material prevented the result being pushed further, so as to ascertain the range of maximum effect with this form of conductor. With increased battery surface, the effect was also increased; so that with eight elements of his battery arranged as a single pair (of 12 square feet) the spark on breaking contact "resembled the discharge of a small Leyden jar highly charged." With the flat spiral, no increase of effect was observable on the introduction of a soft iron core into the axis of the spiral, forming a magnet. With a helical or cylindrical coil about nine inches long, enclosing an iron core, "the spark appeared a little more intense than without the iron." The inference is also drawn "from these experiments, that some of the effects heretofore attributed to magneto-electric action are chiefly due to the reaction on each other of the several spirals of the coil which surround the magnet."

In these researches it was found that when the two plates of a single pair were placed even fourteen inches apart in an open trough of 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." It was also shown that "the spiral conductor produces however, little or no increase of effect when introduced into a galvanic circuit of considerable intensity." When for example an "intensity" battery of two Cruickshank's troughs, each containing 56 elements was employed with the larger copper spiral, "no greater effect was perceived than with a short thick wire:" in either case, only a feeble spark being given.* An abstract of the results thus announced (and which were obtained by Henry during the summer of 1834,) was communicated by Dr. A. D. Bache, as a Secretary of the American Philosophical Society, to the Franklin Journal, in order to give these interesting facts an earlier currency.† The date of original discovery was however so well established, that this friendly effort was scarcely necessary.‡

Combined Circuits.—In 1835, or early in 1836, wires had been extended across the front campus of the College grounds at Princeton from the upper story of the library building to the Philosophical Hall on the opposite side, through which signals were occasionally sent, distinguished by the number of taps of the electro-magnetic bell, first exhibited five years previously in the hall of the Albany Academy. It has already been noticed, that contrary to all the antecedent expectations of physicists, Henry had established the fact that the most powerful form of magnet (designated by him the "quantity" magnet) is not the form best adapted to distant action through an extended circuit. The ingenious idea occurred to him that notwithstanding this fundamental fact, it would be quite easy to combine the two systems so as to enable an operator to produce the most energetic mechanical effects, at almost any required distance.

* *Trans. Am. Phil. Soc.* vol. v. n. s. art. x. pp. 223–231.

† *Journal of the Franklin Institute*, March, 1835, vol. xv. pp. 169, 170.

‡ M. Becquerel in his elaborate Treatise on Electricity, in the chapter on "The influence of an electric current on itself by induction," says with regard to the increase of tension in a feeble current when passing through a long spiral conductor, "The effects observed in these circumstances appear to have been noticed for the first time by Professor Henry." (*Traité experimental de l'Électricité et du Magnétisme*, 8vo. 7 vols. Paris, 1824–1840, vol. v. art. 1261, p. 231.)

It is simply necessary to employ with the distant "intensity" magnet an oscillating armature with a suitable prolongation so arranged as to open and close the short circuit of an adjoining "quantity" magnet of any practicable power:—a work which indeed could be accomplished by the mere swing of the most delicate galvanometer needle. Professor Henry had constructed for his own laboratory a large electro-magnet designed to surpass the celebrated magnet made for Yale College; and with it he was enabled to exhibit to his class, by employing a small portion of his "quantity" battery, an easy lifting power of more than three thousand pounds.* Such was the mechanical agency he called into action through his telegraphic circuit, by simply lifting its galvanic wire from a mercury thimble, or by again dipping it into the same. Although this special combination has not found any important application, its principle underlies all the various forms and uses of the "relay" magnet and local battery since employed.

Visit to Europe.—In order to give Professor Henry a much-needed rest from his diligent services and close application of the past four years, the Trustees of his College liberally allowed him a year's absence with full salary: thus affording him for the first time a long coveted opportunity of visiting Europe.

In February of 1837, in company with his valued and faithful friend, Professor Bache, he arrived in England; where the two American physicists formed ready and lasting intimacies with some of the most distinguished worthies of Great Britain. Everywhere received with courteous and cordial consideration, they both ever carried with them agreeable memories of their holiday sojourn abroad.

In London, many pleasant interviews with Faraday, formed a memorable circumstance. Wheatstone, then Professor of Experimental Philosophy in King's College, was engaged in developing his system of needle telegraph: and Henry had the satisfaction of finding that his own early investigations were recognized and appreciated, and their results successfully adopted. Wheatstone unfolded freely to his visitors his numerous projects; and particularly his arrangement of supplementary local circuit from an additional battery, for sounding an electro-magnetic signal, by being brought into action by a movement from the main line circuit.† Henry had then the pleasure of detailing to him his own similar combination of two electro-

* It is said that this magnet has been made to sustain 3500 pounds.

† This was early in April, 1837. (*Smithsonian Report* for 1857, p. 111.) Two months later, or June 12th, 1837, Wheatstone in conjunction with W. F. Cooke had secured a patent on his system of telegraph, including the combination of circuits.

magnetic circuits, experimentally tried more than a year previously.

Nearly a year was employed in foreign travel, most pleasantly and beneficially both for mind and body: the greater portion of the time however being spent in London, in Paris, (where Henry formed the acquaintance of Arago, Becquerel, De la Rive, Biot, Gay-Lussac, and other celebrities,) and in Edinburgh, where he also found a galaxy of eminent and congenial minds.

In September of the same year (1837) he attended the meeting of the British Association at Liverpool; where being invited to speak, he made a brief communication on some electrical researches in regard to the phenomenon known as the "lateral discharge;"—a study to which he had been led by some remarks of Dr. Roget on the subject. "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 side or the other of a jar, and not the whole discharge." Hence we could increase or diminish the lateral action by any means which affect the quantity of free electricity:—as by "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:"—forming a continuous electrical brush. It was also stated "that the same quantity of electricity could be made to remain on the wire, if gradually communicated [by a point]; but when thrown on in the form of a spark, it is dissipated as before described:"—as though possessing a kind of momentum. When two or more wires are arranged in parallel lines (in electrical connection), only the outer sides of the exposed wires become luminous: and "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." In like manner when a ball was attached to the middle of a vertical lightning-rod having a good earth-connection, "when sparks of about an inch and a half 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." *

At the same meeting, before the section on Mechanics and

* *Report of Brit. Association*, for 1837, pp. 22–24, of Abstracts.

Engineering, Henry gave by request an account of the great extension of the Railway and Canal systems in the United States: which was listened to with great attention and interest. He also referred to the inland or river navigation in our country, describing the improvements introduced into our large river steam-boats, especially on the Hudson river in New York State; where the usual speed was fifteen miles per hour or more.*

In November, 1837, Henry returned from his foreign tour greatly invigorated,—bringing with him some new apparatus: and with increased zest he re-embarked upon the duties of his professorship. Continuing his studies of electrical action, he presented verbally to the American Philosophical Society, February 16th 1838, a notice of further observations on the "lateral discharge" of electricity while passing along a wire. going to show that even with good earth connection, free electricity is not conducted silently to the ground.†

In May, 1838, he announced to the Society the production of currents by induction from ordinary or mechanical electricity, analogous to that first obtained by Faraday from galvanism in 1831: and the further curious fact that on the discharge from a Leyden jar through a good conductor, a secondary shock from a perfectly insulated near conductor could be obtained more intense than the primary shock directly from the jar.‡

These investigations having in view the discovery of "inductive actions in common electricity analogous to those found in galvanism" (commenced in the Spring of 1836), led to renewed examination of the secondary *galvanic* current, which since November 24th, 1831, (or for seven years,) had received no special attention. Henry's very interesting series of experiments were detailed in a somewhat elaborate memoir read before the American Philosophical Society, November 2nd, 1838. Employing five different sized annular spools of fine wire (about one-fiftieth of an inch thick) varying from one-fifth of a mile to nearly a mile in length (which might be called "intensity" helices); and six flat spiral coils of copper ribbon varying from three-quarters of an inch to one inch and a half in width, and from 60 to 93 feet in length (which might be called "quantity" coils), he was able to combine them in various ways both in connection and in parallelism. A cylindrical battery of one and three quarters square

* Same *Report*, Abstracts, p. 135. It was on this occasion that Dr. Lardner, generalizing probably from his observations on the Thames, ventured (not very courteously) to doubt whether any such speed as fifteen miles per hour on water, could ordinarily be effected. (Sill. *Am. Jour. Sci.* Jan. 1838, vol. xxxii. p. 296.) The same authority affirmed the futility of attempting oceanic steam navigation.

† *Proceedings Am. Phil. Soc.* Feb. 16, 1838, vol. i. p. 6.

‡ *Proceedings Am. Phil. Soc.* May 4, 1838, vol. i. p. 14.

feet of zinc surface was principally used; and the galvanic circuit was interrupted by drawing one end of the copper ribbon or wire over a rasp in good metallic contact with the other pole of the battery.

From the energetic action of the flat ribbon coil in producing the induction of a current on itself, it was inferred that the secondary current would also be best induced by it. With the single larger ribbon coil in connection with the battery, and another ribbon coil placed over it resting on an interposed glass plate, at every interruption of the primary circuit, an induction spark was obtained at the rubbed ends of the second coil; though the shock was feeble. With a double wire spool (one within the other) of 2650 yards, placed above the primary coil (having about the same weight as the copper ribbon) the magnetizing effects disappeared, the sparks were much smaller, "but the shock was almost too intense to be received with impunity." The secondary current in this case was one of small "quantity" but of great "intensity." With a single break of circuit in the primary, it was passed through a circle of 56 students of his senior class, with the effect of a moderate charge from a Leyden jar. From various experiments, the limit of efficient length for a given galvanic power was ascertained; beyond which the induced current was diminished. Employing a Cruickshank battery of 60 small elements (4 inches square) he found with the ribbon coil that the induced currents were exceedingly feeble, but with the long wire helix as the primary circuit that strong indications were produced. By the alternations of the ribbon and wire coils, the fact was established "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;" a result which has had an important bearing on the subsequent development of the electro-magnetic "Induction-Coil." With a long ribbon coil receiving the galvanic current from 35 feet of zinc surface, sensible induction shocks could be felt from a large annular coil of four feet diameter (containing five miles of wire) when placed in parallelism at a distance of four feet from the primary coil: while at the distance of one foot the shock became too severe to be taken. With this arrangement an induction shock was given from one apartment to another, through the intervening partition.

Successive orders of Induction.—When it is considered that the primary current in such cases has a considerable duration, while the secondary current is but momentary, being developed only at the instant of change in the primary, it could certainly not have been expected that this single instantaneous electrical impulse of reaction would be capable of acting as a primary current, and of similarly inducing an action on a third independent circuit: and during the seven years in which galvanic induction had been known, no physicist ever thought of making the trial. Theoretically it might perhaps have been inferred, if such tertiary induction had any existence, as it would be coincident not with the instantaneous secondary induction, but with the initiation and termination of such momentary current, and hence in opposite signs—separated by an inappreciable interval of time, that the whole phenomenon would probably be entirely masked by a practical neutralization. The experiments of Henry fully established, however, the new and remarkable result of a very appreciable tertiary current. By connecting the secondary coil with another at some distance from the primary so as not to be influenced by it directly, but forming with the secondary a single closed circuit, not only was the distant coil capable of producing in an insulated wire helix placed over it, a distinct current of induction at the interruption of the primary, but sensible shocks were obtained from it. The experiment was pushed still further; and inductive currents of a fourth degree were obtained. "By a similar but more extended arrangement, shocks were received from currents of a fourth, and a fifth order: and with a more powerful primary current, and additional coils, a still greater number of successive inductions might be obtained. . . . 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." As Henry simply remarks: "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." By means of the small magnetizing helix introduced into each circuit, the direction of these successive currents was found to be alternating or reversed to each other.

The concluding section of this important memoir is occupied with an account of "The production of induced currents of the different orders from ordinary electricity." An open glass cylinder about six inches in diameter was provided with two long narrow strips of tin foil pasted around it in corresponding helical courses, the one on the outside and the other on the inside, directly opposite to each other. The inner coiled strip had its extremities connected with insulated wires which formed a circuit outside the cylinder, and included a small magnetizing helix. The outer tin foil strip was also connected with wires so that an electrical discharge from a half-gallon Leyden jar could be passed through it. The magnetization of a small needle indicated an induced current through the inner tin-foil ribbon corresponding in direction with the outer current from the jar.* By means of

* About a year later, the distinguished German electrician Peter Riess, apparently unaware of Henry's researches, discovered the secondary cur-

a second glass cylinder similarly provided with helical tin-foil ribbons in suitable connections a tertiary current of induction was obtained, analogous to that derived from galvanism. "Also by the addition in the same way of a third cylinder, a current of the fourth order was developed."

Similar as these successive inductions from an electrical discharge were to those previously observed in the case of the galvanic current, they presented one puzzling difference in the directions of the currents of the different orders. "These in the experiments with the glass cylinders, instead of exhibiting the alternations of the galvanic currents, were all in the same direction as the discharge from the jar, or in other words they were all *plus*." On substituting for the tinned glass cylinders, well insulated copper coils, "alternations were found the same as in the case of galvanism." The only difference apparently between the two arrangements, was that the tin-foil ribbons were separated only by the thin glass of the cylinders, while the copper spiral coils were placed an inch and a half apart. By varied experiments, the direction of the induced currents was found to depend notably on the distance between the conductors;—the induction ceasing at a certain distance, (according to the amount of the charge and the characters of the conductors,) and the direction of the induced current beyond this critical distance being contrary to that of the primary current.* "With a battery of eight half-gallon jars, and parallel wires 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." With Dr. Hare's battery of 32 gallon jars, and a copper wire about one-tenth of an inch thick and 80 feet long stretched across the lecture-room and back on either side toward the battery, a second wire stretched

rent induced from mechanical electricity, by a very similar experiment. (Poggendorff's *Annalen der Physik und Chemie*, 1839, No. 5, vol. xlvii. pp. 55–76.)

* The variation in the direction of polarization (without reference to induction currents) appears to have been first noticed by F. Savary, some dozen years before. In an important memoir communicated to the Paris Academy of Sciences July 31, 1826, M. Savary announced that "The direction of the magnetic polarity of small needles exposed to an electric current directed along a wire stretched longitudinally, varies with the distance of the wire:"—the action being found to be periodical with the distance. M. Savary observed three periods, and also the fact that the distances of maximum effect and of the nodal zeros "vary with the length and diameter of the wire, and with the intensity of the discharge." He also found that "when a helix is used for magnetizing, the distance at which the needle placed within it is from the conducting wire, is indifferent; but the direction and the degree of magnetization depends on the intensity of the discharge, and on the ratio between the length and size of the wire." (Brewster's *Edinburgh Jour. Sci.* Oct. 1826, vol. v. p. 369.)

parallel with the former for about 35 feet and extended to form an independent circuit (its ends being connected with a small magnetizing helix,) was tested at varying distances beginning with a few inches until they were twelve feet apart: at which distance of the parallel wire, its induction though enfeebled, still indicated by its magnetizing power, a direction corresponding with the primary current. The form of the room did not permit a convenient separation of the two circuits to a greater distance.*

The eminent French electrician Becquerel, in a chapter on Induction in his large work, remarks: "Quite recently M. Henry, Professor of Natural Philosophy in New Jersey, has extended the domain of this branch of physics: the results obtained by him are of such importance, particularly in regard to the intensity of the effects produced, that it is proper to expound them here with some detail." Twenty pages are then devoted to these researches.†

A memoir was read before the Society, June 19th, 1840, giving an account of observations on the two forms of induction occurring on the making and on the breaking of the primary galvanic circuit, the two differing in character as well as in direction. In these experiments he employed a Daniell's constant battery of 30 elements; the battery being "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." As the initial induction had always been found so feeble as to be scarcely perceptible, (although in quantity sufficient to affect the ordinary galvanometer as much as the terminal induction,) most of the results previously obtained (such as the detection of successive orders of currents) were derived from the strong inductions at the moment of breaking the circuit. It became therefore important to endeavor to intensify the initial induction for its more especial examination: and this it was found could be effected in two ways,—by increasing the "intensity" of the battery, and by diminishing within certain limits the length of the primary coil.

"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

* *Trans. Am. Phil. Soc.* vol. vi. new series, art. ix. pp. 303–337. In the Proceedings of the Society for November 2d, 1838, when this memoir was read, it is recorded "Professor Henry made a verbal communication during the course of which he illustrated experimentally the phenomena developed in his paper." (*Proceed. Am. Phil. Soc.* Nov. 2, 1838, vol. i. p. 54.)

† *Traité experimental de l'Électricité et du Magnétisme*, vol. v. pp. 87–107.

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 greater than the latter, the difference continually increasing until all the thirty elements were introduced into the circuit. . . . 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 "was employed; and the length of the copper ribbon coil was successively reduced from 60 feet, by measures of 15 feet. With 45 feet, the initial induction was stronger than with 60 feet: 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." At the same time it was found that "the intensity of the shock at the *ending* of the battery current diminishes with each diminution of the length of the coil. . . . 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 its intensity remains the same."

Having thus succeeded in exalting the initial induction, Henry proceeded in his investigation. Distinct currents of the third, fourth, and fifth orders were readily obtained from it; and as was anticipated, with their signs (or directions) the reverse of the corresponding orders derived from the terminal induction. In other respects "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."

In the course of these investigations the idea having occurred to him "that the intense shocks given by the electrical fish may possibly be from a secondary current," as it appeared to him that "this is the only way in which we can conceive of such intense electricity being produced in organs imperfectly insulated and immersed in a conducting medium," he endeavored to simulate the effect by arranging a secondary wire coil furnished with terminal handles, over a primary copper ribbon coil, the two being insulated as usual. "By immersing the apparatus 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."

The former experiment of obtaining an induction shock from one room to another through a partition, was repeated on a still larger scale. All the coils of copper ribbon having been united in a single continuous conductor of about 400 feet in length, "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 147 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."

The second section of the memoir is mainly occupied with details of experiments on the screening effect of conducting plates (of non-magnetic metals) when interposed between the primary and secondary coils: showing remarkable contrasts in the "quantity" and "intensity" classes of galvanic effects. When the annular spool or helix (of nearly one mile of copper wire) was employed with the large spiral coil of copper ribbon, "the coil 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 the 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. A similar effect was observed when the galvanometer and the magnetizing helix were together introduced into the circuit. The interposition of the plate entirely neutralized the magnetizing power of the helix (in reference to tempered steel) while the deflections of the galvanometer were unaffected." The induction currents of the third, fourth, and fifth orders, were found to be of considerable "intensity;"—magnetizing steel needles, giving shocks, not being interrupted by a drop of water placed in the circuit between the ends of the severed wire,—and yet being screened or neutralized by a metallic plate interposed between the coils.*

* *Trans. Am. Phil. Soc.* June 1840, vol. viii. n. s. art. i. pp. 1–18.

A continuation of the memoir was read before the Philosophical Society November 20th, 1840, discussing further the theoretical differences between an initial or an increasing galvanic current, and a decreasing or a terminal current, in producing the phenomena of induction. On the same occasion 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." About ten years before, he had devised the first electro-magnetic engine (operating by intermittent magnetic attractions and repulsions); and now he had contrived the first galvanic engine, operating by the analogous intermittent attractions and repulsions of the electric current.*

In June 1842, he presented a communication to the Society recounting an investigation of some anomalies in ordinary electrical induction. While with the larger needles ("No. 3 and No. 4") subjected to the magnetizing helix, the polarity was always conformable to the direction of the discharge, he found that when very fine needles were employed, an increase in the force of the electricity produced changes of polarity. About a thousand needles were magnetized in the testing helices in these researches. This puzzling phenomenon was finally cleared up by the important discovery that an electrical equilibrium was not instantaneously effected by the spark, but that it was attained only after several oscillations of the flow. "The discharge, whatever may be its nature is not correctly represented by the single transfer 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."† In every case therefore of the electrostatic discharge, the testing needles were really subjected to an oscillating alternation of currents, and consequently to successive partial de-magnetizations and re-magnetizations. The complications produced by this residual action, satisfactorily explained for the first time, the discordant results obtained by different investigators. This singular reflux of current was ingeniously applied by Henry to explain the apparent change of inductive current with differing distances. Should the primitive discharge wave be in excess of the magnetic

* *Proceedings Am. Phil. Soc.* Nov. 20, 1840, vol. i. p. 301.

† *Proceedings Am. Phil. Soc.* June 17, 1842, vol. ii. pp. 193.—Helmholtz some five years later (in 1847), but quite independently, suggested "a backward and forward motion between the coatings" when the Leyden jar is discharged. And still five years later (in 1852) Sir William Thomson made the same independent conjecture. To F. Savary however is due the credit of having first thrown out the hypothesis of electrical oscillations, as early as in 1827.

capacity of the needle at a given position, the return wave might be just sufficient to completely reverse its polarity, and the diminished succeeding wave insufficient to restore it to its former condition; while at a greater distance, the primitive wave might be so far reduced as to just magnetize the needle fully, and the second wave, being still more enfeebled, would only partially demagnetize it, leaving still a portion of the original polarity; and so for the following diminished oscillations.

In the course of these extended researches the presence of inductive action was traced to most surprising and unimagined distances. "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 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." For obtaining the results above alluded to, a thick wire was soldered to the edge of the tin roof of his dwelling and passed into his study through a hole in the window frame; while a similar wire passing out to the ground, terminated in connection with a metal plate in a deep well close by. Between the wire ends within his study, various apparatus, including magnetizing helices of different sizes and characters could be attached, so as to be within the line of conduction from the roof to the ground. The inductions from atmospheric discharges were found to have the oscillatory character observed with the Leyden jar; and by interposing several magnetizing helices with few and with many convolutions, he was able to get from a needle in the former the polarity due to the direct current, and in the latter, that due to the return current; thus catching the lightning (as it were) upon the rebound.

In examining the "lateral discharge" from a lightning-rod in good connection with the earth, he had often observed that while a spark could be obtained sufficiently strong to be distinctly felt, it scarcely affected in the slightest degree a delicate gold-leaf electroscope. How explain so incongruous a phenomenon? Henry detected the very simple solution, by a reference to the self-induction of the rod,—a negative wave passing followed immediately by a positive wave so rapidly as to completely neutralize the effect upon the electroscope before the inertia of the gold leaf could be overcome, while actually producing a double spark (sensibly co-incident) to and from the recipient.

A few months later, "he had succeeded in magnetizing needles by the secondary current, in a wire more than two hundred and twenty feet distant from the wire through which the primary current was passing, excited by a single spark from an electrical machine."* In this case the primary wire was his telegraph line stretched seven years before across the campus of the College grounds in front of Nassau Hall: the secondary or induction wire being suspended in a parallel direction across the grounds at the rear of Nassau Hall, with its ends terminating in buried metallic plates:—the large building intervening between the two wires.

This brilliant series of contributions to our knowledge of a most recondite and mysterious agent, placed Henry, by the concurrent judgment of all competent physicists, in the very front rank of original investigators. His persevering researches in the electrical paradoxes of induction, perhaps more than any similar ones, tended to strengthen the hypothesis of an ætherial dynamic agency; although he himself had for a long time been inclined to favor the material hypothesis.†

INVESTIGATIONS IN GENERAL PHYSICS: FROM 1830 TO 1846.

In order to give a proper connection to the experimental inquiries undertaken by Henry in various fields, it is necessary to pause here, and to recur to some of his earlier scientific labors.

Meteorology.—From an early date Henry took a deep interest in the study of meteorology: not only on account of its practical importance, but from its relation to cosmical physics, and because from the very complexity and irregularity of its conditions, it challenged further investigation and stood in need of larger generalizations. His early association with Dr. T. Romeyn Beck in the first development of the system of meteorological observations established in the State of New York, has already been referred to in the sketch of his "Early Career." This active and zealous co-operation continued from 1827 to 1832; or as long as he resided in Albany.

In September of 1830, he commenced a series of observations for Professor Renwick of Columbia College, to determine the magnetic intensity at Albany. With the assistance of his brother-in-law, Professor Stephen Alexander, these observations

* *Proceedings Am. Phil. Soc.* Oct. 21, 1842, vol. ii. p. 229.

† In a paper "On the Theory of the so-called Imponderables" published some years later, in referring to the phenomena of electrical oscillation in discharge, and of the series of inductions taking place and "extending to a surprising distance on all sides," he remarks: "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." (*Proceed. Amer. Association*, Albany, Aug. 1851, p. 89.)

were continued daily for two months.* In April, 1831, a second series of observations was commenced; in the course of which his attention was attracted by a great disturbance of the needle during the time of a conspicuous "aurora" on the 19th of April, 1831. At noon of the 19th the oscillations were found to be perfectly accordant with previous ones, but at 6 o'clock P. M. a remarkable increase of magnetic intensity was indicated. At 10 o'clock of the same evening, during the most active manifestation of the aurora, the oscillations of the needle were again examined. "Instead of still indicating as at 6 o'clock an uncommonly high degree of magnetic intensity, it now showed an intensity considerably lower than usual." Thus, designating the normal intensity at the place as unity, at 6 o'clock it had increased to 1.024, and at 10 o'clock had subsided to 0.993, which according to Hansteen's observations is the usual relation of magnetic disturbance by an aurora.† An account of these results was communicated by Henry to the Albany Institute, January 26, 1832; and was also published in the Report of the Regents of the New York University. A little more than a month later (to wit on March 6, 1832,) he had been able to collate the various published accounts of this aurora; and he learned "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." This circumstance led him to make a careful comparison of the notices of auroral displays given in the meteorological reports in the Annals of Philosophy for 1830 and 1831, with those of the Reports of the New York Regents for the same period. "By inspecting these two publications it was seen that from April, 1830, to April, 1831,

* The needles employed in these observations were a couple received by Professor Renwick from Capt. Sabine,—one of which had belonged to Prof. Hansteen of Norway. "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. 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:" the individual observations not differing from the mean number, ordinarily more than one-thousandth. (Silliman's *Am. Jour. Sci.* April, 1832, vol. xxii. p. 145.)

† Prof. Hansteen has remarked that "A short time before the aurora borealis appears, the intensity of the magnetism of the earth is apt to rise to an uncommon height; but so soon as the aurora borealis 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." (*Edinburgh Philosoph. Jour.* Jan. 1825, vol. xii. p. 91.)

inclusive, the aurora was remarkably freqnent 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." From which he argues that "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 action of this cause (whatever it may be) affects simultaneously a greater portion of the northern hemisphere." *

In attempting to classify and digest the meteorological data within his reach, Henry became strongly impressed with the necessity of much more extensive, continuous, and systematic observations than any as yet undertaken: and he omitted no opportunities of directing influence upon the minds of our national legislators, to impress them with the great need—as well as the practical policy of prosecuting the subject by governmental resources. No one at that day seemed so fully awake both to the importance and to the methods of prosecuting such inquiry: and no one more effectually advanced both by direct and by indirect exertions the wide-spread interest in this study, than he.

