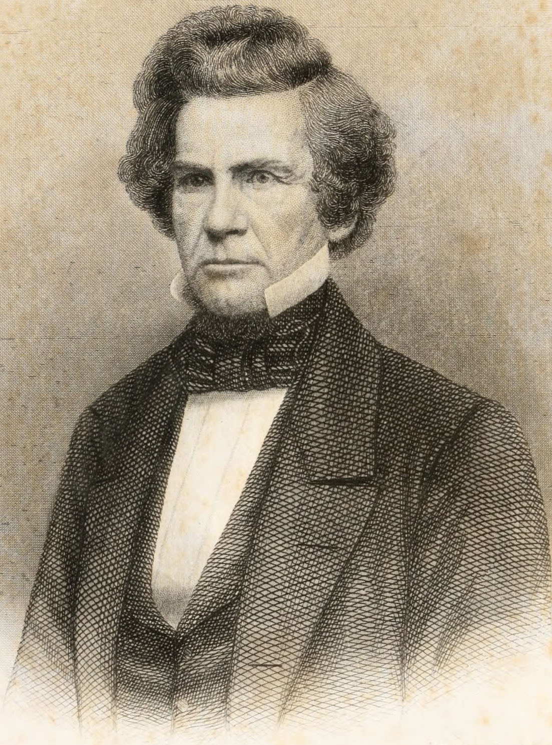

From a Photograph

Eng. by L. S. Punderson.

O. M. Mitchel

Engd. for the Annual of Scientific Discovery 1859

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ANNUAL

OF

SCIENTIFIC DISCOVERY:

OR,

YEAR-BOOK OF FACTS IN SCIENCE AND ART

FOR 1859.

EXHIBITING THE

MOST IMPORTANT DISCOVERIES AND IMPROVEMENTS

IN

MECHANICS, USEFUL ARTS, NATURAL PHILOSOPHY, CHEMISTRY, ASTRONOMY, GEOLOGY, ZOOLOGY, BOTANY, MINERALOGY, METEOROLOGY, GEOGRAPHY, ANTIQUITIES, ETC.

TOGETHER WITH

NOTES ON THE PROGRESS OF SCIENCE DURING THE YEAR 1858; A LIST OF RECENT SCIENTIFIC PUBLICATIONS; OBITUARIES OF EMINENT SCIENTIFIC MEN, ETC.

EDITED BY

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NOTES BY THE EDITOR

ON THE

PROGRESS OF SCIENCE FOR THE YEAR 1859.

THE Twelfth Meeting of the American Association for the Promotion of Science was held at Baltimore, Md., April 28th to May 5th, 1858. In the absence of both President and Vice-President elect, the chair was taken by Professor Caswell. The attendance of members was somewhat smaller than usual, and the whole number of papers presented was ninety-five; of these thirty-three pertained to the section of Astronomy, Physics, and Mathematics; nine to Meteorology; fourteen to Geology and Geography; eighteen to Chemistry, Mineralogy, and Geology; and twenty-one to Philology and Miscellaneous.

The meeting adjourned to meet at Springfield, Massachusetts, on the first Wednesday of August, 1859. Professor Stephen Alexander, of Princeton, was chosen President for the ensuing year, and Professor Edward Hitchcock, of Amherst, Vice-President.

The Association was addressed at length by Dr. Hayes, the Surgeon of the Kane Arctic Expedition, in behalf of a renewed effort to reach the open Polar Sea, described by Dr. Kane. He proposes to organize and lead an expedition, starting in the spring of 1860, and following the route pursued by Dr. Kane. The details of the plan, as given by Dr. H., were as follows:

The expedition would require two years for its operations, and in view of the rich and valuable experience of the last, he could not but deem it probable that the next attempt would prove successful. There was needed for the expedition one vessel of one hundred tons, equipped for two and a half years, and twelve men. It would greatly add to the convenience of the party to be provided with a small steam-tender of thirty tons, with a shifting screw; except for the necessity of conveying provisions, even so large a vessel as one of one hundred tons would not be necessary. The party should leave the States early in April, giving time to lay in additional fresh provisions on the Greenland coast, and so materially to reduce the cost of outfit. Before the last of August it should push up Smith's Sound to the ice-belt, with the intention of wintering as high as the 80th parallel, if possible. Smith's Sound, fortunately for this route, runs diagonally to the course of the general current — thus operating to keep Grinnell's Land free of floating ice. Under this western shore it might be possible to work the steam-tender through the leads left by the southward drifting ice, even into the heart of the Polar Sea. But this was a doubtful reliance on which they would not too much depend. It would be necessary by three or four journeys with the dog-sledges to make depots of provision as high in Grinnel's Land as on the 82d parallel. This was perfectly feasible ; each dog could be depended on to carry seventy pounds weight, thirty-two miles a day, on a ration of thirteen ounces of pemmican. In April, the party should leave the vessel, the men conveying the boats upon sledges until (and the inference was that it would be by the middle of May) the ice-belt had been crossed and the open sea reached. Dogs could not drag the boats — they were not competent to the conveyance of any such dead weight. But experience had shown that over the smooth ice, as this was likely to be, men could easily walk sixteen miles a day, dragging on sledges a weight of one hundred and ten pounds for each. To avoid the incumbrance of so much canvas, they would take no tents, but rely for shelter upon the snow-house, which could be constructed in an hour, and which was better than the tent for protection. Furs and carbonaceous food must be their reliance for warmth. While travelling, the pemmican (dried meat and tallow, of which four pounds is equal to fourteen pounds of green meat) must be the sole reliance as food. The Doctor believed that the climate was eminently a wholesome one. The danger from cold and scurvy had been greatly lessened by the experience of the last few years. Dr. Kane sailed too early to avail himself of the wonderful advantages now furnished in the concentrated fresh meats and vegetables, for protecting from and curing scurvy.

At the conclusion of Dr. Hayes's address, a resolution was adopted, referring the matter to a committee of seven, with instructions to report on the expediency of uniting with Dr. Hayes in his efforts to fit out an expedition.

The Twenty-eighth Annual Meeting of the British Association for the Advancement of Science, was held at Leeds, September 22—26, Professor Owen in the chair. The attendance was good, and the papers of more than ordinary interest.

The meeting for 1859 was appointed to be held in Aberdeen, Prince Albert being the President elect.

The annual report of the Council stated, that since the discussion at the last meeting at Dublin, relative to the formation of a " Catalogue of the philosophical papers contained in the various scientific transactions and journals of all countries," this important work has been commenced under the auspices and at the expense of the Royal

Society. It is purposed that it should include the titles (in the original languages) of all memoirs published in such works, in the mathematical, physical, and natural sciences, from the foundation of the Royal Society to the present time, the titles to be so arranged as to form ultimately three catalogues — one chronological, or in the order of the memoirs in the several series; one alphabetical, according to authors' names, and, lastly, a third, classified according to subjects.

The Council, moreover, lament that their application to the Government for an expedition to the vicinity of Mackenzie's river, for the purpose of observations in terrestrial magnetism, was not successful; but they anticipate an important accession to scientific knowledge from the expedition to the Zambesi river, which was sanctioned by the Government, and sent out under Dr. Livingstone.

The following resolutions were adopted: Resolved, that representations be made to the Meteorological department of the Board of Trade, of desirableness of connecting with its arrangements a system for the observation and record of Oceanic and Littoral Earthquakes, and of the occasional occurrence upon the coasts of Great Sea Waves, and, if practicable, of bringing such into immediate operation.

That it is highly desirable that a series of Magnetical and Meteorological Observations, on the same plan as those which have been already carried on in the Colonial Observatories for that purpose under the direction of Her Majesty's Board of Ordnance, be obtained to extend over a period of not more than five years, at the following stations: 1. Vancouver's Island; 2. Newfoundland; 3. The Falkland Isles; 4. Pekin, or some near adjacent station.

That an application be made to Her Majesty's Government to obtain the establishment of Observatories at these stations for the abovementioned term, on a personal and material footing, and under the same superintendence as in the Observatories (now discontinued) at Toronto, St. Helena, and Van Diemen's Land.

That provision be also requested at the hands of Her Majesty's Government, for the execution, within the period embraced by the observations, of magnetic surveys in the districts immediately adjacent to those sattions, viz., of the whole of Vancouver's Island and the shores of the Strait separating it from the main land, — of the Falkland Islands, and of the immediate neighborhood of the Chinese Observatory (if practicable), where situated, — on the plan of the surveys already executed in the British possessions in North America and in the Indian Archipelago.

An interesting map has been prepared, under the auspices of the Association, by Robert Mallet, of Dublin, with a view of illustrating the surface distribution of earthquakes, the position and situation of all volcanoes, fumaroles, and solfataras, now active, or presumed to have been so, within historic or recent geologic periods, as well as the *seismic* (from the Greek word signifying earthquakes) bands in position and relative intensity. The area of supposed land and sub-oceanic subsidences are also indicated. The map conveys at a glance the portions of the globe in which volcanic eruptions are most prevalent. These appear by contrast to be the islands and oceans surrounding Borneo, where alone are given upwards of one hundred indications, the Gulf of Mexico, and the Andes of South America. In the northern regions, Iceland alone stands out in marked prominence; whilst the whole of Africa, with the exception of the Cape and its northern boundary, and the continent of South America east of the Andes, appear to be totally unaffected by the laws of earthquakes. The greatest area of subsidence appears to be in the Pacific Ocean, extending in a direction southeast from the Philippine Islands to Pitcairn's Island.

The National Association (Great Britain) for the Advancement of Social Science, held its second anniversary meeting in Liverpool, in October, with distinguished success, about three thousand persons being in attendance. The special object of the association, as stated in the constitution of the Society, is "to form a point of union among social reformers, so as to afford those engaged in all the various efforts now happily begun for the improvement of the people, an opportunity of considering social economics as a whole." At the Liverpool meeting, Lord John Russell presided, and delivered the introductory address. During the continuance of the session, an address of great interest was also delivered by Lord Brougham, on "Popular Education and Popular Literature." From this address we make the following extract, in which the author illustrates the benefit accruing from the labors of the Society for diffusing useful knowledge, and defends the publications issued by it from the charge of encouraging superficial acquirements: "When it is said, or sung, by such objectors, that 'a little learning is a dangerous thing,' we can see no harm in adding, that there is another thing somewhat more dangerous — great ignorance; not to mention that the one cures itself, while the other perpetuates itself — ay, and spreads and propagates, too; for it is almost as true in point of fact that they who have learned a little have their half-satisfied curiosity excited to obtain more full gratification, as it is false in point of fact that sobriety results from excess of drinking. We object, therefore, to this hackneyed maxim, not because it is hackneyed, but because it is unfounded; as illogical when delivered in plain prose as inapposite when clothed in humorous verse — the falsehood of the position in the one case being equal to that of the metaphor in the other. 'Better half a loaf than no bread,' is the old English saying. 'All wrong,' say the objectors, 'a little food is a dangerous thing; rather starve than not have your fill.' 'Better be purblind than stone blind,' is the French saying. 'No,' cry the objectors; 'if you can't see quite clearly, what use is there in seeing at all?' 'In the country of the blind,' says the proverb, 'the one-eyed man is king.' Our objectors belonging to the people there would dethrone the monarch by putting out his eye. But they had better couch their blind brethren to restore their sight, and then his reign would cease at once without any act of violence, any *coup d' état.* Here is a well of precious water, and we have got a little of it in a tankard. 'What signifies,' say the objectors, 'such a paltry supply? It would not wet the lips of half a dozen of the hundreds who are athirst.' True, but it enables us to wet the sucker of the pump, instead of following their advice to leave it dry; and, having the handle, we use it to empty the well and satisfy all. A person gains some information, it may be only a little. Say the objectors, 'He is superficial.' Would he be more profound if he knew nothing? The twilight is unsafe for his steps. Would he be more secure from slipping in the dark? But he may be self-sufficient, may think he knows much, and look down upon others as knowing little. Is this very likely to happen if the knowledge he has acquired is within reach of all and by the greater number possessed? The distinction is the ground of the supposed influence upon his demeanor towards others; when that difference no longer exists, the risk of his manners being spoiled is at an end. The most trifling instruction which can be given is sure to teach the vast majority of those who receive it the lesson of their own deficiency, and to inspire the wish for further knowledge. But suppose, as must happen in many cases, that no great progress shall be afterwards made, at least it is certain that the proportion is most inconsiderable of those who are not the better for what they have learned, and of those who are the worse for it the number cannot really be said to have any existence at all. It must always be kept in mind that there are two descriptions of persons to whom popular literature is addressed, and who may in different ways profit by it — those who from their natural capacity and natural inclination, as well as from possessing a certain leisure, can so far improve themselves as to become really accomplished in the branches of knowledge which they study, and the great bulk of the community who can never go beyond giving a very moderate attention to books, can in fact read but very little. Let us first consider the former class, which, though small compared with the mass, is yet again divided into two, those of ordinary talents, but anxious to learn, and those whose thirst for knowledge is not only very great, but accompanied with capacity to excel, possibly even with original genius. Both classes benefit incalculably by the helps which popular literature extends to them. Their love of knowledge is both excited and gratified, and let us observe their progress. The different works which are prepared encourage and enable them to proceed. At first they are attracted by some tale or anecdote, or biographical account. Soon after they find in the same paper a popular exposition of a subject in science or literature. This inclines them to go further, and the treatises furnished by the Useful Knowledge Society are within their reach on different subjects, suiting the line they desire to follow, and in various kinds in point of difficulty, and thus adapted to the progress they may have made, from Mr. Marcet's plain and elementary explanations, up to the treatises of learned professors like those of the Astronomer Royal, Sir D. Brewster, and Professor de Morgan. So great and varied are the helps afforded to students in humble life that it has been said that there can be no such thing now as a self-taught person. Let us only reflect how mighty would have been the comfort to such students in former times could they have enjoyed such facilities. What would Franklin have given for them, who, living on a vegetable diet on purpose to save a few pence from his day's wages for the purchase of books, was fain to learn a little geometry from a treatise on navigation he had been happy enough to pick up at a bookstall, something of arithmetic by having fallen upon a copy of *Cocker*, and from an odd volume of the *Spectator* gained a notion of the style he afterwards so powerfully used? What would Simpson have given for access to books, who could only get, from the accident of a peddler passing the place where he was kept by his father working at his trade of a weaver, the copy of *Cocker* containing a little Algebra, and even when grown up could only, by borrowing Stone's translation of *L' Hopital* from a friend, obtain an insight into the science of infinitesimals, on which two years after he published an admirable work, while continuing to divide his time between his toil as a weaver and as a teacher? Brindley, the great engineer, was through life an uneducated man; Rannequin is said never to have learnt the alphabet; and both executed great works, but with difficulties and delays which reading would have spared them. Harrison, too, though he had received an ordinary education, yet only while working in his trade of a carpenter, became acquainted with science by some manuscript lectures of Sanderson falling in his way; and so hard did he find it to obtain adequate knowledge on the subjects connected with his mechanical pursuits, that forty years were spent in perfecting his admirable improvements on the construction of time-keepers, and bringing them into use. It would be going too far to hold that Franklin's genius, both in physical and political science, could have done greater things had his original difficulties in self-education been removed; but we may safely affirm that both Brindley, Rannequin, and Harrison, would have effected far more with the helps which their successors have had; and of Simpson no doubt can be entertained that, even amid the distractions of his trade, his short life would have been illustrated by far greater steps in mathematical science. For it is an entire mistake to suppose, with some of his biographers, that his genius was not original, and fitted to make great advances in his favorite study. The late proceedings respecting Sir Isaac Newton's monument, have led to ascertaining that Simpson had made the same approaches towards the modern improvement of the calculus which its illustrious inventor himself had done, but kept concealed; and no doubt can be entertained that the germ of the great discovery of Lagrange and Laplace on the stability of the solar system, is to be found in the last and most remarkable work of Simpson. It is truly delightful to contemplate such feats of genius, so scantily aided, in a hard-working mechanic, patronized by none."

The Thirty-fourth meeting of the German Association of Science and Medicine, was held at Carlsruhe, September 17th, 1858, under the presidency of Prof. Eisenlohr. Nearly twelve hundred members, representing all the states of Europe, were present, and many papers of great interest were offered.

The "Societe Zoölogique d'Acclamation," of France, still continues in the full tide of successful experiment. A foreign correspondent of *Silliman's Journal* enumerates the following as a part of the services it has already rendered :

In 1854, it purchased half of the only herd of yaks which had come to Europe ; and now the yak, through its care, has become acclimated without difficulty, and has prospered. In 1855, it distributed nearly a million of bulbs of the yam *(Dioscorea Batatas)* ; now the yam is cultivated at large over Europe, and it promises to rival the potatoe, when through successive sowings it shall have lost its long form. The Society has spread everywhere the Sorghum sugar cane, (*Holcus saccharatus*), which already furnishes to the departments of middle and southern France abundant forage of good quality, while at the same time, through its saccharine juices, it promises to be as valuable to the southern provinces of France as the beet to the northern. It has procured young plants of the *Loza*, a species of *Rhamnus*, from which the Chinese extract the fine green color called the *Kao.* It has imported two herds of Angora goats, which reproduce perfectly in Europe without manifesting any symptoms of degenerating. It has not only acclimated the silk worm of the Ricinus (*Bombyx Cynthia*, or Palma Christi silkworm, a native of India), which is already in France to its twenty-fifth generation, but it has done more, in succeeding in varying its nourishment, by substituting the leaf of the very common *Fuller's Teasel* (*Dipsacus Fullonum*) for the Ricinus (*R. Europœus*), which is rare, and does not grow in our climate without great care ; and it has almost succeeded in regulating the time of hatching, so as to make the birth of the worms correspond with the development of the leaves on which they are nourished. It has already nearly accomplished the propagation in the open air of a silkworm living on the oak. In has raised, in the Jardin des Plantes, two new kinds of Chinese oaks. It has undertaken to grow the white nettle of China, with which fabrics may be made more firm than those of linen or the indigenous hemp. It has promoted the culture of the oleaginous pea, excellent as food, and affording an abundant oil. It has received in portable greenhouses the wax-tree and varnish-tree in good condition, with the insects which frequent them. Finally, through

M. D. de L'Huys, its Vice-President, it has succeded in procuring from the slopes of the Cordilleras numerous tubercles of potatoe, in order to renew in Europe this so valuable species, which, through exaggerated culture and long-continued disease, has lost a part of its qualities.

The report of the Meteorological Department of the English Board of Trade, published June, 1858, states that much information relative to winds and currents has been recently collected from various seas, from many foreign stations on land, and from the Pacific and Atlantic oceans. By very numerous trials, the specific gravity of nearly all the oceanic surface has been ascertained; and it is believed that these results will render further observations of the kind unnecessary, except in peculiar and limited localities, for some special object. Distilled water being taken as 1·000, the specific gravity of oceanic water is found to be nearly 1·027. The lowest temperature hitherto recorded (between 2·300 and 2·500 fathoms below the surface) has been 35 deg. in the North Atlantic, South Atlantic, and Indian oceans, and 86 deg. the highest temperature anywhere at sea on the surface. The total pressure of the barometer varies so little throughout the year within certain limits of latitude near the equator, or rather at about 5 deg. of north latitude in the Atlantic, that (allowing for the six-hourly change) any ship crossing that part of the sea may actually compare her barometer with a natural standard, invariable within those small limits of 2-100ths to 3-100ths of an inch. Hygrometric inquiries are steadily, though slowly, proceeding. Magnetism has not occupied much thought, because it is zealously attended to in other departments of the Government. The report rather gives a general idea of what is being done, than the actual results of the labors of the department.

The managers of the Royal Institution, London, intrusted with the award of the Actonian prize for the best essay " On the wisdom and goodness of the Creator as displayed in the Radiation of Heat," have reported that, in their judgment, no essay had been received by them within the period of seven years since the last award in 1851, of sufficient merit to entitle the author thereof to the prize of £105; that, consequently, no prize was awarded this year; and that the £105 intended to have been awarded, would, pursuant to the trust-deed, be retained, and awarded, with another sum of £105, in 1865, of which due notice would be given.

The present Emperor of the French, in 1852, decreed that a prize of £2,000 should be awarded to the person who, in the course of the ensuing five years, should make the most useful application of the Voltaic pile, and he charged a commission, consisting of M. Dumas, M. Becquerel, M. Pelouze, and M. Despretz, of the Institute, and of other eminent scientific men, to select the recipient of the prize. This commission has recently reported to his Majesty, that, after carefully examining all the improvements in the application of the Voltaic pile made during the last five years in all the countries of Europe, it does not think any of them of sufficient importance to merit the prize; and accordingly, the Emperor, in compliance with its recommendation, has decreed that the prize shall remain open for a second period of five years.

A French gentleman, named Breant, who died some years back, bequeathed the sum of £4,000 to the Academy of Sciences of Paris, to be given to the author of a sovereign cure for the cholera. In 1854, the Academy reported that, though numerous persons had competed for the prize, none of them had obtained it; and during the last year it again reported that though, since 1854, as many as fifty-three memoirs or communications on the subject had been sent in, not one was deserving of the promised reward. The field, consequently, is still open to competitors.

An imperial ukase has been issued at St. Petersburg suppressing the teaching of Latin in all the colleges of the empire. The time hitherto devoted to this study is to be added to that of the positive sciences.

The London Geographical Society has awarded the Victoria Gold Medal for 1858, to Prof. A. D. Bache, Superintendent of the United States Coast Survey, for his extensive and most accurate surveys of America, and for the additions made by him to our knowledge of geography and hydrography. Another gold medal has been also presented to Capt. R. Collinson, R. N., for his successful discoveries in the Arctic Regions, and for having, in Her Majesty's ship *Enterprise*, penetrated further to the eastward, through Behring Strait, than had been reached by any other vessel.

At a recent meeting of this society, also Sir R. I. Murchison read an account of a highly interesting journey through the Elboorz Chain of Central Asia, and of the ascent of the lofty volcanic mountain of Demavend, by Mr. R. F. Thomson and Lord Schomberg Kerr, both attached to the Persian mission. Having succeeded in reaching the summit of Demavend with instruments, the adventurous diplomatists have determined its height to be 21,500 feet, and have thus deprived Mount Ararat of the reputation, so long enjoyed, of being the highest point in Central Asia.

The directors of the Geological Survey of Great Britian, have recently presented to the State Cabinet of New York, and the Museum of the Geological Survey of Canada, at Montreal, a complete set of the duplicate fossils collected during the survey of the United Kingdom. These collections are carefully labelled, and, in their future locations at Albany and Montreal, will constitute an important addition to the resources of American geologists.

Some discussions of interest have taken place during the past year in reference to the existence of an ethereal medium in the interplanetary spaces; and M. Babinet, before the French Academy, has asserted that we have no evidence whatever upon the subject. Encke, however, has taken occasion of the reappearance of the comet bearing his name to again promulgate his belief in the existence of the ether, and claims that its resisting influence on the above-named comet is more manifest than ever; while M. Faye, following Babinet, has replied that he is unable to see how Encke's views can be maintained.

Mr. Hind, the English astronomer, in a recent publication, offers a protest against the names given to the young members of our planetary family. He says:

"A few months since, my attention was directed, by Sir John Herschel, to the inconvenience and confusion which are being gradually introduced into the nomenclature of the minor planets by the acceptance of names easily mistaken, either in speaking or writing, for others belonging to planets previously discovered. I have been fully sensible of the liability to error or misapprehension thereby induced, and am desirous of recording a protest against any further continuance of what must eventually become a positive nuisance to those who are more particularly occupied with the observations and calculations bearing upon this numerous group of planets. Thus, we have already: Thetis, Themis; Lutetia, Lætitia; Iris, Isis; Vesta, Hestia; Pallas, Pales. It will naturally be the wish of every discoverer of a planet that his *enfant trouve* should be known to posterity by the name which it has borne during his lifetime; but if the practice to which allusion is here made, be suffered to continue much longer, there is certainly a probability that a day will arrive when, for the sake of their general convenience, astronomers will consign these troublesome names to oblivion, and substitute others less liable to engender confusion. This consideration alone, we might suppose, would prove sufficiently powerful to induce hesitation on the part of the discoverer before accepting any name likely to be objected to on the score of similarity with that of a planet previously found."

The following is a list of the comets now known or supposed to be periodical, or which belong to our solar system. The periodicity of the last twelve, or of those whose computed times or revolution exceeds fourteen years, with the exception of that of Halley, can, however, only be rendered certain by their actual return at the expiration of the predicted time. It will be seen that, in all but seven instances, the comets bear the names of the astronomers by whom they were first observed. In these seven exceptions, titles have been selected by the discoverers, principally from the names of individuals whom they have desired to honor:

Comet of	Period in years.	Discovered by	
Encke	3.3	Pons	1818
Blanpain	4.8	Blanpain	1819
De Vico	5.5	De Vico	1844

Comet of	Period in years.	Discovered by	
Brorsen — Bruhns	5.6	Brorsen Bruhns	1846 1857
Lexell	5.6	Messier	1770
Pons — Winnecke	5.6	Pons Winnecke	1819 1848
D'Arrest	6.4	D'Arrest	1851
Biela	6.6	Montaigne	1772
Faye	7.4	Faye	1843
Peters	12.8	Peters	1846
Mechain — Tuttle	13.6	Mechain Tuttle	1790 1858
Westphal	69.0	Westphal	1852
Pons	70.7	Pons	1812
De Vico	72.8	Jesuits	1846
Olbers	74.0	Olbers	1815
Brorsen	75.0	Brorsen	1847
Halley	76.1	Apien	1531
Flamsteed	190.0	Flamsteed	1683
Olcott	241.0	Peters	1857
Charles V.	292 0	Fabricius	1556
Bremiker	344.0	Bremiker	1840
Brorsen	401.0	Brorsen	1846
Perry	4220.	Perry	1793

An able article in the July (1858) number of the *Westminster Review*, on " Recent Astronomy, and the Nebular Hypothesis," takes decided ground against the results of what it terms " the rash speculations of late years," as embraced in the belief that all nebulæ are galaxies of stars. The writer defends the nebular hypothesis with much force of argument, and asserts that " the various appearances these nebulæ present are clearly explicable as different stages in the precipitation and aggregation of diffused matter." He asserts that, on the one hand, all the leading phenomena of the solar system, and the heavens in general, are explicable " by the nebular hypothesis," and, on the other hand, that " the common cosmogony is not only without a single fact to stand upon, but is at variance with all our positive knowledge of nature.

M. Wolf, of Zurich, in a letter addressed to General Sabine, states that further researches into the phenomena of the relation between the spots on the sun and terrestrial magnetism have led to the discovery that there is even a greater correspondence between the solar spots and terrestrial magnetism than he had originally imagined, and that sufficient data now exist to satisfy even the most skeptical of the actual correspondence between these phenomena.

The European Statistics of Suicide, recently published in France by Mr Lisle, show that England is no longer at the head of the dreary list. The French author proves that France is the highest in the scale, and Russia lowest. In London, we have one suicide in 8,250 people. Paris gives one in 2,221. For the whole English population, the suicides reckon one in 15,900; France, one in 12,489. The north of France is the most prolific in suicides, that district yielding nearly half of the whole number in the entire empire.

The following is an abstract of a paper recently presented to the Berlin Academy by M. Dieterici on the population of the earth: The author estimates the actual population of the earth at 1,283 millions, viz., Europe, 272; Asia, 750; America, 59; Africa, 200; and Australia, 2. The average of the valuations made by geographers gave, he says, the number of the inhabitants of Europe at 258 millions; but as most of them, owing to the period when their works were published, had not the advantage of the several census taken within the last fifteen years, the number of 272,000,000 was that which came nearest to the truth. The progressive increase in the population of Europe was, moreover, enormous. In 1787, according to a calculation made by order of Louis XVI., it amounted to 150 millions; and in 1805, it had reached 200 millions. It was more difficult to estimate the population of Asia, for geographers who had written during the last twenty-five years on the subject, had shown extraordinary differences of opinion. Some had given to that part of the world only 390 millions of inhabitants, whereas China alone contained a greater number. The figure of 750 millions might be considered as near as possible to the truth, when the difficulties of getting at any exact estimate was looked to. As far as regards Africa, still greater uncertainty prevailed. The author, however, has carefully availed himself of the works of the last explorers of the central portion of that country, as well as of the official returns from Algeria, Senegal, and the Cape of Good Hope. The estimate made by him may be, perhaps, about one-quarter or one-fifth too high.

At the last meeting of the British Association (Leeds, 1858), Mr. William Fairbairn, in an address, thus briefly reviewed the prospects and recent progress of Mechanical Science in Great Britain: Malleable iron, now applied to the construction of bridges, was capable of great development, and there was no span between the limit of one thousand feet which might not be compassed by the hollow girder bridge. With respect to steam navigation much remained to be done, with the object of giving uniformity of strength and security of construction. The *Leviathan*, with all her misfortunes, was a magnificent specimen of naval architecture, the cellular system, so judiciously introduced by Mr. Brunel, being her great source of strength. He was so persuaded of the security of the principle upon which she had been constructed, that he had no doubt she would stand the test of being suspended upon the two extreme points of stem and stern, with all her machinery on board; or she might be poised upon a point in the middle, like a scale-beam, without fracture or injury to the material of which she is composed. He expressed the hope that the necessary funds would be forthcoming to complete her equipment, and that we should then see her dashing aside the surge of the Atlantic at a speed of eighteen to twenty knots an hour. In Great Britain there are now 9,500 miles of railway; and taking, at a rough calculation, one locomotive engine with a force of 200 horses power to every three miles of railway, and assuming each to run 120 miles per day, we might thence calculate the distance travelled over by trains to be equal to 380,000 miles per day, or 138,000,000 miles per annum. To transport these trains required a force equivalent to 200,000 horses in constant operation thoughout the year. In the locomotive engine there has been no improvement of consequence during the last two years, excepting only its adaptation to burning coal instead of coke; but in the formation of the permanent way, considerable improvements had been effected, especially in the jointing of the rails by the process known as fish-jointing.

Admiral Moorson in alluding to the lack of progress in some departments of Naval Architecture, and especially as regards the capabilities of marine steamers, expressed his opinion that if experiments were conducted at sea under a vast variety of conditions as to form, size, and circumstances, rules might be established which would serve to determine much of what was now the subject of controversy, and go far to remove the reproach on the great maritime nations of the world, which was contained in the following passage of a work by Mr. Scott Russell: "It is admitted that out of every three steam-vessels that are built, two fall very far short of fulfilling the intention with which they were constructed."

During the past year the publication of an American Mathematical Journal, edited by Mr. J. D. Runkle, of Cambridge, has been commenced, under the endorsement of the American Association for the Promotion of Science, and the best mathematical talent of the country. It proposes to include in its pages solutions, demonstrations, and discussions, in all branches of the science, as well as in all its various applications; also notes and queries, with notices and reviews of all the principal mathematical works issued in this country or in Europe.

A gallery of portraits of distinguished scientific men is now in the course of publication at Vienna, and will consist, when complete, of a folio volume of one hundred lithographic plates, executed in the highes style of art, — each portrait being accompanied with a leaf of text. The gallery commences with Humboldt, and following him there are three or more in each of the departments, — Mathematics, Physics, Chemistry, Astronomy, Meteorology, Geography, Geology, Mineralogy, Botany, Zoölogy, Anatomy, and Physiology. The physicists included are: Amici, Baumgartner, Biot, Brewster, Ettingshausen, Faraday, Hansteen, Herschel, Jacobi, Magnus, Müller, Neumann, Plucker, Poggendorff, Pouillett, Weber, and Zantedeschi.

A portion of the report of the Canadian Geological Survey on the organic remains of Canada, has been recently published by Mr. Billings, Palæontologist of the survey, and treats of the Cystideæ, Starfishes, and bivalve Crustaceans. It is a work of great merit, enlarging our knowledge of one of the obscurest departments of Palæontology. Besides Cystideæ and Star-fishes, a new genus called *Cyclocystoides*, containing discoid species of Echinoderms, is described by E. Billings and J. W. Salter. It is remarkable that the Canadian Lower Silurian rocks have furnished twenty-one species of Cystideæ, while in New York only one has been found in the rocks of that age.

During the past year, the remarkable work of De la Rive, on Electricity, has been completed by the publication of the third volume, and the entire work may now be regarded as the most complete treatise on the subject of electricity extant. As to the cause of terrestrial magnetism, the author inclines to the theory, that it resides in the sun, which acts upon the earth as in the ordinary experiment by rotation a magnet acts upon a body having a rotating movement. But whence the magnetism of the sun?

Within a very recent period a newspaper in the Maori, or native New Zealand language, has been started at Wellington, N. Z. It is called the "Messenger of Port Nicholson."

The London *Athenæum* for March 13, 1858, contains a letter from Capt. Freeling, Surveyor-General at Adelaide, in respect to the explorations undertaken in the central part of Australia to determine whether a navigable inland sea existed there, as has been supposed. No water was found on which a boat would float. "I was away," says Capt. Freeling, "more than two months, and during that time must have travelled a thousand miles, and I verily believe that there is no other country in the world where so much barren land exists in a similar space. It is really wonderful to see the masses of stone which lie on the hills and plains as on a newly Macadamized road, as well as the absence of grass in places where the stones are not so thickly spread; but all this barrenness may easily be accounted for by the fact that but little rain falls to promote fertility. Occasionally, as in March of this year, an extraordinary rain-fall occurs; then the creeks, which are for years together dry, pour down an amazing volume of water, flooding the lands in their neighborhood, and eventually discharging themselves over a vast, slightly hollowed plain, which then has all the appearance of a large inland sea. Test it, however, as I did, by walking three miles into it, and you then see its true character, and are able to state positively that the summer heats will not have continued long before the whole is evaporated."

During the past year another European expedition into Central Africa has been organized. Its projector is Baron von Krafft, whose intention is to visit the interor of Soudan. He has embarked for Tripoli, and will probably take the route from Ouargla to Djebel Hoggar, a route which has never been followed by Europeans.

A letter from Baron Krafft to Humboldt, dated April 10, 1858, from Algiers, expresses the desire of the author to continue the discoveries of Dr. Barth, so far as limited resources will allow. He will travel in the incognito of a Turkish physician, provided with allopathic and homœopathic medicines, and attended by an Algerian Moor, who is acquainted with the native method of practice. An aneroid barometer, several thermometers, two compasses, a chronometer, a sextant, and a telescope, are the instruments which he carries. He intends, however, to devote himself chiefly to such observations as can be made without his instruments; to collect minerals and plants; to inquire into the trade, language, history, and literature of the people whom he visits; and to determine with the greatest possible accuracy the various routes of caravans, and their various stopping-places. The route of travel which Baron Krafft has marked out for himself is from Tripoli to Ghadames, and thence to Ain Salah and Timbuctoo. Then he proposes to visit Lake Tsad, and afterwards to go, according to his strength and means, either east to Wara and Dar Fur, or north to Bilma, Seggadem and Murzuk.

Robert Stephenson, the eminent English engineer, in a letter addressed to the *London Times*, thus expresses his views in reference to the feasibility of the proposed ship-canal across the Isthmus of Suez: "I should be delighted," he says, "to see a channel like the Dardanelles or the Bosphorus penetrating the isthmus that divides the Red Sea from the Mediterranean; but I know that such a channel is impracticable, — that nothing can be effected, even by the most unlimited expenditure of time, and life, and money, beyond the formation of a stagnant ditch between two almost tideless seas, unapproachable by large ships under any circumstances, and only capable of being used by small vessels when the prevalent winds permit their exit and their entrance. I believe that the project will prove abortive in itself and ruinous to its constructors; and entertaining this view, I will no longer permit it to be said that by abstaining from expressing myself fully on the subject, I am tacitly allowing capitalists to throw away their money on what my knowledge assures me to be an unwise and unremunerative speculation."

At a recent show of the Royal Agricultural Society, held at Chester, England, five steam ploughs contested for the handsome prize of £500 ($2,425). Four of the ploughs were operated by steam-engines fixed on the field, and moving the "shares" back and forth by ropes and windlasses. The fifth plough (Boydells') had a traction engine which moved over the field. Each of these turned over four furrows at once, and the work was well done by them, — all but one, which broke down. The soil was a hard, dry, stiff clay. Furrows of nine inches depth were turned over, and the competition was very spirited. The successful plough was Fowler's; it executed one and three-quarters of an acre in two hours.

At the present time, the Sorgho, or Chinese sugar cane, is extensively cultivated in the South of France, and its products have constituted a prominent feature in recent agricultural exhibitions of

France. At an exhibition at Avignon, M. Prieur exhibited a group of samples illustrative of the metamorphoses to which he has subjected it. Nothing could be more curious than the succession of transformations there shown. In one corner could be seen the sorgho in stalk, such as it is when cut; a little further, were its fibres converted into thread, in skein; then a piece of linen woven with the thread; then a handsome cloak, bordered with furs, which M. Prieur designs for the Prince Imperial.

The most curious and complete array of the products of the sorgho, however, at the same exhibition, was that of Dr. Sicard, of Marseilles. From the pith, he has obtained sugar; from the seeds, flour and fecula, which have been worked up into a great variety of palatable products. He extracts also from the plant alcohol, and a variety of wine, and a variety of dyes, well adapted to wool and cotton; and finally, from the refuse stalks he has manufactured a fair article of paper.

THE

ANNUAL OF SCIENTIFIC DISCOVERY.

MECHANICS AND USEFUL ARTS.

ADDRESS OF PROF. OWEN BEFORE THE BRITISH ASSOCIATION, 1858.

THE following is an abstract of an address delivered by Professor Owen, on assuming the chair as President of the Twenty-eighth Annual Meeting of the British Association for the advancement of Science, September 22, 1858:

GENTLEMEN OF THE BRITISH ASSOCIATION: We are here met, in this our Twenty-eighth Annual Assembly, to continue the aim of the Association, which is the promotion of Science, or the knowledge of the Laws of Nature; whereby we acquire a dominion over nature, and are thereby able so to apply her powers as to advance the well-being of society and exalt the condition of mankind. It is no light matter, therefore, the work that we are here assembled to do. God has given to man a capacity to discover and comprehend the laws by which His universe is governed; and man is impelled by a healthy and natural impulse to exercise the faculties by which that knowledge can be acquired. Agreeably with the relations which have been instituted between our finite faculties and the phenomena that affect them, we arrive at demonstrations and convictions which are the most certain that our present state of being can have or act upon. Nor let any one, against whose prepossessions a scientific truth may jar, confound such demonstrations with the speculative philosophies condemned by the Apostle; or ascribe to arrogant intellect, soaring to regions of forbidden mysteries, the acquisition of such truths as have been or may be established by patient and inductive research. For the most part, the discoverer has been so placed by circumstances, — rather than by predetermined selection, — as to have his work of investigation allotted to him as his daily duty; in the fulfilment of which he is brought face to face with phenomena into which he must inquire, and the result of which inquiry he must faithfully impart. The advance of natural as of moral truth has been and is progressive; but it has pleased the Author of all truth to vary the fashion of the imparting of such parcels thereof as He has allotted, from time to time, for the behoof and guidance of mankind. Those who are privileged with the faculties of discovery are, therefore, to be regarded as preördained instruments in making known the power of God, without a knowledge of which, as well as of Scripture, we are told that we shall err. Great and marvellous have been the manifestations of this power imparted to us of late times, not only in respect of the shape, motions, and solar relations of the earth, but also of its age and inhabitants. In regard to the period during which the globe allotted to man has revolved in its orbit, present evidence strains the mind to grasp such sum of past time with an effort like that by which it tries to realize the space dividing that orbit from the fixed stars and remoter nebulæ. Yet, during all those eras that have passed since the Cambrian rocks were deposited, which bear the impressed record of creative power, as it was then manifested, we know, through the interpreters of these "writings on stone," that the earth was vivified by the sun's light and heat, was fertilized by refreshing showers and washed by tidal waves. No stagnation has been permitted to air or ocean. The vast body of waters not only moved, as a whole, in orderly oscillations, regulated, as now, by sun and moon, but were rippled and agitated here and there successively by winds and storms. The atmosphere was healthily influenced by its horizontal currents, and by ever-varying clouds and vapors rising, condensing, dissolving, and falling in endless vertical circulation. With these conditions of life, we know that life itself has been enjoyed throughout the same countless thousands of years; and that with life, from the beginning, there has been death. The earliest testimony of the living thing, whether shell, crust, or coral, in the oldest fossiliferous rock, is at the same time proof that it died. It has further been given us to know, that not only the individual but the species perishes; that as death is balanced by generation, so extinction has been concomitant with creative power, which has continued to provide a succession of species; and furthermore, that as regards the varying forms of life which this planet has witnessed, there has been "an advance and progress in the main." Geology demonstrates that the creative force has not deserted this earth during any of her epochs of time; and that in respect to no one class of animals has the manifestation of that force been limited to one epoch. Not a species of fish that now lives, but has come into being during a comparatively recent period: the existing species were preceded by other species, and these again by others still more different from the present. No existing genus of fishes can be traced back beyond a moiety of known creative time. Two entire orders (Cycloids and Ctenoids) have come into being, and have almost superseded two other orders (Ganoids and Placoids), since the newest or latest of the secondary formations of the earth's crust. Species after species of land animals, order after order of air-breathing reptiles, have succeeded each other; creation ever compensating for extinction. The successive passing away of air-breathing species may have been as little due to exceptional violence, and as much to natural law, as in the case of marine plants and animals. It is true, indeed, that every part of the earth's surface has been submerged; but successively, and for long periods. Of the present dry land, different natural continents have different Faunæ and Floræ; and the fossil remains of the plants and animals of these continents respectively show that they possessed the same peculiar characters, or characteristic *facies*, during periods extending far beyond the utmost limits of human history. Such, gentlemen, is a brief summary of facts most nearly interesting us, which have been demonstratively made known respecting our earth and its inhabitants. And when we reflect at how late and in how brief a period of historical time the acquisition of such knowledge has been permitted, we must feel that, vast as it seems, it may be but a very small part of the patrimony of truth destined for the possession of future generations.

In reviewing the nature and results of our proceedings during the last twenty-seven years, and the aims and objects of our Association, it seems as if we are realizing the grand Philosophical Dream or Prefigurative Vision of Francis Bacon, which he has recounted in his "New Atlantis." In this noble parable the father of Modern Science imagines an Institution which he calls "Solomon's House," and informs us by the mouth of one of its members, that "The end of its Foundation is the Knowledge of Causes and Secret Motions of Things; and enlarging of the bounds of Human Empire to the effecting of all things possible." As one important means of effecting the great aims of Bacon's "six days' college," certain of its members were deputed, as "merchants of light," to make "circuits or visits of divers principal cities of the kingdom." This latter feature of the Baconian organization is the chief characteristic of the "British Association." But we have striven to carry out other aims of the "New Atlantis," such as the systematic summaries of the results of different branches of science, of which our published volumes of "Reports" are evidence; and we have likewise realized, in some measure, the idea of the "Mathematical House" in our establishment at Kew. The national and private observatories, the Royal and other scientific Societies, the British Museum, the Zoölogical, Botanical, and Horticultural Gardens, combine in our day to realize that which Bacon foresaw in distant perspective. Great, beyond all anticipation, have been the results of this organization, and of the application of the inductive methods of interrogating nature. The universal law of gravitation, the circulation of the blood, the analogous course of the magnetic influence, which may be said to vivify the earth, permitting no atom of its most solid constituents to stagnate in total rest; the development and progress of Chemistry, Geology, Palæontology; the inventions and practical applications of Gas, the Steam-engine, Photography, Telegraphy, — such, in the few centuries since Bacon wrote, have been the rewards of the followers of his rules of research. Prof. O. then dwelt on the importance of direct observation, as illustrated in the history of Astronomy — referred to the discovery of Galileo, the application of his discovery by Kepler and Horrocks, and continued: Without stopping to trace the concurrent progress of the science of motion, of which the true foundations were laid, in Bacon's time, by Galileo, it will serve here to state that the foundations were laid and the materials gathered for the establishment by a master-mind, supreme in vigor of thought and mathematical resource, of the grandest generalization ever promulgated by science — that of the universal gravitation of matter according to the law of the inverse square of the distance. The same century in which the "Thema Cœli" of Lord Verulam and the "Nuncius Sidereus" of Galileo saw the light, was glorified by the publication of the "Philosophiæ Naturalis Principia Mathematica" of Newton. Has time, it may be asked, in any way affected the great result of that masterpiece of human intellect? There are signs that even Newton's axiom is not exempt from the restless law of progress. The mode of expressing the law of gravitation as being "in the inverse proportion of the square of the distances" involves the idea that the force emanating from or exercised by the sun must become more feeble in proportion to the increased spherical surface over which it is diffused. So indeed it was expressly understood by Halley. Professor Whewell, the ablest historian of Natural Science, has remarked, that "future discoveries may make gravitation a case of some wider law, and may disclose something of the mode in which it operates." The difficulty, indeed, of conceiving a force acting through nothing from body to body, has of late made itself felt; and more especially since Meyer of Heilbronn first clearly expressed the principle of the "conservation of force." Newton, though apprehending the necessity of a medium by which the force of gravitation should be conveyed from one body to another, yet appears not to have possessed such an idea of the uncreateability and indestructibility of force as that which, now possessed by minds of the highest order, seems to some of them to be incompatible with the terms in which Newton enunciated his great law, viz., of matter attracting matter with a force which varies inversely as the square of the distance. The progress of knowledge of another form of all-pervading force, which we call, from its most notable effect on one of the senses, "Light," has not been less remarkable than that of gravitation. Galileo's discovery of Jupiter's satellites supplied Römer with the phenomena whence he was able to measure, in 1676, the velocity of light. Descartes, in his theory of the rainbow, referred the different colors to the different amount of refraction, and made a near approximation to Newton's capital discovery of the different colors entering into the composition of the luminous ray, and of their different refrangibility. Hook and Huyghens, about the same period, had entered upon explanations of the phenomena of light conceived as due to the undulations of an ether, propagated from the luminous point spherically, like those of sound. Newton, whilst admitting that such undulations or vibrations of an ether would explain certain phenomena, adopted the hypothesis of emission as most convenient for the mathematical propositions relative to light. The discoveries of achromatism, of the laws of double refraction, of polarization circular and elliptical, and of dipolarization, rapidly followed: the latter advances of optics, realizing more than Bacon conceived might flow from the labors of the "Perspective House," are associated with and have shed lustre on the names of Dollond, Young, Malus, Fresnel, Biot, Arago, Brewster, Stokes, Jamin, and others. Some of the natural sciences, as we now comprehend them, had not germinated in Bacon's time. Chemistry was then alchemy: Geology and Palæontology were undreamt of: but Magnetism and Electricity had begun to be observed, and their phenomena compared and defined, by a contemporary of Bacon, in a way that claims to be regarded as the first step towards a scientific knowledge of those powers. It is true that, before Gilbert ("De Magnete," 1600), the magnet was known to attract iron, and the great practical application of magnetized iron — the mariner's compass — had been invented, and for many years before Bacon's time had guided the barks of navigators through trackless seas. Gilbert, to whom the name "electricity" is due, observed that that force attracted light bodies, whereas the magnetic force attracted iron only. About a century later the phenomena of repulsion as well as of attraction of light bodies by electric substances were noticed; and Dufay, in 1733, enunciated the principle, that "electric bodies attract all those that are not so, and repel them as soon as they are become electric by the vicinity of the electric body." The conduction of electric force, and the different behavior of bodies in contact with the electric, leading to their division, by Desaguliers, into conductors and non-conductors, next followed. The two kinds of electricity, at first by Dufay, their definer, called "vitreous" and "resinous,"— afterwards, by Franklin, "positive" and "negative,"

— formed an important step, which led to a brilliant series of experiments and discoveries, with inventions, such as the Leyden jar, for intensifying the electric shock. The discovery of the instantaneous transmission of electricity through an extent of not less than 12,000 feet, by Bishop Watson, together with that of the electric state of the clouds, and of the power of drawing off such electricity by pointed bodies, as shown by Franklin, were a brilliant beginning of the application of the science to the well-being and needs of mankind. Magnetism has been studied with two aims: the one, to note the numerical relations of its activity to time and space, both in respect of its direction and intensity; the other, to penetrate the mystery of the nature of the magnetic force. In reference to the first aim, my predecessor adverted, last year, to the fact, that it was in the committee-rooms of the British Association that the first step was taken towards that great magnetic organization which has since borne so much fruit. Thereby it has been determined that there are periodical changes of the magnetic elements depending on the hour of the day, the season of the year, and on what seemed strange intervals of about eleven years. Also, that, besides these regular changes, there were others of a more abrupt and seemingly irregular character — Humboldt's "magnetic storms" — which occur simultaneously at distant parts of the earth's surface. Major-General Sabine, than whom no individual has done more in this field of research since Halley first attempted "to explain the change in the variation of the magnetic needle," has proved that the magnetic storms observed diurnal, annual, and undecennial periods. But with what phase or phenomenon of earthly or heavenly bodies, it may be asked, has the magnetic period of eleven years to do? The coincidence which points to, if it does not give, the answer, is one of the most remarkable, unexpected, and encouraging, to patient observers. For thirty years a German astronomer, Schwabe, had set himself the task of daily observing and recording the appearance of the sun's disc, in which time he found the spots passed through periodic phases of increase and decrease, the length of the period being about eleven years. A comparison of the independent evidence of the astronomer and magnetic observer has shown that the undecennial magnetic period coincides, both in its duration and in its epochs of maximum and minimum, with the same period observed in the solar spots.