In 1839, while at Princeton, he induced the American Philosophical Society officially to memorialize the National Government to establish stations for magnetic and meteorological observations: a movement which was partly successful, though not to the extent desired. On the subject of international systems of observation and register, he justly remarks at a later date: "In order 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 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. . . . A general plan of this kind, for observing the meteorological and magnetical changes, more extensively than had ever before been projected, was digested by the British Association

* Sill. *Am. Jour. Sci.* April 1832, vol. xxii pp. 150–155.

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." *

A large collection of original notes of various meteorological observations,—on magnetic variations, on auroras with attempts at ascertaining their extreme height, on violent whirl-winds, on hail-stones, on thunder-storms, and the deportment of lightning-rods,—unfortunately never published nor transcribed were lost (with much other precious scientific material) by a fire in 1865. The phenomena of thunder-storms were always studied by Henry with great interest and attention. A very severe one which visited Princeton on the evening of July 14th 1841, was minutely described in a communication to the American Philosophical Society, November 5th, 1841.†

On November 3d, 1843, he made a communication to the Society "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 thunder-storm, he had found that the cloud was colder than the adjacent blue space."‡

On June 20, 1845, he read a paper before the Society on "a simple method of protecting from lightning, buildings covered with metallic roofs;" urging the importance in such cases of having the vertical rain pipes always in good electrical connection with the earth, since "on the principle of electrical induction, houses thus covered are evidently more liable to be struck than those furnished either with shingle or tile." It is of course necessary to have the metallic roof in good metallic connection with the gutters and pipes; and the latter may conveniently have soldered to the lower end a ribbon of sheet copper two or three inches wide, continuing into the ground surrounded with charcoal and extending out from the house till it terminates in moist ground.§

In this paper he incidentally meets the much debated question whether a lightning-rod is efficient as a conductor by its solidity,

* *Agricultural Report* of Com. of Patents, for 1855, p. 367.

† *Proceed. Am. Phil. Soc.* vol. ii. pp. 111–116.

‡ *Proceed. Am. Phil. Soc.* vol. iv. p. 22.

§ *Proceed. Am. Phil. Soc.* vol. iv. p. 179. Henry appears to have been much impressed with the conducting value of the tinned sheet-iron pipes commonly used as rain spouts, from observing that amid the strange vagaries of the circuitous path pursued by the lightning (in cases of houses struck by this destructive agent), the rain pipe was not unfrequently selected as part of the route;—marks of explosive violence being exhibited at its lower end, and sometimes at its top as well.

or by its surface only. While he had been able to magnetize small needles placed transversely to the edges of broad strips of copper, through which electrical discharges were passed, he could obtain no signs of magnetism in needles when placed transversely near the sides of such strips about mid-way from the edges. In like manner he failed to discover any action in a small magnetizing helix placed within a section of gas-pipe and connected with it at either end, when transmitting through the system an electrical spark; while he easily obtained magnetic effects with a galvanic current passed through the same arrangement.* From these and other experiments he was led to believe that mechanical electricity tends to pass mainly along the exterior surface of a conductor, and accordingly that Ohm's law of conduction is not applicable to mechanical electricity.†

Some popular uneasiness having been excited in 1846, in consequence of telegraph poles being occasionally struck by lightning, and of the supposed danger to travellers along highways likely to result therefrom, a communication on the subject addressed to Dr. Patterson, one of the Vice-Presidents of the American Philosophical Society, was read before the Society, and referred to Professor Henry for report. This was in the very infancy of the electro-magnetic telegraph; as it had not then been in existence more than a couple of years. Henry responded in a communication read June 19th, 1846, to the effect that while telegraph wires as long conductors were eminently liable to receive discharges of atmospheric electricity both from charged clouds and from the varying electrical condition of the air at distant points along the line (as for example even by a fog or precipitation of vapor at one station) as also from induction at a distance, the danger to travellers along a telegraph road would be very slight, unless a person should be standing or passing quite close to a pole at the moment of its being struck. He however recommended that for the protection of the poles, they should be provided with conductors. "The effects of powerful discharges from the clouds may be prevented in a great degree by erecting at intervals along the line and beside 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

* In passing a galvanic current through an iron tube, he obtained the evidence of an induction from both the inside and the outside of the tube, but in opposite directions.

† This very important question cannot be regarded as even yet decisively settled:—eminent authorities maintaining that electricity *in flow* —of whatever origin—observes equally the ratio of proportionality to area of cross section in the conductor. Probably the law of conductivity varies with circumstances.

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 dischage falling on the wire near the station might send a current into the house of sufficient quantity to produce serious accidents."* This precaution has now been largely adopted, especially on the telegraph lines of the central portion of the United States, which are more liable to the effects of lightning.†

Molecular Physics.—Among other inquiries many original examinations were made by Henry in the domain of molecular physics. While Professor in the College of New Jersey in 1839, his attention was attracted to a curious case of metallic capillarity. A small lead tube about eight inches long happening to be left with a bent end lying in a shallow dish of mercury, he noticed a few days afterward that the mercury had disappeared from the dish, and was spread on the shelf about the other end of the tube. On a careful examination of the tube by incision, it appeared that the mercury had not passed along the open canal of the tube, but had percolated through its solid substance. To test this, a solid rod of lead about one-fourth of an inch thick and seven inches long was bent into a siphon form, and the shorter end immersed in a small shallow vessel of mercury; a similar empty vessel being placed under the longer end. In the course of 24 hours a globule of mercury was found at the lower end of the lead rod; and in five or six days it had all passed over excepting what appeared in the form of crystals of a lead amalgam in the upper vessel.‡ A long piece of thick lead wire was afterward suspended in a vertical position, with its lower end dipping into a cup of mercury. In the course of a few days, traces of the mercury were found in the rod at the height of three feet above the cup: thus showing that a metal impervious to water or oil (excepting under very great pressure) was easily penetrated to great distances by a liquid metal.

Some years later on a visit to Philadelphia he endeavored with the assistance of his friend Dr. Patterson (then Director of the United States Mint), by melting a small globule of gold on a plate of clean sheet-iron, to obtain its capillary absorption; but without effect; probably owing to the interposition of a thin film of oxide. Applying to another personal friend, Mr. Cornelius of Philadelphia, a very intelligent and ingenious manufacturer of bronzes, and plated ornaments for chandeliers, etc. to try

* *Proceed. Am. Phil. Soc.*, vol. iv. p. 266.

† Prescott. *Electricity and the Electric Telegraph*, 8vo. N. York, 1877, chap. xxiii. pp. 296. and 411.

‡ *Proceed. Am. Phil. Soc.*, vol. i. p. 82.

whether a piece of silver-plated copper heated to the melting point of silver would show any absorption of that metal, he learned that it was a common experience under such circumstances to find the silver disappear; but that this had always been attributed to a volatilization of the silver, or in the workman's phrase,—to its being "burnt off." At Henry's request the experiment was tried: the heated end of a silver-plated piece of copper exhibited on cooling and cleaning, a copper surface, the other end remaining unchanged. Henry next had the copper surface slightly dissolved off by immersion for a few minutes in a solution of muriate of zinc, when as he had anticipated, the silver was again exposed, having penetrated to but a very short and tolerably uniform distance below the original surface.*

In 1844, he made some important observations on the cohesion of liquids. Notwithstanding that Dr. Young early in the century maintained that "the immediate cause of solidity as distinguished from liquidity is the *lateral adhesion* of the particles to each other," and had shown that "the resistance of ice to extension or compression is found by experiment to differ very little from that of water contained in a vessel,"† all the most popular text-books on physics continued to teach that the cohesion of the liquid state is intermediate between that of the solid and the gaseous states.‡ It seemed therefore desirable to test the question by some more direct means than the resistance of liquids contained in closed vessels; and for this purpose Henry employed the classical soap-bubble. "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." In fact the actual tenacity of pure water is greater than that of soap-water.

The first set of experiments was directed to determine "the quantity of water which adhered to a bubble just before it burst." The second set of experiments was devised to measure the contractile force of a soap-bubble blown on the wider end of a U shaped glass tube half filled with water, by the barometric column sustained in the narrower stem of the tube; the difference of level being carefully observed by means of a microscope. The thickness of the soap-bubble film at its top was estimated by the

* *Proceed. Am. Phil. Soc.* June 20, 1845, vol. iv. p. 177.

† Young's *Lectures on Nat. Philos.* Lect. 50, vol. i. p. 627.

‡ "If we attempt to draw up from the surface of water a circular disk 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 experiment 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." (*Agricultural Report* for 1857, p. 427.—Paper on Meteorology.)

last of the Newton rings shown previous to bursting. The result arrived at from both sets of experiments was that water instead of having a cohesion of 53 grains to the square inch (as was very commonly stated), has a cohesive force of several hundred pounds to the inch; or that the intermolecular cohesion of a liquid is fully equal to that of the substance in the solid state.*

In 1846, he presented to the Philosophical Society an epitome of his views on the molecular constitution of matter; giving the reasons for accepting the atomic hypothesis of Newton. He pointed out that the discovery and establishment 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:" and he urged that the hope of further advancement lies in the assumption "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." He therefore felt "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."

"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 aeriform, the liquid, and the solid."† In referring to this postulated *fourfold state of matter*, Henry was accustomed to point out the remarkable analogy between this conception, and that of the four elements of the ancients,—fire, air, water, and earth

"In conclusion, it should be remembered that the legitimate use of speculations of this kind, is not to furnish plausible expla-

Proceed. Am. Phil. Soc. April 5, and May 17, 1844, pp. 56, 57, and 84, 85. The original notes of these interesting experiments containing the numerical results obtained under a great variety of conditions, laid aside for further reductions and comparisons, were destroyed by fire in 1865. Since the density of most solid substances differs very slightly from that of their liquid state, being indeed less in many,—unless at considerably lower temperatures, (as in the case of ice, and most of the metals.) it appears quite improbable that the difference between solidity and liquidity could depend in any case on the degree of cohesion. On the contrary, the cohesion of water should be sensibly greater than that of ice, since its constituent molecules are closer together. Of the nature of that "lateral *adhesion*" which resists the flow of solids (excepting under the conditions of great strain—long continued), and whose absence is mark d in liquids by their almost perfect and frictionless mobility, our present science affords us no intimation.

† Two hundred years ago, Newton speculating on the unity of matter, ventured the suggestion, "Thus perhaps may all things be originated from æther."—Letter to the Secretary of the Royal Society—Henry Oldenburg, January, 1676 (*Hist. of Roy. Soc.* by Thomas Birch, vol. iii. p. 250).

nations 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."*

Light and Heat.—Henry also made important investigations on some peculiar phenomena connected with light and heat. For the purpose of experimenting on sun light he devised in 1840, a very simple form of heliostat, based on the suggestion of Dr. Young, whereby the solar ray was received into an upper room in a direction parallel to the earth's axis, requiring therefore only an equatorial movement of the reflector; which was effected by the aid of a common cheap pocket watch placed on a small hinged board set by a screw to the angle of latitude. The mirror mounted on a swivel and properly balanced, presented no sensible resistance to the running of the watch, which was arranged for the 24 hour rotation by a watch-maker of Princeton. The whole cost of the completed instrument (including the time-movement) was but sixteen dollars. If any particular direction of the ray was required, it was only necessary to place a stationary mirror in the fixed path of the ray, adjusted to the desired angle.†

In 1841, on repeating experiments of Becquerel and Biot on "Phosphorescence," he discovered some new characteristics in the emanation (particularly when excited by electrical light) which had not before been observed.‡ These were more fully detailed in a communication made to the American Philosophical Society, in 1843, "On Phosphorogenic Emanation." This phenomenon had been first observed in the diamond, when taken into a dark room immediately after exposure to direct sun-light, or to a vivid electric spark; and was afterward observed in several other substances,—notably in the chloride of calcium—"Homberg's phosphorus."§ It had also been shown by Becquerel that

* *Proceed. Am. Phil. Soc.* Nov. 6, 1846, vol. iv. pp. 287–290.

† *Proceed. Am. Phil. Soc.* Sept. 17, 1841, vol. ii. p. 97.

‡ *Proceed. Am. Phil. Soc.* April 16, 1841, vol ii. p. 46.

§ Homberg's phosphorus is chloride of calcium prepared by melting one part of sal ammonia with two parts of slaked lime. Canton's phosphorus is sulphide of calcium formed by a mixture of three parts of sifted and calcined oyster shells, and one part of flowers of sulphur, exposed for an hour to a strong heat.

while this phosphorescence may be fully excited in the sensitive body by rays which have passed through transparent sulphate of lime, or through quartz, the effect is entirely arrested by a plate of transparent mica, or glass. Henry by a long series of experiments greatly extended these lists, including in them a large number of liquids. He also subjected both the exciting rays (especially of the electric spark) and the luminous emanation, to various treatment, by reflection, refraction, polarization, etc. The Nicol prism was found to obstruct this peculiar exciting ray so much as to permit scarcely any impression; but what was remarkable and unexpected, a pile of thin mica plates which seemed to cut off entirely the phosphorogenic impression, was found when placed obliquely at the best polarizing angle, to distinctly excite a surviving luminous spot. On examination of the phosphorescence excited by polarized light, no effect was produced by a rotation of the analyser: "when the beam was transmitted through crystals in different directions with reference to their optical axis, no difference could be observed." The phosphorescence was completely depolarized, as if taking an entirely new origin in the sensitive substance: a fact re-discovered by Professor Stokes some ten years later, with regard to fluorescent emanations.

That the phosphorogenic effect does not depend on a heating of the substance, appeared to be shown by the fact that "the lime becomes as luminous under a plate of alum as under a plate of rock-salt." The emanation was examined by a prism of rock-crystal, and by one of rock-salt:—science had not then the spectroscope. While the impression could be readily made by a reflected beam from a metallic mirror, it failed entirely when directed from a looking-glass. The luminous effect on the phosphorescent substance was found to be defined in location by the form of the opening made in sheet metal screens. On testing with different portions of the electric spark by means of a narrow slit in the screen, the two terminals of the spark were found to be much more active (as measured by the subsequent duration of the phosphorescence) than the middle portion. By a suitable arrangement of double screens with three slits each, he was able to make simultaneous star-like photographs on the substance, of the two extreme portions of the spark and of a middle point: and while the latter point "exhibited a feeble phosphorescence for two or three seconds" only, the two former "continued to glow for more than a minute:" and yet the middle of the spark appeared to the eye quite as vivid as its extremities. It was also observed that while a sensitive daguerreotype plate received no impression from the electric spark, inversely another similar plate exposed for several minutes to the direct light of the full moon received a photographic impression, while the lime similarly exposed, exhibited no phosphorescence.*

Henry was afterward accustomed on the occurrence of a bright aurora, to expose a sheet of paper written or figured with a solution of bisulphate of quinia to the auroral light, when the characters (quite invisible by lamp-light or even by day-light) would distinctly glow with a pale blue light;—indicating the electrical nature of the meteor.

In January, 1845, in conjunction with Professor Stephen Alexander, he instituted a series of experimental observations on the relative heat-radiating power of the solar spots. On the 4th of January a large spot through which our terrestrial globe could have been freely dropped, (having been estimated at more than 10,000 miles in diameter,) favorably situated near the middle of the disk, was examined with a telescope of four inches aperture. A screen having been arranged in a dark room, with a thermo-electric apparatus behind it and having its terminal or pile just projecting through a hole in the screen, the image of the spot was received upon it, giving a clearly defined outline about two inches long and one inch and a half wide. By a slight motion of the telescope the spot could readily be thrown on or off the end of the pile as desired. A considerable number of observations indicated very clearly by the differing deflections of the galvanometer needle "that the spot emitted less heat than the surrounding parts of the luminous disk."† A brief account of the results obtained by these researches given in a letter to his friend Sir David Brewster, was read by him at the Cambridge Meeting of the British Association in June, 1845.‡ The determinations arrived at have been fully confirmed by the later observations of Secchi and others §

In 1845, he contributed a paper to the Princeton Review, on "Color Blindness;" which although in the modest form of a literary review of two Memoirs then recently published, (that of Sir David Brewster in the Philosophical Magazine; and that of

* *Proceed. Am. Phil. Soc.* May 26, 1843, vol. iii. pp. 38–44. This interesting but obscure subject although apparently connected with the phenomenon of "fluorescence" has yet an entirely distinct phase in its abnormal continuance of luminosity,—similar to the familiar effect of a thermal impression. It is possible however that the conversion of wave-periodicity (wave-length), shown by Stokes to be the characteristic of fluorescence, may require time for its full development.

† *Proceed. Am. Phil. Soc.* June 20, 1845, vol. iv. p. 176.

‡ *Report Brit. Assoc.* 1846, part ii. p. 6.

§ P. Angelo Secchi—during the years 1848, and 1849, was Professor of Mathematics at the College of Georgetown, D. C.: and in the preparation of his "Researches on Electrical Rheometry" published in the third volume of the *Smithsonian Contributions*, (art. ii. p. 60,) he received from Henry the friendly assistance of apparatus and suggestions. He was appointed Director of the Observatory at Rome, in 1850.

Professor Elie Wartman, of Lausanne, in the Scientific Memoirs,) supplied original observations on this interesting department of the physiology of vision.

Miscellaneous Contributions.—Henry's miscellaneous contributions to physical science are so numerous and varied, that only a brief allusion to some of them can be afforded. In 1829, he published quite an elaborate "Topographical sketch of the State of New York, designed chiefly to show the general elevations and depressions of its surface."* And in later years he devoted much attention to physical geography He performed at various times, a good deal of chemical work (chiefly of an analytical character),—first as Dr. T. Romeyn Beck's assistant,† and afterward independently, as well as mediately in directing his own pupils and assistants. In 1833, he devised an improvement of Wollaston's mechanical scale of the chemical equivalents, for the benefit of his pupils in chemistry:—a contrivance which was much used and highly appreciated at the time.

The suggestion had been thrown out by more than one astronomer, that carefully timed observations on characteristic meteors or "shooting-stars" might be made available for determining differences of longitude between the stations of observation.‡ For many years however the proposition had been generally regarded as offering rather a speculative than a practical method of solving a problem of so great nicety. Henry in concert with his brother-in-law, Professor Alexander, and with his friend Professor Bache, determined to ascertain by actual trial the availability and value of the system. On the 25th of November, 1835, Professor Bache observing at his residence in Philadelphia (assisted by Professor J. P. Espy,)—simultaneously with Professor Henry and Professor Alexander, at the Philosophical Hall at Princeton, they obtained seven co-incidences:—the instant of disappearance of the meteor being in each case selected as the most accurately attainable epoch. These seven observations (whose greatest discrepancies amounted to but a trifle over 3 seconds) gave a mean result of 2 minutes 0.61 second (time longitude) differing only

* *Trans. Albany Institute*, vol. i. pp. 87–112.

† "Henry was then Dr. Beck's chemical assistant, and already an admirable experimentalist." Address before the Albany Institute, by Dr. O. Meads, May 25, 1871. (*Trans. Albany Instit.* vol. vii. p. 21.)

‡ "The merit of first suggesting the use of shooting stars and fire-balls as signals for the determination of longitudes is claimed by Dr. Olbers and the German astronomers for Benzenberg, who published a work on the subject in 1802. Mr. Bailey however has pointed out a paper published by Dr. Maskelyne twenty years previously, in which that illustrious astronomer calls attention to the subject, and distinctly points out this application of the phenomena." This was dated Greenwich, November 6th, 1783. (*L. E. D. Phil. Mag.* 1841, vol. xix. p. 554.)

one second and two-tenths from the mean estimate of relative longitude arrived at by other methods.*

In 1840, Henry gave an account of "electricity obtained from a small ball partly filled with water, and heated by a lamp."†

In 1843, he read a communication to the Society, "On a new method of determining the velocity of Projectiles:" for this purpose employing two screens of fine insulated wire each in circuit with a galvanometer, and at determined near distances in the path of the projectile;—whereby the galvanic currents would be successively interrupted at the instants of penetration. To record the interval, each galvanometer needle is provided at one end with a marking pen touching a horizontally revolving cylinder, which is divided by longitudinal lines into 100 equal parts, and is driven by clock-work at the rate of ten revolutions per second, giving therefore to the interval of passage between two consecutive lines, the thousandth part of a second.‡ Another still more ingenious method is suggested, whereby the galvanometer may be dispensed with: each circuit including an induction coil, one end of whose secondary circuit is connected with the axis, and the other end placed very nearly in contact with the surface of the graduated paper on the revolving cylinder, so as to give the induction spark through the paper at the instant of the interruption of the primary circuits by the projectile passing through the wire screens. This is really a much neater and more direct application of the electric interruption than the employment of a galvanometer needle for making the record. If desirable, the cylinder may be made to have a very slow longitudinal movement by a screw, so as to give a helical direction to the tracings; and different pairs of screens similarly arranged at distant points in the path of the projectile may be employed to determine the variations of velocity in its flight.§

Henry was always a watchful student of psychological and subjective phenomena. Witnessing on one occasion the performance of an athlete before a large assembly, he noticed with a curious interest the "inductive" sympathy manifested by nearly

* *Proceed. Am. Phil. Soc.* Dec. 20, 1839, vol. i. pp. 162, 163. "This appears to have been the first actual determination of a difference of longitude by meteoric observations." (*L. E. D. Phil. Mag.* 1841, vol. xix. p. 553.) Several years later (in 1838) similar meteoric observations were made between Altona and Breslau; and also between Rome and Naples.

† *Proceed. Am. Phil. Soc.* Dec. 18, 1840, vol. i. p. 323.

‡ It appears that Wheatstone devised his ingenious electro-magnetic "chronoscope" in 1840; though he unfortunately published no account of it till 1845; or two years after the publication by Henry. And this was called out as a reclamation, on the publication of a similar invention by L. Breguet, of Paris, in January of the same year.

§ *Proceed. Am. Phil. Soc.* May 30, 1843, vol. iii. pp. 165-167.

every spectator (himself included) in being swayed by a movement as of assistance to the performer. In remarking the impression of being moved, while steadily watching a series of passing canal boats,—on each boat reaching a certain point, he referred the impression (amounting almost to a sensation of movement), to the relative angle of vision formed by the moving body.

He made a number of experiments on the flow of water jets under varying conditions: also observations on sonorous flames when passing into a stove-pipe of eight inches diameter and about ten feet in length: on the comparative rates of evaporation from fresh and from salt water: on the slow evaporation of water from the end of a tube, and the much greater rapidity of evaporation when the tube is open at both ends: extended notes of which, with a great number of other researches, perished in the flames.

In 1844, he published a Syllabus of his Lectures at Princeton. In December of that year he presented to the Philosophical Society a communication of a somewhat more theoretical character than usual,—on the derivation and classification of mechanical motors. He refers these to two classes;—the first, those derived from celestial disturbance (as water, tide, and wind powers),—and the second, those derived from organic bodies or forces (as steam and other heat powers, and animal powers). The forces of gravity, cohesion, and chemical affinity are not included, since these tend speedily to stable equilibrium; and they become sources of mechanical power only as they are disturbed by some of those before mentioned. It is not the running down of the water-fall, or the clock-weight, which is the true origin of their useful work, but the lifting of them up. The same is true of the power derived from combustion. He then adds that his second class (the forces derived from the organic world) might perhaps by a similar process of reasoning be derived from the first class; (that of celestial disturbance;)—regarding "animal power as referable to the same sources as that from the combustion of fuel," and the action of the vegetative power as "a force derived from the divellent power of the sunbeam," being simply a case of solar de-oxidation. Organism—vegetable and animal, he considers as built up under the *direction* of a vital principle, which is not itself a mechanical force. Volcanic power is neglected as comparatively feeble and limited, and not practically utilized.*

* *Proceed. Am. Phil. Soc.* Dec. 20, 1844, vol. iv. pp. 127–129. This appears to be the first—as it is probably the best—analysis of physical energy, which has been proposed. Twenty years later, a similar analysis (with certainly no improvement in the classification) was adopted by Prof. Tait, in an essay on "Energy;" (*North British Review*, 1864, vol. xl. art. iii. p. 191, of Am. edition:) and by Dr. Balfour Stewart, in his *Elementary Treatise on Heat*, Oxford, 1866 (book iii. chap. v. art. 388, p. 354.)

This interesting digest presents one of the earliest and clearest theoretical statements we have, of the correlation and transformation of the physical forces; including with these the so-called organic forces.

ADMINISTRATION OF THE SMITHSONIAN INSTITUTION.

By an Act of Congress approved August 10, 1846, the liberal bequest to the United States, for the promotion of Science, by James Smithson of London, England, was appropriated to the foundation of the Institution bearing his name; the establishment being made to comprise the chief dignitaries of the Government as the supervising body, and a Board of Regents being created for conducting the business of the Institution after completing its organization. As the testator had bequeathed his fortune,* in simple terms "for the increase and diffusion of knowledge among men," there arose not unnaturally a great diversity of opinion both among Congressmen, and among the Regents, as to the most desirable method of executing the purpose of the Will: and the organizing Act was itself a sort of compromise, after many years of discussion and disagreement in both branches of Congress. To literary men, no instrument of knowledge could be so important as an extensive Library:—to the professional, a seat of education or public instruction—general or special—supplemented by elaborate courses of public lectures, appeared the obvious and necessary means of diffusing useful learning,—to the "practical," a large agricultural and polytechnic institute—supplemented perhaps by a museum, was the only fitting plan of developing the resources of our country:—to the artistic, extensive galleries of art were the most worthy and instructive objects of patronage. The Regents sought counsel from the distinguished and the learned: and several of them applied to Professor Henry for his opinion. He gave the subject a careful consideration; and announced very decided views. As Smithson was a man of scientific culture, a Fellow of the Royal Society, an expert analytical chemist, and devoted to original research, Henry held that the language of his Will must receive its most accurate and scientific and at the same time most comprehensive interpretation; that the words "increase and diffusion of knowledge among men" were deliberately and intelligently employed; and that no local or even national interests were as broad as its terms,—that no merely educational projects of whatever character, no schemes of material and practical advancement however useful, could justly be regarded as fulfilling the obvious intent—expressed by a scientific thinker

* The whole amount of the bequest was a trifle over 100,000 pounds, or about 540,000 dollars.

and writer—first of all the *increase* of knowledge by the promotion of original research,—the addition of new truths to the existing stock of knowledge, and secondly—its widest possible diffusion among mankind.*

These wise and far-reaching views exerted a marked influence; and though hardly then in accord with the opinion of the majority, yet led to his election December 3d, 1846, as the "Secretary" and practical Director of the infant institution. A second time was Henry called upon to sever dearly prized associations,—the prosperous and congenial pursuits of fourteen years within the classic halls of Princeton. One motive turned the wavering scale. Here was a rare occasion offered by the enlightened provision of James Smithson, to secure for abstract science and unpromising original research, a much needed encouragement and support; and an obligation upon the scientific few to resist and if possible prevent the perversion of the trust to the merely popular uses of the short-sighted many. That years would be required for shaping the character and conduct of the institution as he desired, was certain;—that this could not be effected without much opposition and various obstacles, he very clearly foresaw. That during these years of active supervision and direction, he must abandon all hope of personal opportunity for original research, he as freely accepted in the expressive remark made to a trusted friend in consultation on the occasion: "If I go, I shall probably exchange permanent fame for transient reputation."

With the assurance of the Trustees of the College of New Jersey, that should he fail to realize his programme, or should he satisfactorily accomplish his apostolic purpose, his chair should always be at his command, with a hearty welcome back, Henry, neither spurred by over-confidence, nor depressed with undue timidity, though filled with anxious solicitude for the future, accepted the appointment tendered to him. He removed with his family to Washington, and at once commenced his administration of the duties assigned to him by the Regents of the Institution.

Summoned thus to the occupancy of a new and untried field, and to the discharge of essentially executive functions, he from the first displayed a clearness and promptness of judgment, a singleness and steadiness of aim, a firmness and consistency of decision, combined with a practical sagacity and moderation in adapting his course to the exigencies of adverse conditions, which stamped him as a most able and successful administrator. Without concealment and without diplomacy, his distinctly avowed principle of action was steadily and patiently pursued. With

* "Programme of Organization." Smithsonian Report for 1847.

honest submission to the controlling Act of Congress, he made as honest avowal of his desire and of his endeavor to have that legislation modified. Hampered by provisions he deemed unwise and injurious, he yet skilfully managed to reconcile contestant interests, and to secure the entire confidence and concurrence of the Regents. Henceforth his purpose and his effort were to be directed to the unique object of encouraging and fostering the development of what has so flippantly been designated "useless knowledge;" and merging self in the community of physical inquirers and collaborators, to become the high-priest of abstract investigation;—prepared to lend all practicable assistance to that small but earnest band of nature-students, who inspired by no aims of material utility, seek from their mistress as the only reward of their devotion, the higher knowledge of truth.*

Of the two distinct objects of endowment specified by Smithson's Will,—"the *increase*—and the *diffusion*—of knowledge," Henry forcibly remarked: "These though frequently confounded, are very different processes, and each may exist independent of the other. While we rejoice that in our country above all others, so much attention is paid to the *diffusion* of knowledge, truth compels us to say that comparatively little encouragement is given to its *increase*.† There is another division with regard to knowledge which Smithson does not embrace in his design; viz. the application of knowledge to useful purposes in the arts. And it was not necessary he should found an institution for this purpose. There are already in every civilized country, establishments and patent laws for the encouragement of this department of mental industry. As soon as any branch of science can be brought to bear on the necessi-

* Henry has finely said: "Let censure or ridicule fall elsewhere,—on those whose lives are passed without labor and without object; but let praise and honor be bestowed on him who seeks with unwearied patience to develop the order, harmony, and beauty of even the smallest part of God's creation. A life devoted exclusively to the study of a single insect, is not spent in vain. No animal however insignificant is isolated; it forms a part of the great system of nature, and is governed by the same general laws which control the most prominent beings of the organic world." (*Smithsonian Report* for 1855, p. 20.)

† Swainson the Naturalist, the countryman and friend of Smithson, has very pointedly marked this recognized distinction. "The constitution of the Zoological Society is of a very mixed nature, admirably adapted indeed to the reigning taste. It is more calculated however to *diffuse* than to *increase* the actual stock of scientific knowledge." (*Discourse on the Study of Natural History*, Cabinet Cyclopædia, 16mo. London, 1834, part iv. chap. i. sec. 221, p. 314.) And again: "It is very essential when we speak of the diffusion or extension of science, that we do not confound these stages of development with discovery or advancement; since the latter may be as different from the former as depth is from shallowness." (Same work, part iv. chap. ii. sec. 240, p. 343.)

ties, conveniences, or luxuries of life, it meets with encouragement and reward. Not so with the discovery of the incipient principles of science. The investigations which lead to these, receive no fostering care from Government, and are considered by the superficial observer as trifles unworthy the attention of those who place the supreme good in that which immediately administers to the physical needs or luxuries of life. If physical well-being were alone the object of existence, every avenue of enjoyment should be explored to its utmost extent. But he who loves truth for its own sake, feels that its highest claims are lowered and its moral influence marred by being continually summoned to the bar of immediate and palpable utility. Smithson himself had no such narrow views.* The prominent design of his bequest is the promotion of abstract science. In this respect the Institution holds an otherwise unoccupied place in this country; and it adopts two fundamental maxims in its policy;—first to do nothing with its funds which can be equally well done by other means; and second to produce results which as far as possible will benefit mankind in general." †

Congress—naturally with a prevailing tendency to the literary, the showy, and the popular, had (after eight years of dilatory controversy) directed in its organizing Act (sec. 5,) the erection of a building "of sufficient size, and with suitable rooms or halls for the reception and arrangement upon a liberal scale, of objects of natural history including a geological and mineralogical cabinet, also a chemical laboratory, a library, a gallery of art, and the necessary lecture-rooms." By the 9th section of the Act, the Board of Regents were authorized to expend the remaining income of the endowment "as they shall deem best suited for the promotion of the purpose of the testator." Out of an annual income of some 40,000 dollars, the Regents in full accord with their Secretary (whose carefully elaborated programme they officially adopted December 13, 1847,) succeeded in creditably inaugurating all the objects specified in the charter; and at the same time in establishing the system of publication of original Memoirs, to which Henry justly attached the first importance.

An incident in itself too slight to produce a visible ripple on the current of Henry's life, is yet too characteristic to be here

* In regard to the value of scientific truth, Smithson in a communication dated June 10th 1824, has forcibly expressed his strong "conviction that it is in his *knowledge* that man has found his greatness and his happiness, the high superiority which he holds over the other animals who inhabit the earth with him; and consequently that no ignorance is probably without loss to him, no error without evil." (*Thomson's Annals of Philosophy*, 1824, vol. xxiv. or new series vol. viii. p. 50.)

† *Smithsonian Report* for 1853, p. 8.

omitted. Dr. Robert Hare having in 1847 decided upon resigning his Professorship of Chemistry in the Medical Department of the University of Pennsylvania, (the largest and best patronized in the country,) the vacant chair was tendered by the Board of Trustees to Professor Henry. His friend Dr. Hare himself used his influence to induce Henry to become his successor; particularly dwelling on the large amount of leisure afforded for independent investigations. The income of this professorship was more than double the salary of the Smithsonian Secretaryship. The position tempting as it might have been under different circumstances, was however declined. Henry felt that to leave his present post before his cherished policy was fairly settled and established, would be most probably to abandon nearly all the results of the experiment: and having set before himself the one great object of directing the resources of the Smithsonian Institution as far as possible to the advancement of science, in conformity with the undoubted intention of its founder, (and as the execution therefore of a sacred trust,) he resolutely put aside every inducement that might divert him from the fulfillment of his task.*

Of the half a dozen objects of attention specified in the 5th section of the organizing Act, (the various inspiration of different partisans,) not one directly tended to further the primary requirement of the Will:—even the Laboratory being avowedly introduced simply as a utilitarian workshop for mining and agricultural analysis. Regarded as methods of *diffusing* existing knowledge they were obviously local and limited in their range: and as compared with the instrumentality of the Press, were certainly very inefficient for spreading the benefits of the endowment among men.†

* Some six years later, a somewhat similar temptation was presented. In 1853, on the resignation of President Carnahan of the College of New Jersey at Princeton, an effort was made to induce the return of Professor Henry to his academic seat, by a movement to obtain for him the Presidency of the College. Such a token of affectionate remembrance could not but be grateful and touching to his feelings; but a sense of obligation was upon him, not to be laid aside. He had undertaken a work and a responsibility which must not be left to the hazard of failure. He declined the proffered honor—with thanks; and warmly recommended Dr. Maclean to the vacant position: who thereupon was duly elected. (Maclean's *Hist. of College of New Jersey*, vol ii. p. 336.)

† "The objects specified in the Act of Congress evidently do not come up to the idea of the testator as deduced from a critical examination of his will. A library, a museum, a gallery of arts, though important in themselves, are local in their influence. I have from the beginning advocated this opinion on all occasions, and shall continue to advocate it whenever a suitable opportunity occurs." (*Smithsonian Report* for 1853, p. 122 (of Senate edit.) p. 117, (of H. Rep. edit.) The superficial pretext was not wanting on the part of some, that the words "increase and diffusion" were not to be taken too literally, but to be considered as the

Henry with a rare courage dared maintain against most powerful influence, that the interests specifically designated must all be subordinated to the fundamental requirement, the promotion of original research for increasing knowledge; and that this was amply sustained by the residuary grant of authority to the Regents (under the 9th section of the Act) "to make such disposal as they shall deem best suited *for the promotion of the purposes of the testator*, anything herein contained to the contrary notwithstanding," of any income of the Smithsonian fund "not herein appropriated, or not required for the purposes herein provided." Henry's carefully studied programme comprised two sections: the first, embracing the details of the plan for carrying out the explicit purpose of Smithson; the second, indicating the proper steps for carrying out the provisions of the Act of Congress. The first and principal section proposed as methods of promoting research,—the stimulation of particular investigations by special premiums,—the publication of such original memoirs furnishing positive additions to knowledge by experiment and observation as should be approved by a commission of experts in each case,—the active direction of certain investigations by the provision of instruments as well as of the necessary means, the appropriations being judiciously varied in distribution from year to year,—the prosecution of experimental determinations and the solution of physical problems,—the extension of ethnology (especially American) and in general the conduct of such varied explorations as should ultimately result in a complete physical atlas of the United States. As methods of promoting the diffusion of knowledge, it was proposed to give a wide circulation to the published original memoirs or Smithsonian "Contributions to Knowledge" among domestic and foreign libraries, institutions, and scientific correspondents, to have prepared by qualified collaborators, series of careful reports on the latest progress of science in different departments, and to provide facilities for the distribution and exchange of scientific memoirs generally.

It is unnecessary here to follow closely the slow steps by which —through all the obstructions of narrow prejudice and ignorant misconstruction, of selfish interest and pretended philanthropy, of friendly remonstrance and hostile denunciation,—the policy originally marked out by the Secretary was with unwavering resolution and imperturbable equanimity steadily pursued, until it gained its assured success; the vindication and the unpretentious triumph of "the just man tenacious of purpose."

The most formidable of the specialist schemes both in Congress

tautology of legal equivalents, applicable to the development of the individual mind; since school-boys (if not the pundits) were evidently capable of an "increase" of knowledge.

and elsewhere, was that of the Library faction, which prosecuted with remarkable zeal and energy, threatened by the acknowledged ability of its leading advocates to control the action of the Regents, even to the neglect and abandonment of all the other interests indicated by the statute. In Henry's judgment the Institution should possess simply a working library,* an auxiliary for those engaged in scientific research, a repertory well supplied with the published Proceedings and Transactions of learned Societies, but which so far from aiming at an encyclopædic or a literary character, should be mainly supplementary to the large National Library already established at the Capital. "The idea ought never to be entertained that the portion of the limited income of the Smithsonian fund which can be devoted to the purchase of books will ever be sufficient to meet the wants of the American scholar. On the contrary it is the duty of this Institution to increase those wants by pointing out new fields for exploration, and by stimulating other researches than those which are now cultivated. It is a part of that duty to make the value of libraries more generally known, and their want in this country more generally felt."†

Processes of Divestment.—Henry's declaration that the moderate means at command were insufficient to support worthily either a Library, or a Museum, alone, was early justified. The Library though slowly formed of only really valuable scientific works, and this largely by exchanges with the Smithsonian publications,‡ in the course of a dozen years amounted to about 50,000 volumes: and the annual cost of binding, superintendence, and the constant enlargement of room and of cases, was becoming a serious tax upon the resources of the Institution. The propriety of transferring the custody of this valuable and rapidly increasing collection to the National Library established by Congress, was repeatedly urged upon the attention of that body:

* To carry on the operations of the first section a working library will be required, consisting of the past volumes of 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." (*Smithsonian Report* for 1847, p. 139, of Sen. ed.: p. 131, of H. Rep. ed.)

† *Smithsonian Report* for 1858, p. 224, (of Sen. ed.) p. 216, (of H. Rep. ed.)

‡ "It is the intention of the Regents to render the Smithsonian library the most extensive and perfect collection of Transactions and scientific works in this country, and this it will be enabled to accomplish by means of its exchanges, which will furnish it with all the current journals and publications of societies, while the separate series may be completed in due time as opportunity and means may offer. The Institution has already more complete sets of Transactions of learned societies than are to be found in the oldest libraries in the United States." (*Smithsonian Report* for 1855, p. 29.)

and by an Act approved April 5th, 1866, such transfer was at last effected.

"Congress had presented to the Institution a portion of the public reservation on which the building is situated. In the planting of this with trees, nearly 10,000 dollars of the Smithson income were expended." Ultimately however opportunity was taken to have the Smithsonian park included in the general appropriation by the Government for improving the public grounds.

The courses of Lectures which were continued from their establishment in 1849, to 1863, were then abandoned. In conformity with the judicious policy entertained from the beginning not to consume unprofitably the limited means of the Institution by attempting to do what could be as well or better accomplished by other organizations, its herbarium comprising 30,000 botanical specimens and other allied objects, was transferred to the custody of the Agricultural Department. Its collection of anatomical and osteological specimens was transferred to the Army Medical Museum. And its Fine-Art collections were transferred to the custody of the "Art-Gallery" established at Washington (with a larger endowment than the whole Smithsonian fund) by the enlightened liberality of Mr. W. W. Corcoran.

Such were the successive processes by which much of the early and injudicious legislative work of organization, intended for popularising the activities of the Institution, was gradually undone; greatly to the dissatisfaction and foreboding of many of its well-meaning friends. "It should be recollected," said Henry, "that the Institution is not a popular establishment."*

The National Museum.—The last heritage of misdirected legislation—the National Museum, still remains in nominal connection with the Institution; although Congress has recognized the justice of making special provision for its custody by an annual appropriation ever since its establishment in 1842,—four years before the organization of the Smithsonian Institution. The Government collection of curiosities had accumulated from the contributions of the various exploring expeditions; and Henry from the first, had objected to receiving it as a donation, foreseeing that it would

* *Smithsonian Report* for 1876, p. 12. A distinguished politician, now many years deceased, (an influential Member of Congress—and *possible* statesman,) in the confidence of friendship pointed out with emphasis, how by a few judicious expedients—involving only a moderate reduction of the income of the Institution, golden opinions might be won from the press, and the Smithsonian really be made quite a "popular" establishment. Unseduced by these friendly suggestions of worldly wisdom, Henry astonished his adviser by the smiling assurance that his self-imposed mission and deliberate purpose was to prevent, as far as in him lay, precisely that consummation. Had the philosopher repudiated the "breath of his nostrils" he could not have been looked upon by the politician, as more hopelessly demented.

prove more than "the gift of an elephant."* In his first Report, he ventured to say: "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."† In his third annual Report he remarked: "The formation of a Museum of objects of nature and of art requires much caution. With a given income to be appropriated to the purpose, a time must come when the cost of keeping the objects will just equal the amount of the appropriation: after this no further increase can take place. Also, the tendency of an Institution of this kind unless guarded against, will be to expend its funds on a heterogeneous collection of objects of mere curiosity." Justly jealous of any dependence of the Institution, designed as a monument to its founder, upon the varying favors or caprices of a political government, or of any confusion between the National Museum, and its own special collections for scientific study rather than for popular display, he added: "If the Regents accept this Museum, it must be merged in the Smithsonian collections. It could not be the intention of Congress that an Institution founded by the liberality of a foreigner, and to which he has affixed his own name, should be charged with the keeping of a separate Museum, the property of the United States. . . The small portion of our funds which can be devoted to a museum may be better employed in collecting new objects, such as have not yet been studied, than in preserving those from which the harvest of discovery has already been fully gathered." Nor was he reconciled to the gift by the suggestion that a suitable appropriation would be granted by the National Government, for the expense of its custody. "This would be equally objectionable; since it would annually bring the Institution before Congress as a supplicant for government patronage."‡

In his Report for 1851, he forcibly stated in regard to the requirements of a general Museum, that "the whole income devoted to this object would be entirely inadequate:" and he strongly urged a National establishment of the Museum on a basis and a scale which should be an honor and a benefit to the people and their Capital city. "Though the formation of a general collection is neither within the means nor the province of the Institution, it is an object which ought to engage the at-

* His friend Prof. Silliman in a letter dated December 4th, 1847, wrote: "If it is within the views of the Government to bestow the National Museum upon the Smithsonian Institution, the very bequest would seem to draw after it an obligation to furnish the requisite accommodations without taxing the Smithsonian funds: otherwise the gift might be detrimental instead of beneficial."

† *Smithsonian Report*, 1847, p. 139, (Sen. ed.), p. 132, (H. Rep. ed.)

‡ *Smithsonian Report* for 1849, pp. 181, 182, (of Senate ed.) pp. 173, 174, (of H. Rep. ed.)

tention of Congress. A general Museum appears to be a necessary establishment at the seat of government of every civilized nation. . . . An establishment of this kind can only be supported by government; and the proposition ought never to be encouraged of putting this duty on the limited though liberal bequest of a foreigner."* This project was urged in almost every subsequent Report. "There can be but little doubt that in due time ample provision will be made for a Library and Museum at the Capital of this Union, worthy of a government whose perpetuity depends upon the virtue and intelligence of the people. It is therefore unwise to hamper the more important objects of this Institution by attempting to anticipate results which will be eventually produced without the expenditure of its means."† "The importance of a collection at the seat of government, to illustrate the physical geography, natural history, and ethnology, of the United States, cannot be too highly estimated: but the support of such a collection ought not to be a burden upon the Smithsonian fund."‡

The popular mind did not however appear to be prepared to accept these earnest presentations; and in 1858, the National Museum was transferred by law to the custody of the Smithsonian Institution, with the same annual appropriation (4,000 dollars) which had been granted to the United States Patent Office when in charge of it.

So rapidly were the treasures of the Museum increased by the gathered fruits of various government explorations and surveys, as well as by the voluntary contributions of the numerous and wide-spread tributaries of the Institution, that the policy was early adopted of freely distributing duplicate specimens to other institutions where they would be most appreciated and most usefully applied. And in this way the Smithsonian became a valuable centre of diffusion of the means of investigation in geology, mineralogy, botany, zoology and archæology. The clear foresight which announced that the Museum must very soon outgrow the entire capacity of the Smithsonian resources, has been most amply vindicated:§ and to-day a large Government Building is stored from basement to attic, with boxed up rarities of art and nature, sufficient more than twice to fill the Smithsonian

* *Smithsonian Report* for 1851, p. 227 (of Sen. ed.) p. 219, (of H. Rep. ed.)

† *Smithsonian Report*, for 1852, p. 253, (of Sen. ed.) p. 245, (of H. Rep. ed.)

‡ *Smithsonian Report* for 1853, p. 11, (of Sen. ed.) p. 9, (of H. Rep. ed.)

§ Although from the rapid growth of the national collection after it was transferred to the custody of the Smithsonian Institution, the annual appropriation of 4,000 dollars by Congress very soon became wholly insufficient to defray even one-half its necessary expenses, it was not till 1871 that the appropriation was raised to 10,000 dollars. In 1872, it was increased to 15,000 dollars, and in 1874, to 20,000 dollars.

halls and galleries, in addition to their present overflowing display. The strong desire of Henry to see established in Washington a National Museum on a scale worthy of our resources, and in which the existing overgrown collections might be so beneficially exhibited, he did not live to see gratified. That the realization of this wise and beneficent project is only a question of time, is little doubtful; and when established, its being and its benefits will in no small degree be due to him who first realizing its necessity, and most appreciating its importance, with unwearying perseverance for twenty-five years omitted no opportunity of urging upon members of Congress its importunate claims.

Meteorological Work.—In the conduct of what were appropriately called the "active operations" of the Institution—under the first section of the programme (in contradistinction to the local and statical objects of the second section), a rare energy and promptness were exhibited. The very first Report of the Secretary announced not only the acceptance and preparation for publication of an elaborate work on explorations by Messrs. Squier and Davis of "Ancient Monuments of the Mississippi Valley," but the commencement of official preparations "for instituting various lines of physical research. Among the subjects mentioned by way of example in the programme, for the application of the funds of the Institution, is terrestrial magnetism. . . . 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. . . . 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 Northern 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."*

* *Smithsonian Report* for 1847, pp. 146, 147, (of Sen. ed.), pp. 138, 139, (of H. Rep. ed.) Prof. Loomis (to whom among others "distinguished for their attainments in meteorology" letters inviting suggestions, had been addressed,) recommended that there should be at least one observing

An appropriation for the purpose having been made by the Regents, a large number of observers scattered over the United States and the Territories became voluntary correspondents of the Institution. Advantage was taken of the stations already established under the direction of the War, and of the Navy Departments, as well as of those provided for by a few of the States. The annual reports of the Secretary chronicled the extension and success of the system adopted; and in a few years between five and six hundred regular observers were engaged in its meteorological service. The favorite project of employing the telegraph for obtaining simultaneous results over a large area was at once organized; and in 1849, a system of telegraphic despatches was established, by which (a few years later) the information received in Washington at the Smithsonian Institution was daily plotted upon a large map of the United States by means of adjustable symbols. Espy's generalization that the principal storms and other atmospheric changes have an eastward movement,* was fully established by this rapidly gathered experience of the Institution; so that "it was often enabled to predict (sometimes a day or two in advance) the approach of any of the larger disturbances of the atmosphere."†

Eminently efficient as the enterprise approved itself, increasing experience served to demonstrate the increasing demands of the service; and it was seen that to prosecute the subject of meteorology over so large a territory, with the fulness necessary, would require a still larger force of observers, and a greater drain upon the resources of the Institution, than could well be spared from other objects; and as the great value of the system was fully recognized by the intelligent, the propriety of maintaining a meteorological bureau by the national support was early presented to the attention of Congress. This most important depart-

station within every hundred square miles of the United States; and he sagaciously pointed out that "When the magnetic telegraph [then an infant three years old] is extended from New York to New Orleans and St. Louis, it may be made subservient to the protection of our commerce." This interesting letter was published in full as "Appendix No. 2," to the *Report*. In 1848, a paper was read before the British Association by Mr. John Ball, "On rendering the Electric Telegraph subservient to Meteorological Research: in which the author suggested that simultaneous observations so collected, might reveal the direction and probable time of arrival of storms. (*Report Brit. Assoc.* Swansea. Aug. 1848. Abstracts, pp. 12, 13.)

* Franklin is said to have been the first who stated the general law, that the storms of our Southern States move off to the northeastward over the Middle and Eastern States.

† *Smithsonian Report* for 1864, p. 44. An interesting and instructive résumé of results accomplished within fifteen years was given in this *Report*, pp. 42–45: and continued in the succeeding *Report* for 1865, pp. 50–59.

ment of observation had been advanced by Henry to that position, in which a larger annual outlay than the entire income of the Institution was really required to give just efficiency to the system. In his Report for 1865, he remarked: "The present would appear to be a favorable time to urge upon Congress the importance of making provision for the reorganizing all the meteorological observations of the United States under one combined plan, in which the records should be sent to a central depot for reduction, discussion, and final publication. An appropriation of 50,000 dollars annually for this purpose would tend not only to advance the material interest of the country, but also to increase its reputation. . . . 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."*

Five years later, in support of the proposition that the subject from its magnitude now appealed to the liberality of the nation, he briefly recapitulated the work accomplished by the limited means of the Institution. "The Smithsonian meteorological system was commenced in 1849, and has continued in operation until the present time. . . . It has done good service to the cause of meteorology; 1st in inaugurating the system which has been in operation upwards of twenty years: 2nd in the introduction of improved instruments after discussion and experiments: 3rd in preparing and publishing at its expense an extensive series of meteorological tables: 4th in reducing and discussing the meteorological material which could be obtained from all the records from the first settlement of the country till within a few years: 5th in being the first to show the practicability of telegraphic weather signals: 6th in publishing records and discussions made at its own expense, of the Arctic expeditions of Kane, Hayes, and McClintock: 7th in discussing and publishing a number of series of special records embracing periods of from twenty to fifty years in different sections of the United States,—of great interest in determining secular changes of the climate: 8th in the publication of a series of memoirs on various meteorological phenomena, embracing observations and discussions of storms, tornadoes, meteors, auroras, etc.: 9th in a diffusion of a knowledge of meteorology through its extensive unpublished correspondence and its printed circulars. It has done all in this line which its limited means would permit; and has urged upon Congress the establishment with adequate appropriation of funds, of a meteorological department under one comprehensive plan, 'in which the records should be sent to a central depot for reduction, discussion, and final publication.'"†

* *Smithsonian Report* for 1865, p. 57.
† *Smithsonian Report* for 1870, p. 43.

In 1870, a meteorological department was established by the Government under the Signal Office of the War Department, with enlarged facilities for systematic observation: and agreeably to the settled policy of the Institution, this important field of research was in 1872, abandoned in favor of the new organization.* Of the voluminous results of nearly a quarter of a century of systematic records over a wide geographical area which have been slowly digested and laboriously discussed, only a small portion has yet been published. The publication of the series when practicable, will yet prove an inestimable boon to meteorological theory.

Although our country can boast of many able meteorologists, who have greatly promoted our knowledge of the laws of atmospheric phenomena, it is safe to say that to no single worker in the field is our nation more indebted for the advancement of this branch of science to its present standing, than to Joseph Henry. Quite as much by his incitement and encouragement of others in such researches, as by his own exertions, does he merit this award. To him is undoubtedly due the most important step in the modern system of observation,—the installation of the telegraph in the service of meteorological signals and predictions.† While giving however his active supervision to the extensive system he had himself inaugurated, publishing many important reductions of particular features, as well as various circulars of detailed instructions to observers, of the desiderata to be obtained by those having the opportunities of arctic, oceanic, and southern explorations, directing the constant observations recorded at the Institution as an independent station, he made many personal investigations of allied subjects;—as of the Aurora, of Atmospheric electricity and Thunder-storms, of the supposed influence of the Moon on the weather,—and contributed a valuable series of Memoirs on Meteorology, embracing a wide range of physical exposition, to the successive Agricultural Reports of the Commissioner of Patents, during the years 1855, '56, '57,

* As an illustration of the popular favor in which this Signal service is held, it may be stated that the annual appropriation by Government for its support now exceeds not merely the entire Smithsonian income, but *sixteen times* that amount; or in fact its whole endowment.