A few weeks ago, during a visit of inspection to our establishment at Kew, I observed the successful operation of the photo-heliographic apparatus in depicting the solar spots as they then appeared. The continued regular record of the macular state of the sun's surface, with the concurrent magnetic observations now established over many distant points of the earth's surface, will, ere long, establish the full significance and value of the remarkable, and, in reference to the observers, undesigned coincidence above mentioned. Not to trespass on your patience by tracing the progress of Magnetism from Gilbert to Oersted, I cannot but advert to the time, 1807, when the latter tried to discover whether electricity in its most latent state had any effect on the magnet, and to his great result, in 1820, that the conducting wire of a voltaic circuit acts upon a magnetic needle, so that the latter tends to place itself at right angles to the wire. Ampère, moreover, succeeded, by means of a delicate apparatus, in demonstrating that the voltaic wire was affected by the action of the earth itself as a magnet. In short, the generalization was established, and with a rapidity unexampled, regard being had to its greatness, that *magnetism and electricity are but different* *effects of one common cause.* This has proved the first step to still grander abstractions, — to that which conceives the reduction of all the species of imponderable fluids of the chemistry of our student days, together with gravitation, chemicity, and neuricity, to interchangeable modes of action of one and the same all-pervading life-essence. Galvani arranged the parts of a recently-mutilated frog, so as to bring a nerve in contact with the external surface of a muscle, when a contraction of the muscle ensued. In this suggestive experiment, the Italian philosopher, who thereby initiated the inductive inquiry into the relation of nerve force to electric force, concluded that the contraction was a necessary consequence of the passage of electricity from one surface to the other by means of the nerve. He supposed that the electricity was secreted by the brain, and transmitted by the nerves to different parts of the body, the muscles serving as reservoirs of the electricity. Volta made a further step by showing that, under the conditions or arrangements of Galvani's experiments, the muscle would contract, whether the electric current had its origin in the animal body, or from a source external to that body. Galvani erred in too exclusive a reference of the electric force producing the contraction to the brain of the animal; Volta, in excluding the origin of the electric force from the animal body altogether. The determination of "the true" and "the constant" in these recondite phenomena, has been mainly helped on by the persevering and ingenious experimental researches of Mateucci and Du Bois Reymond. The latter has shown that any point of the surface of a muscle is positive in relation to any point of the divided or transverse section of the same muscle; and that any point of the surface of a nerve is positive in relation to any point of the divided or transverse section of the same nerve. Mr. Baxter, in still more recent researches, has deduced important conclusions on the origin of the muscular and nerve currents, as being due to the polarized condition of the nerve or muscular fibre, and the relation of that condition to changes which occur during nutrition. From the present state of neuro-electricity, it may be concluded that nerve force is not identical with electric force, but that it may be another mode of motion of the same common force. It is certainly a polar force, and perhaps the highest form of polar force:

> "A motion which may change, but cannot die;
> An image of some bright eternity."

The present tendency of the higher generalizations of Chemistry seems to be towards a reduction of the number of those bodies which are called "elementary;" it begins to be suspected that certain groups of so-called chemical elements are but modified forms of one another; that such groups as chlorine, iodine, bromine, fluorine, and as sulphur, selenium, phosphorus, boron, may be but allotropic forms of some one element. Organic Chemistry becomes simplified as it expands; and its growth has of late proceeded, through the labors of Hoffmann, Berthelot, and others, with unexampled rapidity. An important series of alcohols and their derivatives, from amylic alcohol downwards; as extensive a series of ethers, including those which give their peculiar flavor to our choicest fruits; the formic, butyric, succinic, lactic, and other acids, together with other important organic bodies, are now capable of artificial formation from their elements, and the old barrier dividing organic from inorganic bodies is broken down. To the power which mankind may ultimately exercise through the light of synthesis, who may presume to set limits? Already natural processes can be more economically replaced by artificial ones in the formation of a few organic compounds,— the "valerianic acid," for example. It is impossible to foresee the extent to which Chemistry may not ultimately, in the production of things needful, supersede the present vital agencies of nature, "by laying under contribution the accumulated forces of past ages, which would thus enable us to obtain in a small manufactory, and in a few days, effects which can be realized from present natural agencies only when they are exerted upon vast areas of land, and through considerable periods of time." Since Niepce, Herschel, Fox Talbot, and Daguerre, laid the foundations of Photography, year by year some improvement is made,— some advance achieved, in this most subtle application of combined discoveries in Photicity, Electricity, Chemistry, and Magnetism. Last year M. Poitevin's production of plates in relief, for the purpose of engraving by the action of light alone, was cited as the latest marvel of Photography. This year has witnessed photographic printing in carbon by M. Pretsch. Prof. Owen continued by alluding to the application of Photography for obtaining views of the moon, of the planets, of scientific and other phenomena. After referring to the discoveries in Electro-magnetism, the lecturer continued: Remote as such profound conceptions and subtle trains of thought seem to be from the needs of everyday life, the most astounding of the practical augmentation of man's power has sprung out of them. Nothing might seem less promising of profit than Oersted's painfully-pursued experiments, with his little magnets, voltaic pile, and bits of copper wire. Yet out of these has sprung the electric telegraph! Oersted himself saw such an application of his convertibility of electricity into magnetism, and made arrangements for testing that application to the instantaneous communication of signs through distances of a few miles. The resources of inventive genius have made it practicable for all distances; as we have lately seen in the submergence and working of the electro-magnetic cord connecting the Old and the New World. More remains to be done before the far-stretching engine can be got into working order; but the capital fact, viz., the practicability of bringing America into electrical communication with Europe has been demonstrated; consequently, a like power of instantaneous interchange of thought between the civilized inhabitants of every part of the globe becomes only a question of time. The powers and benefits thence to ensue for the human race can be but dimly and inadequately foreseen. After referring to the labors of Ray, Linnæus, Jussieu, Buffon, and Cuvier, he said: To perfect the natural system of plants has been the great aim of botanists since Jussieu. To obtain the same true insight into the relations of animals has stimulated the labors of zoölogists since the writings of Cuvier. To that great man appertains the merit of having systematically pursued and applied anatomical researches to the discovery of the true system of distribution of the animal kingdom; nor, until the Cuvierian amount of zoötomical science had been gained, could the value and importance of Aristotle's "History of Animals" be appreciated. There is no similar instance, in the history of Science, of the well-lit torch gradually growing dimmer and smouldering through so many generations and centuries before it was again fanned into brightness, and a clear view regained, both of the extent of ancient discovery, and of the true course to be pursued by modern research. Rapid and right has been the progress of Zoölogy since that resumption. Not only has the structure of the animal been investigated, even to the minute characteristics of each tissue, but the mode of formation of such constituents of organs, and of the organs themselves, has been pursued from the germ, bud, or egg, onward to maturity and decay. To the observation of outward characters is now added that of inward organization and developmental change, and Zoötomy, Histology, and Embryology, combine their results in forming an adequate and lasting basis for the higher axioms and generalizations of Zoölogy, properly so called. Three principles, of the common ground of which we may ultimately obtain a clearer insight, are now recognized to have governed the construction of animals,— unity of plan, vegetative repetition, and fitness for purpose. The independent series of researches by which students of the articulate animals have seen, in the organs performing the functions of jaws and limbs of varied powers, the same or homotypal elements of a series of like segments constituting the entire body, and by which students of the vertebrate animals have been led to the conclusion, that the maxillary, mandibular, hyoid, scapular, costal, and pelvic arches, and their appendages sometimes forming limbs of varied powers, are also modified elements of a series of essentially similar vertebral segments,— mutually corroborate their respective conclusions. It is not probable that a principle which is true for *Articulata* should be false for *Vertebrata:* the less probable since the determination of homologous parts becomes the more possible and sure in the ratio of the perfection of the organization.

After pointing out the distinction between Affinity, which indicates an intimate resemblance, and Analogy, which indicates a remote one, he continued: The study of homologous parts in a single system of organs — the bones — has mainly led to the recognition of the plan or archetype of the highest primary group of animals, the Vertebrata. The next step of importance will be to determine the homologous parts of the nervous system, of the muscular system, of the respiratory and vascular system, and of the digestive, secretory, and generative organs, in the same primary group or province. I think it of more importance to settle the homologies of the parts of a group or animals constructed on the same general plan, than to speculate on such relations of parts of animals constructed on demonstratively distinct plans of organization. What has been effected and recommended, in regard to homologous parts in the Vertebrata, should be followed out in the Articulata and Mollusca. In regard to the constituents of the crust or outer skeleton and its appendages in the Articulata, homological relations have been studied and determined to a praiseworthy extent, throughout that province. The same study is making progress in the Mollusca; but the grounds for determining special homologies are less sure in this sub-kingdom. The present state of homology in regard to the Articulata has sufficed to demonstrate that the segment of the crust is not a hollow expanded homologue of the segment of the endo-skeleton of a vertebrate. There is as little homology between the parts and appendages of the segments of the Vertebrate and Articulate skeletons respectively. The parts called mandibles, maxillæ, arms, legs, wings, fins, in Insects and Crustaceans, are only "analogous" to the parts so called in Vertebrates. A most extensive field of reform is becoming open to the homologist in that which is essential to the exactitude of his science,— a nomenclature equivalent to express his conviction of the different relations of similitude. Most difficult and recondite are the questions in face of which the march of Homology is now irresistibly conducting the philosophic observer;— such, for instance, as the following: Are the nervous, muscular, digestive, circulating, and generative systems of organs more than functionally similar in any two primary provinces of the animal kingdom? Are the homologies of entire systems to be judged of by their functional and structural connections, rather than by the plan and course of their formation in the embryo? It may be doubted if embryology alone is decisive of the question, whether homology can be predicated of the alimentary canal in animals of different primary groups or provinces. It is significant, however, of the lower value of embryological characters, to note that the great leading divisions of the animal kingdom, based by Cuvier on Comparative Anatomy, have nearly been confirmed by Von Baer's later developmental researches. And so, likewise, with regard to some of the minor modifications of Cuvier's provinces, the true position of the Cirripeda was discerned by Straus Durkheim and Macleay, by the light anatomy, before the discovery of their metamorphoses by Thomson. If, however, embryology has been over-valued as a test of homology, the study of the development of animals has brought to light most singular and interesting facts, and I now allude more especially to those that have been summed up under the term "Alternate-generation," "Parthenogenesis," "Metagenesis," etc. John Hunter first enunciated the general proposition, that "the propagation of plants depended on two principles, the one that every part of a vegetable is 'a whole,' so that it is capable of being multiplied as far as it can be divided into distinct parts; the other, that certain of those parts become reproductive organs, and produce fertile seeds." Hunter also remarked, that "the first principle operated in many animals which propagate their species by buds or cuttings;" but that, whilst in animals, it prevailed only in "the more imperfect orders," it operated in vegetables "of every degree of perfection." The experiments of Trembly on the freshwater polype, those of Spalanzani on the Naiads, and those of Bonnet on the Aphides, had brought to light the phenomena of propagation by fission, and by gemmation or buds, external and internal, in animals to which Hunter refers. Subsequent research has shown the unexpected extent to which Hunter's first principle of propagation in organic being prevails in the animal division. But the earliest formal supercession of Harvey's axiom, "*omne virum ab ovo*," appears to be Hunter's proposition of the dual principle above quoted. The experiments of Redi, Malpighi, and others, had progressively contracted the field to which the "*generatio æquivoca*,' could with any plausibility be applied. The stronghold of the remaining advocates of that old Egyptian doctrine was the fact of the development of parasitic animals in the flesh, brain, and glands of higher animals. But the hypothesis never obtained currency in this country; it was publicly opposed in my "Hunterian Lectures," by the fact of the prodigious preparation of fertile eggs in many of the supposed spontaneously developed species; and in then suggesting that the *Trichina spiralis* of the human muscular tissue might be the embryo of a larger worm in course of migration, I urged that a particular investigation was needed for each particular species.

Among the most brilliant of recent acquisitions to this part of Physiology, have been the discoveries which have resulted from such special investigations. Kuchenmeister and Von Siebold have been the chief laborers in this field. After noticing some of the results of those labors, he said: Since the time when it was first discovered that plants and animals could propagate in two ways, and that the individual developed from the bud might produce a seed or egg, from which also an individual might spring capable of again budding, — since this alternating mode of generation was observed, as by Chamisso and Sars, in cases where the budding individual differed much in form from the egg-laying one, — the subject has been systematized, generalized, with an attempt to explain its principle, and greatly advanced, especially, and in a highly interesting manner, in Von Siebold's late treatise, entitled "Wahre Parthenogenesis bei Schmeterlingen und Bienen," in which the virgin production of the male or drone-bee is demonstrated. Von Siebold, having subjected to the closest microscopic scrutiny and experiment the conclusion to which the practical Bee-master, Dzierson, had arrived, relative to the cause of queen-bees with crippled wings producing a swarm exclusively of drones, has demonstrated that the male bee is produced from an egg which has been subjected to no influence save that of the maternal parent; whilst such egg, if impregnated, would have produced a female or worker bee. The now well-investigated phenomena of parthenogenesis in Hydrozoa, have resulted in showing, as in the analogous case of Entozoa, that animals differing so much in form as to have constituted two distinct orders or classes, are really but two terms of a cycle of metagenetic transformations, — the acalephan Medusa being the sexual locomotive form of the agamic rooted budding polype, just as the cestoid tænia is of the cystic hydatid. In Hydrozoa (hydroid polypes, or sertularians) the young are propagated, as in plants, by "buds," and also, as in most plants, by "germs" or "seeds:" these latter are contained in "germ-sacs" projecting from the outer surface, which is another analogy to the flowering parts of plants. The first acquaintance with these marvels excited the hope that we were about to penetrate the mystery of the origin of different species of animals; but as far as observation has yet extended, the cycle of changes is definitely closed. And, since one essential step in the series is the fertilized seeds or egg, the Harveian axiom, "*omne vivum ab ovo*," if metagenetic phases be ascribed to one individual, may be still predicated of all organisms which bear unmistakable characters of plants or of animals. The closest observations of the subjects of these two kingdoms most favorable to clear insight into the nature of their beginning, accumulate evidence in proof of the essential first step being due to the protoplasmic matter of a germ-cell and sperm-cell; the former preëxisting in the form of a nucleus or protoplast, the latter as a granulous fluid. In flowering plants it is conveyed by the pollen-tube, in animals and many flowerless plants, by locomotive spermatozoids. The changes of form which the representative of a species undergoes in successive agamically propagating individuals are termed the "metagenesis" of such species. The changes of form which the representative of a species undergoes in a single individual, is called the "metamorphosis." But this term has practically been restricted to the instances in which the individual, during certain phases of the change, is free and active, as in the grub of the chaffer, or the tadpole of the frog, for example. In reference to some supposed essential differences in the metamorphoses of insects, it had been suggested that stages answering to those represented by the apodal and acephalous maggot of the Diptera, by the hexapod larva of the Carabi, and by the hexapod antenniferous larva of the Meloe, were really passed through by the orthopterous insect, before it quitted the egg. Mr. Andrew Murray has recently made known some facts in confirmation of this view. He had received a wooden idol from Africa, behind the ears of which a Blatta had fixed its egg-cases, after which the whole figure had been rudely painted by the natives, and these egg-cases were covered by the paint. No insect could have emerged without breaking through the case and the paint;

but both were uninjured. In the egg-cases were discovered,—1st, a grub-like larva in the egg; 2d, a cocoon in the egg containing the unwinged, imperfectly-developed insect; 3d, the unwinged, imperfectly-developed insect in the egg, free from the cocoon, and ready to emerge.

The microscope is an indispensable instrument in embryological and histological researches, as also in reference to that vast swarm of animalcules which are too minute for ordinary vision. I can here do little more than allude to the systematic direction now given to the application of the microscope to particular tissues and particular classes, chiefly due, in this country, to the counsels and example of the Microscopical Society of London. A very interesting application of the microscope has been made to the particles of matter suspended in the atmosphere; and a systematic continuation of such observations by means of glass slides prepared to catch and retain atmospheric atoms, promises to be productive of important results. We now know that the so-called red-snow of Arctic and Alpine regions is a microscopic single-celled organism which vegetates on the surface of snow. Cloudy or misty extents of dust-like matter pervading the atmosphere, such as have attracted the attention of travellers in the vast coniferous forests of North America, and have been borne out to sea, have been found to consist of the "pollen" or fertilizing particles of plants, and have been called "pollen showers." M. Daneste, submitting to microscopic examination similar dust which fell from a cloud at Shanghai, found that it consisted of spores of a confervoid plant, probably *Trichodesmium erythræum*, which vegetates in, and imparts its peculiar color to, the Chinese Sea. Decks of ships, near the Cape de Verde Islands, have been covered by such so-called "showers" of impalpable dust, which, by the microscope of Ehrenberg, has been shown to consist of minute organisms, chiefly "Diatomaceæ." One sample collected on a ship's deck 500 miles off the coast of Africa, exhibited numerous species of fresh water and marine diatoms, bearing a close resemblance to South American forms of those organisms. Ehrenberg has recorded numerous other instances in his paper printed in the "Berlin Transactions"; but here, as in other exemplary series of observations of the indefatigable microscopist, the conclusions are perhaps not so satisfactory as the well-observed data. He speculates upon the self-developing power of organisms in the atmosphere, affirms that dust-showers are not to be traced to mineral material from the earth's surface, nor to revolving masses of dust material in space, nor to atmospheric currents simply; but to some general law connected with the atmosphere of our planet, according to which there is a "self-development" within it of living organisms, which organisms he suspects may have some relation to the periodical meteorolites or aërolites. The advocates of progressive development may see and hail in this the first step in the series of ascending transmutations. The unbiased observer will be stimulated by the startling hypothesis of the celebrated Berlin professor to more frequent and regular examinations of atmospheric organisms. Some late examinations of dust-showers clearly show them to have a source which Ehrenberg has denied. Some of my hearers may remember the graphic description by Her Majesty's Envoy to Persia, the Hon. C. A. Murray, of the cloud of impalpable red dust which darkened the air of Bagdad, and filled the city with a panic. The specimen he collected was examined by my successor, at the Royal College of Surgeons, Professor Quekett, and that experienced microscopist could detect only inorganic particles, such as fine quartz sand, without any trace of Diatomaceæ or other organic matter. Dr. Lawson has obtained a similar result from the examination of the material of a shower of moist dust or mud which fell at Corfu, in March, 1857; it consisted for the most part of minute angular particles of a quartzose sand. Here, therefore, is a field of observation for the microscopist, which has doubtless most interesting results as the reward of persevering research.

Observations of the characters of plants have led to the recognition of the natural groups or families of the vegetable kingdom, and to a clear scientific comprehension of that great kingdom of nature. This phase of botanical science gives the power of further and more profitable generalizations, such as those teaching the relations between the particular plants and particular localities. The sum of these relations, forming the geographical distributions of plants, rests, perhaps at present necessarily, on an assumption, viz., that each species has been created, or come into being, but once in time and space; and that its present diffusion is the result of its own law of reproduction, under the diffusive or restrictive influence of external circumstances. These circumstances are chiefly temperature and moisture, dependent on the distance from the source of heat and the obliquity of the sun's rays, modified by altitude above the sea-level, or the degree of rarefaction of the atmosphere and of the power of the surface to wastefully radiate heat. Both latitude and altitude are further modified by currents of air and ocean, which influence the distribution of the heat they have absorbed. Thus large tracts of dry land produce dry and extreme climates, while large expanses of sea produce humid and equable climates. Agriculture affects the geographical distribution of plants, both directly and indirectly. It diffuses plants over a wider area of equal climate, augments their productiveness, and enlarges the limits of their capacity to support different climatal conditions. Agriculture also effects local modifications of climate. Certain species of plants require more special physical conditions for health; others more general conditions; and their extent of diffusion varies accordingly. Thus the plants of temperate climates are more widely diffused over the surface of the globe, because they are suited to elevated tracts in tropical latitudes. There is, however, another law which relates to the original appearance, or creation, of plants, and which has produced different species flourishing under similar physical conditions, in different regions of the globe. Thus the plants of the mountains of South America are of distinct species, and for the most part of distinct genera, from those of Asia. The plants of the temperate latitudes of North America are of distinct species, and some of distinct genera, from those of Europe. The Cacteæ of the hot regions of Mexico are represented by the Euphorbiaceæ in parts of Africa having a similar climate. The surface of the earth has been divided into twenty-five regions, of which I may cite as examples that of New Zealand, in which Ferns predominate, together with generic forms, half of which are European, and the rest approximating to Australian, South African, and Antarctic forms; and that of Australia, characterized by its Eucalypti and Epacrides, chiefly known to us by the researches of the great botanist, Robert Brown, the founder of the Geography of Plants.

Organic Life, in its animal form, is much more developed, and more variously, in the sea, than in its vegetable form. Observations of marine animals and their localities have led to attempts at generalizing the results; and the modes of enunciating these generalizations or laws of geographical distribution are very analogous to those which have been applied to the vegetable kingdom, which is as diversely developed on land as in the animal kingdom in the sea. The most interesting form of expression of the distribution of marine life is that which parallels the perpendicular distribution of plants. Edward Forbes has expressed this by defining five bathymetrical zones, or belts of depth, which he calls, — 1, Littoral; 2, Circumlittoral; 3, Median; 4, Infra-median; 5, Abyssal. The life-forms of these zones vary, of course, according to the nature of the sea-bottom; and are modified by those primitive or creative laws that have caused representative species in distant localities under like physical conditions, — species related by analogy. Very much remains to be observed and studied by naturalists in different parts of the globe, under the guidance of the generalizations thus sketched out, to the completion of a perfect theory. But in the progress to this, the results cannot fail to be practically most valuable. A shell or a sea-weed, whose relations to depth are thus understood, may afford important information or warning to the navigator. To the geologist the distribution of marine life according to the zones of depth, has given the clue to the determination of the depth of the seas in which certain formations have been deposited. Had all the terrestrial animals that now exist diverged from one common centre within the limited period of a few thousand years, it might have been expected that the remoteness of their actual localities from such ideal centre would bear a certain ratio with their respective powers of locomotion. With regard to the class of Birds, one might have expected to find that those which were deprived of the power of flight, and were adapted to subsist on the vegetation of a warm or temperate latitude, would still be met with more or less associated together, and least distant from the original centre of dispersion, situated in such a latitude. This, however, is not only not the case with birds, but is not so with any other classes of animals. The Quadrumana, or order of apes, monkeys, and lemur, consists of three chief divisions — Catarhines, Platyrhines, and Strepsirhines. The first family is peculiar to the "Old World"; the second to South America; the third has the majority of its species and its chief genus (Lemur), exclusively in Madagascar. Out of twenty-six known species of Lemuridæ, only six are Asiatic, and three are African. Whilst adverting to the geographical distribution of Quadrumana, I would contrast the peculiarly limited range of the orangs and chimpanzees with the cosmopolitan powers of mankind. The two species of orang (Pithecus) are confined to Borneo and Sumatra; the two species of chimpanzee (Troglodytes) are limited to an intertropical tract of the western part of Africa. They appear to be inexorably bound by climatal influences regulating the assemblage of certain trees and the production of certain fruits. Climate rigidly limits the range of the Quadrumana latitudinally; creational and geographical causes limit their range in longitude. Distinct genera represent each other in the same latitudes of the New and Old Worlds; and also, in a great degree, in Africa and Asia. But the development of an orang out of a chimpanzee, or reciprocally, is physiologically inconceivable. The order of Ruminantia is principally represented by Old World species, of which one hundred and sixty-two have been defined; whilst only twenty-four species have been discovered in the New World, and none in Australia, New Guinea, New Zealand, or the Polynesian Isles. The camelopard is now peculiar to Africa; the musk-deer to Africa and Asia; out of about fifty defined species of antelope, only one is known in America, and none in the central and southern divisions of the New World. Palæontology has expanded our knowledge of the range of the giraffe; during Miocene or old Pliocene periods, species of Camelopardalis roamed in Asia and Europe.

Geology gives a wider range to the horse and elephant kinds than was cognizant to the student of living species only. The existing Equidæ and Elephantidæ properly belong, or are limited to, the Old World; and the elephants to Asia and Africa,—the species of the two continents being quite distinct. The horse, as Buffon remarked, carried terror to the eye of the indigenous Americans, viewing the animal for the first time, as it proudly bore their Spanish conqueror. But a species of Equus, coëxisted with the Megatherium and Megalonyx, in both South and North America, and perished apparently with them, before the human period. Elephants are dependent chiefly upon trees for food. One species now finds conditions of existence in the rich forests of tropical Asia; and a second species in those of tropical Africa. Why, we may ask, should not a third be living at the expense of the still more luxuriant vegetation watered by the Oronooko, the Essequibo, the Amazon, and the La Plata, in tropical America? Geology tells us that at least two kinds of elephant (*Mastodon Andium* and *M. Humboldtii*) formerly did derive their subsistence, along with the great Megatherioid beasts, from that abundant source. We may infer that the general growth of large forests, and the absence of deadly enemies, were the main conditions of the former existence of elephantine animals over every part of the globe. We have the most pregnant proof of the importance of Palæontology in rectifying and expanding ideas deduced from recent Zoölogy of the geographical limits of particular forms of animals, by the results of its application to the proboscidian or elephantine family. But such retrospective views of life in remote periods, in many important instances, confirm the Zoölogist's deductions of the originally restricted range of particular forms of mammalian life. The sum of all the evidence from the fossil world in Australia proves its mammalian population to have been essentially the same in pleistocene, if not pliocene times, as now; only represented, as the Edentate mammals in South America were then represented, by more numerous genera, and much more gigantic species, than now exist. But Geology has revealed more important and unexpected facts relative to the marsupial type of quadrupeds. In the miocene and eocene tertiary deposits, marsupial fossils of the American genus Didelphys have been found, both in France and England; and they are associated with Tapirs like that of America. In a more ancient geological period remains of marsupials, some insectivorous, as Spalacotherium and Triconodon, others with teeth like the peculiar premolars in the Australian genus Hypsipromnus, have been found in the upper oolite of the Isle of Purbeck. In the lower oolite at Stonesfield, Oxfordshire, marsupial remains have been found having their nearest living representatives in the Australian genera Myrmecobius and Dasyurus. Thus it would seem, that the deeper we penetrate the earth, or, in other words, the further we recede in time, the more completely are we absolved from the present laws of geographical distribution. In comparing the mammalian fossils found in British pleistocene and pliocene beds, we have often to travel to Asia or Africa for their homologues. In the miocene and eocene strata some fossils occur which compel us to go to America for the nearest representatives. To match the mammalian remains from the English oolitic formations, we must bring species from the Antipodes. These are truly most suggestive facts. If the present laws of geographical distribution depend, in an important degree, upon the present configuration and position of continents and islands, what a total change in the geographical character of the earth's surface must have taken place since the "Stonesfield slate" was deposited in what now forms the county of Oxfordshire! These and the like considerations from the modifications of geographical distribution of particular forms or groups of animals, warn us how inadequate must be the phenomena connected with the present distribution of land and sea to guide to the determination of the primary ontological divisions of the earth's surface. Some of the latest contributions to this most interesting branch of natural history have been the result of endeavors to determine whether, and how many, distinct creations of plants and animals have taken place. But I would submit, that the discovery of two portions of the globe, of which the respective Faunæ and Floræ are different, by no means affords the requisite basis for concluding as to distinct acts of creation. Such conclusion is associated, perhaps, unconsciously, with the idea of the historical date of creative acts: it presupposes that the portion of the globe so investigated by the botanist and zoölogist has been a separate and primitive creation, — that its geographical limits and features are still in the main what they were when the creative fiat went forth. But Geology has demonstrated that such is by no means the case with respect to the portions of dry land now termed continents and islands. The incalculable vistas of time past into which the same science has thrown light, are also shown to have periods during which the relative position of land and sea have been ever changing.

Already the directions, and to a certain extent the forms, of the submerged tracts that once joined what now are islands to continents, and which once united now separate or nearly disjoined continents by broad tracts of continuity, begin to be laid down in geological maps, addressing to the eye such successive and gradually progressive alterations of the earth's surface. These phenomena shake our confidence in the conclusion that the Apteryx of New Zealand and the Red-grouse of England were distinct creations in and for those islands respectively. Always, also, it may be well to bear in mind that by the word "creation" the zoölogist means "a process, he knows not what." Science has not yet ascertained the secondary causes that operated when "the earth brought forth grass, and herb yielding seed after his kind," and when "the waters brought forth abundantly the moving creature that hath life." And supposing both the fact and the whole process of the so-called "spontaneous generation" of a fruit-bearing tree, or of a fish, were scientifically demonstrated, we should still retain as strongly the idea which is the chief of the "mode" or "group of ideas" we call "creation," viz., that the process was ordained by and had originated from an all-wise and powerful First Cause of all things. When, therefore, the present peculiar relation of the Red-grouse (*Tetrao scoticus*) to Britain and Ireland — and I cite it as one of a large class of instances in Geographical Zoölogy — is enumerated by the zoölogist as evidence of a distinct creation of the bird in and for such islands, he chiefly expresses that he knows not how the Red-grouse came to be there and there exclusively; signifying also by this mode of expressing such ignorance, his belief that both the bird and the islands owed their origin to a great first Creative Cause. And this analysis of the real meaning of the phrase "distinct creation," has led me to suggest whether, in aiming to define the primary zoölogical provinces of the globe, we may not be trenching upon a province of knowledge beyond our present capacities; at least, in the judgment of Lord Bacon, commenting upon man's efforts to pierce into the "dead beginnings of things."

On the few occasions in which I have been led to offer observations on the probable cause of the extinction of species, the chief weight has been given to those gradual changes in the conditions of a country affecting the due supply of sustenance to animals in a state of nature. I have also pointed out the characters in the animals themselves calculated to render them most obnoxious to such extirpating influences: and on one occasion I have applied the remarks to the explanation of so many of the larger species of particular groups of animals having become extinct, whilst smaller species of equal antiquity have remained. In proportion to its bulk is the difficulty of the contest, which, as a living organized whole, the individual of such species has to maintain against the surrounding agencies that are ever tending to dissolve the vital bond and subjugate the living matter to the ordinary chemical and physical forces. Any changes, therefore, in such external agencies as a species may have been originally adapted to exist in, will militate against that existence in a degree proportionate, perhaps in a geometrical ratio, to the bulk of the species. If a dry season be gradually prolonged, the large mammal will suffer from the drought sooner than the small one; if such alteration of climate affect the quantity of vegetable food, the bulky herbivore will first feel the effects of stinted nourishment; if new enemies are introduced, the large and conspicuous quadruped or bird will fall a prey, while the smaller species conceal themselves and escape. Smaller animals are usually also more prolific than larger ones. "The actual presence, therefore, of small species of animals in countries where larger species of the same natural families formerly existed, is not the consequence of any gradual diminution of the size of such species, but is the result of circumstances which may be illustrated by the fable of the 'Oak and the Reed;' the smaller and feebler animals have bent and accommodated themselves to changes which have destroyed the larger species." No doubt the type-form of any species is that which is best adapted to the conditions under which such species at the time exists; and as long as those conditions remain unchanged, so long will the type remain; all varieties departing therefrom being in the same ratio less adapted to the environing conditions of existence. But, if those conditions change, then the variety of the species at an antecedent date and state of things will become the type-form of the species at a later date, and in an altered state of things. Observation of animals in a state of nature is required to show their degree of plasticity, or the extent to which varieties do arise, whereby grounds may be had for judging of the probability of the elastic ligaments and joint-structures of a feline foot, for example, being superinduced upon the more simple structure of the toe with the non-retractile claw, according to the principle of a succession of varieties in time. Observation of fossil remains is also still needed to make known the antetypes, in which varieties, analogous to the observed ones in existing species, might have occurred, so as to give rise ultimately to such extreme forms as the Giraffe, for example. The aboriginal laws of the geographical distribution of plants and animals have been modified from of old by geological and the concomitant climatal changes; but they have been much more disturbed by man since his introduction upon the globe. The serviceable plants and animals which he has carried with him in his migrations have flourished and multiplied in lands the most remote from the habitats of the aboriginal species. Man has, also, been the most potent and intelligible cause of the extirpation of species within historic times. He alone, with one of the beasts which he has domesticated — the dog — is cosmopolitan. The human species is represented by a few well-marked varieties; and there is a certain amount of correspondence between their localities and general zoölogical provinces. But, with regard to the alleged conformity between the geographical distribution of man and animals, which has of late been systematically enunciated, and made by Agassiz, in Gliddon & Nott's "Varieties of Mankind," the basis of deductions as to the origin and distinction of the human varieties, many facts might be cited, affecting the conformity of the distribution of man with that of the lower animals and plants, as absolutely enunciated in some recent works. Nor can we be surprised to find that the migratory instincts of the human species, with the peculiar endowment of adaptiveness to all climates, should have produced modifications in geographical distribution to which the lower forms of living nature have not been subject. Ethnology is a wide and fertile subject, and I should be led far beyond the limits of an inaugural discourse were I to indulge in an historical sketch of its progress. But I may advert to the testimony of different witnesses — to the concurrence of distinct species of evidence — as to the much higher antiquity of the human race, than has been assigned to it in historical and genealogical records.

Mr. Leonard Horner discerned the value of the phenomena of the annual sedimentary deposits of the Nile in Egypt as a test of the lapse of time during which that most recent and still operating geological dynamid had been in progress. In two Memoirs communicated to the Royal Society in 1855 and 1858, the result of ninety-five vertical borings through the alluvium thus formed are recorded. In the excavations near the colossus of Rameses II., at Memphis, there were nine feet four inches of Nile sediment between eight inches below the present surface of the ground and the lowest part of the platform on which the statue had stood. Supposing the platform to have been laid in the middle of the reign of that king, viz., 1361 B. C., such date added to A. D. 1854, gives 3,215 years during which the above sediment was accumulated; or a mean rate of increase of three and a half inches in a century. Below the platform there were thirty-two feet of the total depth penetrated; but the lowest two feet consisted of sand, below which it is possible there may be no true Nile sediment in this locality, thus leaving thirty feet of the latter. If that amount has been deposited at the same rate of three and a half inches in a century, it gives for the lowest part deposited an age of 10,285 years before the middle of the reign of Rameses II., and 13,500 years before A. D. 1854. The Nile sediment at the lowest depth reached is very similar in composition to that of the present day. In the lowest part of the boring of the sediment at the colossal statue in Memphis, at a depth of thirty-nine feet from the surface of the ground, the instrument is reported to have brought up a piece of pottery. This, therefore, Mr. Horner infers to be a record of the existence of man 13,371 years before A. D. 1854: — "Of man moreover, in a state of civilization, so far, at least, as to be able to fashion clay into vessels, and to know how to harden them by the action of a strong heat." Professor Max Müller has opened out a similar vista into the remote past of the history of the human race by the perception and application of analogies in the formation of modern and ancient, of living and dead, languages. From the relations traceable between the six Romance dialects,— Italian, Wallachian, Rhætian, Spanish, Portuguese, and French,— an antecedent common "mother-tongue" might be inferred, and, consequently the existence of a race anterior to the modern Italians, Spanish, French, etc., with conclusions as to the lapse of time requisite for such divisions and migrations of the primitive stock, and for the modifications which the mother-language had undergone. History and preserved writings show that such common mother-race and language have existed in the Roman people and the Latin tongue. But Latin, like the equally "dead" language Greek, with Sanscrit, Lithuanian, Zend, and the Gothic, Sclavonic, and Celtic tongues, can be similarly shown to be modifications of one antecedent common language; whence is to be inferred an antecedent race of men, and a lapse of time sufficient for their migration over a tract extending from Iceland in the north-west to India in the south-east, and for all the above named modifications to have been established in the common mother "Arian" tongue.

Agriculture has of late years made unusual progress, and much of that progress is due to the application of scientific principles; chiefly of those supplied by Chemistry; in a less degree of Zoölogy and Physiology. Geology now teaches the precise nature and relations of soils: a knowledge of great practical importance in guiding the drainer of land, in the modifications of his general rules of practice. Palæontology has brought to light unexpected sources of valuable manures, in phosphatic relics of ancient animal life, accumulated in astounding masses in certain localities of England. But quantities of azotic, ammoniacal, and phosphatic matters are still suffered to run to waste; and, as if to bring the wastefulness more home to the conviction, those products, so valuable when rightly administered, become a source of annoyance, unremunerative outlay and disease, when, as at present in most towns, imperfectly and irrationally disposed of.

In the operations of Nature, there is generally a succession of processes coördinated for a given result; a peach is not directly developed as such from its elements; the seed would, *à priori*, give no idea of the tree, nor the tree of the flower, nor the fertilized germ of that flower of the pulpy fruit in which the seed was buried. It is eminently characteristic of the Creative Wisdom, this far-seeing and prevision of an ultimate result, through the successive operations of a coördinate series of seemingly very different conditions. The further a man discerns, in a series of conditions, their coordination to produce a given result, the nearer does his wisdom approach — though the distance be still immeasurable — to the Divine wisdom. One philanthropist builds a fever hospital, another drains a town. One crime-preventer trains the boy, another hangs the man. One statesman would raise money by augmenting a duty, or by a direct tax, and finds the revenue not increased in the expected ratio. Another diminishes a tax, or abolishes a duty, and through foreseen consequences the revenue is improved. Water is the cheapest and most efficient transporter of excreta; but it should be remembered that the application of the water-supply as a transporting power is to be limited to all that comes from the interior of the abodes; this alone can be practically and successfully applied to agriculture. Whatever flows from the outside of houses, together with the general rainfall of the town area, should go to the nearest river by channels wholly distinct from the hydraulic excretory system. Agriculture, let me repeat, has made, and is making, great and encouraging progress; but much yet remains to be done. Were agriculture adequately advanced, the great problem of the London sewage would be speedily solved. Can it be supposed, if the rural districts about the metropolis were in a condition to avail themselves of a daily supply of the pipe-water not more than equivalent to that which a heavy shower of rain throws down on 2,000 acres of land, but a supply charged with thirty tons of nitrogenous ammoniacal principles, that such supply would not be forthcoming, and made capable of being distributed when called for within a radius of one hundred miles? To send ships for foreign ammoniacal or phosphatic excreta to the coast of Peru, and to pollute by the waste of similar home products the noble river bisecting the metropolis, are flagrant signs of the desert and uncultivated state of a field where science and practice have still to coöperate for the public benefit.

Some of our sciences are deeply concerned in one progressive step — the uniformity of standard in measure and weight throughout the civilized world; in urging on which step, energetic and unwearied efforts are now being made by a committee of our fellow-laborers of the Royal Society of Arts, amongst whom the name of the prime promoter of this and kindred reforms, Mr. James Yates, deserves special and honorable mention. Chemistry is more concerned in the uniform expression of the results of her delicate balances amongst her cultivators of different countries; Natural History is no less interested in the use, by all observers, of one and the same scale for measuring, and of one set of terms for expressing the superficial dimensions of her subjects.

But not by words only would, or does, science make return to governments fostering and aiding her endeavors for the public weal. Every practical application of her discoveries tends to the same end as that which the enlightened statesman has in view. The steam-engine in its manifold applications, the crime-decreasing gas-lamp, the lightning conductor, the electric telegraph, the law of storms, and rules for the mariner's guidance in them, the power of rendering surgical operations painless, the measures for preserving public health, and for preventing or mitigating epidemics, — such are among the more important practical results of pure scientific research with which mankind have been blessed and States enriched. They are evidence unmistakable of the close affinity between the aims and tendencies of science and those of true State policy. In proportion to the activity, productivity, and prosperity of a community is its power of responding to the calls of the Finance Minister. By a far-seeing one, the man of science will be regarded with a favorable eye, not less for the unlooked-for streams of wealth that have already flowed, but for those that may in future arise, out of the applications of the abstract truths to the discovery of which he devotes himself. This may, indeed, demand some measure of faith on the part of the practical statesman. For who that watched the philosophic Black experimenting on the abstract nature of Caloric could have foreseen that his discovery of latent heat would be the stand-point of Watt's invention of a practically operative steam-engine! How little could the observer of Oersted's subtle arrangements for converting electric into magnetic force have dreamt of the application of such discovery to the rapid interchange of ideas now daily practised between individuals in distant cities, countries, and continents! Some medical contemporaries of John Hunter, when they saw him, as they thought, wasting as much time in studying the growth of a deer's horn as they would have bestowed upon the symptoms of their best patient, compassionated, it is said, the singularity of his pursuits. But, by the insight so gained into the rapid enlargement of arteries, Hunter learned a property of those vessels which emboldened him to experiment on a man with aneurism, and so to introduce a new operation which has rescued from a lingering and painful death thousands of his fellow-creatures. Our great inductive physiologist, in his dissections and experiments on the lower animals, was "taking light what may be wrought upon the body of man." The production of Chloroform is amongst the more subtle experimental results of modern Chemistry. The blessed effects of its proper exhibition in the diminution of the sum of human agony are indescribable. But that divine-like application was not present to the mind of the scientific chemist who discovered the anæsthetic product, any more than was the gas-lit town to the mind of Priestley, or the condensing engine to that of Black.

ADDRESS OF LORD BROUGHAM AT THE INAUGURATION OF A STATUE OF SIR ISAAC NEWTON.

The following eloquent and suggestive address was delivered by Lord Brougham, September 21, 1858, on the occasion of inaugurating a statue to Sir Isaac Newton, at Grantham,—the great philosopher's birthplace. Although somewhat foreign to the general subject-matter of the Annual, we think its publication will be most acceptable to our readers. Rising from a venerable arm-chair, in which Newton sat when he composed the Principia, the learned Ex-Chancellor spoke as follows:

To record the names and preserve the memory of those whose great achievements in science, in arts, or in arms, have conferred benefits and lustre upon our kind, has, in all ages, been regarded as a duty, and felt as a gratification, by wise and reflecting men. The desire of inspiring an ambition to emulate such examples, generally mingles itself with these sentiments; but they cease not to operate, even in the rare instances of transcendent merit, where matchless genius excludes all possibility of imitation, and nothing remains but wonder in those who contemplate its triumphs at a distance that forbids all attempts to approach. We are this day assembled to commemorate him of whom the consent of nations has declared that he is chargeable with nothing like a follower's exaggeration or local partiality; who pronounce the name of Newton as that of the greatest genius ever bestowed by the bounty of Providence for instructing mankind on the frame of the universe, and the laws by which it is governed:

> "In genius who surpassed mankind as far
> As does the midday sun the midnight star."—DRYDEN.

But, though scaling these lofty heights be hopeless, yet is there some use and much gratification in contemplating by what steps he ascended. Tracing his course of action may help others to gain the lower eminences lying within their reach, while admiration excited and curiosity satisfied are frames of mind both wholesome and pleasing. Nothing new, it is true, can be given in narrative, hardly anything in reflection, less still, perhaps, in comment or illustration; but it is well to assemble in one view various parts of the vast subject, with the surrounding circumstances, whether accidental or intrinsic, and to mark in passing the misconceptions raised by individual ignorance or national prejudice which the historian of science occasionally finds crossing his path. The remark is common and is obvious, that the genius of Newton did not manifest itself at a very early age. His faculties were not, like those of some great and many ordinary individuals, precociously developed. Among the former, Clairaut stands preëminent, who at nineteen years of age presented to the Royal Academy a memoir of great originality upon a difficult subject in the higher geometry, and at eighteen published his great work on curves of double curvature, composed during the two preceding years. Pascal, too, at sixteen, wrote an excellent treatise on conic sections.