† "However frequently the idea may have been suggested of utilizing our knowledge by the employment of the electric telegraph, it is to Professor Henry and his assistants in the Smithsonian Institution that the credit is due of having first actually realized this suggestion. . . . It will thus be seen that without material aid from the Government. but through the enlightened policy of the telegraph companies, the Smithsonian Institution *first in the world* organized a comprehensive system of telegraphic meteorology, and has thus given—first to Europe and Asia, and now to the United States, that most beneficent national application of modern science—the Storm Warnings." Article on "Weather Telegraphy" by Prof. Cleveland Abbe. (*Am. Jour. Sci.*, Aug. 1871, vol. ii. pp. 83, 85.)

'58, and 1859. Instructive articles on Magnetism and Meteorology were prepared in 1861 for the American Encyclopædia. And as an illustration of his continued interest in such studies, one of his latest published papers comprised a minute account of the effects of lightning in two thunder-storms; one occurring in the spring of last year (1877) at a Light-house in Key West, Florida, and the other occurring in the summer of last year at New London, Connecticut.*

Archæological Work.— One of the earliest subjects taken up for investigation by the Institution, was that of American Archæology; the attempt by extended explorations of the existing pre-historic relics, mounds, and monuments, of the aborigines of our country, to ascertain as far as possible their primitive industrial, social and intellectual character, and any evidences of their antiquity, or of their stages of development. The first publication of "Smithsonian Contributions" comprised in a good sized quarto volume an account of extensive examinations of the mounds and earthworks found over the broad valley of the Mississippi, with elaborate illustrations of the relics and results obtained: and this volume extensively circulated by gift and by sale, attracted a wide-spread attention and interest, and gave a remarkable stimulus to the further prosecution of such researches. "Whatever relates to the nature of man is interesting to the students of every branch of knowledge; and hence ethnology affords a common ground on which the cultivators of physical science, of natural history, of archæology, of language, of history, and of literature, can all harmoniously labor. Consequently no part of the operations of this Institution has been more generally popular than that which relates to this subject."†

Special explorations inaugurated by the Institution, have supplied it with important contributions to archæological information, and with the rich spoils of collected relics; which together with much material gathered from Arctic and from Southern regions, from Europe, from Asia, and from Africa, fill now a large museum hall 200 feet long and 50 feet wide, exclusively devoted to comparative Anthropology and Ethnology. In 1868, the Secretary reported that "during the past year greater effort has been made than ever before to collect specimens to illustrate the ethnology and archæology of the North American continent:" and he dwelt upon the importance of the subject as a study connecting all portions of the habitable earth, pointing out that "it embraces not only the natural history and peculiarities of the different races of men as they now exist upon the

* *Journal of the American Electrical Society*, 1878, vol. ii. pp. 37–43. The communication is dated Oct. 13, 1877; though not published till during his last illness.

† *Smithsonian Report* for 1860, p. 38.

globe, but also their affiliations, their changes in mental and moral development, and also the question of the geological epoch of the appearance of man upon the earth. . . . The ethnological specimens we have mentioned are not considered as mere curiosities collected to excite the wonder of the illiterate, but as contributions to the materials from which it will be practicable to reconstruct by analogy and strict deduction, the history of the past in its relation to the present."*

Two years later he reported: "The collection of objects to illustrate anthropology now in possession of the Institution is almost unsurpassed, especially in those which relate to the present Indians and the more ancient inhabitants of the American continent." Deprecating the frequent dissipation of small private collections of such objects at the death of their owners, he forcibly urges that "the only way in which they can become of real importance, is by making them part of a general collection, carefully preserved in some public institution, where in the course of the increasing light of science, they may be made to reveal truths beyond present anticipation."†

In his last Report—for 1877,—just published (and which he did not live to see in print), he says: "Anthropology, or what may be considered the natural history of man, is at present the most popular branch of science. It absorbs a large share of public attention, and many original investigators are assiduously devoted to it. Its object is to reconstruct as it were the past history of man, to determine his specific peculiarities and general tendencies. It has already established the fact that a remarkable similarity exists in the archæological instruments found in all parts of the world, with those in use among tribes still in a savage or barbarous condition. The conclusion is supported by evidence which can scarcely be doubted, that by thoroughly studying the manners and customs of savages and the instruments employed by them, we obtain a knowledge of the earliest history of nations which have attained the highest civilization. It is remarkable in how many cases, customs existing among highly civilized peoples are found to be survivals of ancient habits." He then argues from the significance thus developed of many trivial practices and unmeaning ceremonies handed down from immemorial time, the importance to a full comprehension of the customs of modern society, of a scientific study of the myths and usages of ancient peoples. "American anthropology," he remarks, "early occupied the attention of the Smithsonian Institution;" and alluding to its first published work, he says "from the time of the publication of this volume until the present, contributions of value have been made annually by the

* *Smithsonian Report* for 1868, pp. 26 and 33.
† *Smithsonian Report* for 1870, pp. 35, 36.

Institution to this branch of knowledge. . . . The collection of the archæology and ethnology of *America*, in the National Museum, is the most extensive in the world: and in order to connect it permanently with the name of Smithson, it has been thought advisable to prepare and publish at the expense of the Smithsonian fund, an exhaustive work on American anthropology, in which the various classes of specimens shall be figured and described."* This great work still remains to be perfected.

Publications.—To attempt the recapitulation of the various branches of original research initiated or directly fostered by the Institution, would be to write its history. The range and variety of its active operations, and the value of their fruits, are in view of the limited income, and the collateral drains of less important objects exacted from it, something quite surprising. Scarcely a department of investigation has not received either directly or indirectly liberal and efficient assistance: and a host of physicists in the successful prosecution of their diverse labors, have attested their gratitude to the Institution, and no less to the ever sympathetic encouragement of its Director.

Over one hundred important original Memoirs, generally too elaborate to be published at length by any existing scientific society, issued in editions many times larger than the most liberal of any such society's issue, most of them now universally recognized as classical and original authorities on their respective topics, forming twenty-one large quarto volumes of "SMITHSONIAN CONTRIBUTIONS TO KNOWLEDGE," distributed over every portion of the civilized or colonized world, constitute a monument to the memory of the founder, James Smithson, such as never before was builded with the outlay of one hundred thousand pounds: and before which the popular Lyceums of our leading cities, with endowments averaging double this amount, pale into insignificance.

Such as these Lyceums with their local culture, admirable and invaluable in their way, but exerting no influence upon the progrèss of science, or outside of their own communities, and scarcely known beyond their cities' walls,—such was the type of institute which early legislators could alone imagine. Such as the "Smithsonian Institution" stands to-day,—such is the monument mainly constructed by the foresight, the wisdom, and the resolution of Henry.† All honor to the Regents, who with

* *Smithsonian Report* for 1877, pp. 22, 23. Circulars broadly distributed by the Institution, have served to give desired direction to popular attention and activity in this field of research; and the extent of coöperation is such as probably only the "Smithsonian" could have secured, unless by a vastly greater outlay.

† "It is not by its castellated building, nor the exhibition of the museum of the Government, that the Institution has achieved its present

an enlightenment so far in advance of the ruling intelligence of former days, and against the pressures of overwhelming preponderance of even educated popular sentiment, courageously adopted the programme of the Secretary and Director they had appointed; and who throughout his career, so wisely, nobly, and steadfastly upheld his policy and his purpose.

Fifteen octavo volumes of "Smithsonian Miscellaneous Collections" of a more technical character than the "Contributions," (including systematic and statistical compilations, scientific summaries, and valuable accessions of tabular "constants,") form in themselves an additional series; and represent a work of which any learned Society or Institution might well be proud. And thirty octavo volumes of annual Reports, rich with the scattered thoughts and hopes and wishes of the Director, form the official journal of his administration.

The Bibliography of Science.—Among the needful preparations for conducting original inquiry, none is more important than ready access and direction to the existing state of research in the particular field, or its allied districts. This information is scattered in the thousands of volumes which form the transactions of learned Societies; and its acquisition involves therefore in most cases a very laborious preliminary bibliographical research. To make this vast store of observation available to scientific students, by the directory of well arranged digests, would appear to fall peculiarly within the province of an Institution specially established for promoting the increase and diffusion of knowledge among men: and was early an object of particular interest to Henry. In his Report for 1851, he remarked: "One of the most important means of facilitating the use of libraries (particularly with reference to science,) is well digested indexes of subjects, not merely referring to volumes or books, but to memoirs, papers, and parts of scientific transactions and systematic works. As an example of this, I would refer to the admirably arranged and valuable catalogue of books relating to Natural Philosophy and the Mechanic Arts, by Dr. Young. This work comes down to 1807; and I know of no richer gift which could be bestowed upon the science of our own day, than the continuation of this catalogue to the present time. Every one who is desirous of enlarging the

reputation; nor by the collection and display of material objects of any kind, that it has vindicated the intelligence and good faith of the Government in the administration of the trust. It is by its explorations, its researches, its publications, its distribution of specimens, and its exchanges, constituting it an active living organization, that it has rendered itself favorably known in every part of the civilized world; has made contributions to almost every branch of science; and brought, more than ever before, into intimate and friendly relations, the Old and the New Worlds." (Memorial to Congress, by Chancellor S. P. Chase, and Secretary Joseph Henry. *Smithsonian Report*, for 1867, p. 114.)

bounds of human knowledge, should in justice to himself as well as to the public, be acquainted with what has previously been done in the same line; and this he will only be enabled to accomplish by the use of indexes of the kind above mentioned."*

At the time, and for years afterward, one-half of the Smithsonian income was diverted by the requirements of Congress to the local objects of the Lyceum: and the hopelessness of attempting a work—additional to that already mapped out, which would require the united labors of a large corps of well-trained and educated assistants for many years, and the subsequent devotion of the whole available income for many years following, to complete its publication, was fully realized. The project however was not abandoned: and in 1854, Henry conceived the plan of taking up the more limited department of *American* Scientific Bibliography; and by the persevering application of a fixed portion of the income annually for a succession of years, of finally producing a thorough subject-matter index, as well as an index of authors, for the entire range of American contributions to science from their earliest date. Inspired with this ambition, he sought to enlist the co-operation of the British Association for the Advancement of Science, in procuring with its large resources, a similar classified index for British and European scientific literature.

The favorable reception of this project, was officially announced to Henry by the Secretary of the Association, in the transmission of the following extract from the proceedings of that body for 1855. "A communication from Professor Henry of Washington having been read, containing a proposal for the publication of a catalogue of philosophical memoirs scattered throughout the Transactions of Societies in Europe and America, with the offer of co-operation on the part of the Smithsonian Institution, to the extent of preparing and publishing in accordance with the general plan which might be adopted by the British Association, a catalogue of all the American memoirs on physical science,—the Committee approve of the suggestion, and recommend that Mr. Cayley, Mr. Grant, and Professor Stokes be appointed a committee to consider the best system of arrangement, and to report thereon to the council."† The report of this committee dated 13th June, 1856, was presented to the succeeding Meeting of the British Association; in which they take occasion to say: "The Committee are desirous of expressing their sense of the great importance and increasing need of such a catalogue. . . The catalogue should not be restricted to memoirs in Transactions of Societies, but should comprise also memoirs in the Proceedings of Societies, in mathematical and scientific journals:"

Smithsonian Report for 1851. p. 225 (of Sen. ed.). p. 217 (of H Rep. ed.)
Report Brit. Assoc. Glasgow, Sept. 18 5, p. lxvi.

etc. . . "The catalogue should begin from the year 1800. There should be a catalogue according to the names of authors, and also a catalogue according to subjects."* The Committee comprising Fellows of the Royal Society of London finally succeeded in interesting that grave body in the undertaking: and the result was that greatly to Henry's satisfaction, the entire work was ultimately assumed by the Royal Society itself.

In the course of ten years that liberal Society aided by a large grant from the British Government gave to the world its half instalment of the great work, in its admirable "Catalogue of Scientific Papers" alphabetically classified by authors, in seven or eight large quarto volumes. In the Preface to this splendid monument of industry and liberality, stands the following history of its inception. "The present undertaking may be said to have originated in a communication from Dr. Joseph Henry, Secretary of the Smithsonian Institution, to the Meeting of the British Association at Glasgow in 1855, suggesting the formation of a catalogue of Philosophical memoirs: this suggestion was favorably reported on by a Committee of the Association in the following year. . . . In March, 1857, General Sabine, the Treasurer and Vice-President of the Royal Society, brought the matter before the President and Council of that body, and requested on the part of the British Association, the co-operation of the Royal Society in the project: whereupon a committee was appointed to take into further consideration the formation of such a Catalogue. . . . No further step was taken by the British Association or by the Royal Society in co-operation with that body: but the President and Council of the Royal Society acting on the recommendations contained in a Report of the Library Committee dated 7th January, 1858, resolved that the preparation of a Catalogue of scientific memoirs should be undertaken by the Royal Society independently, and at the Society's own charge."†

System of Exchanges.—For the diffusion of knowledge among men, one of the methods adopted by Henry from the very commencement of his administration was the organization of a system by which the scientific memoirs of Societies or of individuals from any portion of the United States, might be transmitted to foreign countries without expense to the senders: and by which

* *Report Brit. Assoc.* Cheltenham, Aug. 1856, pp. 463, 464.

† Preface to *Catalogue of Scientific Papers*, (1800–1863) vol. i. 1867, pp. iii., iv. The second and most important division of this great and invaluable work,—the classified Index to Subjects,—still remains to be accomplished. Had the plan adopted been made to include the scientific memoirs of the two preceding centuries, the value of the compilation would have been enhanced in a far greater proportion than the additional expenditure or the increase of bulk.

in like manner the similar publications of scientific work abroad might be received at the Smithsonian Institution, for distribution in this country. This privilege however is properly restricted to *bona fide* donations and exchanges of scientific memoirs;—all purchased publications being carefully excluded and left to find their legitimate channels of trade. By an international courtesy —creditable to the wisdom and intelligence of the civilized Powers,—such packages to and from the Institution are permitted to pass through all Custom-houses, free of duty; an invoice of authentication being forwarded in advance. When it is considered that this large work of collection and distribution (including the constant supply of the Institution's own publications, and the extensive returns therefor of journals, proceedings, and transactions, for its own library) requires the systematic records and accounts in suitable ledgers, with the accurate parcelling and labelling of packages, large and small, to every corner of the globe, it may well be conceived that no small amount of labor and expense is involved in these forwarding operations.* A recognition of the benefits conferred by this generous enterprise, is practically indicated by the rapid enlargement of the operations. The weight of matter sent abroad by the Institution at the end of the first decade, was 14 thousand pounds for the year 1857: the weight sent at the end of the second decade, was 22 thousand pounds for the year 1867: and the weight sent at the end of the third decade, was 99 thousand pounds for the last year 1877. This admirable system has been greatly encouraged and facilitated by the most praiseworthy liberality of the great lines of ocean steamers, and of the leading railway companies, in carrying the Smithsonian freight in many cases free of charge, or in other cases at greatly reduced rates: an appreciative tribute alike to the beneficent services and reputation of the Institution, and to the personal character and influence of its Director.†

* It may be stated that the number of foreign institutions and correspondents receiving the Smithsonian publications exceeds two thousand; whose localities embrace not only the principal cities of Europe (from Iceland to Turkey), of British America, Mexico, the West Indies, Central and South America, and of Australia, but also those of New Zealand, Honolulu in the Sandwich Islands, twelve cities in India, Shanghai in China, Tokio, Yedo, and Yokohama, in Japan, Batavia in Java, Manilla in the Philippine Islands, Alexandria and Cairo in Egypt, Algiers in northern Africa, Monrovia in Liberia, and Cape Town in southern Africa. The co respondents and recipients in the United States, are probably nearly as numerous.

† "The cost of this system would far exceed the means of the Institution, were it not for important aid received from various parties intereste l in facilitating international intercourse and the promotion of friendly relations between distant parts of the civilized world. The liberal aid extended by the steamship and other lines, mentioned in previous reports, in carrying the boxes of the Smithsonian exchanges free of charge, has been

"This part of the system of Smithsonian operations has everywhere received the commendation of those who have given it their attention or have participated in its benefits. The Institution is now the principal agent of scientific and literary communication between the old world and the new. . . . The importance of such a system with reference to the scientific character of our country, could scarcely be appreciated by those who are not familiar with the results which flow from an easy and certain intercommunication of this kind. Many of the most important contributions to science made in America have been unheard of in Europe, or have been so little known, or received so little attention, that they have been republished as new discoveries or claimed as the product of European research."* It would indeed be difficult to estimate rightly the benefit to science in the encouragement of its cultivators afforded by this fostering service. Few Societies are able to incur much expense in the distribution of their publications; and hence their circulation is necessarily very limited. The fructifying interchange of labors and results, dependent on their own resources, would be obstructed by the recurring expenses and delays of customs interventions, and by unconscionable exactions: and indeed without the Smithsonian mechanism, nine-tenths of the present scientific exchanges would be at once suppressed. Let it be hoped that so beneficent a system will not break down from the weight of its own inevitable growth.

Astronomical Telegraphy.—Analogous in principle to the system of exchange, is that adopted for the instantaneous trans-Atlantic communication of discoveries of a special order. In the year 1873, in the interests of astronomy (to which Henry was ever warmly devoted) he concluded "a very important arrangement between the Smithsonian Institution and the Atlantic cable Companies, by which is guaranteed the free transmission by telegraph between Europe and America of accounts of astronomical discoveries which for the purpose of co-operative observation require immediate announcement."† This admirable service to science, so creditable to the intelligence and the liberality of the Atlantic Telegraph Companies, embraces direct reciprocal communication between the Smithsonian Institution, and the foreign Observatories of Greenwich, Paris, Berlin, Vienna, and Pulkova. During the first year of its operation, four new planetoids were tele-

continued, and several other lines have been added to the number in the course of the year." (*Smithsonian Report* for 1867, p. 39.) Notwithstanding this unprecedented generosity, the exchange system has reached such proportions as to require for its maintenance one-fourth of the entire income from the Smithsonian fund.

* *Smithsonian Report* for 1853, p. 25, (of Senate ed.)

† *Smithsonian Report* for 1873, p. 32.

graphed from America, and seven telescopic comets from Europe to this country.

"Although the discovery of planets and comets will probably be the principal subject of the cable telegrams, yet it is not intended to restrict the transmission of intelligence solely to that class of observation. Any remarkable solar phenomenon presenting itself suddenly in Europe, observations of which may be practicable in America several hours after the sun has set to the European observer,—the sudden outburst of some variable star similar to that which appeared in *Corona borealis* in 1866,—unexpected showers of shooting-stars, etc., would be proper subjects for transmission by cable.

"The announcement of this arrangement has called forth the approbation of the astronomers of the world : and in regard to it we may quote the following passage from the fifty-fourth annual report of the Royal Astronomical Society of England: 'The great value of this concession on the part of the Atlantic telegraph and other Companies, cannot be too highly prized, and our science must certainly be the gainer by this disinterested act of liberality. Already planets discovered in America have been observed in Europe on the evening following the receipt of the telegram, or within two or three days of their discovery ' "*

Official Correspondence.—A vast amount of individual work having in view the diffusion of knowledge, has been performed by the correspondence of the Institution; which may be best described in the language of an extract from one of the early Reports. "There is one part of the Smithsonian operations that attracts no public attention, though it is producing important results in the way of diffusing knowledge, and is attended perhaps with more labor than any other part. I allude to the scientific correspondence of the Institution. Scarcely a day passes in which communications are not received from persons in different parts of the country, containing accounts of discoveries, which are referred to the Institution, or asking questions relative to some branch of knowledge. The rule was early adopted to give respectful attention to every letter received, and this has been faithfully adhered to from the beginning up to the present time. . . . Requests are frequently made for lists of apparatus, for information as to the best books for the study of special

* *Smithsonian Report* for 1873, p. 33. In 1876, a stellar outburst in the "Swan" observed by Dr. Schmidt of Athens, on the 24th of November, was announced. Less brilliant than the similar outburst which occurred in the northern "Crown" in May 1866, it continued to decline through the month of December, and at the close of the year, had dwindled from the third to the eighth magnitude. This may possibly be the same "temporary star"—seen in *Cygnus* in 1600, and again in 1670: and having therefore a period of variability of about 69 years.

subjects, for suggestions on the organization of local societies, etc. Applications are also made for information by persons abroad, relative to particular subjects respecting this country. When an immediate reply cannot be given to a question, the subject is referred by letter to some one of the Smithsonian co-laborers to whose line of duty it pertains, and the answer is transmitted to the inquirer, either under the name of the person who gives the information, or under that of the Institution, according to the circumstances of the case. . . . Many of those communications are of such a character, that at first sight it might seem best to treat them with silent neglect; but the rule has been adopted to state candidly and respectfully the objections to such propositions, and to endeavor to convince their authors that their ground is untenable. Though this course is in many cases attended with no beneficial results, still it is the only one which can be adopted with any hope of even partial good."*

The information given to scientific inquirers has been of an exceedingly varied and highly valuable character, not unfrequently involving a large amount of research from special experts; who have been accustomed cheerfully to bestow a degree of attention on difficult questions thus presented, which would have been accorded perhaps less ungrudgingly to others than to the universally honored Smithsonian Director. As to the pretensions and importunities of the unscientific,—such is the judgment pronounced after a quarter of a century of laborious experience with them:

"The most troublesome correspondents are persons of extensive reading, and in some cases of considerable literary acquirements, who in earlier life were not imbued with scientific methods, but who not without a certain degree of mental power, imagine that they have made great discoveries in the way of high generalizations. Their claims not being allowed, they rank themselves among the martyrs of science, against whom the scientific schools and the envy of the world have arrayed themselves. Indeed to such intensity does this feeling arise in certain persons, that on their special subjects they are really monomaniacs, although on others they may be not only entirely sane, but even evince abilities of a high order. . . . Two persons of this class have recently made a special journey to Washington, from distant parts of the country, to demand justice from the Institution in the way of recognition of their claims to discoveries in science of great importance to humanity; and each of them has made an appeal to his Representative in Congress to aid him in compelling the Institution to acknowledge the merits of his speculations. Providence vindicates in such cases the equality of its justice in giving to such persons an undue share of self-esteem

* *Smithsonian Report* for 1853, pp. 22, 23, (of Senate ed.)

and an exaltation of confidence in themselves, which in a great degree compensate for what they conceive to be the want of a just appreciation of the public. Unless however they are men of great benevolence of disposition, who can look with pity on what they deem the ignorance and prejudice of leaders of science, they are apt to indulge in a bitterness of denunciation which might be injurious to the reputation of the Institution, were their effects not neutralized by the extravagance of the assertions themselves."*

To the projectors and propellers of Paine electric engines, and Keely motors, eager for a marketable certificate from such an authority, Henry would calmly reply: "We may say that science has established the great fact—without the possibility of doubt, that what is called power, or that which produces changes in matter, cannot be created by man, but exists in nature in a state of activity or in a condition of neutralization; and furthermore that all the original forces connected with our globe, as a general rule have assumed a state of permanent equilibrium, and that the crust of the earth as a whole (with the exception of the comparatively exceedingly small proportion, consisting of organic matter such as coal, wood, etc.) is as it were a burnt slag, incapable of yielding power; and that all the motions and changes on its surface are due to actions from celestial space, principally from the sun. . . . All attempts to substitute electricity or magnetism for coal power must be unsuccessful, since these powers tend to an equilibrium from which they can only be disturbed by the application of another power, which is the equivalent of that which they can subsequently exhibit. They are however, with chemical attraction, etc., of great importance as intermediate agents in the application of the power of heat as derived from combustion. Science does not indicate in the slightest degree, the possibility of the discovery of a new primary power comparable with that of combustion as exhibited in the burning of coal. Whatever unknown powers may exist in nature capable of doing work, must be in a state of neutralization, otherwise they would manifest themselves spontaneously; and from this state of neutralization or equilibrium, they can be released only by the action of an extraneous power of equivalent energy; and we therefore do not hesitate to say that all declarations of the discovery of a new power which is to supersede the use of coal as a motive-power, have their origin in ignorance or deception, and frequently in both. A man of some ingenuity in combining mechanical elements, and having some indefinite scientific knowledge, imagines it possible to obtain a certain result by a given combination of principles, and by long brooding over this sub-

* *Smithsonian Report* for 1875, pp. 37, 38.

ject previous to experiment, at length convinces himself of the certainty of the anticipated result. Having thus deceived himself by his sophisms, he calls upon his neighbors to accept his conclusions as verified truths; and soon acquires the notoriety of having made a discovery which is to change the civilization of the world. The shadowy reputation which he has thus acquired, is too gratifying to his vanity to be at once relinquished by the announcement of his self-deception; and in preference he applies his ingenuity in devising means by which to continue the deception of his friends and supporters, long after he himself has been convinced of the fallacy of his first assumptions. In this way what was commenced in folly, generally ends in fraud."*

In looking back upon the struggles, conflicts, and obstructions of the past, it really seems quite marvellous that so much should have been accomplished, with so limited expenditure. These large results are partly due to the admirable method of the Secretary, his clear presage of effects, and his high power of systematic distribution and appliance; partly to the intelligent zeal and sympathetic energy of the able assistants whom he had associated with him almost from the organization of the institution; and partly to the personal magic of the man,—to the surprising amount of voluntary co-operation he was able to call forth in almost every direction, by the sheer force of his own earnest industry, and the contagious influence of his own devotion to the cause of scientific advancement.

An unwarranted Arraignment.—In 1855, while still with quiet determination maintaining the fundamental purpose of his Smithsonian administration against the pressure and opposition of powerful influences, (the discussion having been even carried to the halls of Congress,) Henry was made the subject of a most wanton and ungrateful public attack, in a pamphlet by Prof. Morse of Telegraph fame, impugning not only his scientific reputation, but for the first time—the truthfulness of his testimony given in certain telegraph suits some half a dozen years previously, in reluctant obedience to legal summons.† This testimony thus exacted, of course failed to sustain the complainant's exorbitant claims to the absolute invention and ownership of all possible forms of the electro-magnetic telegraph,

* *Smithsonian Report* for 1875, pp. 39, 40.

† The Hon. S. P. Chase, while Governor of Ohio, (subsequently Chief Justice of the Supreme Court of the United States,) in a letter dated Columbus, Nov. 26, 1856, after reciting his professional connection with the litigations of 1849, says: "I remember very well that you were unwilling to be involved in the controversy even as a witness, and that you only submitted to be examined in compliance with the requirements of law. Not one of your statements was volunteered."

and correspondingly failed to satisfy the cupidity of the actual prosecutors: and in this remarkable accusation first published in 1855, could readily be discerned the mercenary inspiration of interested capitalists and assignees—anxious only to stretch the monopoly to its extremest grasp. To Prof. Morse himself in his early efforts, Henry had generously rendered every encouragement and assistance; and in his later successes had as freely extended his congratulations and his testimonials of the practical merits of his invention.*

To descend to a personal controversy with Mr. Morse, was utterly repugnant to Henry's feelings: to permit his serious impeachment to stand untraversed, appeared scarcely less objectionable. With a calm and self-respecting dignity, Henry simply presented the published arraignment to the Board of Regents, for their consideration and action, with a communication dated March 16, 1857, in the following terms:

"Gentlemen: In the discharge of the important and responsible duties which devolve upon me as Secretary of the Smithsonian Institution, I have found myself exposed like other men in public positions to unprovoked attack and injurious misrepresentation. Many instances of this it may be remembered, occurred about two years ago, during the discussions relative to the organic policy of the Institution: but though very unjust, they were suffered to pass unnoticed; and generally made I presume no lasting impression on the public mind. During the same controversy however, there was one attack made upon me of such a nature, so elaborately prepared and widely circulated by my opponents, that though I have not yet publicly noticed it, I have from the first thought it my duty not to allow it to go unanswered. I allude to an article from the pen of Prof. S. F. B. Morse, the celebrated inventor of the American electro-magnetic telegraph. In this, not my scientific reputation merely, but my moral character was pointedly assailed: indeed nothing less was attempted than to prove that in the testimony which I had given in a case where I was at most but a reluctant witness, I had consciously and wilfully deviated from the truth, and this too from unworthy and dishonorable motives. Such a charge, coming from such a quarter, appeared to me then as it appears now, of too grave a character and too serious a consequence, to be withheld from the notice of the Board of

* "It was my wish in every statement to render Mr. Morse full and scrupulous justice. While I was constrained therefore to state that he had made no discoveries in science, I distinctly declared that he was entitled to the merit of combining and applying the discoveries of others in the invention of the best practical form of the magnetic telegraph. My testimony tended to establish the fact that though not entitled to the exclusive use of the electro-magnet for telegraphic purposes, he was entitled to his particular machine, register, alphabet, etc. This however did not meet the full requirements of Mr. Morse's comprehensive claim."

Regents: . . . and I now embrace the first opportunity of bringing the subject officially to your notice, and asking from you an investigation into the justice of the charges alleged against me. And this I do most earnestly, with the desire that when we shall all have passed from this stage of being, no imputation of having attempted to evade in silence so grave a charge shall rest on *me*,—nor on *you*, of having continued to devolve upon me duties of the highest responsibility, after that was known to some of you individually, which if true—should render me entirely unworthy of your confidence. Duty to the Board of Regents, as well as regard to my own memory, to my family, and to the truth of history, demands that I should lay this matter before you, and place in your hands the documents necessary to establish the veracity of my testimony so falsely impeached, and the integrity of my motives so wantonly assailed."*

Professor Felton, President of Harvard University, Chairman of the select Committee appointed by the Regents to investigate the charge, after a careful examination of all the documentary evidence, submitted a full report, from which it is only necessary to make the following extracts with reference to the Morse pamphlet: "The first thing which strikes the reader of this article is that its title is a misnomer.† It is simply an assault upon Professor Henry; an attempt to disparage his character; to deprive him of his honors as a scientific discoverer; to impeach his credibility as a witness and his integrity as a man. It is a disingenuous piece of sophistical argument, such as an unscrupulous advocate might employ to pervert the truth, misrepresent the facts, and misinterpret the language in which the facts belonging to the other side of the case are stated. . . . Your committee come unhesitatingly to the conclusion that Mr. Morse has failed to substantiate any one of the charges he has made against Professor Henry, although the burden of proof lay upon him; and that all the evidence—including the unbiased admissions of Mr. Morse himself—is on the other side. Mr. Morse's charges not only remain unproved, but they are positively disproved." And the committee submitted resolutions of condemnation on the one side, and of respect and confidence on the other, which were unanimously adopted by the Regents, and made a part of the permanent record.‡

* *Smithsonian Report* for 1857, pp. 85, 86.

† "A Defence against the injurious deductions drawn from the Deposition of Professor Joseph Henry, by Samuel F. B. Morse."

‡ *Smithsonian Report* for 1857, pp. 89-98. When Prof. Morse in his letter to Prof. Sears C. Walker, dated Washington, Jan. 1st, 1848, wrote thus: "To Prof. Henry is unquestionably due the honor of the discovery of a fact in science which proves the practicability of exciting magnetism through a long coil or at a distance, either to deflect a needle or magnetize soft iron;" and when again some six years later, the same Prof. Morse in his pamphlet dated Locust Grove, New York, December, 1853,

Scientific Observations.—One of the objects very dear to Henry's heart, was the establishment of a physical observatory (with a physical laboratory in connection) for the systematic observation and record of important points in celestial and terrestrial physics. For the proper maintenance of such an establishment, he thought an income as large as that of the Smithson fund, would not be too large: and on two different occasions he endeavored to enlist the interest of wealthy and public-spirited citizens in such an enterprise. One of these was Mr. McCormick of Virginia; and a letter on the subject was afterward printed (without its address) in the Report for 1870.* The other was Mr. Lick of California: who after some hesitation, decided in favor of an astronomical observatory. Another allied object of great interest to Henry—and one requiring as large an endowment—was a well-equipped chemical laboratory, in which—under judicious restrictions—those really engaged in original researches, should have liberal facilities of appliances and needed materials, furnished them. He considered that an important part of the work to be accomplished by a physical and chemical laboratory, would be the determination and tabulation of "The Constants of Nature and Art" with a much wider range of subject, and on a scale of much greater completeness and accuracy, than had heretofore been attempted: and thus might be realized the great work or works of reference, suggested by Babbage as a scientific *desideratum.*† Had the Smithsonian fund been twice as large as it is, both these great enterprises for the increase of knowledge, would undoubtedly have been successfully inaugurated by Henry.

Loss by Fire.—Early in the year 1865, (on the 24th day of January,) the central portion of the Smithsonian Building suffered from a disastrous fire, the effects of which were aggravated by the extreme severity of the winter cold, which greatly obstructed the efficiency of the engines brought into action.‡ "The progress of the fire was so rapid, that but few of the contents of the upper rooms could be removed before the roof fell in. The conflagration

wrote: "I shall show that I am not indebted to him for any discovery in science, bearing on the Telegraph:" (p. 9,)—it may be confidently assumed from his known but singular unfamiliarity with scientific literature, that equally in either case he but echoed the promptings of others, and equally in either case in entire ignorance of the real facts. To his dying day, he probably sincerely failed to apprehend the nature of his indebtedness to Henry.

* *Smithsonian Report* for 1870, pp. 141–144.

† *Smithsonian Report* for 1856, pp. 289–302.

‡ The accident resulted from the carelessness of some workmen in the upper picture gallery, who in temporarily setting up a stove, inserted the pipe through a wall-lining into a furring space (supposing it a flue), but which conducted directly under the rafters of the roof.

was only stayed by the incombustible materials of the main building:" the flooring of the upper story, forming an iron and brick vaulting over the lower or principal story. Neither wing of the building was reached by the fire; and the valuable Library (not then transferred to the Capitol), and the Museum, fortunately escaped without injury. The Stanley collection of Indian portraits, comprising about 200 paintings, and estimated as worth 20,000 dollars, was entirely destroyed. A fine full-sized copy in Carrara marble, by John Gott, of the antique statue known as "The Dying Gladiator," was crumbled into a formless mass of stone.

The Secretary's office unfortunately fell within the range of the flames. "The most irreparable loss was that of the records, consisting of the official, scientific, and miscellaneous correspondence; embracing 35,000 pages of copied letters which had been sent, (at least 30,000 of which were the composition of the Secretary,) and 50,000 pages of letters received by the Institution; the receipts for publications and specimens; reports on various subjects which have been referred to the Institution; the records of experiments instituted by the Secretary for the Government; four manuscripts of original investigations, [memoirs by collaborators,] which had been adopted by the Institution for publication; a large number of papers and scientific notes of the Secretary; a series of diaries, memorandum and account books."* This truly "irreparable loss" of the original notes of many series of experiments by Henry, of varied character, running back for thirty years, kept for the purpose of reduction and discussion, or further extension (as leisure might permit) and of which but few had been published even by results,—was borne by their author with his characteristic equanimity; and was very rarely alluded to by him, unless when in answer to inquiries respecting particular points of his researches, he was compelled to excuse the absence of precise data.

The Lecture Room—a model of its class—entirely burned out by the fire, was not reconstructed: but the space it occupied on the upper floor, was with the adjacent rooms (used as the apparatus room, and the art gallery) thrown into one large hall, 200 feet long,—at present occupied as the ethnological museum. Advantage was taken of the hazard demonstrated by the fire, to induce Congress in the following year to transfer the custody of the Smithsonian collection of scientific works to the National Library: and the propriety of this change was thus defended. "The east wing of the Smithsonian building, in which the books were deposited is not fire-proof, and is liable to destruction by accident or the torch of the incendiary, while the rooms of the Capitol are of incombustible materials. This wing was more-

* *Smithsonian Report* for 1865, p. 18.

over filled to overflowing; and a more extended and secure depository could not be obtained, except by another large draught on the accumulated funds intended to form part of the permanent capital."*

Second Visit to Europe.—At a meeting of the Board of Regents, held February 3rd, 1870, "General Delafield in behalf of the Executive Committee, stated that they deemed it highly important for the interests of the Institution in the promotion of science, and due to the Secretary for his long and devoted services, that he should visit Europe to consult with the savans and societies of Great Britain and the continent; and he therefore hoped that a leave of absence would be granted to Professor Henry for several months, and an allowance be made for his expenses. On motion of Dr. Maclean it was unanimously Resolved, That Professor Henry, Secretary of the Institution, be authorized to visit Europe in behalf of the interests of the Smithsonian Institution, and that he be granted from three to six months leave of absence, and two thousand dollars for travelling expenses for this purpose."†

It is not necessary here to recount the particulars of this second visit of Henry to Europe, more fully than in the brief account given by him in his annual Report. "Before closing this report, it is proper that I should refer to a resolution adopted by your honorable Board at its last session, granting me leave of absence to visit Europe to confer with savans and societies relative to the Institution, and making provision for the payment of my expenses. The presentation of this proposition was entirely without my knowledge, but I need scarcely say that its unanimous adoption was highly gratifying to my feelings; and that I availed myself of the privilege it offered with a grateful appreciation of the kindness intended. I sailed from New York on the 1st of June, returning after an absence of four and a half months, much improved in health, and with impressions as to science and education in the Old World, which may be of value in directing the affairs of the Institution. Although limited as to time, and my plans interfered with somewhat by the war, I visited England, Ireland, Scotland, Belgium, parts of Germany and France. But deferring for the present an account of my travels, and the observations connected with them, I will merely state that as your representative, I was everywhere kindly received, and was highly gratified with the commendations bestowed on the character and operations of the Institution intrusted to your care."‡

* *Smithsonian Report* for 1866, p. 14.
† *Smithsonian Report* for 1869, p. 89.
‡ *Smithsonian Report* for 1870, p. 45.

Service on the Light-house Board.—While the whole high bent of Henry's mind was rather toward abstract than utilitarian research, there was no well devised system of practical benefit for man, that did not command his earnest sympathy or enlist his active co-operation:—no labor in such co-operation from which he shrank, if he felt that without the sacrifice of other duties, he could make such labor useful. On the establishment of the Light-house Board, in 1852, Henry was appointed as one of its members; and although his valuable time was already fully occupied, he consented to serve on the Board, in the hope of aiding to benefit the interests of navigation. To the requirements of his new position, he brought his accustomed energy, skill, and eminently practical judgment; and soon made his influence felt throughout the Light-house service.

When the steadily advancing cost of whale oil made it necessary to seek for some more economical illuminant, he attacked the problem with his habit of scientific method. Colza oil or rape-seed oil had been used in France with some success; and efforts were made to introduce its culture and production in this country. Lard-oil had been tested by Professor J. H. Alexander of Baltimore, and pronounced by him of very inferior value as an illuminant. For accuracy of determination, Henry caused to be prepared at the Light-house Depot on Staten Island, a long dark fire-proof chamber, and had it painted black on all its interior surfaces for the purpose of photometric observations. In ordinary lamps, the colza oil was found to be nearly equal to whale oil in illuminating power, and lard-oil inferior to it. Petroleum or mineral oil was also tried; but its quality was at that time too variable, and its use was found to be too dangerous. Experiment showed that lard-oil had a greater specific gravity than sperm oil, a less capillarity or ascensional attraction in a wick, and a less perfect fluidity. The conditions were varied; and it was found that with elevation of temperature, the fluidity, and the capillarity, of the lard-oil increased more rapidly than those of the sperm oil, until at about 250° F. the former surpassed the latter in these qualities. With these results, it became important to compare the oils in large lamps, such as were actually required for the lanterns of light-houses. The heat evolved by the large sized Argand burners, would seem peculiarly to favor the lard-oil: a few trials, with a proper adaptation of the lamps, established its supremacy; and conclusively demonstrated—contrary to all the laboratory trials of former experimenters, that for the purpose desired, this contemned article was for equal quantities a more brilliant illuminant than mineral kerosene-oil, or vegetable colza-oil, or animal sperm-oil, while its market price was only about one-fourth that of the latter. Against all the opposition of interested dealers, and prejudiced keepers, the lard-oil was at once introduced into actual use in the years 1865 and 1866, in all the light-houses of the United States; with an economy of at least one dollar on every gallon of the hundred thousand in annual use; that is of 100,000 dollars per annum.

During the progress of these useful labors, no less important investigations were commenced, on the most efficient forms of apparatus for acoustic signalling, as the substitutes for light signals during the prevalence of sea-board fogs. "Among the impediments to navigation, none are perhaps more to be dreaded than those which arise from fogs. . . . 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."*

Gun signals were early abandoned, as inefficient, dangerous, and expensive: inefficient, because of both "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." Innumerable projects eagerly pressed upon the Board by visionary inventors (some of them being rattles, gongs, or organ pipes operated by manual cranks, many of them being varieties of automatic horn or whistle operated by the winds or the waves) were impartially tested, and uniformly rejected as wholly insufficient: very few of their projectors having the slightest practical idea of the requirements of the service. Experiments on steam-whistles of large size and on horns with vibrating steel tongues or reeds, sounded by steam-power, or by hot-air engines, varied and continued for several years under wide changes of conditions, finally determined their most efficient size and character †

In 1867, comparative trials were made at Sandy Hook (on the Jersey shore, at the entrance to Raritan Bay, and to New York Bay,) with three powerful instruments; a large steam whistle whose cup was 8 inches in diameter, and made adjustable in pitch; a large reed trumpet 17 feet long and 38 inches in diameter at its flaring mouth, whose steel tongue was 10 inches long, 2¾ inches wide, and half an inch thick at its smaller vibrating end, and was blown by a hot air engine; and lastly a large

* *Report of L. H. Board* for 1874, p. 83.

† An enterprising inventor had secured a patent for a metallic compound or alloy for steam-whistles, especially adapted to increase greatly their power as fog-signals. In vain was he assured that his "improvement" was a fallacy; that the cylindrical cup of the whistle was not a bell, but only a resonant chamber; and that its material was comparatively unimportant. He was only with difficulty convinced, when Henry had his whistle formally tested, with a stout cord wound tightly around its cylindrical surface: when its tone under steam escape was proved to be as full, as loud, and as penetrating, as with the cord removed.

siren horn operated by steam at different pressures, the aerial vibration being produced by the intermittence of a revolving grating or disk valve in the small end of the horn, driven at high velocities by the steam engine, and its pitch regulated by the adjustable speed of the revolving disk. The trumpet or fog-horn was provided with a series of replaceable steel tongues of different sizes, and the siren was driven at five different pitches of from 250 to 700 impulses per second, and at steam pressures varying from 100 pounds to 20 pounds per square inch. For the purpose of accurate estimation, within short distances, a phonometer or "artificial ear" was employed, having at its smaller upturned end a horizontal drum of stretched membrane, sprinkled with sand, after the plan devised by Sondhauss. Trumpets of the same size, were made of different materials, as of brass, iron, and wood; but these differences were found to exercise little or no influence on the intensity or penetration of the sound. Trumpets were also made of different shapes, straight and curved, and square as well as round, with equal lengths and equal areas of cross section; from whose trials it appeared that the conical form gave nearly double the distance of action on the sand of the "artificial ear," that was given by the pyramidal form. Such investigations—varied and long-continued, serve to show the conscientious earnestness with which Henry sought to give the highest efficiency to the expedients available for the protection of life and property along our extended sea-coast.

The steam-whistle was found to be less powerful than the trumpet, with the same expenditures of fuel. Steam whistles were afterwards tried of 10 inches, 12 inches, and 18 inches in diameter. The largest size was not found to give results proportioned to its increased consumption; and the 10 or 12 inch size was regarded as practically the most efficient. The siren was found to be the most powerful and penetrating of the instruments tested, as it admitted more advantageously the application of a higher steam expenditure. The best result with this instrument was attained with a pressure of from 60 to 80 pounds, and at a pitch between 350 and 400 vibrations per second. Under favorable conditions, this instrument frequently made itself heard at a distance of fifteen and twenty miles. Henry's large experience with the occasional aerial impediments to sound propagation,* and his strong sense of the vital importance of having fog-signals recognized at a distance, under the most adverse conditions, led him to favor the introduction of the most powerful sounders attainable, without absolutely limiting the decision to their relative economy. Hence he was the first to devise improvements in the siren, and to press its adoption at important or

* An abstract of his elaborate and invaluable researches on some abnormal phenomena of Sound—the crowning labor of his life, must be reserved for a concluding section.

dangerous stations, notwithstanding its higher consumption of steam or heat power.*

Partly under the stimulus given to the sale of lard-oil by the striking proofs of its excellence as an illuminant under favorable conditions, furnished by Henry, this article slowly advanced in price; though probably not to an extent of more than a fourth part additional cost. Henry's energies again were called into requisition to devise a remedy. Neither gas, nor electricity, the favorite means of numerous projectors and advisers, appeared justified, on the score of economy.† A new series of elaborate experiments was undertaken to determine whether mineral oil (so abundant as to be easily procurable at one-third the cost of lard oil) could not be made available. The great improvements introduced into its preparation in later years by high distillation, seemed to justify the attempt. Not only was a laborious inquiry into the best conditions of combustion, by precise photometric measurement required, but for the security of the service, equally laborious examinations into the best practicable methods of testing, of handling, and of storing this material.‡ To secure a

* Major G. H. Elliott, commissioned by the U. S. Light-house Board to make a tour of inspection of European Light-house establishments in 1873, in his Report published by the Senate in 1874. says of the British and French systems, "I saw many details of construction and administration which we can adopt to advantage, while there are many in which we excel. Our shore fog-signals particularly, are vastly superior both in number and power." (*Report on European Light-houses*, p. 12.) "To the careful and laborious investigations and experiments of the distinguished Chairman of the Light-house Board, prolonged through a series of years, and prosecuted under a great variety of conditions, is largely to be attributed the acknowledged superiority of our fog-signal service." (*Journal of Franklin Institute*, Jan. 1876, vol. lxxi. p. 43.)

† *Report of L. H. Board* for 1874, p. 11. No agency (for whatever purpose) has proved so enticing to the half-informed as *electricity*. For years past scarcely a month has elapsed without some new form of patent electric-light, or some marvellous application of electric-lights, being pertinaciously urged by sanguine "reformers" upon the Light-house Board for adoption; some of these ideal schemes being the mounting of electric-lights on buoys, or on the masts of light-ships, or their suspension from moored balloons. Many eminently original minds have earnestly desired to obtain contracts for supplying all the Light-houses with oxy-hydrogen lime lights. In a fog, the most powerful electric-light is as useless as the cheapest kerosene lamp.

‡ "It has been established that the ordinary fire-test is insufficient as usually applied, and that an explosive mixture may be formed by confining the vapors given off at a temperature in some cases twenty degrees lower than that certified to by the public inspector. That this inquiry is of great practical importance to the Light-house system, must be evident when we reflect that means must be devised for testing the oil offered for acceptance in accordance with contracts; for storing it: for transporting it to light-house stations; for preserving it in butts at the stations; and for the instruction of the keepers in its daily use." (*Report L. H. B.* 1877, p. 5.)

proper oxygenation in burning, a modification of the lamp was required. "It was soon apparent that the use of mineral oil would necessitate a change of lamps, and attention is now directed to the perfection of one which will produce the best results from this illuminant. It is thought that the lamps now used with lard-oil can be converted at no great expense and successfully used with mineral oil. Our experiments have shown that this oil can be more readily used in the smaller lamps; and it is proposed as soon as suitable ones can be prepared, to put it into use at such stations of the fifth and sixth order, as may be thought expedient; when if it be found satisfactory, an attempt will be made to substitute it for lard-oil in lamps of the higher orders."* "This change is proposed entirely with reference to economy; for it has been found by repeated experiment, that while a somewhat superior light may be obtained from a small lamp charged with kerosene, a larger lamp charged with lard-oil affords the greater illuminating power. So great is this difference in lamps of the first order with five wicks, that the rates of light from kerosene and lard, are as three to four respectively. Since the safety of the keeper and the continuity of the light are essential elements in the choice of an illuminant, a thorough acquaintance with the nature of the substance is essentially necessary. With a view therefore to the introduction of kerosene, a series of experiments have been made during the last two years on the different varieties of this material found in the market."†

In 1871, on the resignation of Admiral Shubrick, Henry was chosen as the Chairman of the Light-house Board; and his energetic labors in behalf of the service, fully vindicated the wisdom of the choice. Punctual in his attendance on the weekly meetings of the Board, he inspired others with a portion of his own zealous devotion. Nor did he fail to urge upon the Government the constant need and responsibility of maintaining an efficient establishment. He emphatically declared that "the character of the aids which any nation furnishes the mariner in approaching and leaving its shores, marks in a conspicuous degree its advancement 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. . . . 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

* *Report L. H. Board*, 1875, p. 6.
† *Report L. H. Board*, 1877, p. 4.

trustworthy attendants."* And rising to a higher argument, he pointed out that "it is not alone in its economical aspect that a light-house system is to be regarded: it is a life preserving establishment founded on the principles of Christian benevolence, of which none can so well appreciate the importance as he who after having been exposed to the perils of the ocean—it may be for months—finds himself approaching in the darkness of night a lee shore. But it is not enough to erect towers, and establish other signals: they must be maintained in an efficient state with uninterrupted constancy."†

A formal report made to the Hon. Secretary of the Treasury by the Naval Secretary of the Light-house Board, dated May 21st, 1878, (very shortly after Henry's death,) simply detailing for information, the character of his gratuitous services to the Light-house establishment during a quarter of a century, (and not intended for the public,) takes the inevitable form of eulogy. A portion of it is here quoted.

"As chairman of this committee, Professor Henry acted as the scientific adviser of the Board. But in addition it was his duty to conduct the experiments made by the Board, not only in the matter of original investigation and testing of the material used, but in examining and reporting on the models, plans, and theories presented by others to the Board. The value of the services he rendered in this position is simply inestimable. He prepared the formula for testing our oils; he conducted the series of experiments resulting in the substitution of lard-oil for sperm oil, which effected an immense saving in cost; and he also conducted the experiments which have resulted in making it possible to substitute mineral oil for lard-oil, when another economy will be made. His original investigation into the laws of sound have resulted in giving us a fog-signal service conceded to be the best in the world. His examinations into the action of electricity, has enabled the Board to almost completely protect its stations from the effect of lightning. The result of his patient, continuous, practical experimentation is visible everywhere in the

* *Report of L. H. Board*, 1873, pp. 3, 4. The coast line of the United States is far more extended than that of any other nation on the globe. "The magnitude of the Light-house system of the United States may be inferred from the following facts: 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 5,000 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. 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." (p. 4, of same Report.)

† *Report L. H. B.* 1874. p. 5.

service. No subject was too vast for him to undertake; none too small for him to overlook. And while he has brought into the establishment so many practical applications of science, he has done almost as much service by keeping out what presented by others seemed plausible, but which on examination proved impracticable.

"Every theory, plan, or machine, which was pressed on the Board, as for the interests of commerce and navigation, was referred to the committee on experiments, when it was examined by its Chairman, and was formally reported upon. If it had no practical value, the report on record simply stated the inexpediency of its adoption: but the Professor often verbally pointed out to the presenter its fallacy; and sent him away—if not satisfied—at least feeling that he had been well treated. He thus prevented not only the adoption of impracticable plans, but avoided the enmity of their inventors.

"Professor Henry made many valuable reports, containing the results of his elaborate experiments into matters which were formally referred to him, which are spread on the records of the Board; and the reports were drawn in such form that his suggestions were capable of and received practical application. But in addition to this, he was constantly extending his scientific researches for the benefit of the service in all directions. His summer vacations were as a rule passed in experimentation at the laboratory of the Establishment at Staten Island, on its steamers, or at its light-stations, pushing his inquiries to their last results. To experimentation in the interests of this service, Professor Henry seemed to give his whole heart. It appeared as if he never lost sight of the needs of the Establishment, and as if he never neglected an opportunity to advance its interests. In addition to his other duties, Professor Henry presided as Chairman of the Light-house Board for the last seven years at its weekly meetings, when he did much to infuse into the different members of the Board, his own spirit of labor for, and devotion to its interests."*

Services to the National Government.—The value of Henry's services to the various Executive Departments of our Government, faithfully and unostentatiously performed through a long series of years and a succession of Presidential Administrations, cannot be estimated, as its history can never be written. Whatever material for it existed in the form of abstracts of inquiries, trials, and reports, prior to 1865, unfortunately perished in the

* *Executive Documents*, No. 94, Forty-fifth Congress, 2d Session, Senate, pp. 2, 3. It is gratifying to know that on the presentation of his report and recommendation to Congress, by the high-minded Secretary of the Treasury, a moderate appropriation in slight recognition of Henry's "inestimable" services was at once passed for the benefit of his bereaved family.

fire of that year. Whenever in any important case a scientific adviser could be useful to the proper conduct of a Bureau, Henry's reputation generally pointed him out as the most suitable expert and arbiter. On the outbreak of the great civil war, the number of such references was naturally very considerably increased. The Departments of War, of the Navy, and of the Treasury, were besieged by projectors with every imaginable and impossible scheme for saving the country, and demolishing the enemy. Torpedo balloons, electric-light balloons, wonderful compounds destined to supersede gunpowder, and revolutionize the art of war; cheap methods for the manufacture of Government bonds and paper-money, multitudinous expedients for the prevention of counterfeiting, by devices in the engraving, by secret markings, by anti-photographic inks, by peculiar textures of paper,—applicable to coupons, to circulating notes, to revenue stamps,—each warranted to be infallible,—such were among the agencies by which patriotic patentees and adroit adventurers were willing to serve their country and to reap their reward by the moderate royalty or percentage due to the magnificence of the public benefit. Such were among the unenviable tasks of examination and adjudication accepted by Henry, only from an intrepid sense of duty.