That Newton cannot be ranked in this respect with those extraordinary persons is owing to the accidents which prevented him from entering upon mathematical study before his eighteenth year; and then a much greater marvel was wrought than even the Clairants and the Pascals displayed. His earliest history is involved in some obscurity, and the most celebrated of men has, in this particular, been compared to the most celebrated of rivers (the Nile), as if the course of both in its feebler state had been concealed from mortal eyes. We have it, however, well ascertained, that within four years, between the ages of eighteen and twenty-two, he had begun to study mathematical science, and had taken his place among its greatest masters; learnt for the first time the elements of geometry and analysis, and discovered a calculus which entirely changed the face of the science, effecting a complete revolution in that and in every branch of philosophy connected with it. Before 1661, he had not read "Euclid;" in 1665, he had committed to writing the method of fluxions. At twenty-five years of age, he had discovered the law of gravitation, and laid the foundation of celestial dynamics,— the science created by him. Before ten years had elapsed, he added to his discoveries that of the fundamental properties of light. So brilliant a course of discovery in so short a time, changing and reconstructing analytical, astronomical, and optical science, almost defies belief. The statement could only be deemed possible by an appeal to the incontestable evidence that proves it strictly true. By a rare felicity these doctrines gained the universal assent of mankind as soon as they were clearly understood; and their originality has never been seriously called in question. Some doubts having been raised respecting his inventing the calculus — doubts raised in consequence of his so long withholding the publication of his method — no sooner was the inquiry instituted than the evidence produced proved so decisive that all men in all countries acknowledged him to have been, by several years, the earliest inventor, and Leibnitz, at the utmost, the first publisher; the only questions raised being, first, whether or not he had borrowed from Newton; and next, whether, as second inventor, he could have any merit at all,— both which questions have long since been decided in favor of Leibnitz. But undeniable though it be that Newton made the great steps of this progress, and made them without any anticipation or participation by others, it is equally certain that there had been approaches in former times by preceding philosophers to the same discoveries. Cavalleri, by his Geometry of Indivisibles (1635), Roberval, by his method of Tangents (1367), had both given solutions which Descartes could not attempt; and it is remarkable that Cavalleri regarded curves as polygons, surfaces as composed of lines, while Roberval viewed geometrical quantities as generated by motion; so that the one approached to the differential calculus, the other to fluxions; and Fermat, in the interval between them, comes still nearer the great discovery by his determination of *maxima* and *minima*, and his drawing of tangents. More recently Hudden had made public similar methods invented by Schoetin; and what is material, treating the subject algebraically, while those just now mentioned had rather dealt with it geometrically. It is thus easy to perceive how near an approach had been made to the calculus before the great event of its final discovery. There had in like manner been approaches made to the law of gravitation, and the dynamical system of the universe. Galileo's important propositions on motion, especially on curvilinear motion, and Kepler's laws upon the elliptical form of the planetary orbits, the proportion of the areas to the times, and of the periodic times to the mean distances; and Huygens's theorems on centrifugal forces, had been followed by still nearer approaches to the doctrine of attraction. Borelli had distinctly ascribed the motion of satellites to their being drawn towards the principal planets, and thus prevented from flying off by the centrifugal force. Even the composition of white light, and the different action of bodies upon its component parts, had been vaguely conjectured by Ant. de Dominis, Archbishop of Spalatro, at the beginning, and more precisely in the middle of the seventeenth century by Marcus (Kronland of Prague), unknown to Newton, who only refers to the Archbishop's work; while the treatise of Huygens on light, Grimaldi's observations on colors by inflexion, as well as on the elongation of the image in the prismatic spectrum, had been brought to his attention, although much less near to his own great discovery than Marcus's experiment. But all this only shows that the discoveries of Newton, great and rapid as were the steps by which they advanced our knowledge, yet obeyed the law of continuity, or rather of gradual progress, which governs all human approaches towards perfection. The limited nature of man's faculties precludes the possibility of his ever reaching at once the utmost excellence of which they are capable. Survey the whole circle of the sciences, and trace the history of our progress in each, you find this to be the universal rule. In chymical philosophy, the dreams of the Alchymists prepared the way for the more rational, though erroneous, theory of Stahl; and it was by repeated improvements that his errors, so long prevalent, were at length exploded, giving place to the sound doctrine which is now established. The great discoveries of Black and Priestly on heat and aeriform fluids, had been preceded by the happy conjectures of Newton and the experiments of others. Nay, Voltaire had well-nigh discovered both the absorption of heat, the constitution of the atmosphere, and the oxydation of metals; and by a few more trials might have ascertained it. Cuvier had been preceded by inquirers who took sound views of fossil osteology, among whom, the truly original genius of Hunter fills the foremost place. The inductive system of Bacon had been, at least in its practice, known to his predecessors. Observations, and even experiments, were not unknown to the ancient philosophers, though mingled with gross errors; in early times, almost in the dark ages, experimental inquiries had been carried on with success by Friar Bacon, and that method actually recommended in a treatise, as it was two centuries later by Leonardo da Vinci, and at the latter end of the next century Gilbert examined the whole subject of magnetic action entirely by experiments. So that Lord Bacon's claim to be regarded as the father of modern philosophy rests upon the important, the invaluable step of reducing to a system the method of investigation adopted by those eminent men, generalizing it, and extending its application to all matters of contingent truth, exploding the errors, the absurd dogmas, and fantastic subtleties of the ancient schools; above all, confining the subject of our inquiry, and the manner of conducting it, within the limits which our faculties prescribe. Nor is this great law of gradual progress confined to the physical sciences; in the moral it equally governs. Before the foundations of political economy were laid by Hume and Smith, a great step had been made by the French philosophers, disciples of Quesnai; but a nearer approach to sound principles had signalized the labors of Gournay, and those labors had been shared, and his doctrines patronized, by Turgôt, when Chief Minister. Again, in constitutional policy, see by what slow degrees, from its first rude elements, the attendance of feudal tenants at their lord's court, and the summons of burghers to grant supplies of money, the great discovery of modern times in the science of practical politics has been effected, the representative scheme which enables states of any extent to enjoy popular government, and allows mixed monarchy to be established, combining freedom with order, a plan pronounced by the statesmen and writers of antiquity to be of hardly possible formation, and wholly impossible continuance. The globe itself, as well as the science of its inhabitants, has been explored according to the law which forbids a sudden and rapid leaping forward, and decrees that each successive step, prepared by the last, shall facilitate the next. Even Columbus followed several successful discoverers on a smaller scale, and is, by some, believed to have had, unknown to him, a predecessor in the great exploit by which he pierced the night of ages, and unfolded a new world to the eyes of the old.

The arts afford no exception to the general law. Demosthenes had eminent forerunners, Pericles the last of them. Homer must have had predecessors of great merit, though doubtless as far surpassed by him as Fra Bartolomeo and Pietro Perugino were by Michael Angelo and Raphael. Dante owed much to Virgil; he may be allowed to have owed, through his Latin mentor, not a little to the old Grecian; and Milton had both the orators and the poets of the ancient world for his predecessors and his masters. The art of war itself is no exception to the rule. The plan of bringing an overpowering force to bear on a given point had been tried occasionally before Frederick II. reduced it to a system; and the Wellingtons and Napoleons of our own day made it the foundation of their strategy, as it had also been previously the mainspring of our naval tactics. It has oftentimes been held that the invention of logarithms stands alone in the history of science, as having been preceded by no step leading towards the discovery. There is, however, great inaccuracy in this statement; for not only was the doctrine of infinitesimals familiar to its illustrious author, and the relation of geometrical to arithmetical series well known, but he had himself struck out several methods of great ingenuity and utility (as that known by the name of Napier's Bones) — methods that are now forgotten, eclipsed as they were by the consummation which has immortalized his name. So the inventive powers of Watt, preceded as he was by Worcester and Newcomen, but far more materially by Causs and Papin, had been exercised on some admirable contrivances, now forgotten, before he made the step which created the steam-engine anew — not only the parallel motion, possibly a corollary to the proposition on circular motion in the "Principia," but the separate condensation, and above all, the governor, perhaps the most exquisite of mechanical inventions; and now we have those here present who apply the like principle to the diffusion of knowledge, aware, as they must be, that its expansion has the same happy effect naturally of preventing mischief from its excess which the skill of the great mechanist gave artificially to steam, thus rendering his engine as safe as it is powerful. The grand difference, then, between one discovery or invention and another, is in degree rather than in kind; the degree in which a person, while he outstrips those whom he comes after, also lives, as it were, before his age. Nor can any doubt exist that, in this respect, Newton stands at the head of all who have extended the bounds of knowledge. The sciences of dynamics and of optics are especially to be regarded in this point of view; but the former in particular; and the completeness of the system which he unfolded — its having been at the first elaborated and given in perfection — its having, however new, stood the test of time, and survived, nay gained by, the most rigorous scrutiny — can be predicated of this system alone, at least in the same degree. That the calculus, and those parts of dynamics which are purely mathematical, should thus endure forever is a matter of course. But his system of the universe rests partly upon contingent truths, and might have yielded to new experiments and more extended observation. Nay, at times it has been thought to fail, and further investigation was deemed requisite to ascertain if any error had been introduced — if any circumstance had escaped the notice of the great founder. The most memorable instance of this kind is the discrepancy supposed to have been found between the theory and the fact in the motion of the lunar apsides, which about the middle of the last century occupied the three first analysts of the age. The error was discovered by themselves to have been their own in the process of their investigation; and this, like all the other doubts that were ever momentarily entertained, only led in each instance to new and more brilliant triumphs of the system. The prodigious superiority in this cardinal point of the Newtonian to other discoveries appears manifest upon examining almost any of the chapters in the history of science. Successive improvements have, by extending our views, constantly displaced the system that appeared firmly established. To take a familiar instance — how little remains of Lavoisier's doctrine of combustion and acidification, except the negative positions, the subversion of the system of Stahl! The substance having most eminently the properties of an acid (chlorine) is found to have no oxygen at all, while many substances abounding in oxygen, including alkalis themselves, have no acid property whatever; and without the access of oxygenous or of any other gas, heat and flame are produced in excess. The docrines of free trade had not long been promulgated by Smith before Bentham demonstrated that his exception of usury was groundless; and his theory has been repeatedly proved erroneous on colonial establishments, as well as his exception to it on the navigation laws; and the imperfection of his views on the nature of rent is undeniable, as well as on the principle of population. In these and such instances as these it would not be easy to find in the original doctrines the means of correcting subsequent errors, or the germs of extended discovery. But even if philosophers finally adopt the undulatory theory of light instead of the atomic, it must be borne in mind that Newton gave the first elements of it by the well-known proposition in the eighth section of the Second Book of the "Principia," the scholium to that section also indicating his expectation that it would be applied to optical science; while M. Biot has shown how the doctrine of fits of reflection and transmission tallies with polarization, if not with undulation also. But the most marvellous attribute of Newton's discoveries is that in which they stand out prominent among all the other feats of scientific research, stamped with the peculiarity of his intellectual character; they were (their great author lived before his age) anticipating in part what was long after wholly accomplished, and thus unfolding some things which at the time could be but imperfectly, others not at all comprehended, and not rarely pointing out the path and affording the means of treading it, to the ascertainment of truths then veiled in darkness. He not only enlarged the actual dominion of knowledge, penetrating to regions never before explored, and taking with a firm hand undisputed possession; but he showed how the bounds of the visible horizon might be yet further extended, and enabled his successors to occupy what he could only descry; as the illustrious discoverer of the new world made the inhabitants of the old cast their eyes over lands and seas far distant from those he had traversed; lands and seas of which they could form to themselves no conception, any more than they had been able to comprehend the course by which he led them on his grand enterprise. In this achievement, and in the qualities which alone made it possible, inexhaustible fertility of resources, patience unsubdued, close meditation that would suffer no distraction, steady determination to pursue paths that seemed all but hopeless, and unflinching courage to declare the truths they led to, how far soever removed from ordinary apprehension,— in these characteristics of high and original genius, we may be permitted to compare the career of those great men. But Columbus did not invent the mariner's compass, as Newton did the instrument which guided his course, and enabled him to make his discoveries, and his successors to extend them by closely following his directions in using it. Nor did the compass suffice to the great navigator without making any observations, though he dared to steer without a chart; while it is certain that by the philosopher's instrument, his discoveries were extended over the whole system of the universe, determining the masses, the forms, and the motions of all its parts by the mere inspection of abstract calculations, and formulas analytically deduced. The two great improvements in this instrument which have been made — the calculus of variations by Euler and La Grange, the method of partial differences by D'-Alembert — we have every reason to believe were known, at least in part, to Newton himself. His having solved an isoperimetrical problem (finding the line whose revolution forms the solid of least resistance), shows clearly that he must have made the coördinates of the generating curve vary, and his construction agrees exactly with the equation given by that calculus. That he must have tried the process of integrating by parts in attempting to generalize the inverse problem of central forces before he had recourse to the geometrical approximation which he has given, and also when he sought the means of ascertaining the comet's path, which he has termed by far the most difficult of problems, is eminently probable, when we consider how naturally that method flows from the ordinary process for differentiating compound quantities, by supposing each variable in succession constant; in short, differentiating by parts. As to the calculus of variations having substantially been known to him, no doubt can be entertained. Again, in estimating the ellipticity of the earth, he proceeded upon the assumption of a proposition of which he gave no demonstration (any more than he had done of the isoperimetrical problem) that the ratio of the centrifugal force to gravitation determines the ellipticity. Half a century later, that which no one before knew to be true, which many probably considered to be erroneous, was examined by one of his most distinguished followers, Maclaurin, and demonstrated most satisfactorily to be true. Newton had not failed to perceive the necessary effects of gravitation in producing other phenomena beside the regular motion of the planets and their satellites in their course round their several centres of attraction. One of these phenomena, wholly unsuspected before the discovery of the general law, is the alternate movement to and fro of the earth's axis, in consequence of the solar (and also of the lunar) attraction combined with the earth's motion. This libration, or nutation, distinctly announced by him as the result of the theory, was not found by actual observation to exist till sixty years and upwards had elapsed, when Bradley proved the fact The great discoveries which have been made by La Grange and La Place upon the results of disturbing forces have established the law of periodical variation of orbits, which secures the stability of the system by prescribing a *maximum* and a *minimum* amount of deviation; and this is not a contingent, but a necessary truth, by rigorous demonstration, the inevitable result of undoubted *data* in point of fact, the eccentricities of the orbits, the directions of the motions, and the movement in one plane of a certain position. That wonderful proposition of Newton, which, with his corollaries, may be said to give the whole doctrine of disturbing forces, has been little more than applied and extended by the labors of succeeding geometricians. Indeed, La Place, struck with wonder at one of his comprehensive general statements on disturbing forces in another proposition, has not hesitated to assert that it contains the germ of La Grange's celebrated inquiry exactly a century after the "Principia" was given to the world. The wonderful powers of generalization, combined with the boldness of never shrinking from a conclusion that seemed the legitimate result of his investigations, — how new and even startling soever it might appear, — was strikingly shown in that memorable inference which he drew from optical phenomena, that the diamond is "an unctuous substance coagulated;" subsequent discoveries having proved both that such substances are carbonaceous, and that the diamond is crystallized carbon; and the foundations of mechanical chemistry were laid by him with the boldest induction and most felicitous anticipations of what has since been effected. The solution of the inverse problem of disturbing forces has led Le Verrier and Adams to the discovery of a new planet, merely by deductions from the manner in which the notions of an old one are affected, and its orbit has been so calculated that observers could find it — nay, its disc, as measured by them, only varies 1-1,200 of a degree from the amount given by the theory. Moreover, when Newton gave his estimate of the earth's density, he wrote a century before Maskelyne, and by measuring the force of gravitation in the Scotch mountains, gave the proportion to water as 4·716 to 1; and, many years after, by experiments with mechanical apparatus, Cavendish (1798) corrected this to 5·48, and Baily, more recently (1842), to 5·66, Newton having given the proportion as between five and six times. In these instances he only showed the way, and anticipated the result of future inquiry by his followers. But the oblate figure of the earth affords an example of the same kind, with this difference, that here he has himself perfected the discovery, and nearly completed the demonstration. From the mutual gravitation of the particles which form its mass, combined with their motion round its axis, he deduced the proposition that it must be flattened at the poles; and he calculated the proportion of its polar to its equatorial diameter. By a most refined process he gave this proportion upon the supposition of the mass being homogeneous. That the proportion is different in consequence of the mass being heterogeneous does not in the least affect the soundness of his conclusion. Accurate measurements of a degree of latitude in the equatorial and polar regions, with experiments on the force of gravitation in those regions, by the different lengths of a pendulum vibrating seconds, have shown that the excess of the equatorial diameter is about eleven miles less than he had deduced it from the theory; and thus that the globe is not homogeneous. But on the assumption of a fluid mass, the ground of his hydrostatical investigation, his proportion of 229 to 230 remains unshaken, and is precisely the one adopted and reasoned from by La Place, after all the improvements and all the discoveries of later times. Surely at this we may well stand amazed, if not awe-struck. A century of study, of improvement, of discovery has passed away, and we find La Place, master of all the new resources of the calculus, and occupying the heights to which the labors of Euler, Clairant D'Alembert, and La Grange have enabled us to ascend, adopting the Newtonian fraction of 1-230 as the accurate solution of this speculative problem. New admeasurements have been undertaken upon a vast scale, patronized by the munificence of rival Governments, — new experiments have been performed with approved apparatus of exceeding delicacy, — new observations have been accumulated with glasses far exceeding any powers possessed by the resources of optics in the days of him to whom the science of optics, as well as dynamics, owes its origin, — the theory and fact have thus been compared and reconciled together in more perfect harmony; but that theory has remained unimproved, and the great principle of gravitation, with most sublime results, now stands in the attitude, and of the dimensions, and with the symmetry, which both the law and its application receive at once from the mighty hand of its immortal author. But the contemplation of Newton's discoveries raised other feelings than wonder at his matchless genius. The light with which it shines is not more dazzling than useful. The difficulties of his course, and his expedients alike copious and refined for surmounting them, exercise the faculties of the wise, while commanding their admiration; but the results of investigations, often abstruse, are truths so grand and comprehensive, yet so plain, that they both captivate and instruct the simple. The gratitude, too, which they inspire, and the veneration with which they encircle his name, far from tending to obstruct future improvement, only proclaim his disciples the zealous because rational followers of one whose example both encouraged and enabled his successors to make further progress. How unlike the blind devotion to a master which for so many ages of the modern world paralyzed the energies of the human mind!

"Had we still paid that homage to a name
Which only God and nature justly claim,
The Western seas had been our utmost bound,
And poets still might dream the sun was drowned,
And all the stars that shine in Southern skies
Had been admired by none but savage eyes."

Nor let it be imagined that the feelings excited contemplating the achievements of this great man are in any degree whatever the result of national partiality, and confined to the country which glories in having given him birth. The language which expresses her veneration is equalled, perhaps exceeded, by that in which other nations give utterance to theirs; not merely by the general voice, but by the well-considered and well-informed judgment of the masters of science. Leibnitz, when asked at the Royal table in Berlin his opinion of Newton, said that, "Taking mathematicians from the beginning of the world to the time when Newton lived, what he had done was much the better half." "The *Principia* will ever remain a monument of the profound genius which revealed to us the greatest law of the universe," are the words of La Place. "That work stands preëminent above all other productions of the human mind." "The discovery of that simple and general law, by the greatness and variety of the objects which it embraces, confers honor upon the intellect of man." La Grange, we are told by Delambre, was wont to describe Newton as the greatest genius that ever existed, but to add how fortunate he was also, "because there can only once be found a system of the universe to establish." "Never," says the father of the Institute of France, one filling a huge place among the most eminent of members — "Never," said M. Biot, "was the supremacy of intellect so justly established and so fully confessed; in mathematical and in experimental science without an equal, and without an example, combining the genius for both in its highest degree." The *Principia* he terms "the greatest work ever produced by the mind of man;" adding, in the words of Halley, that a nearer approach to the Divine nature has not been permitted to mortals. "In first giving to the world Newton's 'Method of Fluxions,'" says Fontenelle, "Leibnitz did like Prometheus: he stole fire from Heaven to bestow it upon men." "Does Newton," L'Hopital asked, "sleep and wake like other men? I figure him to myself as a celestial genius, entirely disengaged from matter." To so renowned a benefactor of the world, thus exalted to the loftiest place by the common consent of all men, — one whose life, without the intermission of an hour, was passed in the search after truths the most important, and at whose hands the human race had only received good, never evil, — no memorial has been raised by those nations which erected statues to tyrants and conquerors, the scourges of mankind, whose lives were passed, not in the pursuit of truth, but the practice of falsehood, — across whose lips, if truth ever chanced to stray towards some selfish end, it surely failed to obtain belief, — who, to slake their insane thirst of power or of preëminence, trampled on all the rights, and squandered the blood of their fellow-creatures, — whose course, like lightning, blasted while it dazzled, — and who, reversing the Roman Emperor's noble regret, deemed the day lost that saw the sun go down upon their forbearance, no victim deceived, or betrayed, or oppressed. That the worshippers of such pestilent genius should consecrate no outward symbol of the admiration they freely confessed to the memory of the most illustrious of men, is not matter of wonder; but that his own countrymen, justly proud of having lived in his time, should have left this duty to their successors, after a century and a half of professed veneration and lip-homage, may well be deemed strange. The inscription upon the cathedral, the masterpiece of his celebrated friend's architecture, may possibly be applied in defence of this neglect: "If you seek for a monument, look around." If you seek for a monument, lift up your eyes to the heavens, which show forth his fame. Nor, when we recollect the Greek orator's exclamation, that the whole earth is the monument of illustrious men, can we stop short of declaring that the Universe itself is Newton's. Yet, in raising the statue which preserves his likeness, near the place of his birth, and on the spot where his prodigious faculties were unfolded and trained, we at once gratify our honest pride as citizens of the same state, and humbly testify our grateful sense of the Divine goodness which deigned to bestow upon our race one so marvellously gifted to comprehend the works of infinite wisdom, and to make all his study of them the source of religious contemplation, both philosophical and sublime.

CAMP'S IMPROVED LIFE-BOAT.

The boat is thirty feet long, eight feet beam, and four feet hold, and decked over, so that it can be entirely inclosed, and the occupants protected from the washing of the sea, when required during a storm or heavy blow, by means of a water-tight hatch. It is propelled by means of a propeller wheel, worked with a crank by the inmates of the hold, and can be driven at a speed of six to eight miles per hour with less effort than would be required to move it at the same rate with oars. Its capacity is such that fifty persons can be seated in the hold, while thirty more may be lashed to and sustained upon the deck,—giving it a greater capacity for saving life in case of shipwreck than any other improvement yet brought before the public. It is provided with water-tanks and bread-lockers of sufficient size to meet the wants of a full load of persons under any ordinary circumstances. The air-chambers in the stern and stem would keep it buoyant, even if it were stove in, and cause it to right itself in case it should be overturned. Buoyancy is further promoted by a bag of cork attached to the sides below the gunwale, which also serves as a fender to prevent injury to the boat should it be thrown against the side of a vessel in a heavy sea.

The hold of the boat is divided into two compartments by a bulkhead, through which an aperture is made for entrance to the rear compartment from the front, should it be necessary to keep the main hatch closed during a heavy sea. By this means the boat is prevented from being filled with water while loading in a storm.

The boat may be lowered into the water from the vessel with perfect security, even during the running of the heaviest sea, and instantly released from the davit-hooks by means of a novel eye-bolt, invented by Mr. Camp. The peculiarity of this eye-bolt consists in having the upper part, against which the fall-hook bears, made movable upon a pivot and sustained by a catch, which can be removed, even under a heavy load, so as to allow the hook to be freed by a slight effort on the part of a person operating both bolts at the same time, thus setting the boat adrift upon the water with perfect safety. The advantages of having a boat entirely inclosed, so as to protect its occupants from the wash of the sea, and provided with means of propulsion to enable it to be navigated to the shore from the wreck, are apparent. In heavy weather it may be steered from below deck, there being a small raised hatch, with a window in front, for observation; and a binnacle, compass, and all the requisites for navigating the craft in any direction. In addition to the propeller, a mast and sails are lashed to the deck, ready for use in favorable weather.

NOVEL STEAMSHIP.

A steamship of most remarkable form and construction is now in the process of completion at Baltimore, by Messrs. Ross & Thomas Winans, the well-known engineers. The form of this vessel is so different from any hitherto constructed, that it is not an easy matter to describe its peculiarities. Premising, however, that its shape is like that of a cigar, sharp at both ends, and one hundred and eighty feet long, and sixteen feet in diameter in the centre, unbroken in its continuity, except by the wheel-house, which passes around about six feet of its entire centre, above and below the water-line, and over the top as well as under the bottom of the vessel,—the following simple description may be understandable:

Take two elongated and sharp-pointed sugar loaves, and place them but-ends together; put a stick through the centre of the two but-ends, which imagine to be the shaft of the water-wheel, which passes into the two sugar loaves, and is driven by engines at each end of the shaft. Thus it will be seen that it is two entirely separate vessels, united only by the shaft of the water-wheel, and by the wheel-house, which is built completely around the vessel, extending about three feet on each side of the wheel, and raised about three feet, with open sides below the water-line for the water to pass through, and connected with the vessel by upright plates of boiler iron. Having got thus far, imagine the hub of the wheel which is to be placed on the shaft between the two sugar loaves to be exactly the size of the but-ends of the loaves, with twelve iron flanges, or screws, fastened at equal distances on the outer edge of it. It will thus be seen that the wheel forms the centre of the body of the vessel, and revolves transversely; the twelve flanges on the edge of it being the propelling power. When in the water, with her engines, coal, and freight, one-half of the flanges on the wheel will be all the time in the water, and with ninety revolutions per minute some idea of its propelling power may be imagined. The vessel has no deck; but on the upper segment of the tubular surface there are gangways, to pass downward into the two sections, surrounded by iron railings, which extend on either side about thirty feet from the centre. With the exception of the two smoke-pipes and ventilators, these are all the outer works that will be visible. — *Baltimore American.*

STEEL SHIPS.

In 1850, Mr. Ewald Riepe obtained a patent in England for certain improvements in refining steel, which consisted, mainly, in subjecting bars or lumps of raw or crude steel to the action of heat for about four hours, in a furnace closed to the external atmosphere, the temperature being kept a little below the melting-point of the steel. By this method of operation, carburetted hydrogen and oxide of carbon are developed in the furnace in abundance, while the oxygen of the air is entirely prevented from acting upon the steel,—the working door of the furnace, etc., being carefully luted for this purpose.

This patent, says the London Mechanics' Magazine, which was permitted to remain in abeyance for some time, has lately been worked with very beneficial results by Mr. William Clay, of the Mersey Iron Works; the steel produced by means of it having been found to possess a very fine uniform grain, and to be peculiarly suitable for the plating of ships. A new steamer of 170 tons, named the *Rainbow*, intended for the Niger expedition, has been recently constructed of plates of this steel, of the following dimensions: Length, 130 feet; beam, 16 feet. Her engine is high pressure, and of 60 horse power, working up to 200 horse power, indicated; and the boilers, which have also been made of Mr. Clay's steel plates, have been proved up to 200 pounds on the square inch, though they will only require to be worked at 50 pounds to 60 pounds. The advantage of employing this material over the ordinary iron plates is, that, with about half the thickness, they are said to give equal strength with the best iron boiler-plates, so that vessels are able to be constructed of considerably lighter draft of water than formerly,—a result which is likely to be of incalculable benefit in the navigation of the shallow rivers of Africa and India. It will be remembered that Dr. Livingstone took out a small steam yacht, the plates for which were formed of the patent homogeneous metal manufactured by Messrs. Shortridge and Jessop, of Sheffield. The advantage claimed for the Riepe steel is, that, while possessing equal strength and adaptability for the purposes of ship-building, it can be more economically produced. Indeed, it is said that the process of manufacture is so simple, and the cost so little in excess of that of ordinary iron, that, by the saving of weight in the material, as compared with iron of equal strength, it will become absolutely cheaper. *Apropos,* of the strength of the steel, we may state that recent experiments, made by Mr. Clay in testing, at the Liverpool Corporation chain-proving machine, some samples of steel bars manufactured at the Mersey Works, showed that their average tensile strength was 160,832 per square inch, while the strength of Russian iron is only 62,644; of English rolled iron, 56,532; Lowmoor, 56,103; American hammered, 53,913; of tempered cast steel, 150,000, etc.

IMPROVEMENTS IN STEAM-GAUGES.

The only steam-gauges known ten years ago, were the mercury-gauge, the air-gauge, and the piston-gauge for locomotives. The first is costly and cumbersome, its length for measuring 150 pounds pressure being 27 feet. The second breaks easily, the divisions are small, and ascertaining the pressure by looking at the instrument is a work that ordinary firemen carefully dispense with. A piston pressed by steam against a spring is unreliable, on account of friction. In June, 1849, Eugene Bourdon took out a patent in France for a spring-gauge, in which the pressure of steam is indicated by a hand on a graduated dial. Inside the case is an elastic metallic vessel, so shaped as to change its form when steam is let in, and it is united by a proper mechanism to the hand which points out the result. The great superiority of this gauge over former ones is obvious. No liquids are used; there are no joints, consequently no leakage. The gauge is cheap and compact, and its indications are read at a glance from any part of the engine-room. This instrument carried the highest award at the great London exhibition of 1851, and in the subsequent year was patented in the United States, and the patent bought by Ashcroft, under whose name it is generally known. The claim read thus:

"I claim the application of curved or twisted tubes, whose transverse section differs from a circular form, for the construction of instruments for measuring, indicating, and regulating the pressure and temperature of fluids."

Since 1852 about twelve patents have been granted for dial-gauges, in which the elastic vessel was different from Bourdon's. Corrugated Disks, made of steel, or diaphrams made of India rubber, are the main feature in most of them. The first substance is destroyed by rusting, especially where sea water is used. India rubber in a still shorter time undergoes internal chemical changes, and requires to be renewed. In a steam-gauge patented by Victor Beaumont in 1854, and recently perfected, the aim has been to embody all the qualities of Bourdon's by using an elastic vessel of brass closed on all sides except that of the boiler, and to avoid the vibrations of the hand or pointer resulting from the momentum of the tube, held by one end, for each jerk of the locomotive to which the gauge is attached. To obtain this result, the elastic vessel is formed of flattened hollow spheres, communicating together; one side of each sphere is turned inside, so that, under the pressure of steam, one part is extended and the other is compressed. The regularly diminishing motion of the extended parts is thus compensated by the regularly increasing motion of the compressed portions, and the graduation is perfectly uniform for the range of the gauge. Large surfaces are thus exposed to the action of steam, which acts in consequence with a great deal of power; beside which the effect of momentum is so insignificant, that the gauge may be struck with great force on the table without the pointer vibrating in the least.

IMPROVED STEAM-BOILER.

A patent for an improved steam-boiler has recently been issued to Wm. G. Norris, of Philadelphia, the essential feature of which is a *closed* chamber between the fire-box and tube-sheet, for the purpose not only of preventing any combustion going on in actual contact with the tubes of the boiler, but also for the purpose of equalizing the heat before it reaches the tubes.

The advantages claimed for this form of boiler are: lightness, simplicity, and the economical consumption of anthracite and bituminous coal, without emitting smoke, sparks, or gas; a greater amount of heating surface than is obtained in a boiler of the ordinary construction; the fire-box, being placed *over* the axles, may be much longer than usual, to insure sufficient grate surface, while the distance from the centre of the backdriver to the centre of the front trunk-wheel remains shorter than in the ordinary locomotive — a good arrangement, it is said, for short curves. There is no overhanging weight, as the adhesion is all upon the drivers; and, as the centre of gravity is no higher than common, the engine will pull more in consequence, we are informed. In ten-wheel engines the arrangement of the machinery is the same as in those of eight wheels, having a full stroke-pump fastened to the main frame, between front driver and cylinder, while the main rod connects directly to the *front driver*, thereby dispensing with the superfluous friction of "spade handles" or combination stub-ends.

FITTS'S AUTOMATIC BOILER-FEEDER.

The want of a machine which will supply water to boilers, working independently of any other power, and regulating the supply by the amount evaporated, has long been felt. It is, moreover, often desirable to use steam for various purposes, without the necessity of running an engine solely as a means of working a pump for supplying the boilers. To attain this result much labor and skill has been expended, but, thus far, with apparently little success, inasmuch as the pump substantially as arranged by Watt is universally in use.

An arrangement, however, recently put in operation by B. Fitts, of Worcester, Mass., merits attention. The principle involved is, to draw water from the well or reservoir, by means of a vacuum produced by the condensation of steam, which water is subsequently forced into the boilers by the pressure of steam on its surface — thus dispensing with the pump and all the apparatus necessary to drive it.

The machine consists simply of two chambers, each holding six or eight gallons, two valves, an apparatus to change the valves, and the pipes necessary for connecting the boiler, reservoir, etc. The arrangement is double acting — one chamber filling with water while the other is discharging into the boiler. The steam, also, which is used to force the water contained in one chamber into the boiler, is afterwards discharged into the other chamber, and there condenses and heats the water contained in the chamber; the valve is then changed, and the heated water is forced into the boiler. The other chamber, at the same time, being in connection with the water reservoir, is filled with water, by reason of the vacuum produced by the condensation of the steam previously contained in it. There is also an indicator attached, which ingeniously registers the whole amount of water supplied.

SILVER'S MARINE GOVERNOR.

The ordinary ball-governor was invented by James Watt as a part of the steam-engine. In this instrument two heavy balls attached to the extremities of two rods, articulated at their other end with a vertical shaft, are made to rotate with it, receiving motion from the engine. Centrifugal force is thus produced, which overcomes gravity and forces the balls to separate and go up in a circle to a distance proportional to the velocity of the machine. The balls and rods are properly connected with a throttle-valve in the steam-pipe, so that this is wide open when the velocity is small, and entirely closed the moment a velocity is reached beyond which it is not desirable to go. This instrument, depending on gravity for its accurate action, cannot be used on board a ship, where the rolling and pitching would throw it in an inclined position, in which the weight of the balls would make it act at the wrong time. Mr. Thomas Silver, of Philadelphia, obviates this fault by adding two more balls to equilibrate the two first, and by substituting the action of a spring to that of gravity. The four balls of equal size are attached to two rods working on a pin through the centre and through a small shaft. A movable sleeve on this shaft is connected with the two rods half way between the balls and the centre pin, and an adjustable steel spring pushes or pulls constantly on the movable sleeve, in the proper direction to bring the four balls close to the shaft. The movable sleeve is connected with the valve. This instrument is of great service to paddle-wheel steamers, as in a heavy sea it frequently happens that one wheel is entirely out of water, when the engine acquires an undue velocity, technically called "racing," which, suddenly checked by the reïmmersion of the wheel, may result in the breaking of the shaft. But it is of a much greater import for propellers. In these ships the screw is as often brought out of water by a heavy pitch, and as there is no other wheel in the water to resist the power in a measure, the increase of velocity is enormous. Two causes, then, combine for breaking the shaft—the first is, the sudden blow of the sea against the screw when reëntering the water; the other is, the side force exerted against the shaft when the plane of rotation of a rapidly rotating body is suddenly changed.

IMPROVEMENT IN PROPELLER ENGINES.

The several direct-acting screw-propeller engines hitherto constructed are objectionable in the following particulars, viz.:—The horizontal engines occupy too much space transversely in the vessel to admit of being placed in the run. The vertical engines pass through the decks, and project so far above the water-line as to be useless for war purposes; and all approved double engines operate on cranks placed at right angles to each other, which involves as eries of bearings, much friction, and liability to derangement from the shafts getting out of line. In addition to these imperfections, the extreme shortness of the cranks, with the attendant great friction on the crank-pins and journals, to say nothing of the heavy diagonal thrust of the connecting-rods, are serious defects in the direct-acting screw-propeller engines now in common use.

To obviate these difficulties, that well-known able, and veteran inventor, John Ericsson, of hot air celebrity, has invented a useful improvement in steam-engines for working propellers, which consists in the arrangement of the two cylinders of a double engine in such a manner that their base or bottom ranges with a plane passing through the axis of the propeller shaft, or nearly so, in combination with a certain arrangement of rock-shafts, crank-pins, and connecting-rods, for imparting motion from the pistons to the shaft, whereby the inventor is enabled, firstly, to bring the cylinders nearer to the propeller shaft, and hence to economize space, and construct the frame of the engine of great strength and compactness. Secondly, to avoid the diagonal thrust and friction of the slides, unavoidable when the connecting-rod is attached directly to the cross-head. Thirdly, to operate the two connecting-rods nearly at right angles to each other, which enables the inventor to produce a continuous motion with a single crank on the propeller shaft and a single crank-pin in common. Fourthly, to employ a crank on the propeller shaft much longer than half the length of the stroke of the piston, thereby diminishing the heavy pressure on crank-pins and journals which has heretofore caused so much trouble by the overheating of the bearings, and at the same time diminishing the strain on the engine frame. — *Scientific American.*

HYDROSTATIC SCREW-PROPELLER.

The Southampton (Eng.) Star, daily paper, describes the successful working of a hydrostatic screw-propeller, or steamer driven without a shaft, and says: — " All that would be required for the largest ship afloat (by the adoption of this invention) would be one horizontal steam cylinder, placed close to the bottom of the vessel, connected to one pump, also laid on the bottom of the vessel, close to the kelson, working fore and aft the ship without shaft or crank; and by forcing water through the hollow screw-propeller, producing a powerful rotary motion, where only it is required, namely, in the screw, which can by this invention be driven continuously five hundred or more revolutions per minute; and as the whole is immersed in a constant stream of cold water, there is no possible chance of heated bearings. The water surrounding it on all sides becomes a constant lubricator. The power of manœuvring the propeller from the deck, no matter at what rate the vessel may be sailing, is another peculiarity.

ON BOILER INCRUSTATIONS.

A series of papers on boiler incrustations have recently been published in the London Engineer, by Mr. James Napier, a practical chemist of Glasgow. The following is his analysis of scale taken from a boiler in which river water had been used:

Carbonate of lime	79.0
Sulphate of lime	6.3
Peroxyd of iron	3 5
Silica	2.2
Carbonaceous matter	4.0
Water	5.0
	100.

The next analysis is that of scale taken from the boiler of a steamer running between Glasgow and Liverpool, in which no attention was paid to "blowing off." The scale was composed of two layers; the one (that next the metal) was hard and crystalline, the other (or outer coat) was softer and granular. The thickness of the whole crust was about three-eighth of an inch:

Sulphate of lime	81.6
Magnesia	4.2
Silica	2.8
Peroxyd of iron	2.4
Salt	0.7
Water of crystallization	7.7
Carbonic acid	0.6
	100.

The next analysis was that of scale taken from the same boiler, which was worked for the same length of time, as in the former experiment, but care was taken to "blow off" regularly. The scale in this case was only one-sixteenth of an inch thick — only one-sixth the thickness of that formed when "blowing off" was neglected:

Sulphate of lime	94.5
Magnesia	1.5
Peroxyd of iron	0.5
Salt	1.1
Water	2.4
	100.

These analyses show that the sulphate of lime is the main ingredient of the scale deposited by sea water. They also afford very satisfactory evidence regarding the way to prevent incrustations by care in blowing off the saturated water regularly.

The following is the method proposed by Mr. Napier for the prevention of incrustations in all boilers. He analyzes the water to be used, and if found to contain only the bi-carbonate of lime in suspension, there is no difficulty in preventing it from forming scale. The carbonate of lime separates from the water at a high heat, and is kept suspended in the boiler while the water is hot; but when the boiler is stopped, it falls to the bottom in cooling, and and when cold it hardens, adheres to the metal, and forms a crust. A boiler using hard fresh water containing carbonate of lime has thus a thin layer of scale formed every night, and at last it accumulates to a thick stony crust, which almost prevents the passage of the heat from the fire to the water. To prevent such scale, the plan to be adopted is simple. In about an hour after the engine is stopped every evening, and when the fire is cooled down, the engineer should blow off the water freely. This will discharge all the sediment which has been precipitated, and prevent it hardening and adhering to the metal.

Although this method of working boilers will prevent scale, if the water only contains carbonate of lime, it will not entirely suffice to prevent incrustations when the sulphate of lime is the principal ingredient in the water, because it does not precipitate like the carbonate. Having by analysis discovered the quantity of the sulphate of lime in each gallon of the water to be used as feed, a sufficient quantity of the carbonate of soda is to be employed to neutralize the sulphate and convert it into the carbonate. The carbonate of soda dissolved is to be fed regularly into the boiler by a pipe connected with the water feed-pipe. On land boilers, the carbonate of lime thus formed should be blown off every evening when the water has cooled down; in marine boilers, the carbonate will float near the surface when the boiler is working, and it can be blown off by the surface water-cock. Any alkali will neutralize the sulphate of lime in a steam boiler, but the common carbonate of soda is the cheapest which can be used. Care, however, must be exercised not to employ it or any other alkali in excess for such a purpose, as it has a tendency to volatilize with the steam.

CONSUMPTION OF COALS AND RATE OF EVAPORATION FROM ENGINE BOILERS.

At a recent meeting of the Manchester Philosophical Society, Mr. Graham read a paper, in which he described the results of experiments which he had made with a series of small vessels of equal size, the fire being under the first, and the flame-bed alone passing under the others. The evaporative power of the first was found equal to 100, the second to 27, the third to 13, and the fourth to 8. A second set of experiments with larger vessels, in the shape of boilers, corroborates these results. The third series of experiments were made with the view of determining the value of a supplementary boiler as a heating surface, placed under the most favorable circumstances; the result showed an advantage of 15 per cent.

Mr. Graham then detailed the results of a numerous set of experiments on evaporation, on the large scale, with reference to engine boilers. These experiments have extended over a period of several years, observations being made daily, and the results deduced from several hundred recorded observations. Before beginning to register his results, the boilers were in each case reset, and by careful and continuous experiments were put into what was found to be then their best condition for giving the best working result, as regards the admission of air, the draft of the chimney, the size of the fire-place, the distance of the bars from the boiler, the thickness of the fire-bars, and of the fire itself, the form of the flame-bed, flues, and bridges. Mr. Graham stated that in the case of one boiler, the alterations had been repeated at least 30 times for this purpose. The experiments with the boilers were of 12 hours duration each, and number from 30 to 40 for each boiler. A perfect command was maintained of the draft, which varied from 0·5 to 0·7 in. pressure of water, and the temperature of the draft at the bottom of the chimney was generally sufficient to melt lead (612° F.), but never zinc (773° F.) The conclusions which Mr. Graham arrived at by means of these experiments were the following:

1. That the boiler usually called the "Butterfly, or Fishmouth boiler," 25 feet long and 7 feet diameter, will, under favorable, but what may be called ordinary circumstances, give with the Worsley coal, for each pound of coal burnt, 8·29 lbs. of steam; or, not including the heating of the feed-water, from 60° to 212° F., 9·67 lbs.

2. The boiler usually known as James Watt's "wagon-shaped boiler," 25 ft. 6 ins. long, and 6 ft. 6 ins. diameter, will, under similar circumstances, give 8·80 lbs. of steam; or, not including the heating of the feed-water, from 60° F. to 212° F., 10·26 lbs. of steam for each pound of coal burnt.

3. The plain cylindrical boiler, with fire-place underneath, 42 feet long and 6 feet diameter, will, under similar circumstances, give 6·20 lbs. of steam; or, not including the heating of the feed-water, from 60° F. to 212° F., 7·23 lbs. of steam for each pound of coal burnt.

4. The boiler with two internal fire-places joined into one internal flue, known in the neighborhood of Manchester as the "breeches boiler," 23 ft. long and 8 ft. diameter, will, under similar circumstances, give 5·90 lbs. of steam; or, not including the heating of the feed-water, from 60° F. 212° F., 6·88 lbs. of steam for each pound of coal burnt.

5. That a supplementary boiler, under very favorable circumstances, gives a saving of 15 per cent.

6. That flues round a boiler, when cleaned out, and the sides of the boiler scraped once a week, will give a saving of about 2 per cent.

7. That a difference in the setting alone of the same boiler may produce a difference in the result amounting to 21 per cent.

8. That the difference between a good-shaped boiler, properly set, and a bad-shaped boiler, improperly set, but both clean and in good order, may amount to as much as 42 per cent.

9. That a difference in firing only will produce a difference in the result of 13 per cent.

10. That the smallest loss by smoke burning, or by the admission of cold air, either over the furnace door or in front of the bridge, or at the back of the bridge, has been 1·7 per cent.

11. That the loss arising from a scale of sulphate of lime, of not more than one-sixteenth of an inch, amounted to 14·7 per cent.

12. That neither wet coal, nor coal which had been out of the pit three years, nor wet weather, nor a variation of temperature in the atmosphere from 40° F. to 70° F., produced any appreciable difference of result.

13. That windy weather gave a good result.

14. That a comparatively thick and hot fire, with a good draft, uniformly gave the best results.

15. That the difference in the results obtained with different coals, all from the immediate neighborhood, amounted to a loss of 11 per cent.

16. That the same coals, reported to be from the same pits, will vary in their results to the extent of 6 per cent.

17. That when a boiler is worked solely for the purpose of heating by means of its steam, dye-vessels, soap-cisterns, etc., if its available power, with the steam at a pressure of 2 1-2 lbs., be taken as equal to 100, then at 7 lbs. pressure its available power will be 120, and at 10 lbs. pressure it will be 130; the same quantity of coal being consumed in each case. This surprising result, at present unaccounted for, may be thus stated:— That the same weight of coal consumed in the same number of hours will work ten cisterns at 2 1-2 lbs. pressure, twelve cisterns at 7 lbs. pressure, and thirteen cisterns at 10 lbs. pressure.

18. That while we may reasonably look for improvements in the construction of the fire-place, in the form of boiler, in the addition of separate supplementary heating surface, and in cleanliness, and may thereby effect a great saving in the consumption of coal, we cannot, at the same time, expect much saving from extension of flue space, when coated with soot, or for greater length of boiler than four times the length of the fire-place.

Mr. Graham stated in addition, that in consequence of the uniform low results obtained by evaporation from boilers and flues open to the atmosphere, which, according to his experience, never rise higher than from 5·5 to 6·0 lbs. of steam for each pound of coal burnt, also from the increased results obtained with increase of pressure, and apparently due to that condition, he is disposed to suggest that the rate of evaporation of water per pound of coal increases with, and bears some ratio to, increase of pressure.

With regard to the deposition of sulphate and carbonate of lime and mud in boilers, Mr. Graham stated that he had experimented, with more of less success, with caustic soda, quick-lime, muriatic acid, soap liquor, sawdust, spent madder, and logwood chips. Two facts in particular were noticed as regards the tendency of hard water to "scale": 1. That the sulphate of lime separates from the water when in contact with the bottom of the boiler, or with other substances, such as sawdust or other materials floating in the water; but that no precipitation takes place until the water has been concentrated, by continued evaporation, down to the state of a saturated solution, or to that point which may be termed the "salting point." 2. That *carbonate of lime* and mud are principally liberated in the body of the water, and have but little disposition to adhere to the boiler, unless cemented by the sulphate of lime.

Practically, therefore, it has been found that no scale of any consequence will be produced on engine boilers, even with such hard water and hard firing as Mr. Graham has been accustomed to, if 100 gallons of the concentrated liquor of the boiler, equal to 4 per cent. of the amount of feed-water used daily, and 300 gallons, or 12 per cent., be run away on Saturday through the usual mud machine, and if the boiler be run empty every sixth Saturday and brushed out. The water used was so hard as to require from 35 to 40 measures of Clark's test liquid to soften it. There is little loss incurred by this mode of working, since the chief discharge may take place at the close of each day's work; and there is an incredible advantage gained by the saving of coal, the reduced wear and tear of the boiler, and the greater safety of all persons concerned with it.

ON THE RESISTANCE OF TUBES TO COLLAPSE.

It has long been a desideratum in the strength of boilers to determine some definite law by which the engineer could calculate the proportionate strength of internal flues. Ever since boilers became a necessary appendage to the steam-engine, we have acted upon the principle that the internal cylindrical flues subjected to compression were absolutely stronger than the outer shell opposed to tension. These opinions have, in reality, had no foundation in practice, excepting from deductions drawn from occasional explosions, and the failure of vessels under severe pressure. Hitherto, there has been nothing definite, or any known principle by which we could calculate the diameter, thickness of plates, or length of flues corresponding with the strength of the boiler; and even in cases where explosions have taken place in collapse, we have, too frequently, mistaken the original cause from the *débris* surrounding the rupture, and the force which has torn to pieces the scattered remnants of the outer shell. Numerous accidents of this kind have occurred, accompanied by serious loss of life; these have too frequently been caused by the collapse and the rupture of the internal flues, which, acting upon the interior of the boiler with an irresistible force, carries havoc and destruction before it. The relative position and comparative value of these resisting forces have never as yet been clearly ascertained, in so far as respects the cause of rupture, and the anomalous condition in which many of these constructions are affected, have greatly retarded the application of science to improvements in the manufacture. There appears, in fact, to be no rule in existence calculated to attain uniformity of strength in all the parts of a steam boiler, where some of the parts are exposed to internal and some others to external pressure.

The resistance of cylinders, spheres, etc., to internal pressure, have been ascertained from experimental data, such as the form and dimensions of the vessel united to the resisting powers of the material; but we have yet to learn what proportion cylindrical and elliptical tubes bear to each other in their resistance to external and internal pressure.

To supply this want, a series of experiments have recently been instituted, at the joint request of the British Association and the Royal Society, by Mr. Fairbairn, the eminent English engineer, and the law of resistance, under various forms and conditions, ascertained.

The law of resistance for cylindrical tubes, as regards their length, appears to be this: a tube having the same strength of material, and being of the same diameter, will resist double the pressure to one of double the length; or the collapsing pressure, other things being the same, varies inversely as the length, and inversely as their diameters. Experiments made with elliptical tubes showed that in every construction where tubes have to sustain an uniform external pressure, the cylindrical is the only form to be relied upon, and that any departure from the true circle is attended with danger.

The experiments also tended to confirm the conclusions arrived at some years since, that the strengths of riveted joints of malleable iron plates are nearly as the numbers

100 for the plate,
70 for double riveted joints,
50 for single riveted joints.

In conclusion, Mr. Fairbairn remarks: It is interesting to observe how closely nature approximates in her productions to the strongest and best forms. If we look at the tubular forms of grasses, bamboos, and other vegetable constructions of this kind, and taking to account the uses for which they were intended, we shall see that the form contributes greatly to their strength; and we shall, moreover, find that the shoots are telescopic, forming a series of concentric rings, arising from the formation of new and smaller tubes as they emerge in succession from those previously formed. As these again protrude and advance in growth, they leave behind enlarged hoops, or disks, of sufficient rigidity to support and sustain the form of the tubular structure. The same law which pervades natural productions should not be overlooked in art. We have ever before us the lessons of this first great natural teacher; and did we but consult her laws, and in all our applications endeavor to conform to the rules of a philosophy which never errs, and by which nothing is ever made in vain, we should find, to use the words of our aspirations after truth, that Nature's laws, and the constructions derived therefrom, constitute the only true system of philosophy by which we can attain the maximum of strength with the minimum of material.

The sphere is probably the only true form by which we can obtain uniformity of resistance to an uniform pressure, whether external or internal; and to approximate to this, probably, was the reason why our predecessors, from the days of the Marquis of Worcester to those of Watt, adopted the haycock or circular boiler with a hemispherical top and hemispherical bottom, as shown.

This was selected as the strongest form of boiler in the time of Newcomen and Leighton; and it was, probably, for a similar reason that the glass-blower forms the bottom of bottles with an elevated cone penetrating for some distance into the interior of the cylindrical part. This gives great strength to the bottle in resisting internal pressure, and at the same time reduces the quantity of liquid contained in the bottle; a consideration independent of strength, and probably a matter of no small importance to the retail dealers in wine and ardent spirits.

The result of these experiments upon metal tubes subsequently suggested to Mr. Fairbairn the propriety of similarly testing the resisting powers of a perfectly homogeneous, crystalline, and rigid material; in order that our knowledge of the laws that govern the resistance of vessels to collapse might be confirmed and extended. Glass was selected, not only because of its fulfilling better almost than any other material the conditions sought for, and from the ease with which it could be manufactured into the forms required, but also because it was hoped that the results would be of practical value in those cases in the arts and in experimental science, in which it is so extensively employed. The experiments were conducted in a similar manner to those upon iron. Some cylinders and globes, blown out of good flint-glass, were procured direct from the maker. The open ends were hermetically sealed by means of the blowpipe; and the globes, etc., were placed in a strong wrought-iron vessel capable of sustaining a pressure of 2,000 lbs. to the square inch. Water was pumped in by means of a force-pump; the pressure was recorded by a Schaffer's gauge; and the point of rupture was indicated by an explosion within the vessel, and by a sudden decrease of pressure. The first experiments were upon glass globes, intended to be perfectly spherical, but in most instances somewhat flattened upon the side opposite to that from which they were blown. Notwithstanding this ellipticity, some of the globes bore enormously high pressures, especially when the extreme tenuity of the glass was considered, amounting, as it did, from only $\frac{1}{100}$ to $\frac{2}{100}$ of an inch in thickness. In one instance seven globes of glass were submitted to test, three of which were intended to be 5 in. diameter, one 5 1-2 in. and three 8 in., but varying as before mentioned. The bursting pressure of the first four was 292 lbs., 410 lbs., 470 lbs. and 475 lbs. to the square inch,—equivalent in the last case to twenty tons upon a 5 1-2 in. globe, $\frac{1}{50}$ in. thick, before it was fractured. The 8 in. globes burst respectively at 35 lbs., 42 lbs. and 60 lbs. to an inch; but they were unfortunately all elliptical to a serious extent, the diameters of that which burst at 42 lbs. being respectively 8·20 in. and 7·30 in.

In experimenting with homogeneous glass cylinders, blown with hemispherical ends, it was found that the law deduced from the experiments upon iron tubes, applied equally well to the glass, viz., that the strength of cylindrical vessels, exposed to a uniform external pressure, varies inversely as to length. Thus a glass cylinder 4·06 in. diameter, 13 3-4 in. long and ·045 in. thick, collapsed under a pressure of 180 lbs. to the square inch; while another, 4·05 in. diameter, 7 in. long, and ·046 in. thick, yielded only under a pressure of 380 lbs. to the inch.

EVAPORATIVE POWER OF BRASS, COPPER, AND IRON BOILER-TUBES.

A late number of the London *Mechanics' Magazine* contains an article on the above important question, by W. G. Tosh, from a paper read by him before the Institution of Mechanical Engineers at Manchester, England. He constructed small vertical boilers of equal dimensions, and placed in the centre of each a single tube, two inches in diameter, and of No. 14 wire gage thickness. A gas flame was applied to each tube — iron, brass, and copper — successively, during a certain period of time, which was equivalent to the same quantity of fuel consumed in each case. The experiments were first conducted during the day, then at night, at times when there was little probability of a change of pressure in gas pipes. Eight of these were made with the boilers, and the quantity of water evaporated was measured by the number of inches it was lowered in a boiler by each experiment. The result was in favor of the greater evaporating power of the brass over the iron tubes, in the proportion of 125 to 100; that is, two pounds or two tons of coal, or other fuel, will, with the use of brass tubes in a boiler, evaporate twenty-five per cent. more water than iron tubes with the same quantity of fuel, under precisely the same circumstances. In the same proportion that brass surpassed iron in evaporative power, copper was found to surpass brass. The evaporative powers, relatively, of the three metals in tubes for steam boilers, he found were as follows: Iron, 100; brass, 125; copper, 156.

The experiments of Mr. Tosh were subjected to a searching criticism by the engineers of the Institution, and strong doubts were expressed as to their correctness.

PROSSER'S SURFACE CONDENSER.

The principle of this condenser is to use much less condensing water than is usual, by raising its temperature to the boiling point, and to condense the steam arising from this water to supply the loss of water in the boiler. The apparatus is divided into three portions. The condenser proper consists of a number of iron pipes, inside of which runs the escape steam, and outside of which is the condensing water. The condenser for the steam arising from the condensing water is built on the same plan. The heater through which the condensed water is forced to pass on its way back to the boiler, and where it is heated by the escape steam on its way to the condenser, is also of a similar construction. There are merits in this condenser. It is so constructed as to be lasting, and it leaves the boiler clean, even when the most dirty water is used.

ON A NEW SOUNDING APPARATUS.

The following device, proposed by Lieut. E. B. Hunt, U. S. A., has for its chief object to run sounding lines in harbors and water of moderate depth. The principle is that of measuring barometrically the pressure due to the depth, this pressure being transmitted to the barometric basin by a column of atmospheric air.

The method is as follows: Arrange a weighted india-rubber vessel for dragging on the bottom; connect this with a boat or surveying vessel by an air-tight tube of small bore; let this tube open into a cistern of mercury made air-tight; from this cistern arrange a vertical glass column, open at the top, in which the mercury can freely rise to any required height by the pressure due to depth.

The mode of use would be thus: "Throw the weighted air-vessel overboard and let it sink to the bottom, it being connected with the vessel by the air-tube, in turn duly joined to the barometric cistern. Let the boat or vessel be propelled on a course. The air-vessel will be dragged along the bottom, and as the water must have free access to the bag enclosing the air, its pressure will compress the air to a density due to the depth. This pressure will be communicated along the tube to the vessel or boat, and being received on the mercurial surface, will raise a column to such a height that its weight will equal the weight of an equal column of the water, whose height is the depth to which the air-vessel is sunk, or rather that of the lowest part on which the pressure acts. As this depth varies, the height of the column will also vary. The relation of these heights is expressed by the specific gravity of mercury divided by the specific gravity of the particular sea or other water of which the soundings are being made. It would be necessary to make occasional observations with the hydrometer, and in the case of tidal streams these should be quite frequent." The detailed construction of such an apparatus is given by Lieut. Hunt, in Silliman's Journal, No. 76, July, 1858.

GAS-LIGHTS ON RAILROAD CARS.

The following plan for lighting cars with gas has been adopted with great success on the New Jersey railroad. Each car is provided with a wrought-iron cylinder, of a capacity of four and a half cubic feet. The cylinder is of a strength capable of sustaining 500 pounds pressure. The heads, for greater security, are made concave. The gas is compressed under a pressure of twenty atmospheres (300 pounds to the square inch), 90 cubic feet of gas being forced into each cylinder. Each car is provided with a cylinder, which is placed upon a shelf under the car floor, and coupled in the usual manner with a pipe leading to the burner within. An improved regulating contrivance controls the delivery of the gas to the burner under all pressures, and is interposed between the cylinder and burners, so that the light is always steady. The pressure of the gas ensures the continuity of light, no matter what the concussions or roughness of the road.

The method of charging the cylinders with gas, adopted on the New Jersey road, is simple and expeditious. Near the Company's machine shop, at Jersey City, a stack of the cylinders are arranged, into which the gas is forced by the rapid movements of a steam-pump, to a pressure of about 450 pounds. The cylinders are connected together by small pipes, and thus form a strong and capacious reservoir. A conducting-pipe leads from the stack to the large dépôt on the Hudson river, where all the passenger cars arrive and depart, a distance of a quarter of a mile. The conductor terminates in a horizontal pipe running beneath the dépôt platforms, with stop-cock openings at suitable intervals. When the car cylinders are to be charged, an attendant simply couples them to the conducting-pipe, and opens a stop-cock. The gas then instantly rushes into the cylinders and fills them, under the pressure of the reservoir, and they are ready for use. The filling of the cylinders for a whole train occupies only a few minutes, and the work of supplying all the trains with gas is, we are told, easily performed, from beginning to end, by one man. — *Scientific American.*

TUNNEL UNDER THE ALPS.

It is generally known that the immense work of boring a tunnel under the Alps, between Modane and Bardonéche, had commenced; but we have now to record some interesting facts which might, perhaps, never have been discovered but for the peculiar methods employed in this colossal operation. Modane and Bardonéche are situated on opposite sides of the Alpine chain which divides Piedmont from France, and precisely at a point where the valleys of the Arc and the Dora, which lie nearly on the same level, run parallel to each other, and the mountain is narrowest. The thickness of the intervening mountain is 13 kilometres in a straight line; the actual tunnel will be 12 1-2 kilometres. It is designed in the same vertical plane, but, to facilitate drainage, is somewhat higher in the middle than at the orifices, so as to form gentle slopes on both sides, — one not exceeding an inclination of five per thousand, and the other being twenty-three per thousand, in consequence of a difference of level between the two extremities, the numbers being, — Bardonéche (southern orifice), 1,324 meters; culminating point, 1,335 meters; Modane (northern orifice), 1,190 meters above the level of the sea. The crest of the mountain being 1,600 meters higher than the culminating point, the sinking of shafts — which is the method generally employed in order to begin boring tunnels at several points at once — was out of the question; hence the tunnel could only be worked at its extremities, so that the labor, by the ordinary processes, could not be accomplished in less than thirty-six years. Then, how was a depth of gallery of three or four kilometres, and having but one orifice, to be aired? These were all serious obstacles. MM. Elie de Beaumont and Angelo Sismonda having examined the mountain geologically, found it to contain micaceous sandstone, micaceous schists, quartzite, gypsum, and limestone, — all easy to blast, the quartzite alone excepted; but the stratum of this is not likely to be very thick. The other difficulties alone, therefore, remained; and these were at length overcome by three Sardinian engineers, — MM. Sommeiller, Grattone, and Grandis, — who proposed to turn the abundance of water for which the locality was remarkable to account, by applying it to a peculiar system of perforation and ventilation, which we will now endeavor to explain. The first apparatus imagined by these gentlemen consists in a hydraulic air-condenser, which is a syphon turned with its orifices upward, and communicating by one of them with a stream of water, by the other with a reservoir of air. The water, descending into the first branch, enters the second, and by the pressure it exercises condenses the air, which is then forced into the reservoir. This done, a valve is opened, by which the water contained in the syphon is let out, and the operation recommences. The emission and introduction valves are regulated by a small machine operating by means of a column of water; and the air in the reservoir is maintained at a constant degree of pressure by a column of water communicating with another reservoir above. Thus, with a waterfall twenty meters in height, the air is condensed to six atmospheres, equivalent to the pressure of sixty-two meters of water. This condensed air is used for two purposes; first, as a motive power, and then for ventilation. Two kinds of perforators, worked by condensed air instead of steam, are employed, — one invented by Mr. Bartlett, the other by M. Sommeiller, — and the manner in which these machines perform their duty affords the first practical demonstration of the possibility of employing compressed air as a motive power with advantage. By means of these perforators holes for blasting may be bored through the hardest sienite in one-twelfth of the time which would be required if ordinary means were employed. In order to understand the importance of this result, it may be stated that, in tunneling, three-fourths of the time is employed in boring holes, and the remainder in charging and blasting; hence, accelerating the former operation is an immense advantage. The perforators have another advantage: in a place where three couples of miners would hardly find room, eighteen perforators may be set to work; so that, by these ingenious contrivances, as well as by others for clearing away the rubbish, the perforation of the tunnel may be effected in six years instead of thirty-six. The air that has been employed as a motive power is used to feed the gallery; but when the latter shall have reached a considerable depth, it will require 85,924 cubic meters of air per twenty-four hours to replace that which has been vitiated by respiration, torches, and gunpowder; and this quantity, in the form of 14,320 cubic meters of air condensed to six atmospheres, the reservoir can furnish. A new and curious fact has been observed during these works, viz., that when the air, condensed to the degree above mentioned, is shot into the gallery from the machine, any water happening to be near the latter suddenly congeals, although the ambient temperature be about eighteen degrees, centigrade (seventy-two degrees Fahrenheit). Hence, when a large mass of compressed air is driven into a gallery situated at 1,600 meters below the outer surface of the earth, and where, consequently, the temperature must be about 100 degrees Fahrenheit, the dilation of the compressed air produces a diminution of temperature sufficient to counterbalance the excess alluded to. The progress now making per day in boring is three meters on each side of the mountain, or six meters per day in all.

ELASTIC BALL-VALVE PUMP.

This pump, patented in 1857, by Mr. A. Tower of New York, was devised for the purpose of being, at the same time, a lift and force-pump, and a fire-engine. It consists of two cylinders, standing vertically on a bed-plate, between which is an air reservoir, and on the top of which are cast two projections, used as bearing for the working-beam or brake. There is a hollow plunger in each cylinder, connected from its bottom with the brake, and moving through an ordinary stuffing-box, which is so placed that, by turning a few screws, it may be made tighter without stopping the pump. There is one valve-seat directly under each cylinder, and two in the air reservoir, all of which are raised a few inches above the flat surface around. Over each seat stands an India-rubber ball, about two inches in diameter, which is prevented from falling sideways, or rising too high, by being inclosed in a wire cage. Every solid particle which is carried through the valve falls by its own weight by the side of the elevated seat, and cannot, henceforward, choke the pump; and the few particles which may be arrested in their passage, when the ball comes suddenly down, get imbedded in the India-rubber which closes around them, and do not interfere with the action of the valve. This pump is capable of pumping a mixture of grain and water made in the ratio of three quarts of corn to each gallon of water. The working beam is properly moulded for the insertion of levers at each end. When it is advisable to work the pump by steam power, one of the levers is taken away, and a connecting-rod, with one end turned at right angles and shaped like that of the lever, is substituted for it. The pump, mounted on four wheels, and provided with proper hose, is turned into a small fire-engine.

ON THE PROTECTION OF WOOD FROM FIRE.

The attention of practical men has been for some years past directed, from time to time, to the importance of affording to wooden erections some degree of protection from the effects of fire; and numerous plans have been proposed, and to some extent tested, for lessening the combustibility of wood, and for covering its surface with a protective coating more or less unalterable by fire.

The simple application of lime or clay wash, for example, has been found to afford some slight protection to wood, although the tendency of such materials to peel off the surface of the wood (into which they do not in any way penetrate), by exposure to heat, and the rapidity with which the coating is destroyed by atmospheric influence, render them very ineffective agents.

The successful results obtained by the application of alkaline silicates, as protective materials, has recently induced the English War Department to institute an examination, with a view of testing the comparative value of the cheapest of these, the soluble silicate of soda, as an agent for decreasing the combustibility of wood.

The property possessed by the soluble alkaline silicates, of being readily softened by hot water, and thus converted into a state of solution, while they are but slightly affected by cold water, renders their application to wood, either in the form of a bath, or as a wash, very simple. Their dilute solutions being readily absorbed by wood, the surfaces of the latter, as it dries, assume the form of a hard coating.

From the official report, we derive the following extracts, descriptive of the results arrived at, by employing silicate of soda.

"Various specimens of dry wood were prepared with silicate of soda, by being soaked for a few hours in a weak solution. Upon examining the interior of these, after removal from the bath, and subsequent desiccation, the silica was found to have penetrated about a quarter of an inch on all sides. On piling the above over a fire, together with specimens of unprepared wood, and others that had been prepared by different processes, the superiority of the silicate of soda, as a protective agent, was fully demonstrated. Some specimens were then simply painted with a moderately strong solution of silicate; and afterwards placed, together with unprepared wood, in a pool of coal-tar naphtha, and the naphtha ignited. The result was, that while the unprepared wood was speedily consumed, the wood coated with silicate was only scorched, but not burned."

Shortly after the experiments above described were made, the possibility suggested itself of rendering the coating of silicate less destructible by exposure to wet, of increasing its efficiency as a protective, and of rendering its application more economical by combining with its use that of ordinary lime wash.

Some pieces of plank were prepared in the following manner: a dilute solution of the silicate of soda was first applied with a brush; when this had thoroughly soaked into the wood and dried, a thick lime wash (made by slaking some lime, and reducing the hydrate to a smooth wash of the consistence of thick cream) was applied; and, lastly, after the planks had been exposed to the air for two or three hours, they were painted with a second solution of silicate of soda, somewhat stronger than that first used.

The results of trials, similar to those above recorded, proved most satisfactorily that the protective coating resisted to a remarkable degree the action of heat, evinced no symptom of peeling off the highly heated surface of the wood, and protected the fibre to a great degree from the influence of flame playing upon its surface. The durability of the coating was tested by exposing prepared surfaces of wood to a continuous stream of water and to heavy rains for a considerable period. It was found that the rain had no effect upon the coating; in the other more severe test, the material was only to some extent removed, after a time, on that spot where the jet of water first impinged upon the wood. A trial was made of the firmness of the coating, by applying heavy blows to the surface of the wood. The covering was only disturbed in one or two places, where the lime had been laid on rather too thickly.

The above report was accompanied by a communication relating to the cost of the application of the silicate coating, in which it was stated that, provided the silicate of soda employed has been prepared with especial reference to this application (that is, so as to be readily and completely mixable with water), one pound of the material is sufficient to prepare a surface of wood of ten square feet; while the wholesale price of the silicate, in the form of a syrup, of a certain degree of concentration, is £20 per ton; so that the cost of the silicate required to prepare the wood is at the rate of about two pence for a surface of about ten square feet.

The following are the directions adopted for general guidance in preparing wood with the coating of silicate of soda and lime.

The silicate of soda must be in the form of a thick syrup, and the lime wash should be made by slacking some good fat lime, rubbing it down with water until perfectly smooth, and then diluting it to the consistency of thick cream.

The protective coating is produced by painting the wood firstly with a dilute solution of silicate of soda; secondly, with the lime wash; and lastly, with a somewhat stronger solution of the silicate. The surface of the wood should be moderately smooth; and any covering of paper, paint, or other material, should be first removed entirely, by planing or scraping. A solution of the silicate, in the proportion of one part by measure of the syrup to three parts of water, is prepared in a tub, pail, or earthen vessel, by simply stirring the measured proportion of the silicate with the water, until complete mixture is effected. The wood is then washed over with this liquid, by means of an ordinary white-wash brush, the latter being passed two or three times over the surface, so that the wood may absorb as much of the solution as possible. When this first coating is nearly dry, the wood is painted with the lime wash in the usual manner. A solution of the silicate, in the proportion of two parts by measure of the syrup to three parts of water, is then made; and a sufficient time having been allowed to elapse for the wood to become moderately dry, this liquid is applied upon the lime in the manner directed for the first coating. The preparation of the wood is then complete. If the lime coating has been applied rather too thickly, the surface of the wood may be found, when quite dry, after the third coating, to give off a little lime when rubbed with the hand. In that case it should be once more coated over with a solution of the silicate, of the strength prescribed for the second liquid.

THE AMERICAN MANUFACTURE OF WATCH MOVEMENTS.

A correspondent of the New York *Times* furnishes the following interesting description of the works of the American Watch Company, at Waltham, Mass., in which, for the first time, the adaptation of machinery to the construction of uniformly perfect watch movements, in their minutest and most delicate details, has been successfully attempted.

The better to understand the magnitude of the interests affected by this enterprise, it should be remembered that the value of watches and watch movements imported into the United States is about $5,000,000 per annum, exclusive of many more which evade the Custom-house. It is estimated that an equal sum is expended in this country alone in repairing defective watches, and in vain attempts to coax them to run regularly. European watches are made chiefly by hand. The rough parts of the movement are collected usually from several distinct work-shops, all meeting at last upon the bench of the finisher, perhaps in a distant city or some foreign country, where the mechanism is fitted by measurement, and put in motion. Necessarily very much must depend upon the accuracy of eye and steadiness of hand of the workman; for the slightest deviation in size, length, or form of any part of the intricate mechanism, must impair its value, if not render it utterly useless. The variation of the ten thousandth part of an inch in the size of a socket, or the measurements to determine its proper position, may make all the difference between a perfect time-keeper and one which shall be little else than a source of vexation and expense.

In "jewelling," especially, is the highest accuracy of human workmanship required. Jewelling, in watch-making, is the setting of precious stones — usually rubies, sapphires or chrysolites — in positions subjected to friction, in order to avoid the least change of form or size by long wear. Thus holes to receive metal pinions must be made in substances inferior only to the diamond in hardness; and in planing, turning, and drilling the jewels, microscopic exactness is indispensable; for the pinions must move in their holes with perfect ease, and yet without spare room to admit the minutest division of a hair. The mode of piercing jewels, discovered in the year 1700 by M. Facio, of Geneva, was for a long time a distinct art of itself. With this understanding of the delicate mechanical conditions requisite to a true time-keeper, the reader will no longer wonder that there are so few perfect watches, or that none but the best works of the best European finishers heretofore, have approached perfection, nor that there is such absence of correspondence in the practical working of watch movements, which to the eye appear to be precisely alike.

By the employment of ingenious machinery for the construction of each and every part of the movement, the American Watch Company have overcome all the difficulties inseparable from the manufacture by hand. Each of these machines has its peculiar office to perform, doing its special work to a gauge or pattern, with an exactness which handicraft cannot equal. By this means each watch movement, in every part — with the exception of the jewelled holes and the pivots that run in them — is exactly like every other of the same size and style. The holes are drilled in the jewels with a diamond, and then opened with diamond dust on a soft, hair-like wire. The steel pivots designed to run in these jewels, must be exquisitely polished,— by which operation their size is reduced almost inappreciably. After being thus finished, the jewels and pivots are classified by means of a gauge graduated so as to mark the difference of a ten-thousandth part of an inch. The size of the pivots and jewelled holes are recorded at the factory, with the number of each watch; so that if any one of either should ever fail, in any part of the world, its exact duplicate may be obtained at trifling cost by sending the number of the watch to Waltham. All the other parts are made faithfully alike,— any given piece fitting one watch exactly as it does in every other. Nothing is left to the eye or the touch of the workman, for the machinery impresses its own unerring precision upon the whole.

The mechanical principle of true time-keeping is the division of a constant force in a given time, by means of perfectly adjusted mechanism, so arranged as to change the rotary motion of the wheels into the vibratory motion of the balance or pendulum; the only mechanical difference between a clock and a watch being, that the one is so arranged that it will move in *one* position only, while the other will move in *any*,— the pendulum setting the clock in motion, and the balance performing the same service for the watch. The escapement is that part of the time-piece which converts the rotary into vibratory motion, as above, and is made by one tooth of the fastest running wheel in the train escaping at each vibration, which wheel is known as the "scape-wheel." The detached lever escapement, now used in the best English watches, is the one adopted in the Waltham factory, with some valuable modifications in the general construction of the movements. The escapement varies indefinitely in movements of European manufacture, each being fitted by hand to its particular watch, and useless in every other; while in the American factory any one of a thousand escapements will accurately fulfil its office wherever placed. All the ingenious tools by which these remarkable results are obtained, were invented in this establishment, and constructed within its walls.

Having now a general idea of the skill and perfection requisite to successful watch-making, and of the chief points of novelty in the Waltham establishment, let us proceed to a methodical examination of its varied operations.

The factory building is of brick, two stories in height, surrounding a quadrangle court, and covering about half an acre of ground. It accommodates nearly a hundred and fifty operatives, many of them women. We enter, first, the room devoted to the heavier and more massive machinery. Here we find stamps and dies, over a hundred in number, with which the various pieces of the watch are first rudely stamped out of sheets of brass, just as they come from the Connecticut rolling mills. We follow these rough pieces (or "blanks," as they are technically named) to another room, where they are reduced to proper size and form.

Let us follow, for example, the fortunes of this barrel blank,— a simple wheel, about three-quarters of an inch in diameter, stamped out from a sheet of brass three-sixteenths of an inch in thickness, and then hardened by hammering. The blank is placed upon a lathe, on which it revolves with great velocity, and is brought in contact with a series of tools all fastened immovably upon a frame. These speedily reduce it to the required size and form, turn out the chamber to accommodate the main-spring, drill a hole in the centre to receive the barrel arbor to which the main-spring is fastened and around which it is coiled, and turn a flange on the outer edge, on the periphery of which are to be cut the teeth to gear into the "train" of wheels and set it in motion. All this is accomplished in less time than I take to describe it,— the machine setting itself, and invariably executing its work with unfailing precision. A hole is also drilled in one side of the barrel, in which to fasten the opposite end of the spring.

The barrel is now taken from the lathe and placed upon another machine to have its teeth cut. This operation is performed by a minute chisel revolving upon a cylinder at high speed,— the machine automatically moving the piece in a circle so as successfully to present every part of its edge to the cutter, until the entire number of sixty teeth are formed. The teeth of the wheels are cut upon the same principle, although of the smaller sizes from forty to sixty wheels are placed upon the machine at once, and operated upon by the same motion. In another part of this room all the pivots of steel and the shoulders of pinions are cut to their proper size by machinery. Pinions are the axles of wheels, and pivots the bearing ends on which they run. Some of these are very small, and parts of them so slender that they can hardly be manipulated without injury from accidental pressure; nevertheless, machinery strong enough to gouge out metal chips without apparent effort, grasps these diminutive pieces with equal delicacy and firmness, releasing them at last in perfect form and safety. The teeth of these wheels, located at and near the centre of the pinions, are drawn out from the wire by one machine, and are then passed under another which opens them to the precise angle required, and polishes away all superfluous metal, reducing them instantaneously, almost to the exact proportion desired.

The escape-wheels are cut by a machine somewhat similar to that performing the *dental* operation already described. In no part of the mechanism is the perfection of the machinery put to a severer test than here, for the escape must be shaped by it with absolute perfection, as no tool can be applied thereto subsequently. Sixteen of the blanks are put upon the lathe at the same time, and then by three distinct motions — all accomplished in less than one minute — the eccentrically-shaped teeth are cut, and the wheels come out the most exquisitely-delicate little affairs imaginable. Another machine rapidly turns out pillars of brass — each with several minute flanges — used in fastening the large plates of the watch to each other. Others make the various screws required for putting the movement together. Some of these are so minute, that *ninety-five thousand of them weigh only a single pound!* The steel wire of which this number is made costs originally only a dollar,— worth, when thus manufactured, *nine hundred and fifty dollars; the labor thereupon multiplying the value of the raw material nine hundred and fifty times!* A piece of the wire being placed in the machine, is seized firmly and carried forward to a position where a tool meets and points it. This done, another tool advances, cuts it down to a required size,— at the same time forming the shoulder,— and retires. A third tool promptly follows up the work by cutting the thread upon the screw, and a fourth nearly severs it from the wire. The operative now picks up from his bench a sort of rack of steel, perforated with holes, in which the screw fits easily, and, inserting the screw-point into one of these, breaks it off above the head, repeating the operation until the rack is full. The rack now presents to view a long straight row of screw-heads still needing the groove into which the screw-driver is to be inserted when forcing them home. This is promptly supplied by passing the rack horizontally under another instrument which cuts the groove, and, when "case screws" are wanted, saws off a segment of the head at the same time.

A little further on we find machinery for turning the brass plates both large and small. Each piece is perfected in an instant. By similar means the twenty or thirty holes of different sizes are drilled in the larger plates all at once. As the tools are held in their allotted places by iron "muscles," they cannot fail to do this work just where it is wanted,—nor can there be the slightest deviation from the perpendicular in the sides or walls of the holes.

The various parts of the mechanism, leaving the rooms in which they received their shape and size, are carried to the finishing-room. Here each piece is carefully examined with a glass, to see if it is perfect, before it is passed to be hardened, cleaned, and polished. The polishing is all effected by machinery. In the finishing-room, the jewels, also, are set in the pallet which works in the escapement. This pallet is a sort of miniature beam, swinging on a pivot in the centre, and having an angular hook at each end, which works upon the tooth of the escape-wheel. The points of these hooks are opened by machinery, and jewels are inserted in the slits, and then worked down even with the face of the metal. In polishing these pallets, they are placed upon an index to which they are set, so that the angles may be brought down to the microscopic exactitude of shape preëminently indispensable in this part of the mechanism. All the brass pieces, when found perfect, are gilded by electro-metallurgy.

It only remains to describe the dial-rooms, and the system of "putting up" the movements. The dial-rooms are devoted to the production of enamelled dials of all colors. These are made from a species of porcelain manufactured in London, and imported in plates resembling fine China ware. At Waltham, the porcelain is reduced to a paste, and then fused upon thin copper plates — the basis of all dials — at nearly white heat. When cooled, the dial is ground off smoothly, and then subjected again to the furnace, to perfect its surface and give it a smooth glaze. The dial being now ready for painting, the spaces upon its edge are marked off by an index, the figures are put on roughly with a coarse brush, and, after the ink is dried by slow heat, the superfluous edges are removed with a little wooden point. The minute and second points, and the diminutive letters forming the name of the manufacturing firm, are all put on with a fine hair pencil, the artist's eye being assisted by the glass. The hands of the watch are stamped out from thin sheets of steel.

The various pieces of the watch movement, when entirely finished, are collected in sets, and carried to the "putting-up" room, where from thirty-five to forty hands are engaged in putting them together, adjusting and preparing them for market, subjecting each to the most thorough tests, and regulating it perfectly as is possible before its adjustment in the pocket of the wearer. Here, as everywhere else in this establishment, the labor is divided and sub-divided, so as to secure the greatest economy and highest skill. The whole number of pieces in an old-fashioned English lever is between eight and nine hundred, including the chain. In the American movement there are only about a hundred and twenty distinct parts, *each of which passes through the hands of from five to seventy-five operatives*, in the process of manufacture and adjustment!

No one who examines the operations of this Company can doubt that a revolution impends in the watch-manufacturing of the world; for, by the American machinery, watch movements without cases are already produced at just about one-half the cost of imported movements of similar grade, while the former necessarily have the advantage of uniform reliability. A poor time keeper, of machine make, should and doubtless *will* be as rare in the future as a perfect one made by hand has been in the past; and the only difference to be made in the scale of prices must result from the style of finishing the cases and from the number of jewels; for it is as easy to arrange the machinery so as to secure perfect work as to turn out imperfect,—and once set, it cannot fail to serve every movement alike.

REMINISCENCES OF THE FIRST INTRODUCTION OF STEAM NAVIGATION.

The following paper, on the above subject, was recently read before the New York Historical Society, by Professor Renwick, of New York City.

1. The earliest attempt to navigate the ocean by steam was made, and made successfully, by Robert L. Stevens. The circumstances of the case were as follows: He, with his father, who had the misfortune to live half a century too soon, not only for fortune but for fame, had constructed a steamboat propelled by paddle-wheels, which was in motion on the Hudson only a few days later than Fulton's first successful voyage. Being prevented by the exclusive grant from the State of New York to Livingston and Fulton, from plying upon the Hudson, he conceived the bold idea of carrying the vessel, under steam, around Cape May to the Delaware. The vessel reached Philadelphia in safety, and was immediately employed in conveying passengers between that city and Trenton. This passage was made, as I infer from a comparison of other dates, in the spring of 1809. The steamship Savannah, built in New York, made a voyage from New York to Liverpool, and from Liverpool up the Baltic to St. Petersburg, in the year 1818. The voyage from New York to Liverpool was performed partly by sails and partly by steam, and occupied twenty-six days. The same vessel returned *via* Arendal in Norway, and was twenty-five days in making the voyage home from the latter port. This enterprise, however, was, so to speak, no more than a continuation of one of much earlier date and better promise. A steamer of stronger scantling, larger size, and, I believe, more powerful engine, than the Savannah, had been built by a company headed by Cadwallader D. Colden. It was generally understood that this enterprise was undertaken in virtue of a contract with Russia. To this vessel, when launched, the name of the "Emperor Alexander" was given. When nearly ready for sea, her departure was prevented by the declaration of war in June, 1812. Under the name of the "Connecticut," this vessel was long known upon the Sound.

England was, however, before us in forming lines of steamers to navigate stormy seas. The earliest of these was established by the aid of the Government for the transportation of the Irish mail between Holyhead and Dublin, in 1819 or 1820.

2. The first time that I ever heard of an attempt to use steam for propelling vessels was from a classmate of mine, who resided during the summer months at Belleville in New Jersey. He had, in the summer of 1803, seen an experiment on the Passaic River, which he stated to have been directed by John Stevens of Hoboken. According to his account, the propulsion was attempted by forcing water by means of a pump from an aperture in the stern of the vessel. From some vague indications, it would appear that the elder Brunel, afterwards so distinguished in Europe, was in the employment of Mr. Stevens on this occasion. In the month of May, 1804, in company with the same gentleman, I went to walk in the Battery. As we entered

the gate from Broadway, we saw what we in those days considered a crowd, running towards the river. On inquiring the cause, we were informed that "Jack Stevens was going over to Hoboken in a queer sort of a boat." On reaching the bulkhead by which the Battery was then bounded, we saw lying against it a vessel about the size of a Whitehall row-boat, in which was a small engine, but there was no visible means of propulsion. The vessel was speedily under way, my late much-valued friend, Commodore Stevens, acting as cockswain; and I presume that the smutty-looking personage who fulfilled the duties of engineer, fireman, and crew, was his more practical brother, Robert L. Stevens.

A few years since, at the last fair of the American Institute, held at Niblo's, I was asked to serve on a committee to report upon a boat and engine exhibited by the Messrs. Stevens, for the purpose of sustaining the claim of their father to the honor of being the first inventor of the propeller. The circumstances I have just recounted had taken so strong a hold on my memory, that I at once recognized the engine exhibited as that which I had seen at the Battery nearly fifty years before.

In respect to the propeller I could say nothing. One of my colleagues on the committee, however, Mr. Curtis, at that time United States Inspector of steamboats for the port of New York, recognized, as distinctly as I had done the engine, the propeller, which he had seen in the hands of workmen by whom it was manufactured. The dates corresponded, the apparatus was avowedly making for Stevens of Hoboken. Thus it happened that an accidental choice had placed upon the committee two persons who were, by the union of their testimony, capable of establishing the fact into the truth of which they were directed to inquire.

In the spring of the year 1807, I had the pleasure to hear from David Gordon, at that time a merchant in this city, afterwards much distinguished in England as a civil engineer, an account of Fulton's trial-trip, and to learn from him that there was every reasonable hope of his success.

In the summer of the same year, while about to sit down to dinner at Gregory's Hotel in Albany, in company with my predecessor in Columbia College, Dr. Kemp, Mr. Selah Strong entered the room, stating that he had just arrived from New York in Fulton's steamboat, after a passage of about thirty-six hours. He went on to say, that, being anxious to reach Albany to transact some business of importance, he had solicited permission to make the voyage in the steamer, which was, after some hesitation, granted. Five other persons followed him, occupying with him the six spare berths which happened to be on board. Mr. Strong, then, was the first passenger who ever paid his fare in a steamer, and his urgency had probably a great influence on the fortunes of the invention; for, up to that time, Fulton's own views were chiefly devoted to the Mississippi and its branches. An opening for a successful traffic seemed to exist on the Hudson; and from that date to the close of navigation the original boat continued to run occasionally, and to convey passengers.

You may readily believe that I did not fail to visit the vessel; and that I could not avoid hearing the imprecations, not loud but deep, with which the Albany skippers saluted what they thought would be the ruin of their occupation. Even the more quiet burghers could not refrain from lamenting that in Fulton's success was involved the ruin of their trade, and the transfer of their business to New York. The vessel was very unlike any of its successors, and even very dissimilar from the shape in which it appeared a few months afterwards. With a model resembling that of a skiff, it was decked for a short distance at stem and stern. The engine was open to view, and from the engine aft a house like that on a canal boat was raised to cover the boiler, and the apartments for the officers. In these, by the addition of a few berths, the passengers were accommodated. There were no wheel-guards. The rudder was of the shape used in sailing vessels, and moved by a tiller. The boiler was of the form then usual in Watt's engines, and was set in masonry. The condenser was of the size habitually used in land engines, and stood, as was and still is the practice in them, in a large cold-water cistern. The weight of the masonry, and the great capacity of the cold-water cistern, diminished most materially the buoyancy of the vessel.

At this point Fulton's ingenuity and fertility of invention were called into play. The experiment was to the eye of the world successful, yet was withal so imperfect as to be liable to continual accident and annoyance. The rudder had so little power that the vessel could hardly be managed, and could not be made to veer around even in the whole breadth of the Hudson at New York. The spray from the wheels dashed over the passengers, and the skippers of the river craft, taking advantage of the unwieldiness of the vessel, did not fail to run foul of her as often as they thought they had the law on their side. Thus, in several instances, the steamboat reached one or the other of the termini of its route with but a single wheel.

Before the season closed, the wheel was surrounded by a frame of strong beams, and the paddles were covered in; the rudder had taken the shape of a rectangle, of large iron horizontal dimensions, such as is now seen in all American river-boats; this rudder was worked by a wheel, the ropes from which were attached to the end most distant from the pintles. The vessel, by the last mentioned arrangement, became so manageable as to be capable of veering at Albany; and by the first was more likely to inflict than to receive injury in an encounter with a sailing vessel. I was even at that time of opinion— and a careful attention to the working of the patent laws has confirmed me in it — that, had Fulton been less sanguine in relation to his first patent, and had added to it by a new instrument the improvements which circumstances led him to make during the summer of 1807, but which he allowed to become public property, he might have maintained his exclusive privileges as patentee in all parts of the Union. To put a pair of paddle-wheels on the axle of the crank of one of Boulton and Watt's engines is a step almost too simple to admit of specification, and had been in some degree indicated by Watt himself; but the practical difficulties which lay in the way, and could not have been foreseen, required the application of remedies, all of which were original. Among them, unquestionably, was the substitution of a condenser, enlarged fourfold in its capacity, for the old condenser and the cold-water cistern, together with the use of standing pipes instead of the cold-water pump. These made their appearance the ensuing season.

During the winter of 1807-8, the "Clermont"—for by this name the vessel was now known — was almost wholly rebuilt. The hull was considerably lengthened, and covered from stem to stern with a flush deck. Beneath this, two cabins were formed, and surrounded by double ranges of berths, fitted up in a manner then unexampled for comfort. The vessel was then advertised to run at stated periods between New York and Albany, as a packet, the first time of departure being the first Wednesday (I think) of May. On that day I embarked in her; the officer in command was named Jenkins; and Fulton himself, accompanied by the lady whom he had recently married, was on board. The first marked incident was the leaving of several passengers who had ventured to trust to the want of punctuality then usual in the departure of vessels. The rule of starting at an exact hour was then enforced for the first time, and from that rule there was for the future no deviation. One or two of the dilatory parties jumped into a boat that was towing astern, the others were left behind.

Leaving Cortlandt-street at five o'clock, we were at the base of Butler Hill about daybreak the next morning. A delay of a couple of hours took place at Chancellor Livingston's seat, Clermont, and the whole passage was made in less than forty hours. Symptoms of difficulty were manifest, however, even on the upward passage. Mr. Fulton appeared anxious and abstracted. Finally, steam began to make its appearance in very minute jets through the joints of a wooden trunk, that was first considered by the passengers as the case of the boiler. It was at last found to be the boiler itself, and it was whispered that Fulton had been overruled by his associates, and that a cylinder of wooden staves, containing fire-place and flues of copper, had been substituted for the boiler of Watt, instead of replacing it by a new boiler of copper. This form of boiler had been proposed, but as far as I can learn, had never been used by Watt. On the return voyage the leaks in the boiler continued to increase; the speed of the vessel, although aided by a flood in the river, became less and less; and after fifty-seven hours of struggling, the engine ceased to work. We were then at the foot of Christopher street. The flood-tide made itself felt in opposition to our progress, and the passengers considered it better to make a landing, and find their way on foot to the peopled parts of the city.

It took some weeks to obtain a new boiler, after the expiration of which the Clermont resumed her proposed trips.

In the month of September, 1809, I was a partaker in the exciting scene, then first enacted, of a steamboat race. A company at Albany had been formed for the purpose of competing with Fulton. The first vessel of this rival line was advertised to leave Albany at the same time with Fulton's. Parties ran high in the hotels at Albany. The partisans of Fulton were enrolled under Professor Kemp of Columbia College, those of the opposition under Jacob Stout. The victory was long in suspense; and it was not until after the thirtieth hour of a hard struggle that the result was proclaimed by Dr. Kemp, standing on the taffrail of Fulton's vessel, and holding out, in derision, a coil of rope to Captain Stout, for the purpose, as he informed him, of towing him into port. When the age, high standing, and sedate character of these two gentlemen are considered, it did not surprise me, who witnessed their excitement, when I afterwards heard of Western women having devoted their bacon to feed the fires of a steamboat furnace.

Although I became intimately acquainted with Fulton about the year 1810, I have nothing of interest to mention to you, except that this intimacy procured me the privilege of accompanying him on the trial-trips of two of his vessels — I think the Paragon and the Fulton. The latter was intended for the navigation of the Sound, but was prevented from plying on that route by the presence of British cruisers. On one of these occasions we had the opportunity of seeing the respect in which Fulton's genius was held by enemies of the country. On issuing from the Narrows, we saw, close in with the Point of Sandy Hook, a large English vessel, the Razee Saturn, by which the port was then blockaded. Our direct course for the anchorage at the Hook, whither we were bound, lay across the east bank, and we thus had the appearance of bearing down on the cruiser. As soon as we were fairly in sight, and as our smoke could well be seen by the Saturn, that vessel was put about, a press of sail was spread, and every effort was evidently made on board to obtain an offing, by standing away close hauled with a strong wind from S.W. After we got quietly anchored under the Hook, the Saturn resumed her station just outside of the bar.

Although it has been said, on English authority, that Sir Thomas Hardy, while occupying the Sound with a powerful squadron, and carrying his flag in a seventy-four, never remained at anchor during the night, and rarely left the deck except by day, in order to insure safety from Fulton's torpedoes, a more certain if not more terrific mode of attack was, at the date of which I speak, afloat, and nearly ready for service in the waters of New York. This was the steam *Battery*, miscalled *Frigate*, Fulton. This vessel, formidable enough in reality, had been represented by correspondents of English newspapers as a monster of prodigious powers. An hundred guns of enormous calibre were said to be enclosed in fire and bomb-proof shelters; the upper deck was reported to be defended by thousands of boarding-pikes and cutlasses wielded by steam, while showers of boiling water were ready to be poured over those that might escape death from the rapidly whirling steel. In reality, the vessel presented above the surface of the water the figure of an oval, whose greatest length was about the same as that of an English seventy-four. This was covered by a continuous spar-deck, at either extremity of which was mounted, on a revolving carriage, a chambered gun, capable of throwing a solid ball of 100 pounds, but intended, as is well known, to throw shells. Beneath the spar-deck was the gun-deck, also continuous, except in the middle, where space was left for the working of a large paddle-wheel; and on this gun-deck was mounted a battery of thirty-two 32-pounders. The sides of the vessel were thickened by cork and wood, not only between the guns, but as low as the water's edge, until incapable of being penetrated by a 32-pound ball. Beneath the gun-deck the hull was formed as if of a vessel cut in two, leaving a passage from stem to stern for water to reach and to be thrown backwards from the wheel. Two rudders were placed in this passage, moving on their centres. The boilers and the greater part of the machinery were below the reach of shot, and even the wheels could only be reached by a stray shot, passing unimpeded and in a proper direction through the port-holes, until the sides of the vessel had been destroyed by a long-continued battering. The central wheel, and the peculiar rudders, had already been successfully used by Fulton in a ferry-boat. This seems to have been placed on the Brooklyn ferry about the year 1811.

My scene must now be changed to the opposite side of the Atlantic. The war with England being at an end, I took an early opportunity to visit Europe, and reached England in the month of May, 1815. In July of the same year, in company with some other Americans, I made a pedestrian excursion to the neighborhood of Runcorn, in Cheshire. On our return through the beautiful grounds of what once was a park belonging to Lord Sefton, was then laid out in sites for villas, and has since been included in the town of Liverpool, we saw beneath us in the Mersey an object which puzzled our English friend, but which the rest of the party knew to be a steamboat. On reaching Liverpool, we learned that Bell, who had been put forward by a Committee of Parliament, as the rival, indeed as the instructor, of Fulton, had brought his vessel, the Comet, round from Greenock. It seems that he had been driven from the Clyde by the competition of larger and more perfect vessels. In passing between the two towns he had made the first English voyage on the ocean by steam. This date, you will perceive, is six years later than the similar voyage of Robert L. Stevens from New York to Philadelphia. The length of the Comet's keel was no more than forty feet, her engine was of but three-horse power.

On the 21st of March, 1816, I left Southampton for Havre, in a cutter packet of about forty tons. The following night was very stormy, and our captain thought it prudent to return for some hours to Cowes, until the violence of the gale had abated. On entering the basin at Havre, we were moored alongside of a steam vessel of about the same size and similar model, which had, during the gale we had feared to encounter, crossed the channel from Brighton. This vessel ascended the Seine to Rouen, and, if I am not mistaken, to Paris. I do not recollect her name, nor am I aware of her fate; but she was unquestionably the first steam vessel specially built in Great Britain for sea navigation.