"The course which has been pursued of rendering the Government in its late trials, every aid which could be supplied by scientific research, has been warmly approved. As most persons are probably entirely ignorant of the services really rendered to the Government by the Institution, I may here state the fact that a large share of my time, (all indeed which could be spared from official duties,) has been devoted for the last four years to investigations required by the public exigencies. Within this period, several hundred reports, requiring many experiments, and pertaining either to proposals purporting to be of high national importance, or relating to the quality of the multifarious articles offered in fulfillment of legal contracts, have been rendered. The opinions advanced in many of these reports, not only cost much valuable time, but also involved grave responsibilities. While on the one hand the rejection of a proposition would be in contravention to the high importance claimed for it by its author, on the other the approval of it would perhaps incur the risk of the fruitless expenditures of a large amount of public money. It is not necessary, I trust, to say that the labor thus rendered was entirely gratuitous, or that in the judgment pronounced in any case, no regard was paid to the interested solicitations or personal influence of the parties concerned: on the contrary it has in some instances resulted from the examination of materials sold to the Government, that attempted fraud has been exposed, and the baffled speculator received his due reward in condemnation and punishment. These facts it is thought will be deemed a sufficient answer to those who have seemed disposed to reproach the Institution with the want of a more popular demonstration —but of a really far less useful or efficient aid in the support of the Government."*

In the performance of these troublesome and often disagreeable labors, conducted with the single aim necessitated by all his scientific habits and instincts, it of course resulted that a great majority of his judgments and recommendations were decidedly adverse to the hopes and wishes of the aspirants to fame and fortune. Having once satisfied himself of the frivolity or the chicanery of an article or project, his decision was inflexible; and although importunate appeals to the Department Secretary, abetted by a prostituted political or other influence, in one or two instances succeeded in fastening for a time upon the public Treasury a worthless or a noxious leech, the vast number of such, excluded from experimental imbibitions by Henry's critical supervision, must have been a protection to the public interests quite beyond the reach of estimation: while the supplies of honest contractors awarded their just commendation, and the rare proposals of real merit favorably reported upon, which from a hasty survey might have been confounded and overlaid with the mass of untried puerilities, no less served to strengthen and assist the Government during its years of greatest trial, need, and exhaustion.

From the outset of the unnatural sectional revolt, fully appreciating the vastness of the interests, the sacrifices, and the dangers involved, Henry contemplated the crisis—not with despondency, but with a profound sorrow and solicitude. While his sympathies and his hopes were all for the preservation of the national integrity of jurisdiction, he was little given to public exhibitions of his feelings. Undemonstrative—less from temperament than from the deliberate and habitual subjection of emotional expression to reason, during those times of feverish excitement apprehension and circumspection necessarily attendant on the prevalence of a gigantic rebellion (unparalleled in incentive, in temper, and in magnitude) many of whose leaders had been among his personal friends, he was not unnaturally looked upon by many as lukewarm in his patriotism, if not disloyal in his citizenship. To the occasional innuendoes of the press, he deigned no answers: he was the last man to accord compliance with the urgency of a popular clamor. And yet during the entire period of the Southern Insurrection, he was the personal and trusted friend of President Lincoln.†

* *Smithsonian Report* for 1864, p. 15.

† Early in the war (in the autumn of 1861,) a caller at the Presidential Mansion very anxious to see the Chief Magistrate of the nation, was informed that he could not then be seen, being engaged in an important private consultation. The caller not to be repulsed, wrote on a piece of paper

CONTRIBUTIONS TO SCIENCE AT WASHINGTON.

In addition to what may be called the public labors of Henry so diligently performed in various fields after his advent to the Smithsonian Institution, it is well briefly to contemplate the special scientific work he was able to accomplish in the intervals of his exacting occupations; that some estimate may be formed of the independent value of his later contributions, as well as of his wonderful industry. While still engaged in his difficult task of organizing and shaping the policy of the Institution, in 1850, on taking occasion to present before the American Association at New Haven, Conn., a résumé of the electrical phenomena exhibited by the Leyden jar, and their true interpretation, he remarked that "for the last three and a half years, all his time and all his thoughts 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 that 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."*

Thermal Telescope.—Shortly after his establishment at Washington, he continued a series of former experiments with the "thermo-galvanic multiplicator" devised by Nobili and Melloni in 1831; and by some slight but significant modifications of the apparatus, he succeeded in imparting to it a most surprising delicacy of action. With the thermo-electric pile carefully adjusted at the

that he must see Mr. Lincoln personally, on a matter of vital and pressing importance to the public welfare. This of course secured his admission to the presence of Mr. Lincoln, who was sitting with a middle-aged gentleman. Observing the hesitancy of his visitor, the President told him he might speak freely, as only a friend was present. Whereupon the visitor announced that for several evenings past he had observed a light exhibited on the highest of the Smithsonian towers, for a few minutes about nine o'clock, with mysterious movements, which he felt satisfied were designed as signals to the rebels encamped on Munson's Hill in Virginia.

Having gravely listened to this information with raised eyebrows, but a subdued twinkle of the eye, the President turned to his companion, saying "What do you think of that? Professor Henry." Rising with a smile, the person addressed replied, that from the time mentioned, he presumed the mysterious light shone from the lantern of a watchman who was required at nine o'clock each evening to observe and record the indications of the meteorological instruments placed on the tower.

The painful confusion of the officious informant, at once appealed to Henry's sensibility; and quite unmindful of the President, he approached the visitor, offering his hand, and with a courteous regard counselled him never to be abashed at the issue of a conscientious discharge of duty, and never to let the fear of ridicule interfere with its faithful execution.

* *Proceed. Am. Assoc.* 4th Meeting, New Haven, Aug. 1850, p. 378.

focus of a suitable reflector, his "thermal telescope" when directed to the celestial vault, indicated that the heat radiated inward by our atmosphere when clear, is least at the zenith, and increases downward to the horizon; as was to have been inferred from its increasing mass: when directed to clouds, they were found to differ very widely accordingly as they were condensing or being dissipated; some even indicating a less amount of radiation than the surrounding atmosphere. When directed to a horse in a distant field, its animal heat concentrated on the pile, was distinctly made manifest on the galvanometer needle. Even the heat from a man's face at the distance of a mile could be detected; and that from the side of a house at several miles distance.* These and many similar observations demonstrated to sense the inductions of reason, that there is a constant and universal exchange by radiation in straight lines from every object in nature, following the same laws as the palpable emanation from incandescent bodies; and that even when the amplitude of the thermal vibrations (equivalent to the square root of their dynamic energy) is reduced a million fold, its existence may still be distinctly traced.

Henry showed by experiment, that ice could be employed both as a convex lens for converging heat to a focus, and also as a concave mirror for the same purpose: a considerable portion of the incident rays being transmitted, a large portion reflected, and the remainder absorbed by the ice.

He presented to the American Philosophical Society, a discussion of the problem of the suspension of the ball in a water jet or fountain.†

In 1849, for the purpose of estimating the effects of certain meteorological conditions of the atmosphere, he made some experiments on the lateral radiation from a current of ascending heated air at different distances above the flame; the latter being thoroughly eclipsed.

He also experimented on the radiation of heat from a hydrogen flame, which was shown to be quite small, notwithstanding the high temperature of the flame. By placing an infusible and incombustible solid in the flame, while the temperature is much reduced, the radiant light and heat are greatly increased.‡ Results closely analogous to those he obtained in the differences between the audibility of vibrating tuning-forks when suspended by a soft thread, or when rigidly attached to a sounding-board. These results have also an undoubted significance with regard to celestial radiations; not only as to the differences between gaseous nebulæ and stars or clusters, but as to the differences

* *Sill. Am. Jour. Sci.* Jan. 1848, vol. v. pp. 113, 114.

† *Proceed. Am. Phil. Soc.* Oct. 16, 1848, vol. iv. p. 285.

‡ *Proceed. Am. Phil. Soc.* Oct. 19, 1849, vol. v. p. 108.

between stars in a probably different state of condensation or of specific gravity.

A few years later, he continued his investigation of this subject of radiation, more especially with reference to Rumford's "Observations relative to the means of increasing the quantities of Heat obtained in the Combustion of Fuel:" published in Great Britain in 1802.* He found that Rumford's recommendation of the introduction of balls of clay or of fire brick (about two and a half inches in diameter) into a coal fire, was fully justified as an economic measure: more heat being thereby radiated from the fire into the room, and less being carried up the flue. He also showed however that for culinary purposes, while the incandescent or heated clay increases the *radiation*, and thereby improves the quality of the fire for *roasting*, it correspondingly expends the *temperature*, and thereby diminishes its power for *boiling*. "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."†

In 1851, he read before the American Association at Albany, a paper "On the Theory of the so-called Imponderables:" (mainly a development of his earlier discussion in 1846, of the molecular constitution of matter,) in which he forcibly criticised a frequent tendency to assume or multiply unknown and unrealizable modes of action: holding that with regard to the most subtle agencies of nature, we have no warrant by the strict scientific method, for resorting to other than the observed and established laws of matter and force, until it has been exhaustively demonstrated that these are insufficient: and that time has not yet come. The fundamental laws of 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—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." He regarded the modern kinetic or dynamic theory of gases, by its

* *Journal Royal Institution*, 1802, vol. i. p. 28.

† *Proceed. Am. Assoc.* Providence, Aug. 1855, pp. 112–116. "On the Effect of mingling Radiating substances with Combustible materials."

predictions and verifications, as furnishing almost a complete establishment of the atomic and molecular theory of matter. Referring to the ingenious hypothesis of Boscovich, he thought that though well adapted to embrace the two static laws above mentioned, it did not appear equally well adapted to satisfy in any intelligible sense the three kinetic laws. He contended that every attempt at conforming our conception of the ultimate constitution of matter to the inductions of experience, would seem to conduct us directly to the atomic hypothesis of Newton. A careful study of the dynamics of the so-called "imponderables" certainly tended to their unification. Admitting the difficulty of framing an entirely satisfactory theory of the resultant transverse action of electricity, he suggested that a tangential force was not accordant with any inductions from direct experience; and was incapable of direct mechanical realization. Extending the atomic conception of matter to the ætherial medium of space, he concluded by urging "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."*

Among the various public Addresses delivered by Henry on special occasions, reference may be here made to his excellent exposition of the nature of power, and the functions of machinery as its vehicle, concluding with a sketch of the progress of art, pronounced at the close of the Exhibition of the Metropolitan Mechanics' Institute, in Washington, on the evening of March 19th, 1853. After representing to his hearers the close physical analogy between the human body as a moving machine, and the steam locomotive under an intelligent engineer, he remarked: "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."†

Views of Education.—Another address deserving of special notice (delivered the following year,) is his introductory discourse before the "Association for the Advancement of Education," as its retiring President. In this, he maintained that inasmuch as "the several faculties of the human mind are not simultaneously developed, 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. Memory, imitation,

* *Proceed. Am. Assoc.* Albany, Aug 1851, pp. 84–91.

† *Closing Address* Metr. Mech. Inst. Washington, 1853, p. 19

imagination, and the faculty of forming mental habits, exist in early life, while the judgment and the reasoning powers are of slower growth." Hence less attention should be given to the development of the reasoning faculties, than to those of observation: the juvenile memory should be stored rather with facts, than with principles: and he condemned as mischievous "the proposition 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."—' 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."

Applying this same reasoning to the moral training of youth, he considered that (as in the intellectual culture) the object should be "not only to teach the pupil how to *think*, but how to *act* and to *do;* placing great stress upon the early education of the habits. . . . 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, generosity, and justice. . . . 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 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." Childhood (he intimated) is less the parent of manhood, than of age: the special vices of the individual child though long subdued, sometimes surviving and re-appearing in his "second childhood."

Affirming that culture is constraint,—education and direction an expenditure of force, and extending his generalization from the individual to the race, he controverted the idea so popular with some benevolent enthusiasts, that there is a spontaneous tendency in man to civilization and advancement. The origins of past civilizations—taking a comprehensive glance at far distant human populations—have been sporadic as it were, and their prevalence comparatively transitory. "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. . . . 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. . . . 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 as nothing in comparison with that which may be yet unfolded and applied."*

Experiments on Building-stone.—In 1854, a series of experiments on the strength of different kinds of building-stone, was undertaken by Henry as one of a commission appointed by the President, having reference to the marbles offered for the extension of the United States Capitol. Specimens of the different samples—accurately cut to cubical blocks one inch and a half in height, were first tried by interposing a thin sheet of lead at top and bottom, between the block and the steel plates of the

* *Proceed. Assoc. Adv. Education*, 4th Session, Washington, Dec. 28, 1854, pp. 17–3·. The pregnant thought that human civilization is an artificial and coerced condition, would seem to have a suggestive bearing on the two great theories of *development* and *evolution*, so generally confounded by the superficial. What may be called the radical difference between these two views of organic extension, is that the former assumes an inherent mysterious tendency to progression, whose motto is ever "excelsior;" while the latter assumes a general tendency to variation within moderate limits in indefinite directions; so that elevation is no more normal than degradation, and indeed may be regarded as rarer and more exceptional, since at every upward stage attained by the few, there are probably more further digressions downward than upward, the motto being ever "aptior."

crushing dynamometer. "This was in accordance with a plan adopted by Rennie, and that 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 pounds 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 a single exception occur to vary the result.

"The explanation of this remarkable phenomenon (now that the fact 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 center 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 portions 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. . . . All the specimens tested were subjected to this process, and on their exposure to pressure were found to give concordant results. The crushing force sustained was therefore much greater than that heretofore given for the same material."*

In the same communication, interesting remarks are made on the *tensile* strength of materials, particularly the metals. "According to the views 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:" that is on the difference of lateral *adhesion* of the molecules, as exemplified in ice and water. A bar of soft metal—as lead—subjected to tensile strain, by reason of the greater freedom of the exterior layers of molecules, exhibits a stretching and thinning; while the interior molecules being more confined by the surrounding pressure, are less mobile, permit less elongation

* *Proceed. Am. Assoc.* Providence, Aug. 1855, pp. 102 112.

of the mass, and are therefore the first to commence breaking apart. Accordingly on ultimate separation, each fragment exhibits a hollow or cup-like surface of fracture, where the interior portion of the material has first parted: the depth of the concavity being somewhat proportioned to the malleability of the substance. "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 a 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."*

Hydrometric Experiment.—A novel project for the rectification of spirits by the simple process of static separation of the alcohol and water by the stress of their specific gravities when exposed in long columns, produced in 1854 a considerable sensation. It was alleged that the coercitive compression exerted by the water in a long hydrostatic column greatly accelerated the displacement and separation induced by gravitation, and that only a few hours were necessary to complete the process, if the column were sufficiently high.†

A patent was obtained: affidavits and samples fully attested the wonderful efficiency of the process; and only the co-operation of confiding capitalists was required, to realize fabulous profits, and effect a manufacturing and commercial revolution.

Simply in the interests of truth, Henry undertook the careful investigation of this surprising pretension. One of the towers of the Smithsonian Building supplied a convenient well for the experiment, easily accessible throughout its height. "A series of stout iron tubes of about an inch and a half internal diameter formed the column; the total length of which was one hundred and six feet. Four stop-cocks were provided; one at the bottom, one about four feet from the top, and the other two to the intermediate space equally divided or nearly so." Very careful hydrometer and thermometer registers were made at increasing intervals of time, the last being that of nearly half a year: a portion of the reserved liquor being simultaneously tested. The result stated, is: "There is not the slightest indication of any

* This conclusion is not at all in opposition to the ascertained fact of the increased strength imparted to an iron rod by thermo-tension, discovered by Prof. Walter R Johnson.

† An incidental remark in Gmelin's "Handbook of Chemistry" seemed to give some color of plausibility to the scheme. "Brandy kept in casks is said to contain a greater proportion of spirit in the upper, and of water in the lower part." Gmelin's *Handbook*, Translated by Henry Watts. London, 1841, part i. sect. 4,—vol. i. p. 112.

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." *

Sulphuric-acid Barometer.—In 1856, Henry had constructed for the Smithsonian Institution, at the suggestion of Professor G. C. Schaeffer, a large sulphuric acid barometer, whose column being more than seven times the height of the mercurial column (about 18½ feet) gave correspondingly enlarged and sensitive indications. Water barometers with cisterns protected by oil, (as that constructed by Daniell for the Royal Society,) have always proved instable. With reference to sulphuric acid, "The advantages of this liquid are: 1st that it gives off no appreciable vapor at any atmospheric temperature; and 2nd that it does not absorb or transmit air. The objections to its use are: 1st the liability to accident from the corrosive nature of the liquid, either in the filling of the tube or in its subsequent breakage; and 2nd its affinity for moisture, which tends to produce a change in specific gravity." The latter defect was obviated by a drying apparatus consisting of a tubulated bottle containing chloride of calcium, and connected by a tube with the glass bottle forming the reservoir, which excluded all moisture from the transmitted air. "The glass tube [of the barometer] 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."† This barometer continued in successful and satisfactory use for many years; and had its readings constantly recorded.

Of several of Henry's courses of experiments, no details have been published; and his original notes appear to have perished. In 1861, he made a number of experiments on the effects of burning gunpowder in a vacuum, as well as in different gases.

"A series of researches was also commenced, to determine more accurately than has yet been done, the expansion produced in a bar of iron at the moment of magnetization of the metal by means of a galvanic current. The opportunity was taken with the consent of Professor Bache, of making these experiments with the delicate instruments which had previously been employed in determining the varying length, under different temperatures, of the measuring apparatus of the base lines of the United States Coast Survey."‡ This wonderfully microscopic measuring appa-

* *Proceed. Am. Assoc.* Providence, Aug 1855, pp. 142, 143.
† *Proceed. Am. Assoc.* Albany, Aug. 1856, pp. 135–138.
‡ *Smithsonian Report* for 1861, p. 38.

ratus devised by Mr. Joseph Saxton, was capable of distinguishing by the light-ray index of its contact reflector, a dimension equal to a half wave-length of average light, or the 100,000th part of an inch. The long under-ground vaults of the Smithsonian building having been selected as a suitable place for the precise verification of the residual co-efficient of compensated temperature expansion of the base rods of the Survey, the opportunity was seized by Henry, at the termination of the investigation, to apply the same delicate apparatus to the determination of the polarized or magnetic expansion.

In less than six years from the time of these researches, he was called on to mourn the death of his life-long intimate and honored friend, who had always exhibited so fraternal a sympathy and co-operation with his own varied labors. In consequence of this event—the death of his friend Professor A. Dallas Bache in 1867, Henry was chosen in 1868, to be his successor as President of the National Academy of Sciences. At the request of that body, he prepared a eulogy of his friend the late President, which was read before the Academy April 16th, 1869. In grateful acknowledgement of the wise counsels and valuable services of Dr. Bache as one of the Regents of the Smithsonian Institution for nearly twenty years, he observed: "To say that he assisted in shaping the policy of the establishment would not be enough. It was almost exclusively through his predominating influence that the policy which has given the Institution its present celebrity, was after much opposition finally adopted. . . . Nor would it be possible for him [the speaker] to abstain from acknowledging with heart-felt emotion, that he was from first to last supported and sustained in his difficult position by the fraternal sympathy, the prudent counsel, and the unwavering friendship of the lamented deceased."*

Many minor contributions in various fields of scientific observation, must here be omitted: but it would be inexcusable, in this place and on this occasion, to neglect a reference to the active part he took in the organization and advancement of this Society;† and the unflagging interest ever exhibited in its proceedings, from the date of its convocation, March 13th, 1871, to that of his last illness. All here remember with what punctuality he attended the meetings—whether of the executive committee or of the society, undeterred by inclemencies of the weather which kept away many much younger members. All

* *Biographical Memoirs, Nat. Acad. Sci.* vol. i. pp. 181–212. Republished in the *Smithsonian Report* for 1870, pp. 91–116. The father of Prof. Bache—Richard Bache, was a son of the only daughter of the illustrious Benjamin Franklin.

† The Philosophical Society of Washington.

here, recall with what unpretentious readiness he communicated from his rich stores of well-digested facts, observations—whether initiatory or supplementary, on almost every topic presented to our notice; how apt his illustrations and suggestions in our spontaneous discussions; and with what unfailing interest we ever listened to his words of exposition, of knowledge, and of wisdom: utterances which we shall never hear again; and which unwritten and unrecorded, have not been even reported in an abstract.

Range of information.—It was not alone in those physical branches of knowledge to which he had made direct original contributions, that the mental activities of Henry were familiarly exercised and conspicuously exhibited. There was scarcely a department of intellectual pursuit in which he did not feel and manifest a sympathetic interest, and in which he did not follow with appreciative grasp its leading generalizations. Holding ever to the unity of Nature as the expression and most direct illustration of the Unity of its Author, he believed that every new fact discovered in any of nature's fields, would ultimately be found to be in intimate correlation with the laws prevailing in other fields—seemingly the most distant.* To his large comprehension, nothing was insignificant, or unworthy of consideration. He ever sought however to look beyond the ascertained and isolated or classified fact, to its antecedent cause; and in opposition to the dogma of Comte, he averred that the knowledge of facts is not *science*,—that these are merely the materials from which its temple is constructed by sagacious and attested speculation.

Among his earlier studies, Chemistry occupied a prominent place. The youthful assistant in the laboratory of his former instructor and ever honored friend, Dr. T. Romeyn Beck, and later, himself a teacher of the art and knowledge to others, a skilful manipulator, an acute analyst and investigator of re-actions, he seemed at first destined to become a leader in chemical research. Like Newton, he endeavored to bring the atomic combinations under the conception of physical laws; believing this essential to the development of chemistry as a true science. He always kept himself well informed on the progress of the more recent doctrines of quantivalence, and the newer system of nomenclature.

He had also paid considerable attention to geology; with its relations to palæontology on the one side, and to physical geography on the other.

Familiar with the details—as well of astronomical observation

* "A proper view of the relation of science and art will enable him [the reader] 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." (*Agricultural Report* for 1857, p. 419.)

as of the mathematical processes of reduction, he would have done honor to any Observatory placed under his charge. He was lenient in his judgment of the ancient star-worshippers; and was always greatly attracted by astronomical discoveries.

Well read in the science of Political Economy, he had by observation and analysis of human nature, made its inductive principles his own, and had satisfied himself that its deductions were fully confirmed by an intelligent appreciation of the teachings of financial history. He attributed the lamentable disregard of its fundamental doctrines, by many so-called legislators, to a want of scientific training, and consequent want of perception and of faith in the dominion and autonomy of natural law.

A good linguist, he watched with appreciative interest the progress of comparative philology, and the ethnologic significance of its generalizations, in tracing out the affiliations of European nations. By no means neglectful of lighter literature, he enjoyed at leisure evenings, in the bosom of his cultivated family, the readings of modern writers, and the suggestive interchange of sentiment and criticism. Striking passages of poetry made a strong impression on his retentive memory; and it was not unusual to hear him embellish some graver fact, in conversation, with an unexpected but most apt quotation. With a fine æsthetic feeling, his appreciation and judgment of works of art, were delicate and discriminating.

He held very broad and decided views as to the reign of order in the Cosmos. Defining science as the "knowledge of natural law," and law as the "will of God," Henry was always accustomed to regard that orderly sequence called the "law," as being fixed and immutable as the providence of its Divine Author: admitting in no case caprice or variableness. The doctrine of the absolute dominion of law—so oppressive and alarming to many excellent minds, was to him accordingly but a necessary deduction from his theologic and religious faith.

The series of meteorological essays already referred to as contributed to the Agricultural Reports of the Commissioner of Patents, commences with this striking passage, "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, it might appear that meteorology was an exception to this general proposition, and that the changes of the weather and the peculiarities of climate in different portions of the earth's surface, were of all things the most uncertain and farthest removed from the dominion of law: but scientific investigation establishes the fact that no phenomenon is the result of accident, or 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." And after dwelling on the developments and significance of moral statistics, he adds: "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."*

Organic Dynamics.—The contemplation of these uniformities leads naturally to the great modern generalization of the correlation of all the working energies of nature: and this to the subject of organic dynamics. "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." Some ten years before this, or in 1844, (as already noticed in an earlier part of this memoir) Henry had very clearly indicated the correlation between the forces exhibited by inorganic and organic bodies: arguing that from the chemical researches of Liebig, Dumas, and Boussingault, "it would appear to follow that animal power is referable to the same sources as that from the combustion of fuel:"† probably the earliest explicit announcement of the now accepted view. In the series of agricultural essays above referred to, he endeavored to frame more definitely a chemico-physical theory by which the elevation of matter to an organic combination in a higher state of power than its source, might be accounted for. Regarding "vitality" not as a mechanical force, but as an inscrutable *directing* principle resident in the minute germ—supposed to be vegetative, and enclosed in a sac of starch or other organic nutriment, he considered the case of such provisioned germ (a bean or a potato) embedded in the soil, supplied with a suitable amount of warmth and moisture

* *Agricultural Report Com. Pat.* for 1855, p. 357.

† *Proceed. Am. Phil. Soc.* Dec. 1844, vol. iv. p. 129. The admirable treatise of Dr. Julius R. Mayer of Heilbronn, on "Organic Movement in its relation to material changes," in which for the first time he maintained the thesis that all the energies developed by animal or vegetable organisms, result from internal changes having their dynamic source in external forces, was published the following year, or in 1845. Rumford nearly half a century earlier, had a partial grasp of the same truth. (*Phil. Trans. R. S.* Jan. 25, 1798, vol. lxxxviii. pp. 80–102.)

to give the necessary molecular mobility, soon sending a rootlet downward into the earth, and raising a stem toward the surface, furnished with incipient leaves. Supposing the planted seed to be a potato, on examination we should find its large supply of starch exhausted, and beyond the young plant, nothing remaining but the skin, containing probably a little water. What has become of the starch? "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 into 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, etc., of the tuber, as yet unacconnted 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 oxygen, has evolved the power necessary to the organization of the new plant. . . . 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."*

† *Agricultural Report*, for 1857, pp. 440–444 In May, 1842, Dr. Julius R. Mayer published in Liebig's *Annalen der Chemie*, etc., his first remarkable paper on "The Forces of Inorganic Nature," constituting the earliest scientific ennunciation of the correlation of the physical forces; and (if we except the work of Seguin in 1839,) of the mechanical equivalent of heat. (*Annalen u.s.w.* vol. xlii. pp. 233-240.) In September, 1849, Dr. R. Fowler read a short paper before the British Association at Birmingham, on "Vitality as a Foice correlated with the Physical Forces." (*Report Brit. Assoc.* 1849, part ii. pp. 77, 78.) In June, 1850, Dr. W. B. Carpenter presented to the Royal Society a much fuller memoir "On the Mutual Relations of the Vital and Physical Forces." (*Phil. Trans. R. S.* vol. cxl. pp. 727–757.) Neither of these essays accounts for the

The less difficult problem of the building up of the plant after the consumption of the seed, under the direct action of the solar rays, is then considered; the leaves of the young plant absorbing by their moisture carbonic acid from the atmosphere, which being decomposed by solar actinism, yields the de-oxidized carbon to enter into the structure of the organism. "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. 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." And thus under the expenditure of an external force, the plant (whether the annual cellular herb or the perennial fibrous tree) was shown to be built up from the simpler stable binary compounds of the inorganic world, to the more complex and unstable ternary compounds of the vegetable world. "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." (p. 446.) This important organic principle appears to receive here its earliest enunciation.

It was also pointed out that on the completion of the cycle of growth (however brief or however extended), the decay of the plant not only returns the elevated matter to its original lower plane, but equally returns the entire amount of heat energy absorbed in its elevation: an amount precisely the same, whether the slow oxidation be continued through a series of years, or a rapid combustion be completed in as many minutes. "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."

The elevation of inorganic matter (carbonic acid, water, and

amount of building energy displayed in the development of the seed, under conditions of low and diffused heat: and the expression "Vital Force" used both by Fowler and Carpenter, was studiously avoided by Henry.

ammonia,) to the vegetable plane of power, introduces naturally the consideration of the still higher elevation of vegetable organic matter to the animal plane of power. "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. . . . 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." The consumption of internal power does not however stop with the development of the young animal, as it does in the case of the young plant. "The young animal is in an entirely different condition: exposure to the light of the sun is not necessary to its growth or its existence: the chemical ray by impinging on the surface of its body does not decompose the carbonic acid which may surround it, the conditions necessary for this decomposition, 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 power. . . . 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 carbonic acid and water, and into ammonia. Hence oxygen is constantly drawn into the lungs, and carbon is constantly evolved. . . . The animal is a curiously contrived arrangement for burning carbon and hydrogen, and for the evolution and application of power. A machine is an instrument for the application of power, and not for its creation. The animal body is a structure of this character. . . . 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—steam-engine. 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."

"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 as it were of a part of his body. We are thus constantly renewed and constantly consumed; and in this consumption and renewal consists animal life."*

Seven years after the publication of this highly original and suggestive exposition, (whose topics and line of discussion had been distinctly formulated and sketched out more than two years before, at the commencement of the series in 1855,) the eminent physiologist Dr. Carpenter produced his valuable memoir on the Conservation of Force in Physiology; in which he for the first time distinctly affirms the development of vegetative reproductive energy, by the partial running down of matter to its stabler compounds,—"by the retrograde metamorphosis of a portion of the organic compounds prepared by the previous nutritive operations:" and also the ultimate return by decay, of the whole amount of force as well as of matter, temporarily borrowed from nature's store. Likewise with animal powers, "these forces are developed by the retrograde metamorphosis of the organic compounds generated by the instrumentality of the plant, whereby they ultimately return to the simple binary forms (water, carbonic acid, and ammonia,) which serve as the essential food of vegetables. . . Whilst the vegetable is constantly engaged (so to speak) in raising its component materials from a lower plane to the higher, by means of the power which it draws from the solar rays,—the animal whilst raising one portion of these to

* *Agricultural Report* for 1857, pp. 445-449. This important essay it will be observed, antedates Prof. Joseph Le Conte's paper "On the Correlation of Physical, Chemical, and Vital Force," read before the American Association at Springfield, Aug. 1859, (*Proceed. Am. Assoc.* pp. 187–203: and Sill. *Am. Jour. Sci.* Nov. 1859, vol. xxviii. pp. 305–319,) as well as Dr. Carpenter's second and more mature paper "On the application of the Principle of Conservation of Force to Physiology," published in Crookes' *Quarterly Journal of Science*, for Jan. and April, 1864, (vol. i. pp. 76-87; and pp. 259-277.)

a still higher level by the descent of another portion to a lower, ultimately lets down the whole of what the plant had raised."* So little was Henry's earlier paper known abroad, that his name does not occur in Dr. Carpenter's dissertation.

With regard to the great biologic question of the past fifteen years—the affiliation of specific forms, it was impossible that Henry should remain an unconcerned observer. Brought up (as it may be said) in the school of Cuvier, but slightly impressed with the brilliant previsions of his competitor, Geoffroy Saint Hilaire, accustomed to look upon the recurrent hypotheses of automatic development as barren speculations, and beside all this, ever the warmly attached personal friend of Agassiz, he approached the consideration of this controverted subject, certainly with no antecedent affirmative pre-possessions. His general acquaintance with the ascertained facts of the metamorphic development of the individual organism from its origin, as well as with the remarkable analogies and homologies disclosed by the sciences of comparative physiology and embryology, served however in some measure to prepare his mind to apprehend the significance of the indications which had been so industriously collected, and so intelligently collated: and from the very first, he accepted the problem as a purely philosophical one; employing that much abused term in no restricted sense. With no more reserve in the expression of his views, than the avoidance of unprofitable controversies, (though no one more than he—enjoyed the calm and purely intellectual discussion of an unsettled question by its real *experts*,) he yet found no occasion to write upon the subject. The unpublished opinions however, of one so wise and eminent, cannot be a matter of indifference to the student of nature; and their exposition cannot but assist to enlighten our estimate of the mental stature of the man, and of his breadth of apprehension and toleration.

Whatever may be the ultimate fate of the theory of natural selection, (he remarked in the freedom of oral intercourse with several naturalists,) it at least marks an epoch, the first elevation of natural history (so-called) to the really scientific stage: it is based on induction, and correlates a large range of apparently disconnected observations, gathered from the regions of palæontology or geological successions of organisms, their geographical distribution, climatic adaptations and remarkable re-adjustments, their comparative anatomy, and even the occurrence of abnormal variations, and of rudimentary structures—seemingly so uselessly displayed as mere simulations of a "type." It forms a good "working hypothesis" for directing

* *Quart. Jour. Sci.* 1864, vol. i. pp. 86 and 267.

the investigations of the botanist and zoologist.* Natural selection indeed—no less than artificial (he was accustomed to say), is to a limited extent a fact of observation; and the practical question is to determine approximately its reach of application, and its sufficiency as an actual agency, to embrace larger series of organic changes lying beyond the scope of direct human experience. It is for the rising generation of conscientious zoologists and botanists to attack this problem, and to ascertain if practicable its limitations or modifications.

These broad and fearless views, entertained and expressed as early as 1860, or 1861, exhibiting neither the zealous confidence of the votary, nor the jealous anxiety of the antagonist, received scarcely any modification during his subsequent years. Nor did it ever seem to occur to him that any reconstruction of his religious faith was involved in the solution of the problem. So much religious faith indeed was exercised by him in every scientific judgment, that he regarded the teachings of science but as revelations of the Divine mode of government in the natural world: to be diligently sought for and submissively accepted; with the constant recognition however of our human limitations, and the relativity of human knowledge.† Not inappropriately may be here recalled a characteristic statement of the office of hypothesis, made by him some ten years earlier: presenting a consideration well calculated to restrain dogmatism—whether in science or in theology. "It is not necessary that an hypothesis be absolutely true, in order that it may be adopted as an expression of 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. . . . 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."‡

* "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." (*Agricult. Report*, 1856, p. 456.)

† With reference to the intimations of the comparative antiquity of man, Henry quoted with sympathetic approbation the sentiment so well expressed by the Bishop of London in a Lecture at Edinburgh, that "The man of science should go on honestly, patiently, diffidently, observing and storing up his observations, and carrying his reasonings unflinchingly to their legitimate conclusions, convinced that it would be treason to the majesty at once of science and of religion, if he sought to help either by swerving ever so little from the straight line of truth." (*Smithsonian Report* for 1868, p. 33.)

‡ *Proceed. Am. Assoc.* Albany, Aug. 1851, pp. 86, 87.

INVESTIGATIONS IN ACOUSTICS.

During the last quarter of a century, among the many interests which demanded and engaged his attention, Henry studied with much care various phenomena of acoustics, and added much to our practical as well as theoretical knowledge of this important instrumentality. In 1851, he read a communication before the American Association, "On the Limit of Perceptibility of a direct and reflected Sound," in which he gave as the result of experimental observations, the subjective fact that a wall or other reflecting surface if beyond the distance of about 35 feet from the ear, or from the origin of the sound, gives a distinguishable echo from the sound; but that if the ear or the sounding agent be placed within this distance, the reflected sound appears to blend completely with the original one. From a number of experiments, he found that under the same circumstances, this limit of perceptibility did not vary more than a single foot; but that under differing conditions the limit of distance ranged from 30 to 40 feet, (equivalent to a difference of from 60 to 80 feet of sound travel,) depending partly on the sharpness or clearness of the sound, and partly on the pitch or the length of the soniferous wave, which affected the amount of overlapping of the two series. These results imply a duration of acoustic impression on the ear of about one-sixteenth of a second; serving to show that 16 vibrations to the second must be about the lower limit of a recognizable musical tone.* As applied to lecture-rooms, he pointed out that the ceiling should not be more than about thirty feet high, within which elevation, a smooth ceiling would tend to re-inforce the sound of a speaker's voice.†

Many experiments were afterward made on the resonance of different materials, by means of tuning forks. While a tuning fork suspended by a fine thread continued to vibrate for upward of four minutes with scarcely any appreciable sound, if placed in contact with the top of a pine table, the same vibration continued but ten seconds, but gave a loud full tone. On a marble topped table the sound was much more feeble, and the vibration continued nearly two minutes. While the tuning fork against a brick wall gave a feeble tone continuing for 88 seconds, against a lath and plaster partition it gave a sound considerably louder but continu-

* This does not seem to agree with results obtained by Savart some twenty years previously; who concluded from observations with the siren, "that sounds are distinctly perceptible, and even strong when composed of no more than eight vibrations in a second." (*Rev. Encycl.* July, 1832. Quoted in Sill. *Am. Jour. Sci.* for 1832, vol. xxii. p. 371.) This latter determination is somewhat difficult to reconcile with ordinary observations, as it is certain that intervals of one eighth of a second would give a very appreciable rattle to almost every ear.

† *Proceed. Am. Assoc.* Cincinnati, May, 1851, pp. 42, 43.

ing only 18 seconds. On a large block of soft India-rubber resting on the marble slab, the vibration was very rapidly extinguished, but without giving any sensible sound. This anomaly required an explanation. By means of a compound wire of copper and iron inserted into the piece of rubber, and having the extremities connected with a thermo-galvanometer it was found that in this case the acoustic vibrations were converted into heat. Sheets of India-rubber therefore are among the best absorbers and destroyers of sound. A series of experiments was also made on the reflection of sound, to determine the materials least and those best adapted to this purpose. A résumé of these researches, having reference to the acoustic properties of public halls, was read before the American Association in August, 1856.

In 1865, as Chairman of the Committee of Experiments of the U. S. Light-house Board, Henry commenced an extended series of observations on the conduct and intensity of sound at a distance, under varying meteorological conditions. Well aware that for the practical purposes of giving increased security to navigation, the experiments of the laboratory were of little value, he undertook a number of experimental trips on board sailing vessels, and on steamers, in order to make his observations under the actual conditions of the required service. As many of his investigations required intelligent co-operation, and sometimes at the distances of many miles, he associated with him at different times, among members of the Light-house Establishment, Commodore Powell, Commodore Case, Admiral Trenchard, Commander Walker, Captain Upshur, General Poe, General Barnard, General Woodruff, Mr. Lederle, and other engineers of different Light-house Districts, and outside of the establishment, Dr. Welling and others.

At the outset of his experiments, he found that sound reflectors, which play so interesting a part in lecture-room exhibitions, were practically worthless (of whatever available dimensions) for the purpose of directing or concentrating powerful sounds to any considerable distance. At the distance of a mile or two a large steam whistle placed in the focus of a concave reflector 10 feet in diameter could be heard very nearly as well directly behind the reflector, as directly in front of it. In like manner the direction of bell-mouths and of trumpet-mouths, was found to be of comparatively little importance at a distance; showing the remarkable tendency to diffusion, especially with very loud sounds. Most of the observations made on ship-board were afterward repeated on land; and several weeks were occupied with these important researches.

"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." After citing observations by others apparently confirming the suggestion of some dominant influence in the upper wind, Henry adds: "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 Stokes in the Proceedings of the British Association for 1857,* 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."† A rough attempt was made in the course of these observations (which were undertaken at the light-house near New Haven, Connecticut) to compare the velocity of the wind in the upper regions with that near the surface of the earth. "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."‡

In October, 1867, a series of observations was made at Sandy Hook (New Jersey) with various instruments. A sound reflector being employed, the distance at which the sand on the phonometer drum—carried in front, ceased to move was 51 yards, as compared with a distance of 40 yards, without the reflector. At a greater distance, with a more sensitive instrument, the ratio was very much diminished. Experiments were also made on the relative distances at which the trumpet affected sensibly the drum of the phonometer in different directions, giving as their result a limiting spheroid whose reach in the forward axis of the trumpet was about double that in the rear axis, and at right angles to the axis, was about a mean proportional between the two. With greater distances, these differences were evidently very much reduced, the radii becoming more equalized. In the summer of 1871, Henry made

* *Report Brit. Assoc.* vol. xxiv. 2d part, p. 27.

† *Report of Light House Board*, U. S. for 1874, p. 92.

‡ This difference has since been established by a number of independent observations. Mr. Glaisher from his balloon ascents in 1863–1865, ascertained that the upper currents of air are frequently five or six times more rapid than the surface currents. (*Travels in the Air*, p. 9.) Prof. Cleveland Abbe remarks: "From seven balloon ascensions made on July 4th, 1871, at different points in the United States, I have deduced the velocity of the upper currents as about four times that of the surface wind prevailing." (*Bulletin Philosoph. Soc.* Washington, Dec. 16, 1871, vol. i. p. 39.) And M. Peslin states in general terms: "It is certain according to all observations made both in mountains and in balloons, that the force of the wind increases considerably as we ascend in the atmosphere." (*Bulletin International de l'Observ. de Paris et de l'Observ. Phys. Cent. Montsouris*, July 7, 1872.)

investigations at different light-stations, on our western coast of California.

The very important observation that a sound could best be heard at an elevation when the wind is adverse (that is when it blows from the observer towards the acoustic signal,) and that after it had even been entirely lost to the ear in such case, it might be regained in full force by simply ascending to a suitable elevation,—admitted apparently but one explanation, namely that the line of successive impulse constituting a sound beam was deflected or bent upwards by the action of the opposing wind. If—as had already been shown to be the case sometimes, and as might therefore be expected generally,—the adverse wind were assumed to be a little stronger at the elevation than at the surface such a result would at once follow. "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 them 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 waves upward and directs them above the head of the observer."*

From several observed and reported cases where the sound of a fog-signal was exceptionally heard to a greater distance against the wind than toward the direction of the wind, Professor Henry for a while hesitated to give the hypothesis of Professor Stokes an unqualified acceptance; but forced as he was constantly to recur to it as the only plausible explanation of the ordinary influence of wind on the transmission of sound, he finally was able to satisfy himself that even the apparent exceptions to the rule were really in accord with it. Having more than once observed that when the upper current of air, as indicated by the course of the clouds, is in an opposite or different direction from the lower or sensible wind, the range of audibility is most affected and favored by the upper current, it was a natural induction to extend such a condition in imagination to other cases of abnormal behavior of sound. A large amount of subsequent labor and attention was devoted to the determination of this important question.

* *Report of Light House Board* for 1874, p. 106.

In 1872 it was observed from on board a steamer approaching Portland Head station in the harbor of Portland (Maine) that the fog-signal which had been distinctly heard through many miles; was lost to the ear when within two or three miles of the point, that it continued inaudible throughout the nearer distance of a mile or so, and that it was again heard as the station was neared. At Whitehead light station on a small rocky island about a mile and a half from the coast, (being some 65 miles northeast of Portland Head,) it was observed on board a steamer approaching the station during a thick fog, that the signal (a 10-inch steam whistle) though distinctly heard at the distance of six miles or more, and with increasing distinctness as the steamer advanced, was suddenly lost at about three miles, and was not recovered until within a quarter of a mile from the station; the wind at the time being approximately adverse to the sound. A six-inch steam whistle on board the steamer was meanwhile distinctly heard at the station during the whole time of inaudibility of the larger ten-inch whistle, which had also been sounded without any interruption. This remarkable phenomenon implied a compound flexure of the sound beams, and accorded with previous observations made at the same points by Gen. Duane the engineer in charge of the first and second Light-house Districts.

In 1873 observations were again made at Whitehead station, and at Cape Elizabeth light station, both on the coast of Massachusetts. At Whitehead the steam whistle was heard through a distance of 15 miles, with a light adverse wind. At Cape Elizabeth, with a stronger adverse wind, the siren was heard only about nine miles

In 1874, observations were made at Little Gull Island (off the coast of Connecticut); at Block Island, (off the coast of Rhode Island); and at Sandy Hook (New Jersey). At Little Gull Island the sound of a siren was heard against a moderate wind, only three and a half miles. At Block Island the siren was reported to have been heard under favoring conditions of wind through a distance of more than 25 miles. While it was frequently heard at Point Judith station, and the siren at the latter point was as frequently heard at Block Island, (the distance between the two points being 17 miles,) it was shown on comparison of records, that the two instruments had not been heard simultaneously; the wind when favorable to the one being unfavorable to the other.

At Sandy Hook, for the purpose of making simultaneous observations in different directions, three steamers (the tenders of different light-houses) were employed, with steam whistles specially adjusted to the same tone and power. The latter quality having been carefully tested by the phonometer, the three vessels steamed out abreast on trial; and their whistles sounding in regular succession "became inaudible all very nearly at the same moment." One of the vessels being then anchored at a distance from land, the two others were directed in opposite courses, one with the wind, or eastward, the other against it, or westward. In 15 minutes the whistle of the former ceased to be heard, while that of the latter was very distinctly heard; the anemometer showing a wind of about six miles per hour. About noon the vessels changed positions, but the sound from the west continued audible for about three times the distance of that from the east, though the wind had declined to nearly a calm or to about half a mile per hour. In an hour and a half the wind had changed to "within two points of an exactly opposite direction, blowing from the indications of the anemometer at the rate of ten and a half miles per hour." The vessels once more departing, one with the wind, the other against it, the sound of the whistle coming against the wind was this time heard for the greater distance, contrary to expectation. On the following day a number of small balloons having been provided, a similar series of experiments to that of the preceding day was made; a station being selected at a greater distance from land. On the first trial, with a light wind from the west of about one and a quarter miles per hour as indicated by the anemometer, a balloon was set off which continued rising and moving eastward till lost to sight. Two of the vessels taking opposite courses as before, gave the sound in the direction of the wind about double the duration of that coming against the slight wind. The vessels then changed places in their opposite courses; the wind having subsided to a calm. "A balloon let off ascended vertically until it attained an elevation of about 1,000 feet, when turning east it followed the direction of the previous one. In this case the sound of the whistle coming from the east was heard somewhat longer than the opposite one. At the third trial made after noon, the wind had changed nearly one-third of the circle, its force being about five miles per hour. The vessels once more taking their courses with the wind and against it, "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 traced in the earlier observations." In this case the sound was heard with the wind very slightly farther than against it. It was thus shown that the upper current of wind had remained constant throughout the day, while the changing surface wind was apparently a land and sea breeze "due to the heating of the land as the day advanced:" and the varying behavior of the sound beams was easily explained by the varying differences of velocity in their wave fronts at different heights.

In 1875 Henry continued his observations at Block Island (R. I.) and at Little Gull Island (Conn.). The southern light-house on Block Island standing on the edge of a perpendicular cliff 152 feet above the sea level, and being itself 52 feet high (to its focal plane), this point was selected for making investigations on the effect of altitude in modifying unfavorable conditions of audibility. Observers were accordingly stationed on the beach at the foot of the cliff, and also on the tower 200 feet above, to record simultaneously the duration of the whistle signals of two steamers proceeding in opposite directions toward the right and the left. The sound coming against the wind (of about seven miles per hour) continued audible at the upper station four times longer, (*i. e.*, for four times greater distance) than at the lower station. The sound coming with the wind, was unexpectedly heard at the lower station for a longer period than at the upper one. Another observation (with the wind about five miles per hour) gave for the sound against the wind, rather more than twice the distance of audibility at the upper station; and for the sound favored by the wind, a slightly greater distance at the top than at the bottom station. The next observation gave as before, with the adverse wind, the advantage of more than double the distance of audibility to the upper station; meanwhile one of the observers at the foot of the cliff, after the sound was entirely lost, managed by climbing to a ledge about 30 feet above the beach, to recover the signal quite distinctly, and to hear it for some time. The sound coming with the wind continued to be heard at both the higher and the lower stations for precisely the same time, giving on this occasion no advantage to either. Observations made on board the two steamers while moving in opposite directions, gave for the sound travelling with the wind a duration and distance more than five times that for the sound which came against the wind. Five similar experiments gave very similar results. The two vessels moving in opposite courses, each at right angles to the direction of the wind, gave a very close equality for the reciprocal durations of the sound. In the following month, similar observations were made at Little Gull Island, which were very accordant with those made at the former station. As a result of plotting the ranges of audibility in different directions from a given point, producing a series of circular figures (more or less distorted) of very different sizes, Henry was inclined to believe that the whole area of audition is less in high winds than in gentle winds. These investigations as their author well remarks,—"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."*

* *Report of the Light-house Board* U. S. for 1875, p. 107.

In the summer of last year, 1877, with undiminished ardor, he continued his observations on sound; selecting this time Portland harbor, Monhegan Island, and Whitehead light station, on the coast of Maine. At the latter station, the abnormal phenomenon of a region of inaudibility near the fog-signal, and extending outward for two or three miles, (beyond which distance the signal is again very distinctly heard,) had for several years been frequently observed. This singular effect is noticed only in the case of a southerly wind when the vessel is approaching the signal from the same quarter, and consequently with the wind adverse to the direction of the sound beams, a condition of the wind which is the usual accompaniment of a fog. The observation showed this intermediate "belt of silence" to be well marked on board the steamer both on approaching the station and on receding from it by retracing the same line of travel. Meanwhile the intermittent signal whistle from the steamer was distinctly heard at the station on both the outward and homeward trips of the vessel, throughout its course. The next set of observations was made on the opposite side of the small island, by directing the course of the steamer northward; and in this case the shore signal was distinctly heard throughout the trip, while the signal from the vessel passed through the "belt of silence" to the observers at the station. The hypothesis of a local sound shadow of definite extent, is excluded by the simple fact that the regions traversed were entirely unobstructed, the two points of observation—movable and stationary—being constantly in view from each other when not obscured by fog. The hypothesis of a stationary belt of acoustic opacity is equally excluded by the uninterrupted transmission of sound through the critical region in one direction; and this too whichever order of observation be selected. So that in one of the cases the powerful whistle ten inches in diameter blown by a steam pressure of 60 pounds, failed utterly to make itself heard, while the sound from a much feebler whistle only six inches in diameter and blown by a steam pressure of 25 pounds, traversed with ease and fulness the very same space. The only hypothesis left therefore is that of diacoustic refraction; by which the sound beam from one origin is bent and lifted over the observer, while from an opposite origin the refraction is in a reversed direction; and such a quality in the moving air is referable to no other observed condition but that of its motion, that is to the influence of the wind. Observations were afterward made at Monhegan Island, on some of the more normal effects of the refraction of sound by differences of wave velocity, all fully confirming the supposition which had been so variously and critically subjected to examination.

The principal conclusions summed up in this last Report for 1877, are: 1st. The audibility of sound at a distance depends primarily upon the pitch, the intensity, and the quantity of the sound: the most efficient pitch being neither a very high nor a very low one,—the intensity or loudness of sound resulting from the amplitude of the vibration, and the quantity of sound resulting from the mass of air simultaneously vibrating. 2nd. The external condition of widest transmission of sound through the air is that of stillness and perfect uniformity of density and temperature throughout. 3rd. The most serious disturbance of the audibility of sound at a distance, results from its refraction by the wind, which as a general rule moving more freely and rapidly above than near the earth, tends by this difference to lift the sound-beams upward when moving against the wind, and in a downward curve when moving with it. 4th. When the upper current of air is adverse to the lower or sensible wind, or whenever from any cause the wind below has a higher velocity than that above—in the same direction, the reverse phenomenon is observed of sound being heard to greater distances in opposition to the sensible wind than it is when in the direction of the surface wind. 5th. While suitable reflectors and trumpet cones are serviceable in giving prominent direction to sounds within moderate or ordinary distances, yet from the rapid diffusibility of the sound-beams, such appliances are worthless for distances beyond a mile or two. 6th. The siren has been frequently found to have its clearest penetration through a widely extended fog, and also through a thick snow-storm of large area. 7th. Intervening obstructions produce sound shadows of greater or less extent, which however at a distance but slightly enfeeble the sound owing to the lateral diffusion and closing in of the sound waves. 8th. The singular phenomenon of distinct audibility of sound to a distance with a limited intermediate region of inaudibility where no optical obstruction exists, is due sometimes to a diffusion of upper sound-beams which have not suffered the upward refraction; sometimes to the lateral refraction of sound-beams or to the lateral spread of sound from directions not affected by the upward refraction; and very frequently to a double curvature of the refracted sound-beams under an adverse lower wind, by reason of the wave fronts being less retarded by the lower or surface stratum of wind than by that a short distance above, and at still greater heights being again less retarded, and finally accelerated by the superior favoring wind.