From this date onwards, the attempts at the navigation of the narrow seas which surround England were frequent and partially successful. Private enterprise and patronage were, however, insufficient to insure any important results, and these were not attained until the Government, in 1820, stepped in and established a line of mail steamers between Holyhead and Dublin. The sound principle of aiding individual exertion by government funds and government patronage was first exhibited in this line, and the method has been copied in other English lines, and in the *messageries* of France.

The navigation of the narrow seas by steam, as practised by England, afforded but little hope of success in the navigation of great distances upon the ocean. So small was the expectation of its practicability, that a celebrated, if not a distinguished writer and lecturer of that country concluded, that the result of English experience authorized him to prophesy that no vessels could be built that could carry coal enough to make the passage under steam from Europe to America. Yet at the time of this prophecy the problem had been solved years before by American hands in 1820. With funds chiefly furnished by David Dunham, under the inspection and partly at the cost of Jaspar Lynch, with engines planned by Fulton himself and a hull moulded by Eckford, a steamer was built in New York to run, *via* Havana, from New York to New Orleans. This vessel attained what Fulton, from an imperfect theory, had concluded to be the maximum speed of steamboats — nine nautical miles per hour. And this speed was not exceeded by steamers specially built for sea service before the brilliant opening of the Collins line. The vessel of which I speak had sufficient capacity for the stowage of fuel for each passage; sustained, under skilful management, hurricanes of the utmost violence, and had room for many passengers. No experiment could possibly have been more successful. But the enterprise was a failure, because the cost of maintaining it was not defrayed by the number of passengers who presented themselves. The enterprising Lynch was ruined; the vast fortune of Dunham materially diminished; the vessel, stripped of her machinery, was sold for a cruiser to a South American government, in whose service her speed and sea-worthy qualities well sustained the reputation of Eckford. Thus a triumph well deserved by New York remained to be earned, after an interval of many years, by Bristol, in the repeated voyages of the Great Western.

NEW ALLOY FOR SHEATHING SHIPS.

A patent has recently been taken out in England by Mr. Arthur Wall, of London, for a combination of metals possessing different electric characters for the sheathing of ships. The alloy is made by melting two and a half parts of copper in one crucible; in another nine parts of zinc, eighty-seven of lead, one part of mercury, and a half a part of bismuth; then mixing the contents of both crucibles, covering the surface with charcoal dust, and stirring well until all are incorporated. It is stated that the mercury in this alloy protects both the zinc and copper from the action of sea-water. The contents of the crucible are run into ingots, and rolled into sheets.

The same inventor has also obtained a patent for protecting the bottoms of iron ships from the action of sea-water, by the use of a composition of litharge, made into a smooth, thin paste with turpentine, to which is added an equal weight of resin. The whole is then put into a close iron vessel, placed over a fire, naphtha added through an aperture in the lid from time to time, and the boiling kept up slowly for about two days, until the whole has assumed a tenacious, adhesive character, and a creamy consistency. It is then fit to be applied to the iron of the vessel as a primary coating. A second coating is given to the iron with a composition of resin, combined with one-fifth of its weight of an oxyd of mercury and powdered charcoal mixed in turpentine. This outer coating fills up all cracks or gaps left in the first application, and the nature of the composition is stated to be such that it prevents barnacles adhering to the iron, and resists the corroding action of salt water.

ON THE STRENGTH OF SOME ALLOYS OF NICKEL AND IRON.

At a recent meeting of the Manchester (Eng.) Society of Engineers, Mr. Fairbairn, presented the result of some experiments made to ascertain whether an infusion of nickel, in a given proportion, would increase the tenacity of cast-iron, as originally imagined from the analysis of meteoric iron, which generally contains 2 1-2 per cent. of nickel. Contrary to expectation, however, the cast-iron, when mixed with the precise quantity of nickel indicated by the analysis of meteoric iron, lost considerably in point of strength, instead of gaining by it. Mr. Fairbairn also stated in the course of his paper, that during the last ten years, innumerable tests and experiments had been made for the purpose of obtaining a metal of extraordinary tenacity for the casting of mortars and heavy ordnance; but the ultimate result appeared to be, in the opinion of the author and others, that for the casting, or rather the construction, of heavy artillery, there is no metal so well calculated to resist the action of gunpowder as a perfectly homogeneous mass of *the best and purest cast-iron* when freed from sulphur and phosphorus.

In the discussion which followed the reading of the paper, Mr. Calvert said that it was highly probable that nickel caused the increased brittleness of cast-iron, just as carbon, phosphorus, and sulphur, but that the result with malleable iron might probably be very different; and, as meteoric iron is malleable, the trial could only be complete when soft iron and nickel were united; nevertheless, these experiments, as far as cast-iron is concerned, were decidedly new and of great value.

RUSSIA SHEET-IRON.

It is a popular notion that the process of manufacturing the tenacious and glossy "*Russia sheet-iron*" is a profound secret, and that the vigilance exercised by the Russian Government, and the Russian manufacturers, have hitherto successfully prevented all foreigners from obtaining the slightest information on the subject. The present Commissioner of Patents, in his last report, also alludes to the manufacture of this article, as one of the great, unsolved problems in science, which the industrial interests of the country require should be explained.

Mr. Wells, in his recent work, "Principles and Applications of Chemistry," states that this current belief has no foundation in fact, and that the method of preparing the iron in question is perfectly well known. According to the authority quoted, "Russia sheet-iron is, in the first instance, a very pure article, rendered exceedingly tough and flexible by refining and annealing. Its bright, glossy surface is partially a silicate, and partially an oxyde of iron, and is produced by passing the hot sheet, moistened with a solution of wood-ashes, through polished steel rollers."

Another mythical bubble is thus punctured, and the wonderful story of guarded founderies and ever-watchful officials, as connected with Russia sheet-iron, will take rank with the account of "Symmes's Hole," and the barnacles which turn to Solan geese. — *Scientific American.*

COMPOSITE IRON RAILING.

The process by which this light, elegant, and cheap fabric is manufactured is as follows: — Rods and bars of wrought-iron are cut to the lengths required for the pattern, and subjected to a process called crimping, by which they are bent to the desired shape. These rods are then laid in the form of the design, and cast-iron moulds are affixed at those points where a connection is desired; the moulds are then filled with melted metal and immediately you have a complete railing of beautiful design. The entire process is so systematized, that what was once the work of days is effected in an hour. Casting in iron moulds has this great advantage over the old sand moulding, it does not require any time for cooling, as the metal is no sooner run than the moulds may be removed, and used again immediately upon another section of the work; and besides, it is so much more readily effected. By the combination of wrought and cast-iron in this process, the most curious and complex designs may be produced almost at will. This simplicity of production is attended with a corresponding diminution of cost.

METALLIC ALLOY FOR THE FORMATION OF MEDALS, SMALL FIGURES, ETC. BY M. VON BIBRA.

Six parts of bismuth, three parts of tin, and thirteen parts of lead, are fused together first of all, in a crucible or iron ladle: the mixture is poured out and fused again, if it is to be employed in casting. It is almost as readily fusible as the well-known Rose's metal; but, besides possessing considerable hardness, it has the particular advantage of not being brittle, because it possesses no crystalline structure upon the fracture. If the cast objects be bitten with dilute nitric acid, washed with water, and rubbed with a woollen rag, the elevated spots become bright, whilst the sunken portions are dull, and the casting acquires a dark, gray appearance, with an antique lustre. Without biting, the color is light-gray. Some casts of medals taken with this alloy in plaster of Paris were so successful, that the finest contours and the legend, which in the original was only legible with the lens, were completely reproduced. Calculated for 100 parts, this alloy consists of 27·27 bismuth, 59·09 lead, and 13·64 tin. As bismuth is expensive in comparison both with lead and tin, the quantity of lead might be increased, and that of the bismuth diminished, without injury to the valuable properties of the alloy. It is probable this mixture may be adapted for typographical purposes.—*Polytechn. Centralbl.*, 1857, p. 888.

NEW MATERIAL FOR MOULDS, ETC.

It is proposed to introduce a vast improvement in the casting of metals, by substituting compressed carbon for the sand or clay usually employed. The advantage to be derived is, that the same mould may be used over and over again without injuring the smooth surface of the cast material. The carbon to be employed, which is manufactured under a patent recently granted to Mr. Buhring, of England, is comparatively pure, and can be moulded into any shape and form required. The same material has been sucessfully applied to the manufacture of crucibles, and these crucibles are by many considered superior to any others. Another purpose to which the compressed carbon is applicable is the manufacture of battery plates; and it is anticipated that electric telegraph companies would effect a vast saving in the cost of their batteries, by employing carbon in connection with iron, instead of the zinc and copper plates now used.

ON THE SHAPE OF BRICKS.

Mr. George Gilbert Scott, an eminent English architect, in a recently published work, makes the following observations on the form or shape of bricks. He says: — "The *shape* of a brick has a great influence on the effect in work. Our bricks are too short for their thickness — they should either be thinner or longer. I should say thinner for small buildings, and longer for large ones. If, for instance, we had for large buildings facing-bricks of the usual thickness, but nearly a foot long, they would look well, and would work in with a backing of common bricks, if necessary; but for small buildings, bricks of the usual length and breadth, but only two and a half inches in thickness, would look best. In the north of Germany, bricks were used in the middle ages, for large buildings, of much greater size than we now use them; this would have been good had the thickness been kept moderate; but that being increased in proportion, the bricks were often insufficiently burnt; and, except in buildings of gigantic size, they looked clumsy. The Roman brick, which was twice the length of ours, and little more than half its thickness, was in the other extreme — but it is the better side to err on. Their length ensures good bonding, while their thinness causes them to be thoroughly burnt."

DUTY OF STEAM ENGINES.

The following interesting practical examples of the difference between the actual and theoretic duty in different descriptions of steam-engine , is extracted from the published researches of Prof. Thompson, of England, on the dynamic theory of steam:

"1. The engine of the Fowey Consols mine was reported, in 1845, to have given 125,089,000 foot-pounds of effect for the consumption of one bushel, or ninety-four pounds, of coal. Now, the average amount evaporated from Cornish boilers, by one pound of coal, is eight and a half pounds of steam; and hence, for each pound of steam evaporated, 156,556 foot-pounds of pressure are produced.

"The pressure of the saturated steam in the boiler may be taken as three and a half atmospheres, and consequently the temperature of water will be 150°. Now, by Regnault (end of Memoire x), the latent heat of a pound of saturated steam, at 140° Cent., is 508, and since, to compensate for each pound of steam removed from the boiler in the working of an engine, a pound of water at the temperature of the condenser (which may be estimated at 30°) is introduced from the hot well, it follows that 618 units of heat Cent. are introduced to the boiler for each pound of water evaporated. But the work produced for each pound of water evaporated was found above to be 156,556 foot-pounds; hence, $\frac{156{,}556}{618}$, or 25·3 foot-pounds, is the amount of work produced for each unit of heat transmitted through the Fowey Consols engine.

"2. The best duty on record, as performed by an engine at work (not for merely experimental purposes), is that of Taylor's engine, at the United Mines, which, in 1840, worked regularly for several months at the rate of 98,000,000 foot-pounds for each bushel of coal burned; this is $\frac{98}{125}$ or ·784 of the experimental duty reported in the Fowey Consols engine.

"Hence, the best useful work on record is at the rate of 198·3 foot-pounds for each unit of heat transmitted, and is $\frac{198\cdot3}{440}$, or forty-five per cent. of the theoretical duty, on the supposition that the boiler is at 140° and the condenser at 30°.

"3. French engineers contract (in Lille, in 1847, for example,) to make engines for mill power which will produce 30,000 metre-pounds, or 98,427 foot-pounds, of work for each pound of steam used. If we divide this by 618, we find 159 foot-pounds for the work produced by each unit of heat. This is 36·1 per cent. of 440, the theoretical duty.

"4. English engineers have contracted to make engines and boilers which will require only three and a half pounds of the best coal per horse power per hour. Hence, in such engines, each pound of coal ought to produce 565,700 foot-pounds of work; and if seven pounds of water be evaporated by each pound of coal, there would result 80,814 foot-pounds of work for each pound of water evaporated. If the pressure in the boiler be three and a half atmospheres (temperature 140°), the amount of work for each unit of heat will be found, by dividing this by 618, to be 130·7 foot-pounds, which is $\frac{130\cdot7}{440}$, or 29·7 per cent. of the theoretical duty.

"5. The actual average of work performed by good Cornish engines and boilers is 55,000,000 foot-pounds for each bushel of coal, or less than half the experimental performance of the Fowey Consols engine, and scarcely more than half the actual duty performed by the United Mines engine in 1840; in fact, about twenty-five per cent. of the theoretical duty.

"6. The average performance of a number of Lancashire engines and boilers have been recently found to be such as to require twelve pounds of Lancashire coal per horse power per hour (*i. e.*, for performing $60 \times 33{,}000$ foot-pounds), and a number of Glasgow engines such as to require fifteen pounds (of a somewhat inferior coal) for the same effect. There are, however, more than twenty large engines in Glasgow at present, which work with a consumption of six and a half pounds of dross, equivalent to five pounds of the best Scotch, or four pounds of the best Welsh coal, per horse power per hour. The economy must be estimated from these data, as in the other cases, on the assumption, which, with reference to these, is the most probable we can make, that the evaporation produced by a pound of best coal is seven pounds of steam.

A cubic foot of water weighs 62·32 pounds; allowing five pounds of coal to evaporate this quantity (=12·464 pounds per pound of coal — and 12·9 has been done), we get, as the theoretical duty of one pound of coal:

	lbs.		lbs. of coal per H. P. per hour.	Per centage of duty.
In the engine with initial pressure of 150 lbs., and cutting off at 1-10th,	1,394,065 raised one foot	=	·62	100·00
In engine with initial pressure of 90 lbs., and cutting off at 1-6th,	2,797,511	"	·70	87·58
Utmost duty known to have been performed by a Cornish engine,	1,329,787	"	1·48	41·63
Common amount of duty for a superior Cornish engine,	1,000,000	"	1·98	31·30
Average duty of Cornish engines,	750,000	"	2·64	23·48
" best marine "	450,000	"	4·4	14 09

It has been already said that the best expansive engines have never realized in practice more than sixty per cent. of their theoretical duty. As regards the *composition* of such loss of forty per cent., Mr. Pole found, that if an engine of that kind, expanding three and a half times, were absolutely perfect, each unit of heat given out by the combustion of the fuel ought to develop about 134 units of work; but the amount actually produced in the shape of water raised was only about eighty units, or sixty per cent. less than the theoretical result. He has endeavored to discover at what parts of the engine this loss occurred, and has found it might be distributed about as follows:

Imperfect combustion, and other causes of waste of heat in the boiler,	12½
Heat expended in raising the temperature of the feed-water to a boiling-point, ...	12½
Friction, imperfect vacuum, air, pump, etc., or power wasted in working the engine, ...	15
	40
Useful effect realized, ...	60
Total calorific power of the engine, ...	100

The friction of the machinery of a locomotive engine has been experimentally determined by De Pambour at $\frac{1}{11}$ of the tractive force it exerts, and this exactly coincides with the results of Mr. Pole's analytical investigation of the friction of the direct-acting marine engine with slides. This is, of course, exclusive of the resistance of the air-pump, and of the friction caused by the pressure (when unbalanced) of the steam on the back of the slide-valve.—*Engineer and Arch. Journal.*

EXPERIMENTS ON THE STRENGTH OF SEVERAL KINDS OF BUILDING STONES.

The following paper, showing the results of experiments on the strength of various kinds of building stones in common use, was recently read before the American Institute of Architects, N. Y., by Mr. R. G. Hatfield. The pressure applied was by means of a hydraulic press. The press was constructed for me by Messrs. R. Hoe & Co., in their best style of workmanship; oil, instead of water, is used to avoid corrosion, and consequent friction. The pressure is indicated at all stages of the experiment by an index moving over a scale on a circular arc — the index being operated by levers on knife-edge bearings; one of these levers is pressed by a piston playing in a small cylinder, the piston being operated by the oil under pressure. The press has a capacity of 60,000 pounds.

The Resistance to Crushing. — The specimens submitted to this test were two-inch cubes of freestone. They were dressed to the shape about as accurately as cut-stone used in the erection of buildings. To prevent any unequal pressure on the parts, they were bedded above and below in a thin layer of fine white sand. The results given below are the pounds per square inch of the surface pressed, required to produce the first fracture.

Kind.	Number of Specimens.	Average resistance per square inch.	Specific Gravity.
Belleville, N. J.,	4	3522	2·328
Connecticut,	3	3319	2·452
Dorchester,	2	3059	2·381
Little Falls,	5	2991	2·326
Caen,	4	1088	2·218

Resistance to Cross Strain. — The specimens submitted to this test were about 4 × 5 inches, and sixteen inches long; laid on chairs, with a clear bearing of one foot in length. The figures given below are the reduced results, and exhibit for each kind the constant, s, in the formula $\frac{l\,w}{b\,d\,2} = s$, or the weight required to break a piece of the material one inch square, and one foot long, clear bearing, the weight concentrated at the middle of the length.

Kind.	Number of Specimens.	Average valve of $s = \frac{l\,w}{bd2}$.	Specific Gravity.
Blue stone flagging,	3	125 lbs.	2·707
Quincy granite,	2	104	2·658
Little Falls freestone,	3	96	2·326
Belleville,	3	82	2·328
Granite (blue), (Another quarry)	1	72	2·604
Belleville freestone,	3	71	2·273
Connecticut "	3	52	2·462
Dorchester "	3	43	2·289
Aubigné "	2	37	2·472
Caen "	3	25	2·218

The one specimen of granite giving a result so much below that of the other two specimens, was of a coarse texture, showing in the fracture the crystals of its ingredients, large and distinct in form and color.

THE WELLINGTON SARCOPHAGUS.

In one of the chambers into which the crypt of St. Paul's Cathedral is divided by the massive pillars which help to support that vast structure, and under the very centre of the dome, is a sarcophagus, of black marble, in which are inclosed the remains of England's greatest naval hero. No more worthy resting-place could have been found for the glorious dead; and no more fitting spot could possibly have been chosen than the adjacent chamber for the tomb of the hero who, next to Nelson, holds the highest place in the estimation of his countrymen. They rest there side by side — the great admiral and the great general — examples alike of England's glory and of England's gratitude.

When the country had provided so munificently for the burial of her favorite commander, it was felt that the tomb no less than the monument should testify to the national feeling. The chamber immediately to the east of that in which Nelson lies was appropriated to Wellington, and it was decided to place the coffin in a sarcophagus bearing a general correspondence to Nelson's.

Some difficulty was found in obtaining a suitable block of stone for the sarcophagus, either on the continent or in Great Britain. At length one was discovered in a huge boulder of porphyry — one of several — lying in the parish of Luxulion, on the southern coast of Cornwall. So excessively hard was this stone, that tools had to be constructed specially for the purpose of working it; and as only one man could work there at the same time, the carving of the inside took nearly two years to complete. The sarcophagus was hewn into form, as a geologist would say, *in situ:* it being found far easier to carry workmen and tools to the field, than to carry the stone to the workshop. The cutting was done by hand; the polishing, for the sake of expedition, by steam-power. The boulder was sawn in two to form the sarcophagus, the larger portion being hollowed out to provide a receptacle for the coffin, the smaller forming the lid. Its massiveness will be understood when we state that the sarcophagus as completed weighs upwards of twelve tons: the rude block was some five times that weight. Whilst the sarcophagus was in progress, the chamber was being adapted to receive it; and the whole has, nearly five years after the death of the Great Duke, been at length finished.

The chamber has a very impressive effect. In the centre, the massive sarcophagus, reared on a more massive base, reaches nearly to the low vaulted roof; and no object interferes to lessen its majestic proportions. The porphyry, of which it is composed, is of a deep chocolate color — nearly black, in fact — feldspar crystals varying its surface with splashes of a light but dusky red. In form, it is, of course, oblong, the angles not being rounded; but the massiveness is not destroyed, as in the Nelson sarcophagus, by the lower part being cut away: the full width of the base is preserved, very much to the advantage of the general effect. On one side of the sarcophagus is inscribed, in gold letters, "ARTHUR, DUKE OF WELLINGTON;" on the other, the dates of his birth and death. At each end, on a plain circular boss, is a Greek cross, its shape being indicated by a gilt outline. No other inscription or ornament is perceptible. The pedestal on which the sarcophagus rests is of white granite, from the Cheesewring quarry, Cornwall; extremely solid in form, about the height of the sarcophagus, and having at each of its angles the head of a sleeping lion. The lower part of the walls of the chamber are also lined with rough white granite; and a moulding of polished red granite, which is carried along the sides of the chamber, serves to diffuse the color of the sarcophagus, and of the four large polished granite candelabra which stand at the four corners of the apartment. From a sphere which surmounts each of these candelabra, rise four small jets of gas, which shed a dim, religious light, — subdued, but sufficient to allow the tomb to be distinctly seen. The floor is paved with encaustic tiles.

The sarcophagus, to our thinking, is finer in form than the finest of the Egyptian sarcophagi in the British Museum (of course it admits of no comparison in its workmanship with the elaborate hieroglyphic sculpture on some of them), finer, in fact, than any we know. — *Lon. Lit. Gazette.*

CHURCH OF ST. ISAAC, AT ST. PETERSBURG

This church, which has been thirty-nine years building, was consecrated, with great pomp and military parade, on the 10th of June, 1858. "Visitors to this gorgeous temple," says a correspondent of the London Athenæum, "are dazzled with the profusion of barbaric pearl and gold they meet at every glance. We see no wood, except in the doors; all the rest is granite, Carrara marble, iron, porphyry, malachite, alabaster, lapis lazuli, bronze, silver, and gold. Even the lightning-conductors are of platinum. The five crosses, as well as the cupola of the building, are gilt with a mass of 274 pounds of gold, and are seen glittering at a distance of forty wersts from St. Petersburg. One of the bells weighs 75,000 pounds. Eleven hundred and twelve granite columns, with Corinthian capitals, surround the building. They are each fifty-six feet high, and seven feet in diameter at the base. Each is considered to be of a value of £1800 English money. The cost of the whole magnificent building is reckoned — though this is probably a gross exaggeration — at £13,500,000. The interior, — comprising a space of 60,000 square feet, and taken up neither by seats nor by organs (in the place of the organ there is a choir of 1000 men's voices), — is very imposing."

STEAM HAMMERS.

These tools have gone on increasing in quick gradations, until the climax of a six and a half tons, dead hammering weight, with a fall of seven feet six inches, has been reached. A hammer of this weight has been lately erected, and is now in operation at Glasgow. — *London Builder.*

METHOD OF DETECTING DECAY IN TIMBER.

The French Journal "Cosmos" states that a simple method has been adopted in the shipyards of Venice, from time immemorial, for testing the soundness of the timber. A person applies his ear to the middle of one of the ends of the timber, while another strikes upon the opposite end. If the wood is sound and of good quality, the blow is very distinctly heard, however long the beam may be. If the wood is disaggregated by decay or otherwise, the sound will be for the most part destroyed.

ON THE ESTIMATION OF WEIGHTS OF VERY SMALL PORTIONS OF MATTER: BY PROF. McMAYER.

The chemist, in the course of his analytical investigations, often meets with what are called traces of substances; by which is generally understood, quantities of matter too minute to have any appreciable weight in the analytical balance. Now it sometimes happens that these traces are of as much importance, considered scientifically and commercially, as the ingredients present in appreciable quantities; and in order to estimate these small portions of matter, he is often obliged to go over his work, using very considerable weights of substances, whereby his time and care are nearly doubled. It was this inconvenience that first induced me to try to determine in one operation the components present in large and in very minute quantities; and although I have succeeded beyond my expectations, I am confident that the process is susceptible of improvement, both as regards sensibility and accuracy.

After making many investigations on the sensibility of the most delicate levers as to small weights, this method was found far too rough. It then occurred to me that if instead of using the opposing force of gravity through the intervention of a lever, we could oppose to the gravitating effect of the matter the force of perfect elasticity as manifested in filaments of glass, we might succeed in obtaining the weights of extremely small parts of matter. For that purpose I tried the elasticities both of torsion and flexure, and found the latter only to answer the purpose.

The following is a description of the construction of my apparatus, with which I have succeeded in estimating portions of matter equal in weight to the thousandth part of a milligram. Heating a rod of soft glass in one spot to bright redness, I drew it out quickly, and thereby obtained a filament uniformly cylindrical, of about the diameter of fine human hair. Taking from the middle of this fine glass thread a piece of such a length (about three inches) that its weight would barely reduce it from the horizontal, one end of it was fastened, by means of good sealing-wax, to the edge of a mahogany block, and the other end slightly hooked by approaching quickly a small spirit flame. In order to obtain a pan in which to place the substance whose weight I would estimate, I cut with the common microscopic section-cutter some discs of elder pith from ·001 to ·002 inch in thickness; and drawing out a still finer filament, the end was likewise hooked, and the other extremity being passed through a pith disc, a small knob of glass was made on this end by the spirit flame, just of sufficient size to prevent this disc slipping off the suspending-rod. The filament with attached disc was now hooked on the end of the rod fixed to the block, and was then ready for graduation.

Not being able at the time to procure silver wire of sufficient fineness, I substituted some very fine and long hair, taken from the head of a child; and having brought the centre of gravity and centre of motion of a very sensitive analytical balance almost to coincide, I obtained a piece of the middle of a hair weighing exactly one-half milligram. This being divided into five equal parts (each about one inch long) gave us tenths of a milligram. One of these tenths being placed on the pith-pan, the glass filament was deflected a certain quantity, which was marked on an arc formed of bristol board, and so as to be almost touched by the deflected rod in its revolution about the edge of the block. Another tenth was added, and another division obtained: and so on, until all five divisions were marked. The length of the divisions being about one-fourth of an inch, they were very readily subdivided into ten equal parts, which gave me immediately $\frac{1}{100}$ths of a milligram. The weight of any quantity of matter less than one-half milligram may be now estimated to $\frac{1}{100}$th of a milligram by placing it on the pan and observing the deflection.

For the thousandths, still more care and patience is required, the filament being much finer and somewhat shorter, and the pith disc smaller and as thin as possible. In order to obtain the primary graduations of hundredths, one of the above pieces of hair, equal to $\frac{1}{10}$th milligram, is divided into ten equal parts, which gives us weights of $\frac{1}{100}$th milligram. The deflections caused by these weights, divided into ten equal parts, give $\frac{1}{1000}$th of a milligram.

As the least breath of air interferes with the graduations and weighing, the whole instrument is protected by a glass case, the end of the case next the graduated arc being on a hinge.

In elastic rods of square section, the deflection is proportional to the weight; in those of circular section this law is slightly departed from; but by the above method of ascertaining directly the value of each division, the error is avoided. — *Silliman's Journal.*

IMPROVEMENTS IN MILITARY IMPLEMENTS.

Novel Field Artillery. — General Sir Charles Shaw, of England, has recently perfected a novel piece of field artillery, from which he anticipates extraordinary results in the percentage of destructiveness and economy of expenditure. Napoleon's axiom was that to bring a continuous concentrated fire upon a given point of the enemy's position was the secret of victory. Animated by this idea, the general has turned his attention to the construction of a machine which shall accomplish this object with the least amount of risk to the party using it. The invention may be briefly described as an ambulatory infernal machine, based upon the Fieschi model. It consists of a row of twenty-four rifle barrels, bound together, fitted to an axle, and mounted upon a pair of strong, light wheels. The axle is capable of depression or elevation to any angle within a radius of fifty-five degrees, so that the necessary elevation according to the distance of the enemy may be insured. The barrels may be either breach-loading, upon the revolver principle, or they may, as in the model exhibited, be charged in the ordinary way, at the mouth, and rammed down, and all may be discharged at a single fire, or in four divisions of six each. The whole machine is but 200 pounds weight, and is sufficiently portable to be moved about, turned to the right or to the left, and its fire directed with certainty by a single soldier while with its ammunition-cart, containing a relay of barrels and an ample supply of cartridges, it may be moved from one part of the field to another by a single horse at a handgallop. The general affirms that one of these field-pieces, which may be served effectually by eight men, allowing for casualties, will throw in a more deadly fire than a body of 200 infantry armed with the best description of rifles in existence, and that the ratio of its destructiveness, as compared with ordinary infantry firing in line, is as seventy-five per cent. against four. In addition to their use in ordinary field service, these machines, mounted upon a pivot instead of the wheels, may be employed with great effect in boat service, as an armament for ships' tops, martello towers, or other works of defence.

IMPROVEMENTS IN RIFLES.

Mr. Whitworth, of England, in pursuing a course of experiments with a view of improving the rifle, has adopted a polygonal spiral bore of a uniform pitch, but more rapid than could be attained by grooves. This bore has enabled him to surpass the range and penetration of the Enfield rifle; and the strain of the projectile being distributed evenly over every side of the polygon, iron can be substituted for lead in the projectile. Moreover, Mr. Whitworth has discovered, in the course of his experiments, that according to the quickness of the turn in the polygon is the length of the projectile that may be fired, so that twenty-four pound and forty-eight pound shot have been sent to extraordinary ranges with half the usual charge of powder from an ordinary twelve-pounder howitzer.

MALLET'S THIRTY-SIX INCH MORTARS, AND SHELLS.

At a meeting of the British Association for 1857, Mr. Robert Mallet presented an abstract of a plan he had proposed to the British Government for the employment of shells of a magnitude never before imagined by any one, viz., of a yard in diameter, and weighing, when in flight, about a ton and a quarter, and for the construction of mortars capable of projecting these enormous globes. (See Annual of Scientific Discovery, 1858, page 87.) Since the above-mentioned date, a colossal mortar, constructed on Mr. Mallet's plan, has been practically tested by the English Board of Ordnance, on Woolwich Marshes, with charges (of projection) gradually increasing up to seventy pounds. With this amount of powder, a shell weighing 2550 pounds was thrown a horizontal range of upwards of a mile and a half to the height of probably three-quarters of a mile, and, falling, penetrated the compact and then hard, dry earth of the Woolwich range to a depth of more than eighteen feet, throwing about cartloads of earth and stones by the mere splash of the fall of the empty shell.

The explosive power, it is obvious, is approximately proportionate to the weight of powder; but, by calculations, of which the result only can here be given, Mr. Mallet has shown that the total power of demolition, that is to say, the absolute amount of damage done in throwing down buildings, walls, etc., by one 36-inch shell, is sixteen hundred times that possible to be done by one 13-inch shell; and that an object which a 13-inch shell could just overturn at one yard from its centre, will be overthrown by the 36-inch shell at forty yards' distance.

No bomb-proof arch (so-called) now exists in Europe capable of resisting the fall of one of those huge shells upon it, whose energy of descent may be represented as equal to about eight hundred tons. No means or precautions are possible in a fortress against the tremendous fall of such masses, still more against the terrible powers of their explosion, when 480 pounds of powder, fired to the very best advantage, puts in motion the fragments of more than a ton of iron,— no splinter proof, no ordinary vaulting, perhaps no casemate exists capable of resisting their fall and explosion, either of which would sink the largest ship (even the *Leviathan*) or floating battery. And as no precaution could save either garrison or town from such shells, so their moral effect would be paralyzing.

A single 36-inch shell in flight costs £25, and a single 13-inch £2 2s., yet the former is immeasurably the cheaper projectile; for to transfer to the point of effect the same weight of bursting powder we must give—

55 shells of 13 inches, at £2 2s., - - - - - -	£115	10	0
Against 1 shell of 36 inches, - - - - - - -	25	0	0
Showing a saving in favor of the large shell of	£90	10	0

and this assumes that fifty-five small shells, or any number of them, could do the work of the single great one.

The mortars are, with the exception of one part (the base), and the elm timber ends, formed wholly of wrought iron, in concentric rings, and each entire mortar is separable at pleasure into thirteen separate pieces, the heaviest of which weighs about eleven tons; so that the immense weight, when all put together (about fifty-two tons), is susceptible of easy transport, on ordinary artillery carriages, over rough country, or can be conveniently shipped, stowed, or landed.

NEW METHOD OF PRINTING.

A description of a new method of printing, invented by a journeyman printer, and called by him *Neography*, has recently been published in Paris. The object sought to be attained is to obtain printing surfaces of a better quality than stone, zinc, or any other substance hitherto used; and, moreover, to get impressions of different colors by a single operation, instead of bringing the sheet under the press several times. The *modus operandi* is as follows: The figures or characters to be produced are drawn upon a woven stuff, or any other which may be penetrated by a liquid; the ink used for the purpose is composed of lampblack, Indian ink, gum, sugar, and common salt. This done, the side on which the figures have been drawn receives a slight coating of gutta-percha, and when this is dry the surface is washed. Now, since the ink is composed of soluble matter, this will wash off, and the gutta-percha which covered the characters, and which therefore does not adhere to the stuff, washes off too, by which means the stuff becomes a surface which is only penetrable by liquids in those places where the characters were drawn, and is perfectly impenetrable everywhere else. This done, the wrong side of the stuff receives the inks and colors which are to serve for printing, while the sheet is laid on the right side. Under the action of the press, the ink and colors penetrate through the unprotected places, and a clear impression is obtained. Instead of applying the ink and colors as stated, a permanent kind of cushion, made much like the balls formerly used for inking type, and properly charged with ink or colors, may be placed under the stuff, and thus many sheets may be worked off before it is necessary to renew the ink.

BULLOCK'S MECHANICAL FEEDER FOR PRINTING PRESSES.

This machine, now in successful use in New York, operates as follows: A pile of sheets is placed upon the feeding-board in the manner usual for hand-feeding. Above it and a few inches back of the front edge of the top sheet, a number of small vertical cylinders stand in a row parallel to the printing cylinder. Each of these cylinders is a small engine, closed at top and open at the bottom, inside of which is a piston, provided with a piston-rod sufficiently long to reach the paper when the piston is down. All the rods are articulated, an elongated hole is cut in each for a crank-pin to pass through, and by means of a cranked shaft they are made to move constantly backward and forward. The ends of the piston-rods are so arranged as to slide on the paper when moving backward, and as to carry it forward during the forward stroke. Each piston is pressed down by a coiled spring placed in the cylinder between the piston and the top cover. From each cylinder a pipe extends to the edge of the feeding-board nearest the roller, where it is flattened, and its lower portion resting on the feeding-board, is pierced with a small hole. All the cylinders are also connected with an exhaust air-pump, constantly at work. The machinery operates as follows: The piston-rods working backward and forward in contact with the top sheet, brings it forward to the edge of the feeding-board. The moment it arrives there, the suction of the exhaust pump makes the sheet close hermetically the small holes in the pipes. A vacuum in the cylinders, and the rising of the piston against the coil springs, are the immediate results of this closing. The piston-rods recede from the paper, which is left at rest, till the iron fingers of the roller seize it and carry it to the form. The moment the sheet is carried off, the holes in the pipes are left open, air rushes through them into the cylinders, fills the vacuum, the pistons are pushed down by the coil springs, and the ends of the piston-rods carry the next sheet forward. Several of the cylinders work at right angles with the first, to insure a proper register sidewise. There are also a few incidental arrangements, such as the raising of all the pipes from the paper at the moment the last is clenched. There are several good patented plans for making a mechanical feeder for separate sheets, but none better than the one described. The nature of the work requires an attendant, and as feeding does not require a long apprenticeship, there is little difference between the wages of a feeder and those of a boy. The advantage of the apparatus seems then to consist in the possibility of running presses faster. Book printers cannot avail themselves of it, as, for the purpose of making neater copies, the presses are actually run slower than they could be fed by hand. The apparatus would be advantageous for newspaper rotary presses, in which rapidity is everything; but in this case feeding with endless paper is still a better plan, which, sooner or later, will supersede all others. — *New York Tribune.*

CLAY RETORTS FOR GAS-MAKING.

A paper has been read to the Institution of Civil Engineers, London, "On the Results of the Use of Clay Retorts for Gas-making," by Mr. Jabez Church. The substitution of fire-clay for metal, in the construction of retorts, was attributed to Mr. Grafton, and dated back as far as the year 1820. Originally they were square in transverse section; but that form was soon changed for the ⌂, or oven-shape, which had been since adhered to, both in this country and abroad; this latter form of retort admitting of a stratum of coal being distributed of an equal thickness throughout.

The comparative quantities of gas made by iron and clay retorts, of the ⌂

form, of 15 inches by 13 inches in section, and 7 feet 6 inches in length, had been found by the author to be as follows:

The iron retorts lasting 365 days, and working off 1½ cwt. of coal for each charge, effected the carbonization of 2190 cwt. of coal, which, at 9000 cubic feet of gas per ton, gave a total quantity of 985,500 cubic feet of gas per retort; whilst the clay retorts lasted 912 days, carbonized 5472 cwt. of coal, which, at 9000 cubic feet of gas per ton, gave 2,462,000 cubic feet of gas per retort. It would thus be seen that the clay retorts yielded a greater quantity of gas, from the same weight of coal, than the iron retorts; but the specific gravity of the gas so made was less, and its illuminating power was diminished, in consequence of the increased temperature of the clay retorts, which caused the last portion of the gas to be decomposed.

The most practical method of working clay retorts in large works was with the addition of an exhauster. This reduced the pressure on the retort, and prevented the escape of gas through the pores and fissures; and by that system the quantity made was increased about 200 cubic feet per ton of coal. In small works, the expense of an exhausting apparatus, and steam machinery to work it, would not be compensated by the gas saved.

HEATING BY GAS AND SAND.

Some interesting experiments have recently been made in Albany, by Mr. Calvin Pepper and others, to test the value of sand as a heating medium, especially for railroad cars. The heat is obtained in the first instance by diffusing the gas through sand. If the gas be directed into the body of the sand, it will instantly diffuse itself through the entire mass, and rising to the surface, may, with perfect safety, be instantly set on fire with a match, the flame covering the whole surface of the sand with a pure flame without smoke, no matter how large the extent of the flame, and with perfect and complete combustion. The heat is almost instantaneously diffused through the entire mass of sand, heating it equally throughout, and requiring but one minute of time to heat the sand to such intense temperature that it will retain its heat for hours after the gas is shut off and the light extinguished.

INFLUENCE OF WALL-PAPERS ON THE TEMPERATURE OF APARTMENTS.

Paper-hangings in themselves (says the *Builder*), as materials, maintain a higher temperature than the walls or partitions on which they may be placed; then less condensation of vapor takes place, and the dampness is removed from the room as the progress of ventilation goes on. To a great extent paper is an absorbent; but then the moisture is given off in the same form, or may escape by other means. The reason why dark papers are dryer than light ones, is still due to the same action. All dark materials imbibe more light and heat, and will thus maintain a higher temperature; besides which, many of the very light-colored papers (particularly the better ones) have a glazed or satin face, which is produced by the use of a large quantity of China clay, a material that, from its coldness, at once causes condensation of moisture, and thus facilitates its own decay.

SUBSTITUTE FOR LEATHER.

Samuel Whitmarsh, of Northampton, Mass., has invented a new fabric which is intended to supply the place of leather in many of its applications.

The fabric is composed of cotton or other fibrous substances, either woven into cloth or in an unwoven state, and saturated or coated with a compound of linseed oil and burnt umber, prepared by boiling in every gallon of oil about three pounds of umber in a powdered state, for such a length of time that the composition, when cool, will roll in the hands without sticking. The fabric may be made in forms suitable for the soles of boots and shoes, coverings for trunks, travelling bags, cap-fronts, or as a substitute for carriage or harness leather, or for machine-belting or hose-pipe. The mode of producing the fabric differs, to some extent, according to the use for which it is designed; but the general principles are in all cases the same. The umber is stirred into the boiled oil until it reaches the point desired, when it is ready to be applied, in the manner best calculated to produce special articles.

A NEW CEMENT.

Mr. Edmund Davy prepares a new cement, which is well spoken of, by melting in an iron vessel equal parts of common pitch and gutta-percha. It is kept either liquid under water, or solid, to be melted when wanted. It is not attacked by water, and adheres firmly to wood, stone, glass, porcelain, ivory, leather, parchment paper, feathers, wool, cotton, hemp, and linen fabrics, and even to varnish.—*Cosmos*, vol. xii., p. 41.

LIQUID GLUE.

Take glue of good quality and dissolve it in as small a quantity of hot water as possible; then, while yet hot, remove it from the fire and dilute it to the proper degree of thinness by adding alcohol, after which it should be bottled, and the mouth of the bottle kept covered with a piece of India-rubber, or anything else that will exclude the air. Alcohol will preserve glue made in this way for many years, keeping it from putrefaction in summer and from freezing in winter. In cold weather it requires only a little warming to make it ready for use. This convenient article has been in use in England for many years, but has never been extensively known in this country.

ARTIFICIAL IVORY.

A patent has recently been granted, in England, to Charles Westendarp, jr., for manufacturing a material intended to imitate ivory, bone, horn, coral, or other similar substances, natural or artificial, and which may be used in preference to ivory, on account of cheapness and adaptability, as the same materials may be moulded or turned to the various forms or patterns they may be desired to take, and may be applied to all the purposes in which natural ivory becomes useful, such, for instance, as billiard-balls, door and other knobs, piano-forte keys, rulers, paper-knives, whip, stick, and other mounts, and in imitation, or as a substitute for carved wood, enamelled china, precious stone works, and a variety of fancy, ornamental, and decorative figures.

The process being as follows: Five ounces (or more or less according to the size of the article to be made,) of ivory dust is soaked with a white color, say white lead or zinc white, three ounces, in a solution of white shellac or copal, in sixteen ounces of spirit of wine. After the whole is well mixed, which is best done at a temperature of 212° Fahrenheit, the alcohol is partially evaporated, and the stiff paste or dry powder pressed into a solid mass in the dies or mould, which have been previously heated to about 230° or 280° Fahrenheit; after being so solidified they are polished in the ordinary manner of polishing ivory. Instead of using ivory dust, steamed and finely powdered bones, porcelain, cotton, and various finely powdered materials may be employed, and the colors may be varied according to the tint or shade required; the ivory or other dust may be dyed similar to cotton cloth. Gum dammar, copal, mastic (and if great elasticity is required, bleached India-rubber or gutta-percha), answer the purpose very well, either with or without shellac; bees-wax, camphor, and turpentine, are good for some of the purposes, and, according to the ingredients used, it will be perceived that the preparation must undergo various modifications during the process of manufacture.

MACHINE FOR BURRING WOOL.

Thomas Musgrave, of Northampton, Mass., has invented a machine for removing burrs and dirt from wool, which promises to accomplish the same results with that staple that the gin has with cotton. The machine is very simple, and is in the form of an attachment to the ordinary carding machine. It adds only fifteen or seventeen dollars extra expense to the carder. The value of it will be obvious to all who know how foul the South American wools are with burrs, and how great is the expense of cleaning it by any process hitherto known. After being washed, the wool is placed upon the apron of Mr. Musgrave's machine, and carried by it to two rollers, covered with coarse cards, lying parallel and revolving inwards. As the wool passes these it meets the "burrer," a cylinder of about six inches in diameter, composed of steel rings slid upon a shaft. The circumference of each ring is cut into teeth something like those of a cross-cut saw; between each of the rings is a circular wire to separate them. The whole are then driven together, forming a cylinder, of which the surface is composed of these steel teeth, of which there are eleven to the inch. As the wool passes the feeding rollers it is caught by the teeth; the wool itself is drawn into the space between the teeth, leaving the burr on the surface. Above this cylinder is another of wood, into which are fixed longitudinally spiral blades. This revolves so as nearly to touch the under cylinder, but in a contrary direction. Thus it is obvious that the burrs on the under cylinder coming in contact with the blades of the upper one, are cut off. They are received upon an apron, which removes them. In the ordinary manner the wool freed from the burrs is then delivered to the carding machine. Were the burrs large and hard, like cotton seed, no more would be required; but the small, brittle burrs of the South American wool are apt to be broken, and the fragments get ultimately into the yarn and injure the cloth. To obviate this, Mr. Musgrave has introduced a carding cylinder to take the wool from the first "burrer" and deliver it, better distributed, to a finer one, where the teeth are fourteen to the inch instead of eleven. By this means the wool passes to the carder entirely free from burrs. The thorough mode of its action may be illustrated by the fact that Mestiza wool was passed through, and in a few moments freed from forty per cent. weight of burrs without any apparent injury to the fibre, adding at least fifteen cents per pound to the value of the wool.

IMPROVED KNITTING-MACHINE.

A very ingenions knitting-machine, or loom, has recently been invented by J. B. & W. Aiken, of Franklin, N. H. It resembles a large ring, having a revolving top plate, and a number of under hooks, moving back and forth towards and from the central opening, to receive the thread or yarn from a rotary ring-traveller, to form the loops, interlace them, and then throw them off in the form of a long knit tube hanging down in the centre. To produce a ribbed knit fabric, two sets of needles are required, the one set working vertically through, and transverse to the loops formed by the other set; one set of needles only are required for plain work. A large machine for knitting shirts has five feed bobbins, and a stop motion for each, so that the break of a thread at once stops it. It is a most ingenious loom, and will knit fifty yards in one day.

A stocking-loom occupies no more space than a common sewing-machine; but one is required for knitting the legs, and another the feet. The work of the former is taken off in the form of a long tube; this is cut in proper lengths, put on the footing-machine, which weaves a single square piece to the leg, and this is closed by crotchet work, by hand, to form the foot. One girl can attend eight looms, and produce 100 dozen pairs of stockings in a factory every day. They are the most perfect machines for this purpose we have yet examined, and no less than five patents are embraced in their operation and construction. The cost of a machine to knit ribbed stocking-legs, is $200; one for feet, $100; a family machine, for plain work, $50.

BOOTHE'S IMPROVED GRAIN-CLEANER.

Ordinary smut-machines are built of wood, and are open; the necessary consequence is that they have to be confined in a close room on account of the dust thrown out, and that they catch fire very easily from over-heated journals. Several large mills have been lost from this cause, and the rate of insurance is, on this account, often extremely high. This new grain-cleaner has been devised to avoid dust and danger of fire; it is entirely metallic, and is all encased. On a vertical shaft, a cylinder, or drum, about two feet in diameter and four feet long, is keyed, and made to revolve at a velocity of 550 revolutions per minute. On the periphery of the drum projecting flat arms, denominated "beaters," are screwed in parallel circular rows. They extend a few inches outside, forming an angle of forty degrees with a tangent to the drum, and their external surface, measuring three inches by four inches, is deeply corrugated by vertical grooves a quarter of an inch deep and wide. Around this drum is a stationary cylindrical envelop of such a diameter as to leave scarcely an inch of free space between itself and the ends of the beaters. This envelop is corrugated circularly; the hollow of each corrugation is opposite one row of beaters. This circular envelop is closed below by a curved bottom terminating in a pipe at the centre, and is closed at the top by the case of a horizontal fan blower, which is placed above it; the fans of the blower revolve with the shaft of the machine. There is also a suction-pipe leading from the pipe at the bottom of the machine to the fan blower. To operate, the grain is introduced at the top, between the drum and the cylindrical casing. Before it has had time to fall an inch, it is caught on the inclined face of a beater, and thrown out by centrifugal force; but the beater is inclined, so as to follow the grain and exert upon it a hard friction. The grain is thus thrown into a corrugation of the outside envelop, and in falling down along the lower portion of this corrugation, which acts as an inclined plane, it is brought back toward the centre of the machine, and is caught by the second row of beaters, and by all in succession. The dust which is detached from the grain is carried up by a strong current of air blowing upward between the drum and its envelop. After reaching the bottom of the machine, the grain enters the central pipe, and falls on an inverted cone placed in it, and the last particles of dust remaining are carried away through the outside pipe already mentioned. Machines of this kind are built of different sizes. A two-horse power machine can clean fifty bushels of wheat per hour, and is sold for $150. Those of a larger size cost proportionately less, and do more work for the same power. After a time, the surface of the beaters wears out, and they become perfectly flat; but they are easily replaced by others, at a cost of $3 for the whole set. This machine does its work cheaply and effectually, and, slightly modified, may eventually serve for cleaning cocoa and coffee in Equatorial America and elsewhere. The cleaning of cocoa is at present actually done by hand, in the most primitive manner, at a cost equal to forty per cent. of the price of the grain ready for shipping.—*N. Y. Tribune.*

SELF-INDICATOR BEE-HIVES.

The careful bee-fancier has long desired to possess some method of measuring the daily increase or decrease in the weight of his hive. A recent German authority states that a bee-grower there took the trouble to weigh one of his hives twice a day—before the bees left in the morning and after their return at night—and thus he determined the nightly loss by consumption and evaporation. These observations were continued from May 5 to August 2, a period of ninety-one days, and the results are very interesting. On the 5th of May the hive weighed sixty-four pounds; it lost two swarms weighing twelve pounds; yet on August 2, it weighed 120½ pounds. There was no increase in weight from June 28 to July 21, except of one-quarter pound on one day, and three-quarters on another; and from July 17 to August 2, the whole increase was only three pounds. The work of each day is minutely recorded, and the results go to prove that the bee-keeper should have some means of ascertaining the weight of his hives daily throughout the season. A method of doing this has been invented by Mr. Shirley Hibbard, of Tottenham, England. It consists of a turned pillar, made after the fashion of a telescope, working like a piston in a brass or iron cylinder. Beneath the pillar is a spiral spring, on which the pillar rests. Two slots run down the side or front of the cylinder, and between them an index is marked. A finger is attached to the base of the pillar, and the hive adjusted on top of the latter, so that as it presses down on the spring the finger marks the gross weight of the whole. A thumb-screw passes through the cylinder, and, by pressing against the pillar, holds it in a fixed position whenever it may be desirable. The whole affair is exceedingly simple, and must be readily understood. To the intelligent bee-keeper it will be a very acceptable acquisition.

RECENT IMPROVEMENTS IN AGRICULTURAL IMPLEMENTS.

Stenton's Improved Land-side Cutter, patented 1858, consists of a horizontal knife or cutter, which is attached near the end of the land-side to an ordinary plough. The width of the cutter is one-third that of the plough, and it cuts its own breadth under the land, so that the plough on its succeeding rounds will turn the breadth of the cutter in addition to its usual work. It is affirmed that the saving of power usually lost in friction on the land-side is transferred to the edge of the cutter, and that thus one-third more work is performed by the same team when the cutter is used. Another important advantage is, that the plough thus provided is much more steady, and much more easily kept in the ground. When it is desired to pulverize the ground, two cutters are used, at different heights, the second in advance of the first.

A new form of steam-plough recently patented in England operates as follows: A series of spades is made to enter the land in succession, and cut it into the arc of a circle, when the cut slices are suddenly thrown up against a shield plate, at once reversing and breaking them almost into powder.

A new form of cart-body has also been patented for the purpose of delivering manure over a field without requiring it to be thrown out by hand. The bottom of the cart-body is supplied with longitudinal openings, in which revolve drags or blades attached to an axis under the body. As the cart moves, these drags pull down the manure in a condition of complete pulverization.

PROTECTIVE MATTING FOR HORTICULTURAL AND AGRICULTURAL PRODUCE.

Doctor Guyot, of Paris, the proprietor of extensive vineyards, in Sillery, Champagne, has introduced in France, and is now introducing in England, a simple, but improved, description of straw matting for the protection of horticultural and agricultural produce, together with a loom or apparatus for manufacturing the same.

The fabric is composed of a weft of straw, cane, bass, rush, reed, or other similar material, woven into or combined with a warp, consisting of two sets of warp threads, each set composed of two wires, or stout cords, twisted together; and it is manufactured as follows: The straw, bass, or other material, is cut into even lengths, and spread on a table with a central slot or channel from end to end, where, by means of a comb or reed with conical teeth, the mass is divided into clusters (the thickness of each cluster being according to the space between every two adjoining teeth). The comb is driven into the straw just over the channel. The table is then brought to the weaver, who takes a cluster at a time, and feeds it in a loom or frame, in which the warp, cords or wires, are delivered off in twos from four reels set in the same spindle mounted in the standards of the frame, and are passed through eyes and grooves in plates which act as heddles, being connected by a double escapement or otherwise to treadles, by which they are depressed and brought up again by springs at the top, whereby the warp threads are crossed, two by two, alternately, each set being opened to form a shed, through which the weft is introduced. The fabric, as it is woven, is wound off on a beam made to revolve by a weighted lever; the weight also effects the draft and tension of the warp threads, being brought back from the end of its stroke by hand or otherwise; or the beam may be turned, and the warp threads delivered off and opened to form the shed by steam or other power which may be employed to work the frame. Pins may be let in the fabric to fix it in place, or it may be mounted on stakes with cross-pieces, or on swiveled rods, or on adjustable frames, so that the position of the matting may be varied when used for sheltering a plant; or it may be mounted in rollers like a blind, to cover conservatories, etc.

The breadth of the fabric varies from one foot three inches to two feet. The lesser breadth is the better for protecting plants placed in rows or beds, or in hot-houses and other like places, and the greater breadth for protecting wall fruits, such as peaches, apricots, etc. The matting is made of any desired length, being rolled up into rolls, like carpeting, as it leaves the loom or apparatus in which it is woven, and which has been designed especially for its manufacture. It weighs but little, and may consequently be transported with ease, and at a small expense. It may be handled roughly without risk of injury, arranged in any desired form or manner, cut into any required lengths, and, if desired, be reünited again without difficulty. It is so easily applied in the garden or orchard, that ten men will, in a single day, fix it over thirty thousand feet of plants, and that so firmly and surely that it will resist the most violent storms to which it may be exposed.

IMPROVEMENT IN PAPER-MAKING MACHINERY.

An improvement, invented by Stephen Rossman, of Stuyvesant, New York, has for its object the prevention of the breaking or tearing of the paper, as it passes from the upper one of the second press-rolls to the dryer. This is attained by the use of a small roll arranged parallel with the press-rolls, between the highest part of the upper press-roll and doctor, about opposite the line where the paper should leave the upper press-roll, on its way to the dryer, so that the web of paper will pass between it and the upper press-roll. The slight cohesion of the web to this small roll eases it off the upper press-roll, and prevents its breaking; and if a slight break should occur in the web, it prevents the edge of the break being carried under the doctor, and thereby increased.

MARSTON'S IMPROVED DOOR LOCK.

A few years ago a talented burglar discovered that, by taking with a pincer a firm hold of the end of a key, it could be made to turn, and that thus the door of the sleeping apartments could be opened from the outside. This knowledge having spread rapidly, and led to numerous practical applications, some inventors set to work and devised a number of instruments, called burglar alarms. In some of these a bell is made to ring; in others, powder to explode, or a gas-burner is lighted, with a cracking noise, either by means of electricity, or of a wound-up spring, acting by friction on a match. Mr. Marston accomplishes the same result, by placing the inside key-hole of locks out of line with the outside one. This arrangement renders it impossible to turn the key from the outside; but it leaves the outside key-hole empty, and the lock might be picked through it. To obviate this, the lock is provided with a sliding piece, which receives its motion from the key in exactly the same manner as the bolt, and which moves over the outside key-hole, and closes it hermetically, each time the door is locked from the inside. The key is shaped so as to have no action on this slide when used from the outside.

NEW MUSICAL INSTRUMENT.

The successful efforts of art-mechanics in music have for a long time been exclusively shown in improving the old instruments of past centuries, but not in adding new instruments of high value and rank to the list. We have at last recently examined an instrument, made by Messrs. Hill, of New York, which is essentially novel, and, making every allowance for the inevitable deficiencies of a first out-worked attempt of the principle involved, the result is promising and brilliant. The inventors call it a keyed harp; whereas, its qualities are precisely those which the harp has not, namely: a sustained sound. It is played upon like the piano-forte, and, while the tone-stroke has not the readiness, or crispness, or vitality of that instrument, the sustained vibration is much greater, when not arrested by mechanical means. The note cannot be shaded after once sounded; but the continuation of the vibration, we are assured by the inventors, can, under the extended application of a second and improved manufacture, be secured for a whole minute. The instrument, we heard, wants power, which the inventors say can be more than doubled by doubling the size of the constituents of its sonority; but it has great sweetness — in fact, too saccharine, if anything — and not characterized by vigor.

The principle is that of a vibrator, or fork, with the prongs applied to an aperture in a box or cell. The vibrators have prongs, from one inch to ten inches long, the handles of which are gently, but firmly, held to their places over the hammers and to the cells, which cells are of as many sizes as are the forks. To the prongs of the longer vibrators are wires to receive the hammers, and wings to enable the prongs of the vibrators to take efficient hold on, and thoroughly cause to sound, the air in the cells. The damper-frames and damper-levers are at the back ends of the keys; and the sound is stopped by the fall of the damper against, or near, the ends of the prongs. The damping is perfect, as is also the pedal movement. The covering of the hammers differs much from, and is simpler than that of the piano. The strength of the ordinary piano action is all that can be desired in this; and the inventors would have had much more tone and better adjustment of parts had they used vibrators of double the size of the present ones. A very great difficulty has been so to arrange the parts as to bring them into a convenient compass, as regards the size of the case, and to get sufficient sound-distance, and the best forms and sizes of cells to fit the case and keys, and to produce the right quality and quantity of tone — all of which the inventors aver they can now master to perfection. — *N. Y. Tribune.*

ORGAN BLOWN BY WATER POWER.

The following is a description of the means employed in the Cathedral of Carlisle (England) for blowing the organ by the application of water power:

The water is collected in two cisterns or tanks, placed in the roof over the south aisle, and is drawn from the reservoir supplying the town. From these cisterns the water passes down a pipe, into two cylinders, like those of a steam-engine, standing in a hole, apparently dug to obtain a greater fall of the water. Exactly over these cylinders are two feeders, made like the reservoirs of the organ bellows, each having a diaphragm, or middle leaf, which is moved up and down by means of the pistons. Attached to these leaves are two rods, which pass down to two beautifully-made and very large cocks. The reciprocating motion is attained by one cylinder operating upon the cock of the other; and the blast of air obtained by these feeders is continuous, but varied by a steam equilibrium throttle-valve, which the reservoir of the bellows closes as it becomes thoroughly inflated. The engine is under the immediate control of the organist by suitable gearing leading to valves in the cistern.

RESTORATION OF TARNISHED SILVER.

Sometimes silver instruments become so completely tarnished and discolored, that by no ordinary means can they be cleansed. Professor Böttger states that by electrolysis their color can be restored in an incredibly short period. To effect this a saturated solution of borax in water, or a moderately strong solution of caustic potassa, is brought into a state of active ebullition; and with this the discolored object, laid in a zinc sieve-like vessel, is moistened. If a zinc sieve be not at hand, we may attain the same end by touching the object, when it has been dipped in the boiling fluid, with a zinc rod.

ON THE DURABILITY OF ZINC WHITE.

A curious lawsuit has been tried in Paris during the past year. M. Gudin, the well-known French marine painter, demanded 800*l.* damages from a tradesman, for having sold his canvasses prepared with white of zinc, which is a substance so injurious to oil colors, that several of his paintings became, in a comparatively short time, cracked and spoiled. In support of his demand, he stated that one of his paintings, a *View on the Coast of Asia*, had been returned to him, and he had had to restore 320*l.*, the amount received for it; and that after painting three others, for which he was to have received 700*l.*, he had not been able to deliver them. The court awarded M. Gudin an indemnity of 480*l.*

RAZOR PAPER.

This article supersedes the use of the ordinary strop; by merely wiping the razor on the paper, to remove the lather after shaving, a keen edge is always maintained without further trouble; only one caution is necessary, that is, to begin with a sharp razor, and then the paper will keep it in that state for years. It may be prepared thus:

First procure oxyd of iron (by the addition of carbonate of soda to a solution of persulphate of iron), well wash the precipitate, and finally leave it of the consistency of cream. Secondly, procure some good paper, soft, and a little thinner than ordinary printing paper; then, with a soft brush, spread over the paper (on one side only), very thinly, the moist oxyd of iron; dry it, and cut into pieces two inches square. It is then fit for use.

ASSYRIAN CIVILIZATION.

Sir Henry Rawlinson, the eminent oriental scholar, in a recently published communication, thus concludes a sketch of the range of Assyrian civilization:

"Among them (the ornaments) are some which anticipate inventions believed till lately to have been modern. Transparent glass (which, however, was known also in ancient Egypt) is one of these; but the most remarkable of all is the lens discovered at Nimrud, of the use of which as a magnifying agent there is abundant proof. If it be added to this, that the buildings of the Assyrians show them to have been well acquainted with the principle of the arch, that they constructed aqueducts and drains, that they knew the use of the lever and roller, that they understood the arts of inlaying, enamelling, and overlaying with metals, and that they cut gems with the greatest skill and finish, — it will be apparent that their civilization equalled that of almost any ancient country, and that it did not fall immeasurably behind the boasted achievements of the moderns. With much that was barbaric still attaching to them, with a rude and inartificial government, savage passions, a debasing religion, and a general tendency to materialism, they were, towards the close of their empire, in all the arts and appliances of life, very nearly on a par with ourselves; and thus their history furnishes a warning — which the records of nations constantly repeat — that the greatest material prosperity may coëxist with the decline, and herald the downfall, of a kingdom."

ON SCIENCE AS A BRANCH OF EDUCATION.

The following is an abstract of a lecture on the above subject, recently delivered before the Royal Institution, London, by Professor Faraday. The high position of this gentleman always secures attention for his opinions; but upon this topic especially, his views will be examined with great interest.

The development of the applications of physical science in modern times has become so large, and so essential to the well-being of man, that it may justly be used, as illustrating the true character of pure science, as a department of knowledge, and the claims it may have for consideration by governments, universities, and all bodies to whom is confided the fostering care and direction of learning. As a branch of learning, men are beginning to recognize the claim of science to its own particular place; for, though flowing in channels utterly different in their course and end to those of literature, it conduces not less, as a means of instruction, to the discipline of the mind; whilst it ministers, more or less, to the wants, comforts, and proper pleasure, both mental and bodily, of every individual of every class in life. Until of late years the education for, and recognition of, it, by the bodies which may be considered as giving the general course of all education, have been chiefly directed to it only as it could serve professional services,—namely, those which are remunerated by society; but now the fitness of university degrees in science is under consideration, and many are taking a high view of it, as distinguished from literature, and think that it may well be studied for its own sake,—*i. e.*, as a proper exercise of the human intelligence, able to bring into action and development all the powers of the mind. As a branch of learning, it has, without reference to its applications, become as extensive and varied as literature; and it has this privilege, that it must ever go on increasing. Thus it becomes a duty to foster, direct, and honor it, as literature is so guided and recognized; and the duty is the more imperative, as we find by the unguided progress of science and the experience it supplies, that of those men who devote themselves to studious education, there are as many whose minds are constitutionally disposed to the studies supplied by it, as there are of others more fitted by inclination and power to pursue literature. The value of the public recognition of science as a leading branch of education may be estimated in a very considerable degree by observation of the results of the education which it has obtained incidentally from those who, pursuing it, have educated themselves. Though men may be specially fitted by the nature of their minds for the attainment and advance of literature, science, or the fine arts, all these men, and all others, require first to be educated in that which is known in these respective mental paths; and when they go beyond this preliminary teaching, they require a self-education directed (at least in science) to the highest reasoning power of the mind. Any part of pure science may be selected to show how much this private self-teaching has done, and by that to aid the present movement in favor of the recognition generally of scientific education in an equal degree with that which is literary; but perhaps electricity, as being the portion which has been left most to its own development, and has produced as its results the most enduring marks on the face of the globe, may be referred to. In 1800 Volta discovered the voltaic pile — giving a source and form of electricity before unknown. It was not an accident, but resulted from his own mental self-education. It was, at first, a feeble instrument, giving feeble results; but, by the united mental exertions of other men, who educated themselves through the force of thought and experiment, it has been raised up to such a degree of power as to give us light, and heat, and magnetic and chemical action, in states more exalted than those supplied by any other means. In 1819 Oersted discovered the magnetism of the electric current, and its relation to the magnetic needle; and as an immediate consequence, other men, as Arago and Davy, instructing themselves by the partial laws and action of the bodies concerned, magnetized iron by the current. The results were so feeble at first as to be scarcely visible; but, by the exertion of self-taught men since then, they have been exalted so highly as to give us magnets of a force unimaginable in former times. In 1831 the induction of electrical currents one by another, and the evolution of electricity from magnets, was observed, — at first in results so small and feeble that it required one much instructed in the pursuit to perceive and lay hold of them; but these feeble results, taken into the minds of men already partially educated and ever proceeding onwards in their self-education, have been so developed as to supply sources of electricity independent of the voltaic battery or the electric machine, yet having the power of both combined in a manner and degree which they, neither separate nor together, could ever have given it, and applicable to all the practical electrical purposes of life.. To consider all the departments of electricity fully, would be to lose the argument for its fitness in subserving education in the vastness of its extent; and it will be better to confine the attention to one application, as the electric telegraph, and even to one small part of that application, in the present case. Thoughts of an electric telegraph came over the minds of those who had been instructed in the nature of electricity as soon as the conduction of that power with extreme swiftness through metals was known, and grew as the knowledge of that branch of science increased. The thought, as realized at the present day, includes a wonderful amount of study and development. As the end in view presented itself more and more distinctly, points, at first apparently of no consequence to the knowledge of the science, generally rose into an importance which obtained for them the most careful culture and examination, and the almost exclusive exercise of minds whose powers of judgment and reasoning had been raised first by general education, and who, in addition, had acquired the special kind of education which the science in its previous state could give. Numerous and important as the points are, which have been already recognized, others are continually coming into sight as the great development proceeds, and with a rapidity such as to make us believe that, much as there is known to us, the unknown far exceeds it; and that, extensive as is the teaching of method, facts, and law, which can be established at present, an education looking for far greater results should be favored and preserved. The results already obtained are so large as even in money value to be of very great importance; — as regards their higher influence upon the human mind, especially when that is considered in respect of cultivation, I trust they are, and ever will be, far greater.

No intention exists here of comparing one telegraph with another, or of assigning their respective dates, merits, or special uses. Those of Mr. Wheatstone are selected for the visible illustration of a brief argument in favor of a large public recognition of scientific education, because he is a man both of science and practice, and was one of the very earliest in the field, and because certain large steps in the course of his telegraphic life will tell upon the general argument. Without referring to what he had done previously, it may be observed that, in 1840, he took out patents for electric telegraphs, which included, amongst other things, the use of electricity from magnets at the communicator,— the dial face,— the step-by-step motion,— and the electro-magnet at the indicator. At the present time, 1858, he has taken out patents for instruments containing all these points; but these instruments are so altered and varied in character above the former that an untaught person could not recognize them. The changes may be considered as the result of education upon the one mind which has been concerned with them, and are to me strong illustrations of the effects which general scientific education may be expected to produce. In the first instruments powerful magnets were used, and keepers, with heavy coils, associated with them. When magnetic electricity was first discovered, the signs were feeble, and the mind of the student was led to increase the results by increasing the force and size of the instruments. When the object was to obtain a current sufficient to give signals through long circuits, large apparatus were employed, but these involved the inconveniences of inertia and momentum; the keeper was not set in motion at once, nor instantly stopped; and, if connected directly with the reading indexes, these circumstances caused an occasional uncertainty of action. Prepared by its previous education, the mind could perceive the disadvantages of these influences, and could proceed to their removal; and now a small magnet is used to send sufficient currents through 12, 20, 50, 100, or several hundred miles; a keeper and helix is associated with it, which the hand can easily put in motion; and the currents are not sent out of the indicating instrument to tell their story, until a key is depressed, and thus irregularity contingent upon first action is removed. A small magnet, ever ready for action and never wasting, can replace the voltaic battery; if powerful agencies be required, the electro-magnet can be employed without any change in principle or telegraphic practice; and as magneto-electric currents have special advantages over voltaic currents, these are in every case retained. These advantages I consider as the result of scientific education, much of it not tutorial, but of self: but there is a special privilege about the science branch of education, namely, that what is personal in the first instance immediately becomes an addition to the stock of scientific learning, and passes into the hands of the tutor, to be used by him in the education of others, and enable them, in turn, to educate themselves. How well may the young man, entering upon his duties in electricity, be taught, by what is past, to watch for the smallest signs of action, new or old; to nurse them up by any means until they have gained strength; then to study their laws, to eliminate the essential conditions from the non-essential, and, at last, to refine again, until the encumbering matter is as much as possible dismissed, and the power left in its highly developed and most exalted state. The alterations or successions of currents, produced by the movement of the keeper at the communicator, pass along the wire to the indicator at a distance; there each one for itself confers a magnetic condition on a piece of soft iron, and renders it attractive or repulsive of small, permanent magnets; and these, acting in turn on a propelment, cause the index to pass at will from one letter to another on the dial face. The first electro-magnets, *i. e.*, those made by the circulation of an electric current round a piece of soft iron, were weak; they were quickly strengthened, and it was only when they were strong that their laws and actions could be successively investigated. But now they were required small, yet potential. Then came the teaching of Ohm's law; and it was only by patient study, under such teaching, that Wheatstone was able so to refine the little electro-magnets at the indicator as that they should be small enough to consist with the fine work there employed, able to do their appointed work when excited in contrary directions by the brief currents flowing from the original common magnet, and unobjectionable in respect of any resistance they might offer in the transit of these tell-tale currents. These small transitory electro-magnets attract and repel certain permanent magnetic needles, and the to-and-fro motion of the latter is communicated by a propelment to the index, being there converted into a step-by-step motion. Here everything is of the finest workmanship; the propelment itself requires to be watched by a lens, if its action is to be observed; the parts never leave hold of each other; the vibratory or rotary ratchet-wheel and the fixed pallets are always touching, and thus allow of no detachment, or loose shake; the holes of the axes are jewelled; the moving parts are most carefully balanced, — a consequence of which is, that agitation of the whole does not disturb the parts, and the telegraph works just as well when it is twisted about in the hands, or placed on board a ship, or in a railway carriage, as when fixed immovably.

Now there was no accident in the course of these developments; — if there were experiments, they were directed by the previously acquired knowledge; — every part of the investigations was made and guided by the instructed mind. The results being such (and like illustrations might be drawn from other men's telegraphs, or from other departments of electrical science), then, if the term education may be understood in so large a sense as to include all that belongs to the improvement of the mind, either by the acquisition of the knowledge of others, or by increase of it through its own exertions, we learn by them what is the kind of education science offers to man. It teaches us to be neglectful of nothing; — not to despise the small beginnings, for they precede, of necessity, all great things in the knowledge of science, either pure or applied. It teaches a continual comparison of the small and great, and that under differences almost approaching the infinite: for the small as often contains the great in principle as the great does the small; and thus the mind becomes comprehensive. It teaches to deduce principles carefully, to hold them firmly, or to suspend the judgment — to discover and obey *law*, and by it to be bold in applying to the greatest what we know of the smallest. It teaches us first by tutors and books to learn what is known to others, and then, by the lights and methods which belong to science, to learn for ourselves and for others; — so making a fruitful return to man in the future for that which we have obtained from the men of the past. Bacon, in his instruction, tells us that the scientific student ought not to be as the ant, who gathers, merely; nor as the spider, who spins from her own bowels; but rather as the bee, who both gathers and produces. All this is true of the teaching afforded by any part of the physical science. Electricity is often called wonderful — beautiful; — but it is so only in common with the other forces of nature. The beauty of electricity, or of any other force, is not that the power is mysterious and unexpected, touching every sense at unawares in turn, but that it is under *law*, and that the taught intellect can even now govern it largely. The human mind is placed above, not beneath it; and it is in such a point of view that the mental education afforded by science is rendered supereminent in dignity, in practical application, and utility: for, by enabling the mind to apply the natural power through law, it conveys the gifts of God to man.

NATURAL PHILOSOPHY.

ROTATION PRODUCED BY ELECTRICITY.

At a recent meeting of the Royal Society, England, an ingenious and curious apparatus was exhibited, displaying the rotation of a metallic sphere by electricity. The apparatus was contrived by Mr. Gore, of Birmingham, who states that his experiments had their origin in a phenomenon observed by Mr. Fearn, of Birmingham, in his electro-gilding establishment. — When a tube of brass, half an inch in diameter and four feet long, was placed upon two horizontal and parallel brass tubes, one inch in diameter and nine feet long, and at right angles to them, and the latter connected with a long voltaic battery consisting of from two to twelve pairs of large zinc and carbon elements, this transverse tube immediately began to vibrate, and finally to roll upon the others. Acting upon this, Mr. Gore constructed a disk of wood, provided with two brass rails, level, uniform, and equi-distant; on these rails a hollow and very thin copper ball was placed, and the brass rails being connected with a zinc and carbon battery, the ball began to vibrate, and presently to revolve. In all cases yet observed, Mr. Gore states, that the motion of the ball is attended by a peculiar crackling sound at the points of contact, and by heating of the rolling metal. When the apparatus was exhibited before the Royal Society, electric sparks were seen as the ball rolled from the spectator.

ELECTRICITY OF NERVES AND MUSCLES.

M. de la Rive, in the third volume of his *Treatise on Electricity*, just published, reviews the whole science of electro-physiology; and reminds practitioners that, as the difference between the electricity of the muscles and of the nerves is now clearly established, so must they be careful in applying their remedies, not to waste on the muscles, which are the best conductors, the electric currents intended solely for the nerves.

ACCIDENTS BY LIGHTNING.

From a recent foreign work, "Boudin on Medical Geography," we derive the following memoranda respecting accidents from lightning. As compared with the country, towns, and especially the larger and more populous ones, appear to possess an immunity from accident to life by lightning. Thus, between 1800 and 1851, not a single death by lightning was recorded in Paris; and in 1786 it was calculated that out of 750,000 deaths in London, during thirty years, two only had been produced by this agency. During a century, only three persons were killed by lightning in Göttingen, and two in Halle. Comparing these numbers with the total deaths from this cause, and with the fact that twenty-five per cent. of all happen under trees, he holds it reasonable to conclude "that lightning finds more victims in open country than in cities." Another "most curious phenomenon, beyond contradiction, is the tendency it has to strike the same places and the same edifices at different epochs." Of this, Dr. Boudin produces several illustrations, and quotes M. Poullet in support and explanation.

With regard to the frequency of accidents by lightning, fatal to human life in France, he tells us that from 1835 to 1852, inclusive, 1308 persons were killed.

M. Boudin thinks that the persons injured are at least twice as numerous as those killed. Some United States statistics show the injured to be to the killed as five to one. Many more men than women are killed, and not in France only, but also, though not in so marked a proportion, in Sweden (1815 to 1840), and in England (1838 and 1839). He seems to think that this is not explained by the greater exposure of men in the fields; but still he does not think we are warranted in concluding "that, all things being equal, woman runs less danger than man;" but he considers the question as "worthy of being submitted to the test of observation." And he quotes the following peculiar passage from Arago, declining, however, to "maintain its rigorous exactitude":

"In two conditions altogether alike," says Arago, "one man, by the nature of his constitution, runs more danger than another. There exist persons who arrest abruptly the communication of electricity, and do not feel the shock, even when they occupy the second place in the file. These persons, by exception, are not conductors of the electric fluid. Exceptionally, then, we must rank them amongst non-conducting bodies, which lightning respects, or which, at least, it strikes rarely. Differences so marked cannot exist, without there being also shades of difference; but every degree of conductibility corresponds, during the storm, to a certain measure of danger. The man who is as conducting as metal will be as often struck as metal; the man who interrupts the communication to the chain will scarce have more to fear than if he were glass or resin. Between these limits there will be found individuals whom the lightning will strike as readily as wood or stone. Thus, in the phenomena of thunder, all does not depend on the place which a man occupies; *the physical constitution of the man plays also a certain part.*"

As one would expect, "the configuration of the soil, and its mountainous character," exercise an influence on the frequency of accidents, which, for instance, in proportion to the population, are much rarer in the departments of the Eure and Seine than in those of the Dordogne, Lozere, High Loire, and Low and High Alps. Less danger is run in the house than in the fields, and in towns than in the country.

The effects of lightning on man he makes either curative of preëxisting affections, productive of wounds or injuries, or productive of death. The injuries it may produce seem to be very varied.

To the peculiar images, said to have been observed on the bodies of some persons killed by lightning, he gives the name of Keraunographic images, and he relates some of the most singular instances of it on record, giving the sources, which are not always the most reliable.

PHOTOGRAPHIC EFFECTS OF LIGHTNING.

Statements of impressions of trees, etc., made on the human person by lightning, are not uncommon, and Mr. Poey, of Havana, has published a paper of some length on the subject. (See Annual of Scientific Discovery, 1858, page 226.) In a case of a person struck by lightning, at Salem, Mass., during the past summer, it was currently reported, "that upon his back there was left an impression of a larch tree, situated just outside the window at which he was sitting." The attendant physician has, however, published the following observations on the phenomenon in question:

"There was no laceration, or abrasion of the skin. The appearance was something like what is often seen, of a frosty morning, on the window glass, resembling branches of trees, and was produced by the peculiar action of the lightning on the capillary vessels of the skin, causing them to become enlarged and reddened, in consequence of admitting more blood than usual, and to assume an arborescent character. This appearance was not the fac simile of any tree or bush. The whole surface affected was about ten inches square."

This explanation appears to satisfactorily meet the facts of this particular case; but, as instances are cited by Mr. Poey in which objects other than trees have been delineated on the skin through the agency of lightning, the photographic effects of this agent cannot, therefore, be entirely disputed.

LIGHTING GAS BY ELECTRICITY.

Samuel Gardner, jr., of New York, patented in 1857 an electric apparatus, by means of which a person acting on two keys could light or shut off at will, and at the same moment, all the gas-burners of a building, or any designated number of them. It was applied to the lights of the Broadway Theatre, and was made to work several times every evening, to the great amusement of the audience. The stop-cock of every chandelier, and of every isolated burner, is provided with a rachet-wheel, which is acted upon by a catch connected with an ordinary electro-magnet, and each magnet is connected by a wire with a battery, and with a circuit-breaking key, placed in the operator's room. Over every burner is a coil of fine platina wire; and all these coils, connected together by copper wires, are in the circuit of another electric current, which may be closed or opened by means of another circuit-breaking key. To light the gas, the operator closes the circuit of the coils of platina; these become red hot. He then closes and opens the stop-cocks' circuit as many times as is necessary to make the rachet-wheels describe a quarter of a circle. The stop-cocks are then opened, and the gas, rushing on the burning coils, is lighted. The burners are turned off by playing again on the key till the rachet-wheels have moved another quarter of a circle; then the stop-cocks are closed. By having as many keys as there are burners, or groups of burners, each burner or each group may be operated separately from the others. By throwing all in one circuit, they may be operated with a single key. The use of this invention does away with the causes of fire consequent upon the use of matches; it saves the labor of lighting, and an unnecessary expense of gas in large establishments, where the lighting has to be begun one hour before light is wanted. In the streets it is peculiarly advantageous, as a burner accidentally put out by a puff of wind, is instantly lighted again. An improvement on this invention has been patented, March, 1858, by the original inventor. It consists in placing the platina coil by the side of the burner, instead of above it, and in the flame. The use of platina, though very costly, is necessary, as it is the cheapest metal which does neither melt nor burn under the circumstances described. The apparatus, thus improved, has been lately applied to the 1,500 burners of the Senate Chamber in Washington, and is said to give complete satisfaction.

GATCHELL'S LIGHTNING RODS AND POINTS.

A committee of the Franklin Institute recommend the following improvements in the construction of lightning rods, introduced by Mr. J. L. Gatchell, of Maryland. These improvements consist, first, in the use of a rope of twisted copper wire (containing eighteen strands of wire, about $\frac{1}{16}$ inch in diameter), by which are gained greater conducting power, freedom from breaks or joints in the conductor, and a flexibility which allows it to be adapted to any irregularities of form over which it may be carried.

The second modification is the substitution of a copper for a platina point, and the increasing the angle of the point, so as to approach that recommended by the Committee of the French Academy of Sciences. The advantages here gained are, greater conducting power by the substitution of copper for platina, and secondly, a counteraction of the liability to fusion by rendering the point much less acute. The preservation from oxidation is entrusted to a zinc ball attached below the point.

ON THE ELECTRICAL LIGHT. BY H. W. DOVE.

The experiments, in connection with the results of the prismatic investigation of the spark, appear to me to lead to the following conclusion:

A wire, becoming red-hot by heat, is first red, then orange, and lastly white; so that it behaves like the combination of light which is obtained when a screen is drawn away from the spectrum concealed by it, in such a way that the red end first becomes visible, and to this the violet is finally added. The increase of brilliancy, from the slightly luminous brush to the bright spark, behaves quite otherwise. In this case, it is as if the screen removed first set free the violet end, and then the other colors. This distinction of itself renders it improbable that the phenomena of electrical light, in the state of less brilliancy, can be ascribed to a gradually increasing ignition of solid particles. They rather resemble the weakly luminous flame of hydrogen, which becomes white by solid ignited carbon in the so-called gas-flames, or by other solid matters, as in the Drummond light. The true electrical light is produced at great distances in the surrounding, isolating, aëriform medium, when the latter is attenuated. With this colored light belonging to the strongly refrangible part of the spectrum, phenomena of ignition may be combined, by particles torn away from the positive and negative bodies. If these particles be only at a red heat, the impression of a violet light is produced by their mixture with the electric light. To this class belong the column of light in the electrical egg, and the basal point of the brush, and, lastly, the indented reddish sparks of an electrical machine, at distances to which a white spark does not pass. If particles at a white heat come together, the whole is white, as in the sparks of Leyden jars; in opposition to the bright light of incandescence, the less strongly luminous electric light disappears in the same way as the weak, bluish, lower part in a gas-flame appears black in opposition to the bright mass of light, whilst with the small brilliancy of a wax-light, the latter betrays its color even without optical aids of absorption. Only prismatic analysis, and the action upon uranium glass, indicate the presence of the electric light also. If the particles at a white heat do not reach each other, the spark acquires a spot of interruption, which, however, still shows red light besides the true electric light, when the particles previously at a white heat have become cooled to redness. The basal point of the brush, which retrogrades in proportion to the larger field in which the electric light becomes visible, is to be compared with the spot of interruption of the spark; the particles of the solid body which are here still red-hot may, on reaching a greater distance, be completely extinguished, so that then the electric light alone prevails. The brush could not be colored by a spirit-flame colored yellow with chloride of sodium held under it, as it then becomes converted into a spark. The phenomena of the exhausted tube with mercury, indicate the modification which the electric light undergoes in media other than atmospheric air.—*Phil. Mag.*

ELECTRO-MOTIVE FORCE OF VARIOUS BATTERIES.

M. Petruscheski, a Russian experimenter, gives the following as the results of his investigations on the power of different voltaic combinations:

Grove, with amalgamated zinc,	1.78
Battery of cast iron and amalgamated zinc,	1.72
Bunsen,	1.69
Eisenlohr (Daniell's, with tartrate of potassa in place of sulphuric acid),	1.05
Daniell, with chloride of sodium,	1.05
" chloride of sodium and amalgamated zinc,	1.01
" with dilute sulphuric acid,	1.00
Eisenlohr, with zinc not amalgamated,	0.99
Daniell, dilute sulphuric acid and amalgamated zinc,	0.93
Wollaston, with amalgamated zinc,	0.93

Cosmos, vol. xli., p. 4.

COST OF ELECTRIC LIGHT.

M. Edmond Becquerel has been recently engaged in some experiments with a view to determine the comparative cost of electricity as an illuminating agent. He used a battery of zinc and platinum, made with strict attention to economy, and the results were as follows:

The standard is the light of 350 candles of the best quality, and the comparative cost of

Coal gas at $1 60 per 1000 c. feet, was	$0 35
Oil (Rape Seed), at 17 cents per pound,	0 60
Stearine candles, at 32 cents per pound,	2 52
Wax candles, at 52 cents per pound,	3 12
Electric light,	0 58

Thus showing that, although the electric light is cheaper than candles, it will not at present compete with coal gas, at least until some cheaper battery power be found.

EXPERIMENTS OF ANDREW CROSSE.

The very curious experiments of Andrew Crosse (made famous by republication in the "Vestiges of Creation"), by which animal life seemed to be produced by the action of any continued electrical currents, have recently been repeated by Professor Schulze of Germany. No insects or animal germs, however, made their appearance, — a result which strengthens the probability of the supposition which Mr. Crosse himself never disputed, that the ova of the insects — however the electric current may have operated to stimulate their development — were derived from the atmosphere, or had been conveyed into the apparatus by some natural means which had escaped the attention of the experimenters.

Mr. Crosse, after a life spent in electrical experiments, died July 6, 1855, at the age of 71, and his "Memorials, Scientific and Literary," lately published in London, contain some curious details as to his investigations above referred to, and the way in which they were received by the public.

Being engaged at the time in experiments for the production of mineral crystals by the agency of the voltaic current, in which he had remarkable success, he contrived a little apparatus for the deposition of crystals of silica on a lump of stone, through the agency of a voltaic trough. After this experiment had been going on for a fortnight, he observed a few small whitish specks on the surface of the electrified stone. By the eighteenth day, these specks had expanded, and seven or eight filaments were thrown out from the surface of each; but as embryo minerals exhibited similar phenomena in the process of crystallization, there was nothing so far to excite any surprise. Before long, however, these growing specks assumed the appearance of insects standing erect on the bristles which formed their tails, and by the twenty-eighth day they were distinctly seen to move their legs. By this time the experimenter was greatly astonished. Instead of a mineral for which he had looked as the result of his experiment, he had found an animal, alive and kicking. It was plain they were no mere appearances, for in a few days they detached themselves from the stone and began to move about. They were, to be sure, not creatures of a very inviting and attractive character, for they belonged apparently to the genus *acarus*, which includes some of the most disgusting parasites with which the animal body is annoyed. But they continued to increase, and in the course of a few weeks, at least a hundred made their appearance. Whence did they come? and what was their origin? To these questions, Mr. Crosse, with all his faith in the power of electricity, did not then venture and has not since ventured a decided answer. Many years after, for the experiment was first tried in 1807, he professed himself still unable to form an opinion. He expresses himself thus: "The simplest solution of the problem which occurred to me was that they rose from ova deposited by insects floating in the atmosphere and hatched by electric action. Still I could not imagine that an ovum could shoot out filaments, or that those filaments could become bristles; and moreover, I could not detect, on the closest examination, the remains of a shell. Again, we have no right to assume that electric action is necessary to vitality until such fact shall have been most distinctly proved. I next imagined, as others have done, that they might have originated from the water, and consequently made a close examination of numbers of vessels filled with the same fluid. In none of these could I perceive a trace of an insect, nor could I see any in any other part of the room."

The experiments were repeated in various ways, and with every precaution that could be thought of, yet the insects still appeared, and that, too, under circumstances apparently highly adverse to the development of animal life. They made their appearance under the surface of liquids in which they could not afterward live, even in fluids that were caustic or absolutely poisonous. Though the solid materials employed had been subjected to a heat greater than that of molten iron, and the solutions used had been poured while boiling into the apparatus, still these strange insects made their appearance; nor did an atmosphere impregnated with chlorine or loaded with muriatic acid gas prove any bar to their production. Similar experiments were afterwards undertaken by Mr. Weeks, of Sandwich, with still greater precaution, if possible, to exclude every exterior element of animal life, but still in the end — though a period of twelve or eighteen months sometimes elapsed — the insects appeared.

The publication of these experiments caused a great deal of talk, much of which took the shape of a direct personal attack upon the unlucky philosopher. In the true spirit of the middle ages, which long confounded experimental philosophy with impiety, Mr. Crosse was arraigned as an impious man. If he began by creating animals by electrical power — no matter of how inferior a sort — who could tell where he might stop? It was a plain usurpation of the functions of Deity. Mr. Crosse must certainly be an atheist. Letters were addressed to him in which he was denounced as "a disturber of the peace of families," and a "reviler of our holy religion." "I have met," says Mr. Crosse, "with so much virulence and abuse, so much calumny and misrepresentation, in consequence of these experiments, that it seems in this nineteenth century as if it was a crime to have made them." In fact, he found himself obliged to come out with a public declaration that he was neither an atheist nor a materialist, nor a self-imagined creator, but a humble and lowly reverencer of that great Being, of whose laws those who accused him seemed to have lost sight.

ON THE USE OF ELECTRICITY FOR PRODUCING LOCAL ANÆSTHESIA.

The application of electricity for producing local anæsthesia, as in tooth-pulling, has been recently made with marked success. The arrangement for using or applying this agent is simple, and consists of the common electro-magnetic machine used in medical electricity, a single cell and pair of plates constituting a Smee's battery, and a small electro-magnetic coil with a bundle of wires for graduating the strength of the current. One end of the thin wire conveying the secondary current is attached to the handle of the forceps, and the other end of it to a metallic handle to be placed in the hand of the patient. The instrument touching the tooth completes the circuit, and the current passes instantaneously. The wire attached to the forceps should be made to pass through an interrupting footboard, so that the continuity of the wire may be made or broken in an instant by a movement of the right foot of the operator. The advantage of this arrangement is, that it allows the instrument to be placed in the mouth without risk of producing a shock in coming in contact with the lips, cheeks, or the tongue, which would interfere with the quiet of the patient. A hole drilled in the end of the left handle of the forceps, and the end of the wire tapered to fit rather tightly, allows the substitution of one pair of forceps for another with scarcely a moment's delay.

IMPROVEMENT IN ELECTROTYPING.

The *National Intelligencer* says an improvement in the process of electrotyping has been made, by which electrotypes can be produced with great rapidity and accuracy. The improvement consists in covering the face of the wax, or other material of which the matrix is made, with fine metallic leaf before the impression is taken. In this way a perfect conducting metallic surface is obtained; that is, over the entire face of the letters, as well as over the spaces between the lines.

The sides of the letters do not, as a general thing, have a metallic conducting surface, inasmuch as the types, when the impression is taken, cut the leaf, and force a part of it down into the matrix, thus leaving the wax exposed on the sides of the letters. This cutting of the leaf, however, is rather an advantage, since such exposed parts of the wax are the very parts where a slow deposit is preferred, and which is effected by touching such parts over with plumbago. The advantages are these: The moment that the mould or matrix is placed in the bath and the battery applied, the deposit of metal commences at once on the entire surface, — the deposit being more rapid, however, on the face of the letters, and on the spaces between the lines than on the sides of the letters; and this is just what is wanted, since it prevents, especially when the letters are small and deep, what is termed "bridging over" (hollow letters). By the use of silver leaf an electrotype may be produced with a bright silvered face, — a feature of considerable importance in all cases where the plates are to be laid aside for future use, inasmuch as the face of the letters will not be so easily injured by long and continued exposure to air and moisture, as when of the usual copper face.

ELECTRIC DISCHARGES IN AIR HIGHLY RAREFIED.

On making a current of static electricity to pass through a tube of rarefied air, a luminous arc is obtained, which experiences modifications, when subjected to the action of the poles of a powerful magnet. This fact, which calls to mind the corresponding effect experienced by a luminous arc produced by a powerful galvanic battery, was discovered by De la Rive in 1849. He first used as the source of the electricity the hydro-electric machine of Armstrong, afterwards a common electric machine, and quite recently Ruhmkorff's apparatus. M. Plücker, of Bonn, has tried the same, and his results are published in a recent number of Poggendorff's Annalen.

According to De la Rive, it is necessary for success that the tube or globe should contain some vapor, equivalent in tension to six millimeters of mercury, and the vapors answering best are those of alcohol, sulphuret of carbon, and camphine. De la Rive has applied the experiments to the illustration of the Aurora Borealis, so frequent in the Polar regions.

ON A MODIFICATION OF RUHMKORFF'S INDUCTION COIL.

At the last meeting of the British Association, Mr. W. Ladd, presented the results of a very extensive course of experimenting with Ruhmkorff's induction coils, with a description of the machine, as it is now constructed. His object, he said, was not to make very large machines, but to obtain the greatest results from a three-mile coil, that being sufficiently large for all ordinary purposes. I find the best length for the iron core to be thirteen inches and about 15·8 diameter, composed of fine iron wire not larger than No. 22, very carefully annealed. The primary wire should be of sufficient size to carry freely the whole of the battery current, and of sufficient quantity to thoroughly saturate the iron core with magnetism. For this purpose I use three layers of one continuous No. 12 copper wire carefully annealed; if more layers are used I find that the secondary wire is removed too far from the magnetic influence. The secondary wire ought not to be larger than No. 35, covered with silk, which must be laid on perfectly even and insulated from the primary wire, and also from the layers of the secondary next to it. I find the best insulating medium to be the thinnest gutta-percha made, and which, I believe, to be the only gutta-percha sold which cannot be adulterated; it is true that it has many minute preforations, but by laying on, at least, six thicknesses between each layer of wire, perfect insulation is secured. The greatest care must be taken in protecting the ends of the layers so as to prevent the sparks passing from one to the other. The condenser should be, at least, fifty sheets of tin-foil of about one square foot in size. These sheets must be separated from each other by three sheets of varnished paper or gutta-percha tissue. Every alternate sheet of foil is connected together, thus forming two poles, to be attached one to each side of the break. It may be placed at the bottom of the stand or in a separate box; the latter I prefer. In developing the power of the machine, everything depends upon the contact breaker, which should be capable of retaining contact until the whole of the magnetism is obtained, and capable also of breaking contact as soon as the smallest quantity is induced. *These results are obtained in the break attached to this instrument.* The hammer is made to vibrate freely between the core and the coil, and the brass screw terminating with the platina plate at the back of the hammer, a very small amount of magnetism will be sufficient to attract the hammer and so break the contact. If now I bring this screw (placed half-way up the spring carrying the hammer) to bear upon the spring, it will have the effect of pressing the two platina plates together, so that it takes a greater amount of magnetism to separate them. By this means I can regulate the power of the instrument to the purposes for which it is required. The battery I employ is a five cells of "Grove's," with immersed platina plates 5×3, having an exposed surface of 140 square inches. With such a battery and a coil thus constructed, I can always insure sparks from half an inch to four inches in air. The machine now exhibited contains six miles of wire, and worked with the same battery, gives six and a quarter inch sparks. The position which the induction coil is now taking in this electrical age is one of considerable importance. It has awakened new philosophical ideas, and is being successfully applied to practical purposes of the highest advantage to mankind. For blasting purposes, a three-mile coil is capable of firing fifty charges simultaneously. But, important as its present position is, and successful as its past application has been, it is yet in its infancy, and there can be little doubt that by *patient perseverance* machines can be constructed that will obviate the necessity of employing such ponderous machines, and still more ponderous batteries, as are now at work on the Atlantic Cable.