These remarkable series of acoustic investigations undertaken after the observer had considerably exceeded his three score years, —perseveringly continued weeks at a time, and sometimes for more than a month,—extending through a period of twelve years, and pursued over a wide and extremely irregular range of sea-coast, and under great variety of both topographical and meteorological conditions — untiringly prosecuted by numberless sea trips of 10, 15 and even 20 miles in single stretches, in calm, in sunshine, in storm, with every variety of disregarded exposure, form altogether a labor and a research—quite unequalled and unapproached by any similar ones on record. As a result of so great earnestness and thoroughness in the conduct of an enterprise of so great difficulty, Henry has advanced and enriched our knowledge by contributions to the science of acoustics unquestionably the most important and valuable of the century. By persistent cross-examination of the bewildering anomalies of sound propagation under wide diversities of locality and condition, he has succeeded in evolving order out of apparent chaos, —in reclaiming a new district, now subjected to the orderly reign of recognized law,—and in raising the plausible but long neglected hypothesis of Stokes into the domain of a verified and fully established theory. Only on the subject of the ocean echo had he failed to reach a solution which entirely satisfied his judgment;* and at the ripe age of four score years he had mapped out a further extension of his laborious search after truth, when his beneficent and all unselfish purposes were cut short by death.

With these great labors (a full demand upon the energies of youthful vigor) fittingly closed the life of one whose long career had been dedicated to the service of his race,—no less by the unrecorded incitations and encouragements of others to the prosecution of original research, than by his own earnest efforts on all convenient occasions to extend the boundaries of our knowledge. Nor is it permitted us to indulge in vain regrets that thirty years of such a life were seemingly so much withdrawn from his own chosen ministry at the altar of science, to be occupied so largely with the drudgery and the routine of merely administrative duties. True though it be, that talents adapted to such functions are very much more common and available than those which form the successful interrogator of Nature, who that knows by what exertions Smithson's wise endowment was rescued from the wasteful dissipation of heterogeneous local agencies and objects—by what heroic constancy, and through what ordeals of remonstrance and

* "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 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 their curvilinear direction until they strike 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 prolonged 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." (*Report of Light House Board*, 1877, p. 70.)

misconception, of contumely and denunciation, the modest income of the fund (husbanded and increased by prudent management) was yearly more and more withdrawn from merely popular uses and interests, and more and more applied to its truest and highest purpose, the fostering of abstract research, the founding of a pharos for the future,—the "increasing and diffusing of knowledge among men,"—who that knows all this, can say that Henry was mistaken in his devotion, or that his ripest years were wasted in an unprofitable mission? But in addition to this vast work,—accomplished as probably no one of his scientific compeers would have had the fortitude and the indomitable persistence to carry through, his personal contributions to modern science (as has been shown) have in the meantime been neither few nor unimportant.

One remarkable circumstance relating to Henry's directorship of the Smithsonian publications (which have had so wide a distribution and influence)* must not be here passed over. Having himself amidst the absorbing occupations of his position conducted so valuable original investigations—on the strength of building materials,—on the best illuminants and their proper conditions,—and especially in his last great labor on the philosophy of sound, we should naturally expect to find them displayed in the "Smithsonian Contributions;"—where in interest and importance second to none contained in that extensive and admirable series, these memoirs would have found their fitting place, and have given honor to the collection. But as if to avoid all semblance of a personal motive in his resolute policy of administration, he published nothing for himself at the expense of the Smithsonian fund; his numerous original productions being given to the public through the channel of various official reports. And thus it has occurred that his writings scattered in the different directions which seemed to him at the time most suitable, with little thought of any special publicity or perpetuity, have largely failed to reach the audience which would most appreciate them. And many of his most valuable papers—never by himself collected—must be searched for in unsuggestive volumes of Agricultural or Light-House Board Reports.†

* "The number of copies of the Smithsonian Contributions distributed, is greater than that of the Transactions of any scientific or literary society; and therefore the Institution offers the best medium to be found for diffusing a knowledge of scientific discoveries." (*Smithsonian Report* for 1851, p. 202.)

† Many valuable communications made to the American Association, to the National Academy of Sciences, to the Washington Philosophical Society, and to other bodies, from rough notes, which their author was prevented from writing fairly out, by the unceasing pressure of his multitudinous official and public duties, have unfortunately been published only by title.

For him it seemed enough that what was once established, would not be willingly let die; that the medium or the occasion of communication was of comparatively little consequence, if but a new fact or principle were thrown into proper currency, and duly accepted as part of the world's wealth: and beyond all ordinary men he seemed to feel the insignificance of personal fame as compared with the infinite value of truth. For such a man the most appropriate monument would be a full collection of his writings, produced in a worthy and appropriate style of publication.

PERSONALITY AND CHARACTER.

Of Henry's personal appearance, it is sufficient to say, that his figure, above the medium height, was finely proportioned; that his mien and movement were dignified and imposing; and that on whatever occasion called upon to address an assembly,

> "With grave aspéct he rose, and in his rising seemed
> A pillar of state: deep on his front engraven
> Deliberation sat, and public care."

His head and features were of massive mould; though from the perfect proportion of his form, not too conspicuously so. His expansive brow was crowned with an abundant flow of whitened hair; his lower face always smoothly shaven expressed a mingled gentleness and firmness; and his countenance of manly symmetry was in all its varying moods, a pleasant study of the mellowing, moulding impress of long years of generous feeling, and a worthy exponent of the fine and thoughtful spirit within: wearing in repose a certain pensive but benignant majesty, in the abstraction of study a semblance of constrained severity, in the relaxation of friendly intercourse a genial frank and winning grace of expression. Like his intimate personal friend Agassiz, he seemed to stand and to move among men as the very embodiment of unfailing vigorous health and physical strength, and only a year ago, he walked with as erect and elastic a carriage,—with as firm and sprightly a step, as any one here present.

It is difficult to attempt even a sketch of Henry's intellectual character, without allusion to his moral attributes;—so constantly did the latter dominate the former. It may be said that the most characteristic feature of his varied activities was earnestness, and this as usual was the offspring—as much of a moral as of a mental purpose.

His mind was eminently logical; and this rational power was exhibited in every department of his theoretical or his practical pursuits. He never showed or felt uneasiness at necessary deductive consequences, if the premises were well considered or appeared to be well founded. If presented with the problem of an untried case,—while avowing the necessity of reserve in predicting results, he seemed to have an almost intuitive apprehension of the operation of natural law. If confronted with an unfamiliar phenomenon, whether in the experience of others, or in his own observations, his imagination was fertile in the suggestion of test conditions for eliminating varying influences. While few have ever held the function of hypothesis in higher estimation as an instrument of research, no one ever held hypothesis in more complete subjection.

As a lecturer and instructor, he was always most successful. Free from all self-consciousness,—without attempting oratorical display, his expositions—in simple, direct, and conversational language, were so lucid, satisfying, and convincing, that they enlisted from the start and secured to the close, the attentive interest of his auditors.

In his sympathy with the pursuits of the rising generation of physicists was ever manifested a disposition to frequent consultation and interchange of views with them; as if (aware of the usual tendency to mental ossification with advancing years,) he thus sought by familiar association to drink at the fountain of perennial youth. And surely no one was ever more successful in retaining life's coveted greenness in age;—not more in the geniality of his affections and in his undimmed faith, hope, and charity, for mankind,—than in his intellectual freedom from undue prejudices, and in his readiness calmly to discuss or adopt new theories.

And this leads to the reflection that in the seeming contrasts of his nature were combined qualities which formed in him a resultant of character and of temperament as rare as admirable With this great mobility of aptitude and of circumspection, this adaptability of mental attitude, he yet possessed an unusual firmness of resolution. With a manly sturdiness of conviction he presented an unvarying equability of temper and of toleration; and with perfect candor as perfect a courtesy. With a characteristic dignity of figure of presence and of deportment, he preserved an entire freedom from any shade of arrogance. With a warm and active charity, he still displayed a shrewd perception of character; and while ever responsive to the appeals of real distress, his insight into human nature protected him from being often deceived by the wiles of the designing. Intolerant of charlatanism and imposture, he was capable of exhibiting a wonderful patience with the tedium of honest ignorance. Possessing in earlier life a natural quickness of temper, and always a high degree of native sensibility, his perfect self-control led the casual acquaintance to regard him as reserved and unimpressible. Of him it may be truly said in simple and oft-quoted words:

> "His life was gentle; and the elements
> So *mixed* in him, that Nature might stand up
> And say to all the world—This was a MAN!"

With all his broad humanity, he possessed but little of what is known as "humor." He could more heartily enjoy the ludicrous as drolly narrated by its appreciative victims, than when sarcastically recited at the expense of another. The sparkle of wit he fully appreciated provided it were free from coarseness and from personal satire. From the subordination of his sense of humor to his native instinct of sincerity, he had no approbation—or indeed tolerance of "practical jokes," holding that the shock to the feelings or to the confidence of the dupe, is far too high a price for the momentary hilarity enjoyed by the thoughtless at a farcical situation. Newspaper hoaxes—literary or scientific, in like manner received his stern reprobation, as uncompensated injuries to popular trust, and to the cause of popular enlightenment.

Strong in his unerring sense of justice and of right, he allowed no prospects of personal advantage to influence his judgment in action, in decision, or in opinion. He never availed himself of the opportunities offered by his position, of reaping gain from profitable suggestions or favorable awards: and he never willingly inflicted an injury even on the feelings of the humblest. This was characteristically shown in the pains taken to convince the judgment of those against whose visionary projects he was so often called upon to report in the public interests of the Smithsonian Institution, of the Light-house service, and of the General Government:—often expending an amount of valuable time and of patience which few so situated would have accorded, or could have well afforded. And yet on the other hand when himself the subject of injustice, misconstruction, or abuse, he never suffered himself to be provoked into a controversy;—as if holding life too serious, time too precious, to be wasted in mere disputation Least of all did he ever think of resorting to retaliatory conduct or to the expression of opprobrious sentiments. He calmly put aside disturbing elements, and seemed endowed with the power of excluding from his mental vision all irritating incidents. In that benignant breast there harbored no resentments.

To those who knew the man,—to those who have enjoyed the charm of his more intimate society, and felt the magnetism of his cheery presence, how poor and insufficient must appear these disjointed outlines of that mental, moral, and spiritual nature, which always and at every point was so much larger than it seemed.

Less than a year ago, (on the evening of November 24th, 1877,) he delivered in this place before this Society his annual address, shortly after his re-election as its President;—an address which as we beheld the remarkable fulness and freshness of the speaker's mental and bodily powers,—we little thought was in reality his valedictory. In it he concisely yet lucidly portrayed for the stimulation of more youthful physicists, the processes and the qualities necessary for success in original research;—the awakened attention to "the seeds of great discoveries constantly floating around us," —the careful observation, the clear perception of the actual facts uncolored as much as possible by *a priori* conceptions or expectations,—the faculty of persevering watchfulness, and the judgment to eliminate (with all due caution) the conditions which are accidental,—the importance of a provisional hypothesis,—the conscientious and impartial testing of such by every expedient that ingenuity may suggest,—the lessons taught by failure,—the firm holding of the additional facts thus gleaned, though adverse and disappointing,—the diligent pondering, and the logical application of deductive consequences, to be again examined until as the reward of patient solicitation, the answer of nature is at last revealed.

"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 which I have given of a successful investigation, 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 again 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 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."* Who that listened could fail to see that the speaker was unconsciously giving us precious glimpses into his own experience?

Less than two weeks after this, he suffered at New York a temporary numbness in his hands, which he feared might threaten a paralysis; but a subsequent swelling of his feet and hands revealed

* *Bulletin Phil. Soc.* Washington, Nov. 24, 1877, vol. ii. p. 166.

to his physician the nature of his inward disease as a nephritis, which had been insidiously assailing him before it was suspected, and had doubtless been aggravated by his unremitting scientific labors continued as usual through his last summer vacation. Only a month before he died, he thus described the commencement of his malady: "After an almost uninterrupted period of excellent health for fifty years, I awoke on the 5th of December at my office in the Light-house Depot in Staten Island, finding my right hand 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. Weir Mitchell and Dr. J. J. Woodward pronounced the disease an affection of the kidneys."*

Aware that his illness was fatal, he yet felt lulled by that strange flattery of disease when unattended with a painful wasting, into the thought that he might probably survive the approaching warmer weather; and fully prepared for death, with the sense of life still strong within him, he planned what might yet be accomplished.

But with occasional alternations of more favorable symptoms, with the uræmia steadily increasing, his strength slowly declined: and as he lay at noon of the 13th of last May, [1878,] with growing difficulty of breathing—surrounded by loving and anguished hearts—his last feeble utterance was an inquiry which way the wind came. With intellect clear and unimpaired, calmly that pure and all unselfish spirit passed away—leaving a void none the less real, none the less felt, that the deceased had reached a good old age, and had worthily accomplished his allotted work.

Great as is the loss we have sustained of "guide, philosopher, and friend," we have yet the mournful satisfaction of reflecting that his influence, powerful as it always has been for good, still survives—in his works, his high example, and his unclouded memory;—that our community, our country, the world itself, has been benefited by his existence here; and that as time rolls on, its course will be marked by increasing circles of appreciation, reverence, and gratitude, for the teachings of his high and noble life.

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* Opening Address, written for the meeting of the National Academy of Sciences, April 16th, 1878. (*Proceed. Nat. Acad. Sci*, vol. i. part 2, p. 127.)

LIST OF SCIENTIFIC PAPERS; BY JOSEPH HENRY.

1825. On the production of cold by the rarefaction of Air: accompanied with Experiments. (Presented Mar. 2.) Abstract, *Trans. Albany Institute*, vol. i. part ii. p. 36.

1827. On some Modifications of the Electro-magnetic Apparatus. (Read Oct. 10.) *Trans. Albany Inst.* vol. i. pp. 22–24.

1829. Topographical Sketch of the State of New York; designed chiefly to show the General Elevations and Depressions of its Surface. (Read Oct. 28.) *Trans. Albany Inst.* vol. i. pp. 87–112.

1829. First Abstract of Meteorological Records of the State of New York, for 1828. (In conjunction with Dr. T. Romeyn Beck.) *Annual Report of Regents of University*, to the Legislature of New York.—Albany, 1829.

1829. On the Mean Temperature of Twenty-seven different Places in the State of New York, for 1828. (In conjunction with Dr. T. Romeyn Beck.) Brewster's *Edinburgh Jour. Science*, Oct. 1828, vol. i. pp. 249–259.

1830. Second Abstract of Meteorological Records of the State of New York for 1829. (In conjunction with Dr. T. Romeyn Beck.) *Annual Report of Regents of University*, to the Legislature of New York.—Albany, 1830.

1831. 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 small Galvanic Elements. Silliman's *American Jour. Science*, Jan. 1831, vol. xix. pp. 400–408.

1831. Tabular Statement of the Latitudes, Longitudes, and Elevations, of 42 Meteorological Stations in New York. *Annual Report Regents of University* to Legislature N. Y. 1831.

1831. Third Abstract of Meteorological Records of State of New York for 1830. (In conjunction with Dr. T. Romeyn Beck.) *Annual Report of Regents of University*, to the Legislature of New York.—Albany, 1831.

1831. An Account of a large Electro-magnet, made for the Laboratory of Yale College. (In conjunction with Dr. Ten Eyck.) Silliman's *Am. Jour. Sci.* April, 1831, vol. xx. pp. 201–203.

1831. On a Reciprocating Motion produced by Magnetic attraction and repulsion. Silliman's *Am. Jour. Sci.* July, 1831, vol. xx. pp. 340–343. Sturgeon's *Annals of Electricity*, etc. vol. iii. pp. 430–432.

1832. On a Disturbance of the Earth's Magnetism in connection with the appearance of an Aurora as observed at Albany on the 19th of April, 1831. (Communicated to the Albany Institute, Jan. 26, 1832.) *Report of Regents of University*, to the Legislature of New York.—Albany, 1832. Silliman's *Am. Jour. Sci.* July, 1832, vol. xxii. pp. 143–155.

1832. Fourth Abstract of Meteorological Records of the State of New York for 1831. (In conjunction with Dr. T. Romeyn Beck.) *Annual Report of Regents of University*, to the Legislature of New York.—Albany, 1831.

1832. On the Production of Currents and Sparks of Electricity from Magnetism, Silliman's *Am. Jour. Sci.* July, 1832, vol. xxii. pp. 403–408.

1832. On the effect of a long and helical wire in increasing the intensity of a galvanic current from a single element. Silliman's *Am. Jour. Sci.* July, 1832, vol. xxii. p. 408. Becquerel's *Traité* expérimental de l'Électricité, etc. 1837, vol. v. pp. 231, 232.

1833. Fifth Abstract of Meteorological Records of the State of New York. for 1832. (In conjunction with Dr. T. Romeyn Beck.) *Annual Report of Regents of University*, to the Legislature of New York.—Albany, 1833.

1835. Contributions to Electricity and Magnetism. No. I. Description of a Galvanic Battery for producing Electricity of different intensities. (Read Jan. 14.) *Transactions Am. Philosoph. Society*, vol. v. pp. 217–222. Sturgeon's *Annals of Electricity*, etc. vol. i. pp. 277–281.

1835. 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. (Read Feb. 6.) *Trans. Amer. Phil. Soc.* vol. v. pp. 223–232. Sturgeon's *Annals of Electricity*, etc. vol. i. pp. 282–290. Taylor's *Scientific Memoirs*, vol. i. pp. 540–547.

1835. Facts in reference to the Spark, etc. from a long Conductor uniting the poles of a Galvanic Battery. *Journal of Franklin Institute*, Mar. 1835, vol. xv. pp. 169, 170. Silliman's *Am. Jour. Sci.* July, 1835, vol. xxviii. pp. 327–331.

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1838. A Letter on the production directly from ordinary Electricity of Currents by Induction, analogous to those obtained from Galvanism. (Read to Philosoph. Society, May 4.) *Proceedings Am. Phil. Soc.* vol. i. p. 14.

1838. Contributions to Electricity and Magnetism. No. III. On Electro-dynamic Induction. (Read Nov. 2.) *Trans. Am. Phil. Soc.* vol. vi. pp. 303–338. Silliman's *Am. Jour. Sci.* Jan. 1840, vol. xxxviii. pp. 209–243. Sturgeon's *Annals of Electricity*, etc. vol. iv. pp. 281–310. *L. E. D. Phil. Mag.* Mar. 1840, vol. xvi. pp. 200–210: pp. 254–265: pp. 551–562. Becquerel's *Traité expérimental de l'Electricité*, etc. vol. v. pp. 87–107. *Annales de Chimie et de Physique*, Dec. 1841, 3d series: vol. iii. pp. 394–407. Poggendorff's *Annalen der Physik und Chemie.* Supplemental vol. i. (Nach Band li.) 1842, pp. 282–312.

1839. A novel phenomenon of Capillary action: the transmission of Mercury through Lead. (Read Mar. 15.) *Proceedings Am. Phil. Soc.* vol. i. pp. 82, 83. Silliman's *Am. Jour. Sci.* Jan. 1839, vol. xxxviii. pp. 180, 181. *Biblioth. Universelle*, vol. xxix. pp. 175, 176. Liebig's *Annalen der Chemie*, etc. vol. xl. pp. 182, 183.

1839. A Letter on two distinct kinds of dynamic Induction by a Galvanic current. (Read to Phil. Soc. Oct. 18.) *Proceedings Am. Phil. Soc.* vol. i. pp. 134–136.

1839. Observations of Meteors made Nov. 25, 1835, simultaneously at Princeton and at Philadelphia, for determining their difference of Longitude. (In conjunction with Professors A. D. Bache, S. Alexander, and J. P. Espy.) *Proceedings Am. Phil. Soc.* Dec. 21, vol. i. pp. 162, 163. Silliman's *Am. Jour. Sci.* Oct. 1840, vol. xxxix. pp. 372, 373.

1840. Contributions to Electricity and Magnetism. No. IV. On Electro-dynamic Induction. (Read June 19.) *Trans. Am. Phil. Soc.* vol. viii. pp. 1–18. Silliman's *Am. Jour. Sci.* April, 1841, vol. xli. pp. 117–152. Sturgeon's *Annals Electricity,* etc. vol. vii. pp. 21–56. *L. E. D. Phil. Mag.* June, 1841, vol. xviii. pp. 482–514. *Annales de Chim. et Phys.* Dec. 1841, 3d ser. vol. iii. pp. 407–436. Poggendorff's *Annal. der Phys. und Chem.* 1841, vol. liv. pp. 84–97.

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1840. Electricity from heated Water. (Read Dec. 18.) *Proceedings Am. Phil. Soc.* vol. i. pp. 322–324.

1841. Description of a simple and inexpensive form of Heliostat. (Read Sept. 17.) *Proceedings Am. Phil. Soc.* vol. ii. p. 97.

1841. Observations on the effects of a Thunderstorm which visited Princeton on the evening of the 14th of July, 1841. (Read Nov. 5.) *Proceedings Am. Phil. Soc.* vol. ii. pp. 111–116.

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1845. On Color Blindness. *Princeton Review*, July, 1845, vol. xvii. pp. 483–489.

1845. On the discharge of Electricity through a long wire, etc. (Read Nov. 7.) *Proceedings Am. Phil. Soc.* vol. iv. pp. 208, 209.

1846. Repetition of Faraday's Experiment on the Polarization of Liquids under the influence of a galvanic current. (Read Jan. 16.) *Proceedings Am. Phil. Soc.* vol. iv. pp. 229, 230.

1846. Extrait d'une Lettre à M. de la Rive, sur les Télégraphes Electriques daus les Etats-Unis de l'Amérique. *Biblioth. Universelle. Archives*, 1846, vol. ii. p. 178.

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1853. Address before the Metropolitan Mechanics' Institute, Washington. (Delivered March 19.) 8vo. Washington, 1853, pp. 19.

1854. Meteorological Tables of mean diurnal variations, etc. prepared as an Appendix to Mr. Russell's Lectures on Meteorology. *Smithsonian Report* for 1854, pp. 215–223.

1854. Thoughts on Education; an Introductory Discourse before the Association for Advancement of Education. (Delivered Dec. 28.) *Proceedings Assoc. Adv. Education*, 4th Session, 1854, pp. 17–31.

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1855. On the Induction of Electrical Currents. (Read Sept. 11.) *Proceedings Am. Academy of Arts*, etc. vol. iii. p. 198.

1855. Note on the Gyroscope. Appendix to Lecture by Prof. E. S. Snell. *Smithsonian Report*, 1855, p. 190.

1855. Remarks on Rain-fall at varying elevations. *Smithsonian Report*, 1855, pp. 213, 214.

1855. Directions for Meteorological Observations. (In conjunction with Prof. A. Guyot.) *Smithsonian Report*, 1855, pp. 215–244.

1855. Circular of Inquiries relative to Earthquakes. *Smithsonian Report*, 1855, p. 245.

1855. Instructions for Observations of the Aurora. *Smithsonian Report*, 1855, pp. 247–250.

1855. On Green's Standard Barometer for the Sm. Institution. *Smithsonian Report*, 1855, pp. 251–258.

1855. Circular of Instructions on Registering the periodical phenomena of animal and vegetable life. *Smithsonian Report*, 1855, pp. 259–263.

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1856. On Acoustics applied to Public Buildings. *Proceedings Am. Assoc.* Aug. 1856, pp. 119–135. *Smithsonian Report*, 1856, pp. 221–234. *Canadian Journal*, 1857, vol. ii. pp. 130–140.

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1859. Application of the Telegraph to the Prediction of Changes of the Weather. (Read Aug. 9.) *Proceedings Am. Academy of Arts*, etc. vol. iv. pp. 271–275.

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1860. Circular to Officers of Hudson's Bay Company (April 20). *Smithsonian Miscell. Collections*, No. 137, vol. viii. pp. 1–6.

1860. Description of Smithsonian Anemometer. *Smithsonian Report*, 1860, pp. 414–416.

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1861. Article on "Magnetism" for the American Encyclopædia. Edited by Ripley and Dana. *Am. Encycl.* 1861, vol. xi. pp. 61–63.

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1862. Report of the Light House Board on the proposed Transfer of the Lights to the Navy Department. Exec. Docts. 37th Cong. 2d Sess. Senate, Mis. Doc. No. 61, pp. 2–18.

1863. Introduction to Memoir by Prof. J. Plateau. On the Figures of Equilibrium of a Liquid Mass, etc. *Smithsonian Report*, 1863, pp. 207, 208.

1864. On Materials for Combustion in Lamps of Light-Houses. (Read Jan. 12, before the National Academy of Sciences.) [Not published in Proceedings.]

1865. Report relative to the Fire at the Smithsonian Institution, occurring Jan. 24th, 1865. (In conjunction with Mayor Richard Wallach.) Presented to the Regents February, 1865. *Smithsonian Report*, 1864, pp. 117–120.

1865. Queries relative to Tornadoes: directions to observers. *Smithsonian Miscell. Collections*, No. 190, vol. x. pp. 1–4.

1865. Remarks on the Meteorology of the United States. *Smithsonian Report*, 1865, pp. 50–59.

1865. Remarks on Ventilation: especially with reference to the U. S. Capitol. *Smithsonian Report*, 1865, pp. 67–69.

1866. Report on the Warming and Ventilating of the U. S. Capitol. (May 4.) *Exec. Doc.* No. 100. H. of Rep. 39th Cong. 1st Sess. pp. 4–6.

1866. Report of Building Committee on Repairs to Sm. Institution from Fire. (In conjunction with Genl. Richard Delafield, and Mayor Richard Wallach.) Presented to Regents April 28. *Smithsonian Report*, 1865, pp. 111–114.

1866. On the aboriginal Migration of the American races. Appendix to paper by F. Von Hellwald. *Smithsonian Report*, 1866, pp. 344, 345.

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1866. Investigations in regard to Sound. (Read Aug. 10, before the National Academy of Sciences.) [Not published in Proceedings.]

1867. Circular relating to Collections in Archæology and Ethnology. (Jan. 15.) *Smithsonian Miscell. Collections*, No. 205, vol. viii. pp. 1, 2.

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1868. Appendix to a Notice of Schœnbein. *Smithsonian Report*, 1868, pp. 189–192.

1868. On the Rain-fall of the United States. (Read Aug. 25, before the National Academy of Sciences.) [Not published in Proceedings.]

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1871. Instructions for Observations of Thunder-Storms. *Smithsonian Miscell. Collections*, No. 235, vol. x. p. 1.

1871. Circular relative to Heights. For a topographic chart of N. America. *Smithsonian Miscell. Collections*, No. 236, vol. x. p. 1.

1871. Directions for constructing Lightning-Rods. *Smithsonian. Miscell. Collections*, No. 237, vol. x. pp. 1–3. Silliman's *Am. Jour. Sci.* Nov. 1871, vol. ii. pp. 344–346.

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1871. Suggestions as to Meteorological Observations; during the Expedition toward the North Pole. *Smithsonian Report*, 1871, pp. 372–379.
1871. Meteorological Notes and Remarks. *Smithsonian Report*, 1871, pp. 452, 455, 456, 459, 461.
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