Jan's Improvement. — In using Ruhmkorff's coils, damage is not unfrequently sustained by the electric spark forcing its way through the apparatus itself, in place of passing between the poles. To avoid this, M. Jan has devised a plan of plunging the apparatus in a non-conducting liquid, such as the spirits of turpentine. The liquid filling the interstices of the coil, constituting the immediate instrument of the induction, assures the perfect isolation of the several convolutions, and if a spark too violent passes through the instrument, the presence of a non-conducting fluid stops its passage and insures safety. M. Quet, thus using the Ruhmkorff machine, has produced results which have hitherto defied the Voltaic battery, and, of course, the ordinary electrical machine.

STUDY OF THE THERMO-MULTIPLIER.

This instrument, valuable to the experimentalist for its extreme delicacy, and for its extensive scale, has been the object of careful theoretic and practical study, by M. de la Provostaye, whose results as reported to the Academy of Sciences, at Paris, may be summed up as follows:

First, as to the galvanometer.

1. Whatever may be the position of the needles, the forces which act upon each half are reducible practically to one, perpendicular to the plane of the meridian.

2. That the amount of deviation makes but a slight change in the amount of this resultant: hence, the apparatus may be regarded as a tangent-needle of a very considerable degree of perfection.

Secondly, as to the thermo-electric pile.

1. The progress of heating the thermometer takes place by the same degrees, and in the same time, as if it were placed in a space at the constant temperature which the pile attains under the influence of the source of heat.

2. When the rise of the temperature is sufficiently small, if we withdraw the calorific action, it cools again, in the same time, and by the same degrees, as if heated.

Thirdly. He terminates the integral expression for the movement of the needle; shows that its position of rest under the action of the current is proportioned to the constant quantity of the current when the anterior face of the pile has assumed a stationary excess of temperature, and to the intensity of the incident heat: and then derives expressions for the times corresponding to the maximum and minimum excursions of the needle, and the extent of these excursions; and terminates with the following observation: "If, after making an observation with the thermo-pile in the common way, and awaiting the fixed deviation of the needle, the screen is replaced, the energy of the current diminishes, and the needle returns to zero. I have found that the retrograde motion of the needle, counted from the fixed deviation, takes place by oscillations of the same extent and times as the primitive motion counted from zero."

HUGHES'S TELEGRAPH.

The following is a full description of this somewhat famous instrument, with its latest improvements, as it has been employed during the past year on one of the lines between New York and Philadelphia. By it, messages are transmitted simultaneously to and fro at the rate of two hundred letters a minute each way. With all other telegraphs, the current runs through the magnet of the instrument, and a sign is transmitted by breaking the current for an instant; with this one, the line is connected with the ground, and the current is made to pass through the machine only for an instant, when a sign is to be transmitted. This arrangement constitutes one of its most important advantages, namely: Any surplus of electricity produced by an overcharged atmosphere has full time to flow into the ground, and Hughes's machine may be operated without danger to the attendants, during a storm which stops all the others. To accomplish this, the magnet is made of a natural horse-shoe magnet capable of sustaining five pounds, in contact with the poles of which are placed two pieces of soft iron, surrounded by coils of wire. The armature is provided with a spring nearly as strong as the magnet, the tendency of which is to pull them apart. When the machine is at rest the armature is in contact with the pieces of soft iron where it is kept. As these have become magnetic from their contact with the natural magnet, when a key is pressed down the current is made to pass through the coil in such a direction as to destroy the natural magnet by creating an artificial one with poles reversed. The armature is thus instantaneously let free, and is thrown up by the spring; this motion acts upon a detent, and the other parts of the instrument do their office in transmitting a letter, and also of cutting off the current from the magnet and of forcing back the armature against it. In this manner the natural magnet is not required to attract the armature from a distance, but acts only, when in contact, to hold it in its place; that is, in the position of its greatest power. The parts of the machine which are in constant motion consist of a horizontal main shaft under which is a vertical shaft, connected with the first by beveled wheels; of a train of cog-wheels, of a drum, weight, spring and treadle. The main shaft of the instruments at both extremities of the line move with exactly the same speed. The velocity of each is regulated, like that of a common clock, by an anchor escapement, with this difference, that the vibrations of a slender bar of steel, held by one extremity, are substituted for the beats of a pendulum. A clock is made to go slow or fast by lengthening or shortening the pendulum, and the velocity is regulated here by doing the same with the bar of steel. This escapement acts about sixty times in a second, or twenty times faster that that of a clock; and this result could not be obtained from a pendulum, as this would have to be only 1-80 part of an inch long from the point of suspension to the centre of the ball, to beat sixty times. The upper portion of the vertical shaft is isolated from the lower portion by an intermediate piece made of ivory. The upper part is provided with an articulated horizontal arm which rests on a shorter arm extending from the lower part, and this contact connects together the two portions of the shaft. This arm describes circles a quarter of an inch above the table. Under its extremity a circular row of twenty-eight slats is cut in the table, and as many metal slides are placed vertically in them. Twenty-eight keys are connected with these slides, in such a manner that, when a key is pressed down, the corresponding slide is raised in the slat sufficiently high to reach the arm, which, in revolving, slides up the inclined end of the slide. This operation makes the current pass through the coils of the other instrument, as will be shown hereafter. The main shaft carries by friction a type-wheel, the types of which are inked by a small tangential inking roller. A second horizontal shaft moving with the first, but faster, is provided with a chuck, by means of which it carries the printing shaft once round each time a letter is telegraphed. This printing shaft, by means of a projecting cam, brings at each turn the roller which carries the paper in contact with the type-wheel, and the letter actually there is printed. The slip of paper is carried onward the distance of a letter by a dog acting on a ratchet, when the roller recedes from the type-wheel.

All the parts having been described separately, we will now explain their connection, together with the manner of using the machine. The instruments at both stations are first started, and made to revolve at the same rate, the type-wheels lying in such positions that each time the arm of the vertical shaft of one instrument passes over the slide of key A, the letter A engraved on the type-wheel of the other instrument is opposite the paper roller. To print a letter at the other end of the line, the operator presses down the key on which the letter is engraved; the key raises the corresponding metal slide which raises the revolving arm before one half of a second has elapsed, since the arm makes two revolutions per second. This makes the current pass through the magnet of the instrument at the other station and release its armature, which springs up. The detent acts instantly, makes the chuck catch the printing shaft, and this last raises the printing roller against the type-wheel, the letter is printed, and the armature coming down, the printing shaft is unconnected, and every part returns to its original position except the paper, which has proceeded the distance of one letter forward.

The closing of the current at one station acts only on the magnet of the other station. This allows of the writing both ways at the same time. Generally the despatches travelling in opposite directions are interwoven; and also two different letters may appear to be, the one received and the other sent at the same moment; one, in fact, starts only after the other is arrived and the way is clear; but, each time the same letter is transmitted by both operators, the two machines act at the same mathematical instant. Two batteries are erected, one at each station, and none at intermediate points of the road, and they are connected with the instruments in a peculiar manner, which will be best understood by making a diagram. One pole of each battery is connected with the slides of the instrument, the other with the ground. The wire of the line is connected at each end, through coil No. 1 of the instrument, with the upper portion of the shaft; the lower portion is connected, through coil No. 2, with the ground. When the keys of both machines are at rest the current of both batteries is broken — all the connecting wires and the line are free from electricity. When a key is acted upon in New York, the corresponding slide raises the arm; this disconnects one coil in New York and makes the current of one battery pass through one coil in New York and through both coils in Philadelphia. The power of the springs acting against the armatures is so calculated as to overcome the magnet diminished by the current of one battery through two coils, but to be smaller than the magnet diminished only by one battery through one coil. Consequently the armature in Philadelphia is released, and a letter is printed there, and that in New York is held in place. The explanation for telegraphing the other way is the same. When the sides of the same letter are raised at the same time in both instruments, one coil of each is disconnected; but there is the power of two batteries in the other coil of each; and, as a power of two batteries through one coil is equivalent to that of one through two coils, the result described occurs at both stations, and the letter is printed at both places in the same identical instant.

A second manner of using the instrument is to place the batteries on the line as is usual, and to arrange the arms so that they clear the currents when the slides come in contact with them. With this plan, it is not possible to telegraph both ways at the same time.

A third manner, which is favorite with the inventor, and which it is proposed to apply to the Submarine Atlantic Telegraph, consists in having only local currents, which at the moment they are closed develop an induction current of great intensity that shoots through the entire line. For this purpose a coil of coarse wire is wound in three thicknesses around a piece of soft iron of less than an inch in diameter, and about ten inches in length. The local current is made to pass through it. Another coil of fine wire is wound around the first, and is connected with the cable and with the ground.

The manner of making the type-wheels start right is very simple. The operator stops the type-wheel by depressing a small lever, which catches a pin so placed on the wheel that when stopped blank is opposite the printing roller. This lever is thrown up by the printing roller, so that all the care the operator of the other station has to take is to begin by a blank.

Teeth of a slanting shape are cut on the side of the type-wheel, and each time the lever which carries the printing roller is raised, a projection on its side enters between two teeth of the type-wheel, and makes it slide backward or forward as the case may be. The faster letters are telegraphed the more often this correction is made, and it proves so effective that the vibrating springs doing the office of pendulums require to be set with only a very small degree of accuracy; in fact, one may beat fifty and the other fifty-one without any inconvenience resulting from the difference.

The number of cups per hundred miles requisite for working House's Telegraph is two hundred; Morse's requires fifty, and Hughes's only four to work both ways. A good operator can transmit two hundred letters a minute; but this is the limit of human skill, and not that of the power of the instrument. An average writer can pen one hundred and fifty letters per minute. Consequently, one may play on a key-board or telegraph thirty-three per cent. faster than he can write.

Hughes's instrument is moved by a weight of seventy-five pounds, descending two and a half feet in fifteen. This weight is raised now and then by pressing down a treadle. This winding up does not require the stopping of the machine — an intermediate spring being so arranged as to act during the time the weight is raising.

BONELLI'S AUTOGRAPHIC TELEGRAPH.

The autographic telegraph of M. Bonelli, the Sardinian director of telegraphs, is attracting considerable attention on the continent, and bids fair to supersede many of the existing systems of telegraphic communication. Indeed, the action of this telegraph is sufficiently wonderful, and its advantages sufficiently obvious, to give it a claim to public interest. It reproduces with the utmost exactitude any inscription or design which may be traced upon the strip of metalized or conducting paper, which is given to the sender of a message; and this it does with such rapidity, that four times the number of words that can in any given time be transmitted by the usual system, can, it is confidently asserted, be sent by this method. Many advantages beside that of rapidity are, moreover, to be derived from an unerring process of autographic reproduction. It is well known that the various symbols or ciphers, by means of which secrecy is ensured in confidential and important communications, are a constant source of error, owing to the necessary ignorance of the clerk who transmits the message with regard to the value and significance of the signs employed. In a copying telegraph, where the process is purely mechanical, no such inconvenience can occur, and all errors of manipulation are easily avoided. The method employed by M. Bonelli, is to write the despatch, or draw the sketch to be transmitted, on a metalized or conducting paper in non-conducting ink. It is then placed on the transmitting machine, and passed by clock-work under a number of fine conducting wires, arranged in line like the teeth of a comb. These conducting wires (there are sixty in Bonelli's machine) are insulated separately in a gutta-percha cable, which is stretched between the two places in communication. At the other end, they are spread out in the same comb form. Under this receiving comb is passed, by means of similar clock-work, a chemically prepared paper, the yellow color of which is changed to green by the action of the magnetic currents which pass over the wires. When the wires at the transmitting end are passing over the insulating ink, they of course convey no fluid, and make no change in the color of the receiving paper. The message appears, therefore, in yellow letters on a green ground, the letters being composed of lines, the nearness of which depends on the distance between the wires. To accomplish the same thing with one wire, it is necessary to have the two machines move in exact time with each other, and the point of the wire must pass necessarily over every portion of the written despatch, transmitting it point by point. By Bonelli's method, this exactness of time would not be requisite.

THE ATLANTIC CABLE.

The great scientific event of the year 1858 was the successful submergence of the Atlantic Telegraph Cable, and the temporary transmission of messages between the Old World and the New. The main facts pertaining to the history of this important enterprise are as follows:

The telegraphic fleet, comprising the Niagara and the Agamemnon carrying the cable, with two attendant steam-frigates, sailed from Queenstown, Ireland, on the 17th of July, 1858, and united at the *rendezvous*, lat. 52° 5′ long. 32° 40′ W., on the 28th. On the succeeding day, July 29th, at 1 P. M., lat. 52° 59′, long. 32° 27′ W. the "*splice*" between the two ends of the cable was successfully made, and electrical signals passed perfectly through the whole length on board both ships. Depth of water 1550 fathoms. The distance from the entrance of Valentia harbor was eight hundred and thirteen nautical miles; to the entrance of Trinity Bay. N. F., eight hundred and twenty-two nautical miles, and from there sixty miles to the telegraph house at the head of the Bay of Bulls, equal in all to eight hundred and eighty-two nautical miles. The Niagara had sixty-nine miles farther to run than the Agamemnon. Each ship had on board about eleven hundred nautical miles of cable. The following table presents a condensed view of the Niagara's voyage:

Date.	Lat. N.	Long. W.	Dist. sailed in last 24 hours.	Miles and fathoms of cable paid out.	Excess per cent.	Depth of water in fathoms.
July 30, Friday,	51° 50′	34° 49′	89	131*m*.	48	1550 — 1985
" 31, Saturday,	51° 50′	38° 28′	137	159*m*. 353*f*.	13	
Aug. 1, Sunday,	50° 32′	41° 55′	145	164*m*. 683*f*.	14	1950 — 2424
" 2, Monday,	49° 52′	45° 37′	154	177*m*. 150*f*.	15	1600 — 2385
" 3, Tuesday,	49° 17′	49° 23′	147	161*m*. 763*f*.	10	1742(?)—1827
4, Wedn'y,	48° 17′	52° 43′	146	154*m*. 360*f*.	6	200
5, Thursday, Niagara anchored.			64	66*m*. 382*f*.	4	

It will be observed that the distances run during the five full days were remarkably uniform, 149 miles per day, and the excess of cable paid out was about 15 per cent. more than the ship's record. Twice during the progress of the ship, from some unexplained cause, signals failed to pass between them, viz., at 7 1-2 P. M. July 29, for an hour, and August 2, from $12^h\ 38^m$ A. M. to 5.40 A. M. But during all the remainder of the time signals were constantly received; and at the last, the Agamemnon, August 5, signalized the Niagara that they had paid out 1010 miles of cable. The cable was landed at Valentia Bay on Thursday, August 5, and at 6 A. M. the shore end was carried into the telegraph house, and a strong current of electricity received through the whole cable from the other side of the Atlantic.

On the 6th of August, a message of thirty-one words was transmitted from Ireland to Newfoundland in thirty-five minutes, and on the 17th of August, the Queen of Great Britain transmitted a congratulatory message to the President of the United States, expressing her joy at the completion of this great international bond, to which President Buchanan responded in the same spirit.

After this the electrical condition of the wire became daily more faulty, and it was only with the greatest difficulty, and by constant repetition, that messages were transmitted to Newfoundland, although return messages to Valentia were, in almost every case, clear and distinct. The last intelligible signal received at either end of the line was on the 4th of September; since which date the cable has remained practically inoperative. We are, however, informed, that the electric current is still unbroken, but that its indications are too feeble to admit of any application to telegraphic purposes. The electrician-in-chief has arrived at the conclusion that there are at least two serious faults in the cable, one of them dating from before the submergence, and between 500 and 600 miles from Valentia; the other about 270 miles distant, and at the place where, owing to the sudden change in the depth of the sea, danger had always been apprehended; that one or both of these faults have been aggravated, if not made fatal, by the intense currents used to overcome the difficulty, and that electrical tests indicated, what was otherwise too probable, that at least the Agamemnon's portion of the cable was in a very damaged state before it was submerged.

In the present condition of the line, the great natural currents of electricity, which are continually traversing the surface of the earth in various directions, act by their inductive effects upon the great length of cable submerged, and disturb the needles and galvanometers at both ends of the line to a considerable degree. This nature and action, if properly observed and studied by means of the Atlantic Cable, would, no doubt, throw considerable light upon the phenomena of diurnal magnetic variations, to account for which no satisfactory law has been proposed. On the night of Monday, the 6th of September, one of those extraordinary phenomena called magnetic storms must have passed over the track of the cable; for from half-past eleven to half-past twelve, the reflecting galvanometer in connection with the line was most violently disturbed. The reflections were so rapid and violent that it was only occasionally that the reflected ray of light could be distinguished upon the reading scale.

The London *Times*, in commenting on the present condition of the Atlantic Telegraph enterprise, uses the following language:

For the present, and as regards this particular cable, we feel as people do about a tree languishing from some inscrutable disease, or a child that pines away, it cannot tell why. What is the matter with it? Where is the pain? What part is hurt? Answer, there is none. In a small room on the Irish coast a bit of copper wire is fixed on the table or the wall, and knowing men are coaxing it to tell them what has happened a thousand miles off in the mid abyss of the ocean. Its vitality expires, its pulsation grows weaker; it responds more and more feebly to the tortures of science, and the very means used to rouse it from its stupor, draw on its constitution. In their urgency, the operators cut off their own hopes, and it is now suspected that they have done themselves no small part of the mischief that they deplore. What is this but the old story of the genius, maliciously true, keeping the very letter of his bond, doing superhuman service, but gone forever if a word or a movement be omitted? There is too much reason to fear that the affair is reduced to a post mortem examination. There is a length of wire, but how long no man can say. It is, indeed, almost the greatest wonder of the age, and, fairly considered, beats even the Atlantic Telegraph itself, should that ever be an existent fact, that our men of science can stand at one end of a fine copper wire and ask it how long it is. "Answer, wire, are you 10 miles long, or 270, or 560, or 1,000, or even 2,000? What is the nature of your fracture or injury?" Witchcraft itself cannot beat such divination. It is even some comfort to reflect, that, though for the present science does us no good, yet it gains by our failure, and though we do not yet obtain what we want, we know more.

Since the deposition of the cable, says the London *News*, a great variety of interesting experiments have been performed to show the kind of electricity best suited for working through the line, both in a perfect and imperfect condition; and the results which have been arrived at are both useful and interesting. The high tension electricity from the induced coils was found to burn up and destroy a wire where a fault in the gutta-percha was made. The second experiment was made with discharges from Henley's magneto-electric machine. This was found to suffer a slight loss through the fault, but not to injure the wire at the point of egress in any way as long as negative discharges only were sent through. The direct battery current, of great quantity, but low tension electricity, was found to answer best, and to cause less injury to the cable, and to suffer less loss through the fault; but some copious currents of this low tension electricity are now unable to make the cable show signs of activity even with the most delicate arrangement of Professor Thomson's reflecting galvanometer. Much has been said about this beautifully sensitive little instrument, yet but few know its nature or the advantages it possesses over other galvanometers in observing very minute currents of electricity. It consists of a coil of very fine insulated wire, in the interior of which is suspended a very small mirror of the finest microscopic glass, to the back of which are fastened two magnetic needles not more than a quarter of an inch in length. The two needles, with the reflecting mirror they bear, not weighing more than two grains, are suspended by a single fibre of silk. When the instrument is in use a ray of light is thrown upon the mirror, and is reflected upon an index board. The very faintest currents of electricity are measured by these means; for the very faintest deflection of the needles, caused by an almost imperceptible current of electricity passing through the coil, of course, is very perceptible upon the index board by the motion of the reflected spot of light.

The supposed difficulty of working through the cable, after it was submerged, which was so much talked about, has turned out to be altogether a mistake, and therefore the high tension induction coils, which were made by Mr. Whitehouse, were not only unnecessary, but absolutely hurtful and dangerous — unless the whole length of the line was perfectly insulated in every inch of its vast length, which, of course, it is impossible to expect that anything made by human hands could be. The retarding influences of induction which where so much spoken of while the cable was lying at Keyham, were probably due to the inductive influence of one layer of the cable lying over another in the coil; for so little has it been experienced since the cable has been laid, that Professor Thomson thinks if the line was in fair condition it could be worked through with ease with a few battery cells of Daniel's constant battery.

That the wire is exposed to a considerable extent, in at least two places, is well ascertained, and where a metallic surface through which currents of electricity are passing, is exposed to water, oxidation takes place by the electrolytic decomposition of the water, and thus the wire must soon be eaten away. This may be in a great measure avoided by transmitting all signals with negative currents, which would prevent the direct oxidation of the wire by electrolysis; but at the same time, according to the experiments which have been made, both by Professor Thomson and Mr. Henley, an exposed wire is not entirely free from a species of decomposition, even when negative currents alone are transmitted through it. It soon became encrusted with a light-colored substance, the precise nature of which is not quite ascertained as yet, but it is supposed to be a combination of chloride of copper with some of the organic compounds contained in the gutta-percha. So, no matter what may be the result of the preconcerted experiments at both termini of the wire, one thing cannot be doubted, viz., that, before permanent, certain, and rapid telegraphic communication can be secured between Europe and America, another cable must be laid.

Thus, for the present, the cable must be considered a failure, at least as regards its paying and working properties. Nevertheless, in spite of all obstacles, the attempt has demonstrated the ease, and, indeed, almost certainty, with which, under ordinarily favorable circumstances, a submarine cable across the Atlantic can always be laid; and all the theories of the cross currents which were to break the cable, the floats which were necessary to buoy it up, and, above all, the idea that it could never sink to the bottom, are set at rest forever. The cable has been laid, and has been worked through; and really, when we look for one moment at what that coil has had to undergo, from the day the first mile of it was made up to the present time, it seems nothing less than marvellous how it has been submerged, and still more astounding that any signals ever came through it. On the very place where the splice was made, in the centre of the Atlantic, an air-bubble, almost the size of a coffee-bean, had to be cut out. How many hundreds of similar places might there have been that were never seen! The defective centring of the copper conductor in its gutta-percha covering was also, no doubt, a source of many serious faults; for the reason that the gutta-percha, being very thin in some parts, allows the powerful electric currents from the induction coils to pierce it and touch the outside wires. Once this fault, which is technically termed "blowing a hole" in a cable, takes place, such a loss of the signaling current ensues that the cable is rendered useless, or a great augmentation of battery power is rendered necessary; and when this last remedy is resorted to, the hole becomes larger and larger, until the water, getting freely to the wire, oxidates it away in a very short time. Such accidents have frequently occurred in the cables between England and the Hague, and there is not the least doubt that there are many scores of such faults along the Atlantic wire. A good deal of attention has lately been turned to the question of rope-covered wire, and there is now no doubt but that all future attempts to connect Europe with America will be made with cables so constructed. At the same time it will not be strands of hemp loosely thrown around the gutta-percha covered wires that will make a serviceable cable; but rope-yarns, bound in with the same fineness as the wires are now twisted by the "closing machine." In fact, rope-covered wire would have to be made by Glass and Elliot, or Newall, just in the same manner, and on the same plan, as the wire-covered cables are now made by those firms.

There is one difficulty which many suppose will influence the success of the deep sea cables in no slight degree — namely, the pressure to which all cables must be subjected at great depths. Some people are sufficiently ill-informed to deny that there is any pressure exerted by the water of the ocean at the bottom at all. Such absurd blunders only arise from ignorance of the difference between density and weight. It does not follow that, because water is almost incompressible, it weighs nothing, no more than it follows that, because a block of granite is not elastic, it does not press upon the spot on which it rests. The pressure of the water at the bottom of the Atlantic averages about two tons and a half to a square inch. It is a simple matter of calculation to ascertain the fact. At such a pressure as this, wrought brass can be saturated with water like a sponge, and a block of it so saturated requires days for it to ooze out again.

ON THE SUBMERGENCE AND CONSTRUCTION OF SUBMARINE TELEGRAPH CABLES.

At a recent meeting of the London Society of Civil Engineers, it was mentioned as a practical illustration of the facility with which light cables could be laid, that, although the submarine telegraph between Varna and the Crimea was submerged under considerable difficulties, and during a storm, yet the actual length payed out was only 3 3-4 miles in excess of the distance between those places, which was nearly 350 miles. The depth of the Black Sea, where this cable was laid, was about 70 fathoms. The cable consisted, throughout the greater portion of its length, simply of No. 16 copper wire, covered with gutta-percha, and wholly unprotected. The shore ends had an iron sheathing, extending to a distance of ten miles from Varna, and of six miles from the Crimea. Its insulation was perfect; and it remained uninjured for twelve months, during the time of the Russian war, notwithstanding the many violent storms to which it was exposed in the Black Sea, until, during a storm of more than usual severity, it was broken on the 5th of December, 1855.

With reference to the best form for a submarine cable, it had been proved that, when great depths had to be traversed, one of light specific gravity was to be preferred. The conductor, which constituted the weight to be carried, should, therefore, be as light as possible; and, to insure its continuity, it should be relieved from strain by the external coating. The conductor, when of copper, had a specific gravity of 11, the gutta-percha insulator was nearly equal in weight to sea-water, and the iron external covering had a specific gravity of 7. Probably, aluminium might be substituted for copper in deep sea cables, with advantage, as it was nearly equal in conducting power, and was only one-third of the weight. The outer covering should be of hard material, so as to resist the longitudinal strain during the process of submerging; but it should add as little as possible to the weight. It was considered that no material fulfilled these conditions as well as soft steel.

It had been found that the light and heat of the sun, the mycellium of a fungus, and other substances and conditions, had the power of rendering gutta-percha unfit for the insulation required for the transmission of messages by means of electricity. Several specimens of gutta-percha, in a decayed state, were exhibited; and also a piece of copper wire, five feet in length, covered with gutta-percha, which was strained until it broke; when the gutta-percha, owing to its partial elasticity, contracted, and left seven inches of copper wire uncovered. A newly-made tube of gutta-percha, under a strain of 276 lbs., stretched from 14 inches to 24 inches before breaking; but a similar tube, which had been exposed for about five years to the atmosphere, light, and heat of the sun, was so brittle as to be easily broken by the hand.

The London *Builder* publishes the following curious item of information bearing on the subject of the duration of submarine telegraph cables:

"On examining a piece of submarine cable cut from the end of the La Manche line, long in use, there were noticed an indefinite series of ruptures or subdivisions, as if the wire had been chopped into morsels, or had been disintegrated, under the influence of the electrical vibrations. Since, in the case of the transatlantic cable, currents positive and negative alternately are launched through it, such a disintegration of the wire must be expected to come about even more rapidly. The fact itself is too mysterious to be discussed at present."

ON THE PRESENT STATE OF OUR KNOWLEDGE RESPECTING TERRESTRIAL MAGNETISM.

From the report of a joint committee appointed on the part of the Royal Society and the British Association to consider the expediency of memorializing Government to renew the system of magnetic observations formerly carried on at various colonial and foreign stations, but now suspended, we derive the following information respecting the results thus far obtained from the accumulated observations made at the several observatories at St. Helena, Toronto, Hobarton, and the Cape of Good Hope. In the first place, the committee report, that the mean state of the several elements for each of the stations, as reduced to a fixed epoch, has been obtained with a precision of which nothing previously done has afforded any example — emulating, in this respect, the exactness of astronomical determinations, and competent to serve as a fixed point of departure, to the latest ages; and this for each of the elements in question — the dip, the declination, and the intensity of the magnetic force.

Secondly, that at each station the rate of regular progressive secular change, in all three of the elements above mentioned, has been ascertained, with a degree of precision which contrasts strongly with the loose and inaccurate determinations of former times.

Thirdly, that the laws of the diurnal, annual, and other periodic fluctuations in the value of these elements, as exhibited at each station, have been established in a manner and with a decision to which nothing hitherto executed in any branch of science, astronomy excepted, is comparable; and that the results embodied in the examination of these laws have laid open a view of magnetic action so singular, and so utterly unexpected, as to amount to the creation of a new department of science, and a detection of a completely novel system of physical relations; for that, in the first place, the systems of diurnal and annual magnetic changes have each been separated into two perfectly distinct and physically independent systems, — the one, at any particular station, holding its course according to laws depending solely on the sun's hour-angle at the moment of observation, and his meridian altitude at different seasons, — the other, comprehending all those movements which, under the name of magnetic storms, or "irregular disturbances," have hitherto presented the perplexing aspect of phenomena purely casual, capricious in amount and in the particular occasions of their occurrence when regarded singly, has been shown, by these discussions, to be subject in its totality to laws equally definite with the others, though more dependent for their application on peculiarities of local situation. As regards the first of these fluctuations, they find it demonstrated:

That the sun's regular action on the magnetism of the globe is determined by a law of no small complexity and intricacy, but which, nevertheless, has been traced with precision and certainty, and shown to be referable, in the first place, and for one of its arbitrary coëfficients, to the geographical situation of the place of observation with respect to a certain line or equator on the earth's surface, which cannot yet be precisely traced for want of sufficiently numerous stations (but which seems to approach to the line of least intensity, and is very far from coinciding with the geographical equator), — and in the next, and for its other influential cause, to the fact of the sun's having north or south declination; so that the whole diurnal change in any one of the elements, and at any station, is made up of two portions, one of which retains the same sign and a constant coëfficient all the year round; the other changes sign, and varies in the value of its coëfficient with the annual movement of the sun from one side of the equator to the other.

That, consequently, for a station on the magnetic equator (so defined), the *mean* amount of diurnal change is *nil*, when taken over the whole year, but that on any particular day of the year it has a determinate magnitude, which passes through an annual periodicity, with opposite characters in opposite seasons. And that for a station in middle latitudes the mean diurnal fluctuation is not *nil*, but such as, during every part of the year, to exhibit an easterly deviation in the morning hours, and a westerly in the evening hours, for stations north of the equator, and *vice versa* for those south of it; but that the amount of this deviation, or the amplitude of the diurnal fluctuation, varies with the seasons, being exaggerated or partially counteracted by the alternate conspiring and opposing influence of the sun's declination during the summer and winter seasons.

As regards the irregular disturbances, though arbitrary and capricious in extent, and in the moments when they may be expected, individually, this does not prevent their obeying, with great fidelity, the law of averages, when grouped in masses, and treated separately from those of the former class. So handled they are found to conform, in their average effect, at each of the twenty-four hours of the day, and on each day of the year, to the very same rules, as regards the sun's daily and annual movement, with one remarkable point of difference, viz., that their hours of maxima and minima are not identical with those of the regular class, but that each particular station has, in this respect, its own peculiar hours, analogous to what is called the "establishment" of a port in the theory of the tides. And that, in consequence, the superposition of these two systems of diurnal fluctuation gives rise to a series of compound variations analogous to the superposition of two undulations having the same period, but different amplitudes, and different epochal times. And that, by attending to this principle, many of the most complex phenomena, such as that of a double maximum and minimum, with the occurrence of a nightly as well as a daily movement, are explained in a satisfactory manner.

The discussion of the observations already accumulated has further brought into view, and, in the opinion of your committee fully established, the existence of a very extraordinary periodicity in the extent of fluctuation of all the magnetic elements, and in the amplitude and frequency of their irregular movements especially, which connects them directly with the physical constitution of the sun, and with the periodical greater or less prevalence of spots on its surface — the maxima of the amount of fluctuation corresponding to the maxima of the spots, and these again with those of the exhibitions of the Aurora Borealis, which appears also to be subject to the same law of periodicity; a law which, as it does not agree with any of the otherwise known solar, lunar, or planetary periods, may be considered as, so to speak, personal to the sun itself. And thus we find ourselves landed in a system of cosmical relations, in which both the sun and the earth, and probably the whole planetary system, are implicated.

That the sun acts in influencing the earth's magnetism, in some other manner than by its heat, seems to be rendered very probable by several features of this inquiry; and the idea of a direct magnetic influence exterior to the earth, is corroborated by the discovery of a minute fluctuation in the magnetic elements, having for its period not the solar but the lunar day, and, therefore, directly traceable to the action of the moon. The detection of this fluctuation by Mr. Kreil, from a discussion of the Prague observations, has been confirmed by the evidence afforded by those of our colonial observatories, and appears to be placed beyond all question by the recent deductions from the horizontal force and the declination extending over three years of observation at the Cape of Good Hope, which General Sabine has submitted for your committee's inspection, and in both which the fluctuations in question emerge in a very satisfactory manner, and one calculated to give a high idea of the precision of which such determinations are susceptible, when it is considered that the total amplitude of oscillation due to this cause in the direction of the Cape needle is only about 16″ of angle.

The committee also quote the following extract from a communication addressed to them by Gen. Sabine, on the importance of continuing the system of observations at the present time:

"Recent observations in North America, discussed in the proceedings of the Royal Society for January the 7th, 1858, have made known that the general movement of translation of the isoclinal and isogonic lines, which from the earliest observations have been progressing from west to east, has within a few years reached its extreme eastern oscillation, and that the movement in the reverse direction has already commenced; we live, therefore, at an epoch in the history of terrestrial magnetism, which, we have reason to believe, will be regarded hereafter — when theory shall have more advanced — as a highly important and critical epoch. The geographical position of the maximum force in the northern hemisphere appears to have reached its extreme easterly elongation, and from this time forth may be expected to move for many years to come towards the meridian which it occupied in

Halley's time, accompanied by a corresponding change in the positions and forms of the isodynamic, isoclinal, and isogonic lines in North America; a careful determination of the absolute values and present secular change of the three elements at this critical theoretical epoch, at stations situated on either side of the American continent, and nearly in the geographical latitude of the maximum of the force, would furnish, therefore, data for posterity, of the value of which we may have a very inadequate appreciation at present.

ON THE DEVELOPMENT OF A PHYSICAL THEORY OF TERRESTRIAL MAGNETISM.

At the last meeting of the British Association, Mr. J. Drummond presented a new theory of terrestrial magnetism, an outline of which he submitted to the meeting for '57. (See Annual Scientific Discovery 1858, page 370.) The fundamental principles of this theory are as follows: Assuming the prevailing idea regarding the early condition and present state of the globe, viz., that it has cooled down from a state of fluidity, and now consists of a solid crust inclosing a molten nucleus — the author assumed also that the sun, moon, and other planetary bodies, must exert the same influence upon the inclosed fluid which they exert upon the surface ocean in producing the tides,— that, consequently, a system of internal tides must be occasioned simultaneously with the external tides. Further, accepting the theory of Gauss, that the entire matter of the globe is magnetic, he concluded that the passage of these internal waves must occasion corresponding changes in the position of the needle; and, reasoning from these premises, he arrived at the following conclusions in regard to the changes in position which the needle ought to undergo. A declination needle at any station, resting on the line of the magnetic meridian, ought, upon one of the internal waves coming from the eastward, to make an excursion to meet it; as the crest of the wave approaches the station of observation, the needle ought to return with it; and when it comes immediately beneath the point of observation, the needle ought to coincide again with the meridian. As the wave proceeds westward, the needle ought to follow it, making a westerly excursion equal to the easterly; and as the wave passes further west, and its influence over the needle thereby declines, the latter ought slowly to return again to the meredian. Again, an inclination needle ought to begin slowly to dip as the crest of the wave approaches the station of observation, reaching its maximum when the wave is immediately beneath it, and slowly rising to its former position as the wave passes easterly; and the intensity, as indicated by the oscillating needle, ought to increase as the crest of the wave approaches the station, reaching its maximum when it is immediately beneath it, and decreasing gradually as the wave proceeds to the westward,— the maximum of intensity thus coinciding with the maximum of inclination. The results of observation, Mr. Drummond stated, harmonize completely with the conditions of the theory.

INFLUENCE OF MAGNETISM OVER CHEMICAL ACTION.

The following inquiry, by Mr. H. F. Baxter, originated in an endeavor to ascertain whether *Magnetism* possessed any influence over *Organic Forces;* and the kind of experiments that were undertaken for the purpose of solving this question, was that of submitting seeds *during* vegetation to the influence of magnetism. These experiments, however, having failed to give any definite or decided result, Mr. Baxter was ultimately, and perhaps naturally, led to ask the question — *Does magnetism possess any influence over chemical action?* The solution of this question appeared to be almost a necessary preliminary step to the continuation of Mr. Baxter's original inquiry.

The author's investigations will be found detailed in the *Edinburgh New Philosophical Journal,* No. 10. The following are the general conclusions deduced from his investigations:

1. That *Magnetism* (in its *static* or quiescent condition), does not *excite* or *originate chemical action.*

2. That when substances *undergoing* chemical action are submitted to the *influence* of magnetism (in its *static* or quiescent condition) *no increase* in the *chemical action* is observed; but that,

3. Under certain conditions *during* chemical action, the influence of magnetism is such as to indicate a *directive* influence over chemical action; this influence being shown by a *rotatory motion* of the fluid around the pole of the magnet.

4. That it is not necessary for the production of this *rotatory motion* that the solution should act chemically *upon* the iron bar forming the pole; for, if the pole be surrounded by a metal ring, the *rotation* occurs, provided the solution is capable of *acting chemically* upon this metal ring.

5. That the *influence of the magnet,* as well as the *existence* of the *chemical action,* and its *continuation,* are essential for the production of this *rotation;* and,

6. That the *direction* of the *rotation* is dependent upon the *poles* of the magnet, being *contrary* for each *pole.*

DOES MAGNETISM INFLUENCE VEGETATION?

Mr. H. F. Baxter states that the results of his inquiry into this subject are negative: that is, no positive evidence has been obtained to show that Magnetism either does or does not influence vegetation. After noticing the opinions of Becquerel, Dutrochet, and Wartmann, the author says: — "As it may be considered a law in vegetable physiology that all plants have a tendency, during the germination of their seeds, to develop in two diametrically opposite directions (the root and the stem), the question arose — Might not this direction be influenced or counteracted by submitting the seeds whilst germinating to the influence of magnetic force?" Accordingly, a series of experiments were undertaken by the author, which are classed under two principal heads: 1st, Those in which the line of magnetic force was directed *perpendicularly* to the plants; and 2d, In which the line of force was directed *transversely* to the plants. The author gave details of the experiments, which were varied and multiplied. No definite conclusions, however, could be drawn from them relative to the effects of magnetism. — *Proceedings of the Botanical Society of Edinburgh.*

MAGNETIC DISCREPANCIES.

At Point Barrow, the ultima thule or north cape of the American continent, between Mackenzie's River and Behring's Straits, the British relief ship Plover waited for Sir John Franklin — waited and hoped for two long years, from the summer of 1852 to the summer of 1854, when hope failed, and her crew came home. During those two long, dreary, solid winter nights, Capt. Maguire and his officers amused themselves with observing and recording for seventeen months unremittingly the hourly variations of the needle and the shifting scenery of the Aurora. Their observatory was the sand of the shore with a dome of ice slabs lined with seal-skin fur. Their instruments had come from Woolwich; their observations were as skilful and exact as those of their fellow officers at Toronto, and their results have been reduced, under the eye of the same master-mind in London, Major-General Sabine, the highest authority living in this particular branch of science. To the astonishment of all, these observations at Point Barrow have turned out to be in some respects the direct reverse of those at Toronto. While the *regular* solar declination of the needle follows the same law, the needle bending furthest to the east and west at the same hours of the day at both places, the *disturbance* diurnal variation at the one is just the opposite of what it is at the other — the west of one agrees with the east of the other, and the east of the one with the west of the other,— a difference the more remarkable, since, at both places, there can be no doubt that the sun's heat is the cause of the disturbance of the needle. At the same time the auroral exhibitions keep time with the magnetic disturbances. Out of one thousand seven hundred and eighty-eight hourly observations in three months of 1852–53, four hundred and sixty-one showed an aurora; and out of one thousand eight hundred and thirty-seven in the corresponding quarter of the following year, six hundred and sixteen exhibitions of the aurora took place. Six days out of seven, during these six months of night, the auroral light replaced the sun light. For the first time, then, in the annals of meteorological science, the apparitions of this polar spectre have been studied steadily and long enough to fix them to the different hours of the solar day, and it is found that not a single record of their appearance was made between eleven o'clock in the morning and three in the afternoon, whereas one hundred and two are recorded at one o'clock at night. From this, their favorite hour, their visits regularly decrease in number until midday, and increase as regularly through all the evening hours up to midnight. But this beautiful cycle of illumination for those polar wastes leaves us in total darkness as to its hidden cause. And, to make perplexing conjectures more perplexed, there seems to be a law of agreement between the frequency of the auroras and the disturbances of the needle toward the west, but not toward the east.

ON THE INTENSITY OF THE TERRESTRIAL MAGNETIC FORCE.

Mr. J. Drummond, in a communication to the British Association, 1858, stated that, in comparing the observations of dip with those of intensity, he had found some anomalous results, of which the following is an example. In the diurnal variation the dip is at a minimum about 8 A. M., at a maximum about 11 A. M., after which it decreases to a minimum again about 2 P. M. Turning now to the intensity, the maximum is found to occur about 8 A. M., and the minimum about 11 A. M., after which it again increases, reaching a maximum in the afternoon. From these facts, then, it would appear that, while the earth exerts a greater attracting power over the needle about 11 A. M., than either before that hour or after it, the intensity of the force by which this is accomplished is then at its minimum. In other words, we are driven to the conclusion, that the earth exerts a greater attracting power by a minimum of force than by a maximum, — a conclusion entirely at variance with all our knowledge of the magnetic force. This anomalous result the author traced to the assumption laying at the foundation of the present theory of the intensity, viz., that the terrestrial force is exerted in the direction of the dip; and from an analysis of the phenomena of the dip he arrived at the following laws: — 1. That the true direction in which the earth's force is exerted is in the radial line of its centre, at least so within certain limits, the earth being a spheroid and not a sphere. 2. That the force being at all points upon the earth's surface exerted in the radial line of its centre, and the vibrations of a horizontal needle being therefore, at all stations, made at right angles to the direction of the force, their number at any two or more stations in similar times, or at different periods in similar times, indicates exactly the ratio of the force at each station and at each period.

ON FLUORESCENCE PRODUCED BY THE AURORA.

Mr. T. R. Robinson, of Armagh, in a letter to the Phil. Magazine, writes as follows: On the occasion of an aurora of more than average brightness, on the 14th of March, 1858, I availed myself of the opportunity to try whether this light was rich in those highly refrangible rays which produce fluorescence, and which are so abundant in the light of the electric discharges; and I found it to be so. A drop of desulphate of quinine on a porcelain tablet seemed like a luminous patch on a faint ground; and crystals of platino-cyanide of potassium were so bright, that the label on the tube which contained them (and which by lamplight could not be distinguished from the salt at a little distance) seemed almost black by contrast. These effects were so strong, in relation to the actual intensity of the light, that they appear to afford an additional evidence of the electric origin of the phenomenon.

RELATIONS OF MATTER AND FORCE.

The late Dr. Samuel Brown, of Edinburgh, whose scientific essays have recently been published, was one of the boldest and most original thinkers among the scientists of the present century. His speculations concerning ultimate connection of matter and force, as embodied in the following paragraph, are especially worthy of notice.

"A particle," he says, "is a molecular nucleus, surrounded by five polar spheres of force; the first, that of repulsion, which is never overpassed in the chemical, any more than the first repulsive sphere of the sun is in the astronomical, operations of nature; the second, that of proper chemical affinity; the third, that of repulsion, which hinders the compression of a solid body by surrounding forces; the fourth, the attractive sphere of solidiformity; and the fifth, the repulsive sphere of gasiformity. It is called a molecular nucleus to distinguish from both the point of infinite repulsion defined by Boscovich, and the solid nucleus of Newton, and to indicate that the chemist has no more to do with what is within his ultimate atoms than the astronomer with what is within his stars. Nor is it meant that there are no more than five spheres of force; but only that the chemical atomician, contemplating matter under the conditions of gasiformity, liquidity, solidity, and chemical combination, has to consider these five alone. A particle of hydrogen, revolving like a planet round oxygen on their outermost spheres of repulsion, produces the smallest mass of these gases diffused by Dalton's law in the ratio of particle to particle; revolving round oxygen on the second outermost spheres of repulsion, they should produce the smallest mass of an analogous solidiform substance, which, however, cannot exist, inasmuch as if the mutual repulsion of oxygen to oxygen, and hydrogen to hydrogen, in contiguous molecules, could be so far constrained as to admit of such composition, there were no opponent force to hinder their compression into the more intimate union of chemical combination. And, lastly, a particle of hydrogen revolving round an oxygen on their third outermost (*i. e.* innermost) spheres of repulsion, produces a particle of the compound water."

Speaking of Sir Isaac Newton's theory of chemistry, founded on some facts in astronomy, Dr. Brown says:

"The master of astronomy and the creator of optics, he does not appear to have done anything for concrete chemistry, his laboratory notwithstanding; always saving and excepting his conjecture that the diamond was combustible because it is a strong refractor — a prosperous guess which it is customary to extol as sagacious, in spite of the notorious fact that there are stronger refractors than that crystalline carbon, which are not combustible a whit. Its combustibility has no connection with its refractive power, in fact; and, though the hypothesis was not atrociously inconsequent when it was made, it is as ridiculous as illogical to admire it now. It was just one of those countless little strokes of fortune which are constantly befalling the man of genius and industry. In the game of discovery, long and difficult though it is, Nature always gives her darling loaded dice, because she will have him win the day. But Isaac Newton has almost become the mythical man or demigod of British science, owing partly to the assault of Voltaire, partly to the lofty rhymes of Thomson, partly to the clangorous eloquence of Chalmers, yet chiefly, and all but entirely, to the overwhelming conceptions with which his very name amazes the mind; and one of the consequences is, that all sorts of trumpery stories about falling apples, as well as every kind of encomium, may be heaped with impunity on the Atlantean shoulders of 'the incomparable Mr. Newton,' now that the shade is divinized. If *nil nisi bonum* is to be written on the tomb of the vulgar dead, after all, what shall men not say or sing, if so please their uncrowned majesties, at the shrines of the immortals!"

LIGHT AND ELECTRICITY.

The evidences connecting electricity and magnetism, as forces, with the sun, and other bodies of our system, are, of course, different and inferior to those which establish the relations of light. Yet they are now continually becoming more numerous and significant. Whoever has seen the star of pure and intense light which bursts forth on the approach of the charcoal points completing the circuit of a voltaic battery, or the *flood of light* thence poured by reflection over wide and distant spaces, cannot but suspect that the new "fountain" thus opened to the eyes of men (and certainly not destined to remain an idle and valueless gift of science) may be the same in source and qualities as that higher fountain which diffuses light and heat over the whole planetary system. Sir J. Herschel, who ever makes his highest speculations subordinate to cautious induction, has assigned strong reasons for believing the sun to be in a permanently excited electrical state. Meanwhile the moon also has been found, by delicate observations and averages carefully collected, to exercise a magnetic influence on the earth, — the needle expressing to human eye certain small variations which strictly correspond with the lunar hour angle. The fact has its peculiar interest in indicating, and this not vaguely, a similar influence throughout the whole planetary system, and possibly far beyond. The magnetic conditions and changes of the earth itself come into direct testimony here; so general and strictly coincident over its surface, as to give us assurance that the total globe is in a definite magnetic state; and capable, through this state, of affecting other worlds, as well as the little needle which man makes his index here of this mysterious force. — *Edinburgh Review.*

ON THE NATURE OF FLAME AND THE CONDITION OF THE SUN'S SURFACE.

The following paper, on the above subject, by Professor John W. Draper, is published in the L. E. & D. Phil. Magazine, Vol. XV., page 90:

Among the recent publications on photo-chemistry, there is one by Professor Dove, on the Electric Light (Phil. Mag., Nov., 1857), which will doubtless attract the attention of those interested in that branch of science. Examination by the prism, and by absorbing and reflecting colored bodies, leads him to the conclusion that it is necessary to consider the luminous appearance as having two distict sources: — First, the ignition or incandescence of the material particles bodily passing in the course of the discharge; secondly, the proper electrical light itself. As respects the first, he illustrates its method of increase from low to high temperatures by supposing a screen to be withdrawn from the red end of the spectrum through the colored spaces successively towards the violet; and that of the latter from the bluish brush to the bright Leyden sparks, by a like screen drawn from the violet towards the red.

The true electric light exhibits properties resembling those observed in actual combustions, as though there was an oxidation of a portion of the translated matter when the spark is taken in air. The order of evolution of rays in this instance happens to be the same as in the second illustration of Professor Dove, that is, from the violet to the red. There are certain facts connected with these appearances of color which are not generally known, and deserve to be pointed out,

In the Philosophical Magazine (February, 1848), I showed, experimentally, that there is a relation between the color of a flame and the energy with which the combustion giving rise to it is going on. The more vigorous and complete the combustion, the higher the refrangibility of the light. A flame burning in its most tardy and restricted way emits rays that are red; but burning in its most complete and effective manner, rays that are violet. In intermediate states of combustion, the intermediate colors are evolved in their proper order of refrangibility.

The flame of a candle or lamp consists of a series of concentric luminous shells, surrounding a central dark core. These shells shine with different colors, the innermost one immediately in contact with the dark core being red, and having a temperature of 977° F. Upon this, in their proper order of refrangibility, are shells, the light of which is orange, yellow, green, blue, indigo, violet. When we look upon such a flame, the rays issuing from all the colored strata are received by the eye at once, and impress us with the sensation of white light.

The differently colored shells, of which a flame thus consists, may be easily parted out from one another, and demonstrated by a prism. Their cause is the slower rate at which combustion occurs at points more and more towards the interior. On the outside, which we may say is in contact with the air, the combustion is most vigorous and complete, and hence the light there emitted is violet; but in the most interior portion of the shining shell, resting upon the dark combustible matter, the atmospheric air can hardly penetrate, or, rather, its oxygen is exhausted and consumed. Between the exterior and interior surface, the burning is going on with an activity constantly declining, because the interpenetration or supply of oxygen is gradually less and less.

But, besides this collection of colored shells, constituting what may be termed the actual flame, there is another region exterior thereto, and to be distinguished both in its chemical nature and in its optical relations. Chemically, it consists of the products of combustion and of the unburnt residue of the air, that is to say, carbonic acid, steam, and nitrogen. These are all the time escaping out of the true flame, and envelop it as an exterior cone or cloak. Optically, this portion differs from the true flame in the circumstance, that it is shining as an incandescent, ignited, but not a burning body. For physiological reasons, into the detail of which it is not necessary here to go, the tint of this exterior cloak seems to be a monochromatic yellow. That, however, is, to a considerable degree, a deception; prismatic examination proving that all the other colors are present, and that the yellow merely exceeds the rest in force and intensity.

A flame thus far may be considered as offering three regions: — First, a central nucleus which is not luminous, and consists of combustible vapor; secondly, an intermediate portion, the true flame, arising from the reaction of the air and the combustible vapor, and being composed of a succession of superposed shells, the interior being red, the exterior violet, and the intervening ones colored in the proper order of refrangibility; the cause of this difference of color being the declining activity with which the combustion goes on deeper and deeper in the flame. As to temperature, the inner red shell cannot be less than 977° F., and the exterior violet one probably more than 2500° F. Thirdly, an envelop consisting of the products of combustion, exterior to the true flame, shining simply as an incandescent body, and its light for the most part overpowered by the brighter portion within.

By the aid of the facts thus presented, we can easily explain the nature of the other regional divisions, distinguishable in such a flame. There must be a blue portion below; blue, because it consists of the most refrangible rays, which issue forth in abundance, for there the exterior air is most copiously and perfectly applied. At the upper end of the flame, particularly if the wick be long and the supply of combustible matter abundant, the light emitted is red; for the products of combustion ascending past that part, make it difficult for the exterior air to get access.

Upon these principles we may also predict what color a flame will have when we vary the circumstances of its burning. Tallow or wax, at temperatures greatly beneath their usually understood point of combustion, oxidize with a pale violet phosphorescent light, quite perceptible, nevertheless, in a dark room; and here the light is violet, for the supply of combustible matter is small, and that of the air abundant. The oxidation is therefore thorough and prompt. For a like reason, sulphur, as we commonly see, burns blue; but if a piece of it is thrown into nitrate of potash ignited in a crucible, the light yielded is of intolerable brilliancy, and absolutely white. Its whiteness does not depend upon the physiological fact, that any color, if it be intensely brilliant, will seem white to the eye; but it is optically white, as is proved by prismatic examination, when all the colors are perceptible. And the reason of this is, that at the high temperature to which the sulphur is exposed, it volatilizes faster than the nitrate of potash and air together can oxidize it, and offers every intermediate rate of combustion, and emits rays of every refrangibility.

In like manner it may be shown that carbonic oxide must burn with a blue flame, and cyanogen with a red. We can also foresee what must be the optical result of resorting to unusual methods of combustion, as when we throw into the interior of a flame a jet of air from a blowpipe. In this case we destroy the red and orange strata, replacing them by bluer colors. Examining such a blowpipe cone by the prism, we have a beautiful demonstration that such has actually taken place.

There is one of these special cases which deserves attentive consideration in connection with the appearance of the electric light; it is the production of Fraunhoferian lines, when things have been arranged in such a way that an incombustible material is present in the substance to be burnt. This state is perfectly represented in the case of cyanogen, which contains more than half its weight of incombustible nitrogen. When the peach-colored nucleus of the cyanogen flame is properly examined, it yields a series of dark lines and spaces exceeding in number and strength those of the sunlight itself. These fixed lines are the representatives of dark shells, superposed among the shining ones with definite periodicity. In such a cyanogen flame they bear no relation to the burning of the carbon, but must be attributed to the disengagement of the nitrogen.

In other cases dark lines are replaced by bright ones, as in the well-known instance of the electric spark between metallic surfaces. The occurrence of lines, whether bright or dark, is hence connected with the chemical nature of the substance producing the flame. For this reason they merit a much more critical examination than has yet been given them; for by their aid we may be able to ascertain points of great interest in other departments of science. Thus, if we are ever able to acquire certain knowledge respecting the physical state of the sun and other stars, it will be by an examination of the light they emit. Even at present, by the aid of the few facts before us, we can see our way pretty clearly to certain conclusions respecting the sun. For, since substances which are incandescent, or in the ignited state through the accumulation of heat in them, show no fixed lines, their prismatic spectrum being uninterrupted from end to end, it would appear to follow that the luminous condition of our sun, whose light contains fixed lines, cannot be referred to such incandescence or ignition. At various times those who have studied this subject have offered different hypotheses; one regarding the sun as a solid or perhaps liquid mass in a condition of ignition; another considering the light to be electrical; a third supposing it to be the seat of a fierce combustion. Of such hypotheses we have given reason for declining the first. Prismatic analysis, which demonstrates no resemblance between the light of the sun and that of any form of electric discharges with which we are familiar, enables us in like manner to reject the second; and upon the whole, facts seem most strongly to prepossess us in favor of the third, in artificial combustions similar fixed lines being observed. If such is to be regarded as the physical condition of the sun, we can no longer contemplate it as an immense mass, slowly and tranquilly cooling in the lapse of countless centuries by radiation into space, as so many considerations drawn from other branches of science have hitherto led us to suppose;

but it must be regarded as the seat of chemical changes going on upon a prodigious scale, and with inconceivable energy. If the law designated above, that the more energetically the chemical action in combustion the more refrangible the emitted light, be translated into the conceptions of the undulatory theory, it not only puts us in possession of a distinct idea of the manner in which the combustive union of bodies is accomplished, the quickness of vibration increasing with the chemical energy, but it also enables us to transfer for the use of chemistry some of the most interesting numerical determinations of optics.

OPTICAL PROPERTIES OF PHOSPHORUS.

At the last meeting of the British Association, Dr. Gladstone read a communication from himself and Rev. T. Dale, on some optical properties of phosphorus. He said that phosphorus was known to be highly refractive and disfusive. Its refractive index had been determined at 2·125 or 2·224, a number scarcely exceeded by that of diamond or chromate of lead. This determination was made without reference to temperature, and was that part of the spectrum measured indicated. Their own experiments produced numbers which showed not merely a very high refractive power, but an amount of disfusion unknown in any other substance. The disfusive power was nearly twice that of bi-sulphide of carbon, and largely exceeded that of even oil of cassia; its only rival was that assigned to chromate of lead, but some doubt seemed to rest on that determination. The determinations of the disfusion of phosphorus, made by persons experimenting, had indicated an amount scarcely exceeding that of bi-sulphate of carbon; but a difficulty attending the examination of phosphorus would sufficiently explain this. Phosphorus in a liquid condition had apparently never been examined, as difficulties had arisen from its inflammability, and from the action on cement. An examination of the properties of liquid phosphorus showed a considerable diminution of both the refractive and the disfusive power, it not being in direct ratio with the diminution of density. Liquid phosphorus exhibits a greater amount of sensitiveness than had been observed in any other substance, and it was evidently greater at the high than at the low temperatures. The effect of temperature on disfusion could not be accurately determined. A saturated solution of phosphorus in bi-sulphide of carbon was almost as refractive and disfusive as melted phosphorus itself. There was a certain want of clearness in phosphorus which prevented the lines being distinguished without great difficulty, which did not arise from any opacity, or from the crystalline character of solid phosphorus, or from unmelted pieces floating about; for it occurred in a solution of bi-sulphide of carbon. The addition of phosphorus to bi-sulphide of carbon rendered the spectrum seen through it misty, according to the amount of phosphorus. This was not due to the great refraction, or the great disfusion, or the great sensitiveness, though this had undoubtedly something to do with it. To what was this due? Different specimens of phosphorus differ widely in respect to this property, and it was perhaps connected with some want of homogeneity in the substance. The phosphorus experimented on was generally colorless. It was a curious circumstance that yellow phosphorus cuts off the extreme red ray — this being the opposite of what yellow bodies usually did, and was remarkable also in connection with the red modification of phosphorus.

RESEARCHES ON THE INDICES OF REFRACTION.

Jamin has undertaken to determine the refracting power of water when compressed or when reduced to vapor. The experiments were executed by means of the author's very beautiful apparatus for interferences, described in the 42d volume of the Comptes Rendus. The water examined was enclosed in two parallel tubes, one of which was open, while the other was subject to variable pressure. At every change of pressure the fringes underwent a displacement, which was measured, and from which the variations in the refracting power of the liquid could be calculated. To avoid the error arising from the increase in the length of the compressed column, the two tubes were plunged into a trough full of water, so that the interfering rays traversed the length of the tubes and the spaces separating their extremities from the sides of the trough. If one of the tubes changes its length by a small quantity, the external space diminishes by the same quantity, and thus the effect of the dilatation is sensibly destroyed. The author finds that with this apparatus one millimetre of pressure, more or less, produces an interval of $\frac{4}{100}$ of a fringe, which is easily observed: for an entire atmosphere there is a displacement of 28 fringes. The sensibility of the apparatus could be still more increased by giving the tubes a greater length than that of one metre which was employed. The author found that, in all his experiments, the difference of path produced by pressure was sensibly proportional to the pressure; so that if we calculate the compressibility of water from the optical experiments, we find the coëfficient to be 0·0000590 for common distilled water, and 0·0000511 for water deprived of air. According to the direct measures of Grassi, this coëfficient is 0·0000501. Jamin has also measured with the same instrument the index of refraction for the vapor of water. Two tubes were employed 4 metres in length: one of these was filled with perfectly dry air; the other with air charged with a known proportion of the vapor of water. The difference in the refractive powers could then be observed by the change produced in the fringes. There was generally a difference of 8 fringes between dry and saturated air. More than fifty measurements made under very different circumstances of temperature, pressure, and hygrometric condition, agreed in assigning to the refractive power of vapor at 0° and 760^{mm} the value 0·000521. The author finds farther that the diminution in the index of refraction of air by saturation with vapor would only affect the seventh decimal of the number 1·000292. . . . . found for that index, and that, consequently, in astronomical refractions, it is useless to trouble oneself about the vapor of water. — *Comptes Rendus,* xlv. 892.

NOTES ON THE SCINTILLATION OF STARS.

The following is an abstract from a paper recently read before the Royal Astronomical Society, England, by Prof. Dufour:

Down to the year 1852 no person, as far as I am aware, had undertaken a serious of regular observations on the scintillation of the stars. Struck with the difference which the phenomenon presented from night to night, I commenced in that year to observe it assiduously. At first it occurred to me merely to make it a subject of meteorological inquiry; but I soon found that the question was more complicated than I originally supposed it to be, and that in any case, before entering upon its discussion, it would be necessary to collect together a mass of observations extending over a considerable period of time. This object I have actually accomplished. I have now more than 20,000 observations, and the number is daily increasing. I have commenced the calculations and reductions, relative to meteorological researches, but much yet remains to be done before bringing them to a close. However, as Arago has well remarked, in scientific inquiries, what had not been foreseen has most frequently the lion's share. Although at first I had not the remotest idea of studying the phenomenon of scintillation for its own sake, still I have been led, by the force of circumstances, to consider the subject; and in the course of last year I succeeded in establishing the following propositions, which I have developed in a memoir published in the "Bulletin de la Société Vaudois des Sciences Naturelles:" 1. The scintillation of one star differs from the scintillation of another star, and in general red stars scintillate less than white stars. 2. Except in the case of stars near the horizon, the scintillation is very nearly proportional to the product obtained by multiplying the astronomical refraction of the star by the thickness of the aërial stratum traversed by the rays of light emanating from the star. By adopting the theory of scintillation proposed by M. Arago, who was of opinion that the phenomenon depends entirely upon the principle of the interference of light, I have assigned an explanation of the first of these facts, namely, that red stars scintillate less than white stars. Professor Montigny, of Antwerp, who has devoted much attention to the theory of scintillation, has adduced another explanation of the fact, which he conceives to be established by my observations. He supposes that a ray of homogeneous light — like redlight, for example — is less dispersed by astronomical refraction, so that the pencil of light from a red star reaches the eye in a less expanded state, so to speak, than a pencil of white light, and is, consequently, liable to be partially turned aside or modified by atmospheric disturbances. Hence, according to the theory of M. Montigny, the scintillation of stars, whose light is homogeneous, ought to be more feeble than that of white stars. I do not wish to pronounce an opinion here between the theory of M. Montigny and that which I have proposed. However, it is not difficult to see that the study of the scintillation of the stars may give rise to questions of great importance in regard both to optical and meteorological science. Now, with a view to this object, it would be interesting to study the phenomenon in different climates and at different altitudes. Accordingly, in the year 1856, I spent some time at the Hospice of Great St. Bernard, at an altitude of 2475 metres, in order to make observations on the phenomenon of scintillation, and I found it to be much less intense than on the plains. Since that time Mahmoud Effendi, Director of the Observatory of Cairo, has also resolved to undertake the study of this phenomenon at the Observatory confined to his charge. He has announced to me that he will shortly visit me at Morges, France, to confer with me on the subject, and that he will then return to Egypt and commence his observations.

The following are some of the points upon which I conceive it would be important to call the attention of observers: 1. The observation on each evening of the progress of scintillation, according as the stars are ascending or descending with respect to the horizon. Do the stars scintillate at all altitudes? Is there any altitude at which it ceases to manifest itself? At Morges the stars in general scintillate at all altitudes, although feebly near the zenith; but on the nights when the scintillation is very faint, it ceases completely at a zenith distance of 45°. Is it so also on the peak of Teneriffe? 2 Is there a very marked difference between the scintillation of one evening and that of another? 3. Do the stars scintillate less feebly on the peak of Teneriffe than when observed from the plains below, as in the case of Mount St. Bernard? 4. It would be interesting to observe, upon ascending the peak of Teneriffe, whether the stars Achernard and Canopus, which are invisible in our latitudes, scintillate more or less intensely than certain other stars of comparable magnitude. I hope that you will be able to continue the prosecution of the interesting observations which you have commenced; and since for this expedition it is necessary to obtain the authorization of the government, which must possess adequate materials for forming an opinion beforehand of the importance of the expedition, you might suggest the scintillation of the stars as an additional phenomenon to be observed at that exceptional station. I was glad to learn that on the peak of Teneriffe the stars appeared to scintillate faintly. This was exactly conformable to the observations I made during my residence at St. Bernard, notwithstanding that the altitude was much less considerable (only 2180 metres). It coincides in every respect with the results at which I have arrived. I continue to pursue my observations as formerly, and labor at their reduction; but when there are more than 20,000 observations to compute in various ways, to combine by the hour, by the day, and according to certain meteorological conditions, the labor is immense, and cannot certainly be accomplished in less than several years. The explanation of the phenomenon of scintillation proposed by M. Montigny appears to me admissible, and equally probable with that offered by myself, according to which I attribute the faint scintillation of red stars in the circumstance that the red wave being the greatest wave, it would less readily interfere, and, consequently, would with greater difficulty be destroyed, or increase in intensity. Perhaps the only mode of deciding between these two explanations would consist in observing the scintillation of violet stars. If the theory of M. Montigny is correct, a violet star, like a red star, ought to scintillate less feebly, because it is composed of homogeneous light. If, on the other hand, my explanation of the phenomenon is well founded, the violet star ought to scintillate more intensely than a white star, because the violet wave is the smallest wave. Unfortunately, there is no violet star sufficiently bright for such comparative observations. Accordingly, at present, I do not wish to pronounce between the two explanations, each of which appears to me to be possible. But precisely this question and others suggested by the scintillation of the stars prove that the study of the subject may possess a high degree of interest in several respects.

INFLUENCE OF LIGHT ON THE RESPIRATION OF THE LOWER ANIMALS.

M. Beclard, of France has recently made some curious experiments on the Influence of Light on Animals and finds that those creatures which breathe from the skin, and have neither lungs nor branchiæ, undergo remarkable modifications under different colored rays. He exposed the eggs of flies (*Musca carnaria*) under bell-glasses of six different colors: little maggots were hatched from all; but those under the blue and violet rays were more than a third larger than those under the green. Frogs, which by reason of their naked skin, are very sensitive to light, give off half as much more carbonic acid in a given time under the green ray as under the red; but if the frogs are skinned, and the experiment is repeated, the excess is then with those under the red ray. Frogs placed in a dark chamber lose one-half less of moisture by evaporation, than when placed in common daylight.

ON MOLECULAR IMPRESSIONS BY LIGHT AND ELECTRICITY.

The following is an abstract of a paper on the above subject recently read before the Royal Institution, London, by Mr. Grove:

The term molecular is used in different senses by different authors. It is used in the present connection to signify the particles of bodies smaller than those having a sensible magnitude, or as a term of contradistinction from masses. If there be any distinctive characteristic of the science of the present century, as contrasted with that of former times, it is the progress made in molecular physics, or the successive discoveries which have shown that when ordinary ponderable matter is subjected to the action of what were formerly called the imponderables, the matter is molecularly changed. The remarkable relations existing between the physical structure of matter, and its effect upon heat, light, electricity, magnetism, etc., seem, until the present century, to have attracted little attention: thus, to take the two agents selected for this evening's discourse, Light and Electricity, how manifestly their effects depend upon the molecular organization of the bodies subjected to their influence. Carbon in the form of diamond transmits light but stops electricity. Carbon in the form of coke or graphite, into which the diamond may be transformed by heat, transmits electricity but stops light. Leonard Euler alone conceived that light may be regarded as a movement or undulation of ordinary matter; and Dr. Young, in answer, stated as a most formidable objection, that if this view were correct all bodies should possess the properties of solar phosphorus, or should be thrown into a state of molecular vibration by the impact of light, just as a resonant body is thrown into vibration by the impact of sound, and thus give back to the sentient organ an effect similar to that of the original impulse. In the last edition of his "Essay on the Correlation of Physical Forces" (1855), Mr. Grove has made the following remarks on this question: "To the main objection of Dr. Young that all bodies would have the properties of solar phosphorus if light consisted in the undulations of ordinary matter, it may be answered that so many bodies have this property, and with so great variety in its duration, that *non constat* all may not have it, though for a time so short that the eye cannot detect its duration." The above conjecture has been substantially verified by the recent experiments of M. Niepce de St. Victor, of which the following is a short *résumé:* — An engraving which has been for some time in the dark is exposed to sunlight as to one half, the other half being covered by an opaque screen: it is then taken into a dark room, the screen removed, and the whole surface placed in close proximity to a sheet of highly sensitive photographic paper, the portion upon which the light has impinged is reproduced on the photographic paper, while no effect is produced by the portion which had been screened from light: white bodies produce the greatest effect, black little or none, and colors intermediate effects. Mr. Grove had little doubt that had the discourse been given in the summer instead of mid-winter, he could have literally realized in this theatre the Laputa problem of extracting sunbeams from cucumbers! While fishing in the grounds of M. Seguin, at Fontenay, Mr. Grove observed some white patches on the skin of a trout, which he was satisfied had not been there when the fish was taken out of the water. The fish having been rolling about in some leaves at the foot of a tree, gave him the notion that the effect might be photographic, arising from the sunlight having darkened the uncovered, but not the covered portions of the skin. With a fresh fish a serrated leaf was placed on each side, and the fish laid down so that the one side should be exposed, the other sheltered from light: after an hour or so the fish was examined, and a well-defined image of the leaf was apparent on the upper or exposed side, but none on the under or sheltered side. The number of substances proved to be molecularly affected by light is so rapidly increasing, that it is by no means unreasonable to suppose that all bodies are in a greater or less degree changed by its impact. Passing now to the molecular effects of electricity, every day brings us fresh evidence of the molecular changes effected by this agent. The electric discharge alters the constitution of many gases across which it is passed; and it was shown that by passing it through an attenuated atmosphere of the vapors of phosphorus, this element is changed by the electric discharge into its allotropic variety, which is deposited in notable quantity on the sides of the receiver. In this experiment, the transverse bands or striæ discovered by Mr. Grove, in 1852, are very strikingly shown. The glow which is seen on excited electrics, such as glass, was also shown by Mr. Grove to be accompanied with molecular change. Letters cut in paper, and placed between two well-cleaned sheets of glass, then formed into a Leyden apparatus, by sheets of tin-foil on their outer surfaces, and then electrified, by connection for a few seconds with a Ruhmkorf coil, had invisible images of the letters impressed upon the interior surface, which were rendered visible by breathing on them, and rendered visible, and at the same time permanently etched by exposure, after electrization, to the vapor of hydrofluoric acid. So, again, if iodized collodion be poured over the surface of glass having the invisible image, and then treated as for a photograph, and exposed to uniform daylight, the invisible image is developed in the collodion film, the invisible molecular change being conveyed to the molecular film, and rendering it, when nitrated, more sensitive to light in the parts where it has been in proximity to the electrical impression, than in the residual parts. Here we have a molecular change, produced first by electricity on the glass, then communicated by the glass to the collodion, then changed in character by light, and all this time invisible, and then rendered visible by the developing chemical agent. Mr. Babbage had observed that some plates of glass which had formed the ornamented margin of an old looking-glass, and were backed by a design in gold-leaf covered with plaster of Paris, showed, when this backing was removed by soft soap, an impression of the gold-leaf device, which was rendered visible by the breath on the glass. Some of the plates had been kindly lent by him for this evening; and in one, Mr. Grove had removed a portion of the backing, and the continuation of the gilded design came beautifully out by breathing on the glass while in the frame of the electric lamp, and was projected (as were the previous electrical images) on a white screen. Of the practical results to science of the molecular changes forming the subject of this evening's lecture, a beautiful illustration was afforded by the photographs of the moon by Mr. De la Rue, which afforded, by the aid of the electric lamp, images of the moon, of six feet diameter, in which the details of the moon's surface were well defined, — the cone in Tycho, the double cone in Copernicus, and even the ridge of Aristarchus, could be detected. The bright lines, radiating from the mountains, were clear and distinct. A photograph of the planet Jupiter was also shown, in which the belts were very well marked, and the satellites visible. The following question was suggested by Mr. Grove. As telescopic power is known to be limited by the area of the speculum or object-glass, even assuming perfect definition, as the light decreases inversely as the square of the magnifying power, a limit must be reached at which the minute details of an object become lost for want of light. Now, assuming a high degree of perfection in astronomic photographs, these may be illuminated to an indefinite degree of brilliancy by adventitious light. With a given telescope, could a better effect be obtained, by illuminating the photographic image, and applying microscopic power to that, than by magnifying the luminous image in the usual way by the eye-glass of the telescope? Can the addition of extraneous light to the photograph permit a higher magnifying power to be used with effect than that which can be used to look at the image which makes the photographic impression? In other words, is the photographic eye more sensitive than the living eye; or can a photographic recipient be found which will register impressions which the living eye does not detect, but which, by increased light or by developing agents, may be rendered visible to the living eye? The phenomena treated of, which are a mere selection from a crowd of analogous effects, show that light and electricity, in numerous cases, produce a molecular change in ponderable matter affected by them. The modifications of the supposed imponderables themselves have long been the subjects of investigation; the recent progress of science teaches us to look for the reciprocal effects on the matter affected by them. Few, indeed, if any, electrical effects have not been proved to be accompanied with molecular changes; and we are daily receiving additions to those produced by light. Mr. Grove feels deeply convinced that a dynamic theory, one which regards the imponderables as forces acting upon ordinary matters in different states of density, and not as fluids or entities, is the truest conception which the mind can form of these agents; but to those who are not willing to go so far, the ever-increasing number of instances of such molecular changes affords a boundless field of promise for future investigation, for new physical discoveries, and new practical applications.

NIEPCE DE ST. VICTOR'S DISCOVERIES IN REGARD TO THE ACTION OF LIGHT.

The following is a *résumé* of the highly important discoveries recently made by M. Niépce de St. Victor, in regard to the action of light, as communicated by M. Chevreul to the French Academy.

The conditions now determined are — that *any body, after having been exposed to light, retains in darkness some impression of this light.* M. Niépce remarks — "The phosphorescence and the fluorescence of bodies are well known, but I am not aware that any experiments have ever been made on the subject which I am about to describe."

Expose to the direct rays of the sun, during a quarter of an hour at least, an engraving which has been kept many days in obscurity, and of which one-half has been covered by an opaque screen; then apply this engraving upon a very sensitive photograph paper, and, after twenty-four hours' *contact in darkness*, we shall obtain, *in black*, a reproduction of the white parts of the engraving, which, in the process of insulation, has not been sheltered by the screen.

If the engraving has been kept for many days in profound darkness, and we then apply it upon sensitive paper, without having previously exposed it to light, it is not reproduced. Certain engravings which have been exposed to light are reproduced better than others, according to the nature of the paper; but all kinds of paper, even the filtering paper of Berzelius and the *papier de soie*, with or without a photographic design, and others, are reproduced more or less perfectly after exposure to light. Wood, ivory, parchment, and the living skin, are reproduced perfectly under the same circumstances; but metals, glass, and enamels, are not reproduced. If an engraving is exposed to the rays of the sun for a very long time, it is saturated with light; and the intensity of the impressions obtained by contact in darkness is so great, that M. Niépce hopes to arrive at a process by which, operating upon very sensitive papers — as paper prepared with the iodide of silver, for example, or upon the dry collodion or albumen tablets, and developing the image with gallic or the pyrogallic acid — to obtain proofs sufficiently vigorous to form an original, from which impressions may be taken. A new means for reproducing engravings will thus be secured.

Further results of M. Niépce's experiments are described by M. Chevreul as follows:

If we interpose a plate of glass between the engraving and the sensitive paper, the whites of the engraving are no longer impressed upon it. The same interruption of the radiations takes place if we interpose a plate of mica, or a plate of rock-crystal, or of yellow glass stained with the oxide of uranium. We discover, further, that these substances arrest equally the impression of the phosphorescent rays when placed directly in front of the sensitive paper.

An engraving covered with a film of collodion or of gelatine, is reproduced; but an engraving covered with a layer of varnish or of gum, is not reproduced. An engraving placed at three millimetres' distance from the sensitive paper, is very well reproduced; and if the design is of a bold character, it will be reproduced at the distance of a centimetre.* The impression is not, then, the result of action of contact, or of chemical action. A colored engraving of many colors is reproduced very unequally; that is to say, the colors imprint their image with different intensities, varying with their chemical nature — some producing an impression which is very visible, whilst others scarcely tint the sensitive paper.

It is similar with characters printed with different inks. Printers' ink, whether it be such as is used with type or for copper-plate printing, and the ordinary writing ink, formed of a solution of nutgalls and sulphate of iron, do not give images; while certain "English inks give impressions sufficiently strong." Vitrified characters, traced upon a plate of varnished porcelain, or covered with enamel, are imprinted upon the sensitive paper without the porcelain itself leaving any trace of its presence; but a porcelain not covered with varnish or enamel, such as *biscuit china* or "*la pâte de kaolin,*" produces a slight impression.

If, after having exposed an engraving to the light during one hour, we apply it upon a white card which has remained in darkness during some days, and if, after having left the engraving in contact with the card during twenty-four hours at least, we put *the card in its turn* in contact with a leaf of sensitive paper, we shall have, after twenty-four hours of this new contact, a reproduction of the engraving, a little less visible, it is true, than if the

*The millimetre is 0·03937 of an English inch. The centimetre is 0·39371 of an English inch.

engraving had been applied directly upon the sensitive paper, but yet distinct.

When a tablet of black marble, lightly strewn with white spots, after having been exposed to the light, is applied at once to a sensitive paper, the white parts of the marble only are imprinted upon the paper. Under the same conditions, a tablet of white chalk will produce a sensible impression, while a tablet of charcoal will produce no such effect. When a black and white feather has been exposed to the sun, and applied in darkness to a sensitive surface, the white parts alone imprint their image. The feather of a parrot — red, green, blue, and black — has given scarcely any impression, acting as if the feather had been black. Certain colors, however, have left traces of a very feeble action.

Experiments have been made with textile fabrics of different natures and of various colors. The following are a few of the results:

Cotton—*White* impressed the sensitive paper.
" *Brown* (by madder and alumina). Nothing given.
" *Violet* (by madder, alumina, and iron). Scarcely anything.
" *Red* (by cochineal). Nothing.
" *Turkey Red* (by madder and alum). Nothing.
" *Prussian Blue*, upon white ground, is the blue which produces the best impression.
" *Blue* (by indigo). Nothing.
" *Chamois* (by peroxide of iron). No impression.

Linen, silk, and woollen cloths give equally different impressions, according to the chemical nature of the colors.

M. Niépce calls particular attention to the following experiment, which is, as he says, curious and important:

We take a tube of metal — of tin-plate, for example, or of any other opaque substance — closed at one of its extremities, and cover the interior with paper or white card; the open end of the tube is exposed for about an hour to the direct rays of the sun. Then apply this open end to a sheet of sensitive paper, and preserve it in this state for twenty-four hours, when the circumference of the tube will have designed its image. More than this. *If an engraving upon china paper is interposed between the tube and the sensitive paper, we find the same reproduced.* Reproduced, be it remembered, by the radiations which have been absorbed and redeveloped from the interior of the tube. " If we close the tube hermetically as soon as we cease to expose it to the light, we shall preserve, during an indefinite time, the faculty of radiation, which the insulation has communicated, and we shall see that this is manifested by the impression produced when we apply the tube upon a sensitive paper, after having removed the cover by which the tube was closed."

Niépce then informs us that he has repeated upon images formed in the camera-obscura similar experiments to those which he has made with the direct light. A piece of card which had been kept in darkness was placed in the camera-obscura for about three hours, and on it was projected an image brilliantly illuminated by the sun. Then the *card was applied to sensitive paper*, and after twenty-four hours there was obtained a reproduction of the primitive image of the camera-obscura. There must be a long exposure to obtain an appreciable result.

It will be remembered that, some few years since, Professor Stokes drew attention to some peculiar conditions of light, to which he gave the name of *fluorescence.* M. Niépce has made several experiments with substances which possess this peculiar property. A design was traced upon a sheet of white paper with a solution of sulphate of quinine, one of the most fluorescent bodies; the paper was then exposed to the sun, and subsequently applied to the sensitive paper. The fluorescent parts were reproduced in black, much more intense than that of the paper upon which the design was formed. A plate of glass interposed between the design and the sensitive paper prevented any impression. A plate of glass, colored yellow by the oxide of uranium, produced the same effect. If the design in sulphate of quinine has not been exposed to light, nothing is produced upon the sensitive paper. M. Niépce then tells us that a design traced with phosphorus upon paper, will, without being exposed to light, impress very rapidly the sensitive paper. This impression is, beyond all doubt, due to the formation of phosphide of silver — it is a chemical change quite independent of the luminous effect, and has nothing in common with the other phenomena. He says, however, that the same effects are produced by fluate of lime, rendered phosphorescent by heat.

Such are the principal matters to which M. Niépce now directs attention; and if his results are confirmed by further experiments, they must materially change our views of luminous variations.

Addenda.—The details of the method of reproducing engravings by means of the vapor of phosphorus, alluded to above, are given as follows in the *Cosmos* (Paris):

The engraving to be copied is exposed to the vapors of phosphorus burning slowly in the air, the shadows only absorb the vapors; a sheet of sensitive paper prepared with chloride of silver is applied; after a contact for a quarter of an hour, the engraving is imprinted upon the paper with phosphuret of silver, which, when strong enough, resists the action of dilute chemical agents. The best mode of operating, consists in placing the engraving in a box in front of a sheet of pasteboard, covering one side of the box, whose surface has been sufficiently rubbed with a stick of phosphorus. The pasteboard must be rerubbed for each operation; for if the phosphorus is red, it produces no effect. A sheet of water of a centimetre (0·4 inch) or more in thickness, does not stop the deposition or action of the vapors of phosphorus. The action is exerted on the sensitive paper even through india-paper; that is to say, that if an engraving on india-paper is laid upon a sheet of sensitive paper, and these placed together in the box in face of the phosphorescent wall, a negative image of the engraving will be obtained, as if the shadows had behaved like a screen, and the lights had allowed the vapors to pass through and impress the sensitive paper. Only if the exposure be too long prolonged, the shadows will also impress their image, and this will even prevail over the ground. The vapor of sulphur produces analogous effects, and gives an image or reproduction of the engraving drawn in sulphuret of silver; but this image is not stable.

LUNAR AND STELLAR PHOTOGRAPHS.

At a recent meeting of the Royal Astronomical Society, Mr. De la Rue exhibited a great variety of beautiful photographs of the moon and Jupiter, which, through the aid of a magnifying glass of moderate power, exhibited a considerable amount of detail, not visible to the unassisted eye.

While on the subject of lunar photography, Mr. De la Rue begged to direct the attention of the Society to one or two points of physical interest, which may be thus stated: points on the lunar surface having optically equal intensity of light, do not produce equally brilliant positive and equally obscure negative impressions; the actinic rays are evidently not always in proportion to the illuminating rays. Another curious fact, which he thought he had well made out, is, that those portions of the lunar surface which are illuminated by a very oblique ray from the sun, do not produce an equal effect on the sensitive plate, though they are equally bright to the eye. Such a phenomenon obtains during the afternoon in terrestrial photography, when the sun's rays reach us obliquely through the atmosphere. The moon has no visible atmosphere; nevertheless, from whatever cause it may arise, that portion of the moon which is illuminated by an oblique ray, does not produce a corresponding effect on the sensitive plate which it does to the eye.

By paying particular attention to the state of the collodion film, he had been very successful in reducing the time of exposure, and had produced pictures, not only of the lunar surface, but also of Jupiter, in from three to seven seconds. The photographs of Jupiter show his belts remarkably well. The beauty of the photographs exhibited of the moon, he thought it would be admitted, gave great promise that at a future period photography will be considered as the only correct means of mapping down the lunar surface. When we shall be able to obtain collodion finer in grain and still more sensitive, it will supersede hand-drawing altogether; and even now the results obtained are much more accurate than anything hitherto done by mapping or hand-drawing. It is nearly impossible, by micrometrical measurement, to lay down all the details of the moon, and much, after a sort of triangulation, has to be filled up by the eye. The work is too laborious; and the famous map of Beer and Madler, wonderfully accurate as it is, does not fulfil the conditions of absolute accuracy in all the minute points of detail.

The light proceeding from Jupiter possessed considerably more actinic power than that of the moon, in proportion to its luminosity; or, in other words, although the light of the moon is at least twice as bright as that of Jupiter, its actinic power would appear to be not greater than as from 6 to 5, or 6 to 4. It is not improbable that the blue tint of Jupiter may have something to do with its photogenic power. It may also be stated, that the darkest parts of Jupiter's surface came fully out by an exposure which did not suffice to bring out those portions of the moon situated near the dark limb, and consequently illuminated by a very oblique ray.

Mr. De la Rue also stated that he had compared the philogenic power of Jupiter and Saturn, and that, on a recent occasion, he had turned the telescope alternately on each of these two planets, and found that to produce pictures of equal intensity, the sensitized plate had, on the average, to be exposed five seconds to Jupiter and sixty seconds to Saturn. Hence the chemical rays from Jupiter are twelve times more energetic than those from Saturn — an effect undoubtedly, in a great measure, attributable to the greater brilliancy of the former planet.

ON THE ACTION OF LIGHT UPON OXALATE OF THE PEROXIDE OF IRON.

Professor J. W. Draper, in a paper on the measurement of the chemical action of light, communicated to the London Philosophical Magazine, notices particularly the action of light upon the oxalate of peroxide of iron. The golden yellow solution of this salt undergoes no change when kept in total darkness, but on exposure to a lamp or to daylight is decomposed with evolution of carbonic acid and pecipitation of oxalate of the protoxide as a lemon-yellow powder. In sunlight it effervesces violently. The indigo ray is especially active in producing this effect, and undergoes absorption in doing so, since a sunbeam which has passed through one layer of solution is incapable of affecting another. The author points out several methods of employing this salt in photometry, the most advantageous of which is to collect and measure the quantity of carbonic acid absorbed in a given time. The solution is sufficiently sensitive for all ordinary purposes. When great sensitiveness is required the author recommends the use of the tithonometer invented by him in 1843, and since employed, in a modified form, by Bunsen and Roscoe.

IMPROVEMENTS IN PHOTOGRAPHY.

Sensitiveness of Photographic Reägents. — The sensitiveness of the reägents used in photography is well known. But there are others more sensitive still, which often interfere with photographic results. Photographers are aware that they do not succeed well in the vicinity of a perfumery, and that even the effluvia from the hand that has been wet with an essence, will prevent success when it was thought to be sure. It has even been supposed that in clear weather, when the atmosphere was full of vegetable emanations and vapors drawn from the soil by heat, photographs do not succeed as well, and that these influences must be avoided for the finest results. Evidently the emanations from the soil and plants are themselves sensitive to the chemical rays, and more so than the photographic reägents, especially chloride of silver; and hence, as far as they are not destroyed, may monopolize the chemical action.

These thoughts are suggested by a fact recently made known by Mr. Ford. He had always had perfect success; but afterwards, in spite of all precaution, failed of good pictures. After long seeking the cause, he finally found that it was owing to his room being near a storehouse of "noir animal," for economical uses, — the effluvia was injurious to the photoraphic liquids, the principal of which were nitrate of silver, pyrogallic acid, and hyposulphite of soda. He moved to another place, and had no more difficulty.

Novel application of Photography.— M. Persoz, Professor of Chemistry at the Conservatoire des Arts et Mêtiers of Paris, has just published a most interesting discovery of his, by which photography may be applied to the ornamenting of silk stuffs. The bichromate of potash is a substance commonly used in photography, being extremely sensitive to light. If a piece of silk stuff, impregnated with this salt, be exposed to the rays of light penetrating through the fissures of the window-blinds in a closed room, the points where the stuff has received these rays of light will assume a peculiar reddish tint. Now, suppose a piece of metal or of strong paper to be cut out after a given pattern, and to be laid upon a piece of silk prepared as before; if exposed to the sun, or, better still, to simple daylight, the pattern will be reproduced in a few instants. The pale red, which the parts acted upon by the light assume, is so permanent that nothing can destroy it; nay, it will fix other colors, such as madder, campeachy, etc., just like a mordant, and in that case it will modify the color of those substances in absorbing it. The experiment may be varied as follows: Let a fern leaf be laid upon a piece of prepared silk and kept flat upon it by a pane of glass; then that part of the silk which is protected by the leaf will retain its original color, while all the rest will receive the impression of light, as above described, forming the ground on which the figure of the leaf will appear in white, gray, or whatever other color the silk may have had before the operation. The richest patterns may thus be obtained on plain silks, and at a comparatively small expense.

Photographic Enamels. — MM. Armengaud, in the *Genie Industriel*, announce that "the MM. Bruder, of Neufchatel (Switzerland), have discovered a process by which photographs may be developed on white enamel, incorporated by vitrification, and covered with a glaze of glass also melted and incorporated with the enamel. They apply the same process also upon metals and wood."

Practical Applications of Photography.— During the Paris exhibition of 1855, some of the non-commissioned officers of the Royal Engineers who were employed there received instruction in photography, and on their return to England their knowledge thus obtained was turned to a practical use by Colonel James, the director of the Ordnance Survey, in making reductions of the various maps and plans required in that work. This is the first instance of the scientific use of photography on a large scale, and some idea may be formed of its importance from the fact that the saving in this item only has been *not less than* £20,000.

Since then, a systematic course of instruction in photography has been given to non-commissioned officers in the English engineer corps, and this constant supply of men practiced in the art is maintained. These are distributed in all parts of the world, where a detachment of engineers is maintained, and the orders to officers commanding companies to which photographers are attached, are, to send home, periodically, photographs of all works in progress, and to photograph and transmit to the war department drawings of all objects, either valuable in a professional point of view, or interesting as illustrative of history, ethnology, natural history, antiquities, etc., etc.

New Photographic process for the Printing of Positive Proofs. — A piece of sulphur is dissolved in sulphuret of carbon, in the proportions of twenty-five parts of the former to seventy-five of the latter, and the solution is then filtered. The solution in sufficient quantity is poured on the paper, which is shaken quickly in every direction, in order that it may spread equally, and crystals of opaque sulphur may not form. The paper thus prepared is kept in the dark. At the required moment, the paper is put under an ordinary negative, and exposed to the light for twenty-five seconds to a minute, or five minutes on a dark day. When taken out, nothing is seen on the paper.

Nothing appears upon the surface of the paper when it is removed from the slide. It is then put over a mercury bath, at the bottom of which are placed some grams of this metal. The sulphurized paper from the camera is then exposed at the distance of eight centimetres above the mercury (which is heated meantime), sustained on a frame of paper forming a cover to the bath, on the under side of which, with its face to the mercury, is the prepared paper. The vapor of mercury combines with the portions of sulphur which have received the influence of the sun's rays, forming a yellowish brown sulphuret of mercury, which perfectly reproduces the details of the impression. The picture is then protected by a film of varnish-gum, or albumen, to preserve it.

This is the process of M. Salmon, of Chartres.

McCraw's new, cheap, and permanent process in Photography.— The following account of a new process for producing cheap and permanent photographs without employing either silver or gold salts, or the noxious hyposulphite of soda, was presented to the British Association, 1858, by Mr. W. McCraw:

"The labors of the committee appointed by the Photographic Society of London to inquire into the cause of the fading of photographs, after a elapse of two years, have only amounted to this: that photographs of a certain kind have all faded; and that some of those of the kind that have stood best, have unaccountably faded — the sad presumption being, that in time all photographs produced in the usual way, by the means of chloride of silver, and fixed (as it is called) by hyposulphite of soda, will perish. These considerations, and the fact of a prize being offered by a French nobleman for the discovery of a process for printing photographs in carbon, set me to experiment in that direction. But my experiments with carbon and various pigments led me to think that no material applied mechanically, or that could not be made to take the shape of a dye or chemical solution, would ever give results with the exquisite half-tints of the present beautiful but perishable process. The photographic properties of bichromate of potass were pointed out by Mungo Poutou twenty years ago, giving photographs of a pale, tawny color. A piece of paper is washed over with the saturated solution of the bichromate, and when dried in the dark is of a light yellow color, and very sensitive to light. If a negative photograph, or a piece of lace or a leaf, be placed over the prepared paper, and put in sunshine, in a few minutes a perfect impression of the object is obtained. The light darkens the color of the bichromate, and renders it insoluble in water, while the yellow color washes out from the parts protected from the light by the lace or leaf, or negative photograph, as the case may be. But pictures of this kind have little or no practical value; for although the lights are good enough, the deep black shadows are only represented by a tawny shade. Some eighteen months ago a process was patented for deepening these photographs by treating them with gallic acid and a salt of iron, which went by the name of 'Sella's process.' I tried this process at the time according to the specification of the patent, but failed to make one satisfactory specimen. They wanted everything that a good photograph should have, — pure lights, clear half-tints, and deep shadows, — and as I found that others had not been more successful, I abandoned my experiments. But in the course of further experiments, a year afterwards, with carbon, I was struck with the fact, that a drop of a solution of bichromate of potass allowed to fall on a piece of white paper and afterwards dried and exposed to the sun, when washed with a solution of proto-sulphate of iron, and then with gallic acid, while the spot became perfectly black, the surrounding white paper was unaffected by the liquids. Knowing the photographic properties of the bichromate already described, I believed that this might be the foundation of a good photographic process; and that if the bichromate could be kept from penetrating the pores of the paper, by being kept on its surface, the defects of Sella's process might be avoided. With this view, I began by filling the pores of the paper with albumen, and then, to render it insoluble, immersing the paper in ether. This, however, did not answer. But as it would be tedious to detail all the pains I took to discover what would not do, and to find in what proportions and in what order the right materials could be best applied, I will briefly give the formula which I have adopted, and by which the specimens alluded to were produced: — First, take the white of eggs, and add 25 per cent. of a saturated solution of common salt (to be well beat up and allowed to subside); float the paper on the albumen for thirty seconds, and hang up to dry. Secondly, make a saturated solution of bichromate of potass, to which has been added 25 per cent. of Beaufoy's acetic acid. Float the paper on this solution for an instant, and when dry it is fit for use. This must be done in the dark room. Thirdly, expose under a negative, in a pressure frame, in the ordinary manner, until the picture is sufficiently printed in all its details, — but not over-printed, as is usual with the old process. This requires not more than half the ordinary time. Fourthly, immerse the pictures in a vessel of water in the darkened room, — the undecomposed bichromate and albumen then readily leaves the lights and half-tints of the picture. Change the water frequently, until it comes from the prints pure and clear. Fifthly, immerse the picture now in a saturated solution of protosulphate of iron in cold water for five minutes, and again rinse well in water. Sixthly, immerse the pictures again in a saturated solution of gallic acid in cold water, and the color will immediately begin to change to a fine purple black. Allow the pictures to remain in this until the deep shadows show no appearance of the yellow bichromate; repeat the rinsing. Seventhly, immerse, finally, in the following mixture: Pyrogallic acid, two grains; water, one ounce; Beaufoy's acetic acid, one ounce; saturated solution of acetate of lead, two drams. This mixture brightens up the pictures marvellously, restoring the lights that may have been partially lost in the previous parts of the process, deepening the shadows, and bringing out the detail; rinse, finally, in water, and the pictures are complete, when dried and mounted. The advantages of this process may be briefly stated as follows: — First, as to its economy. Bichromate of potass, at 2*d.* per ounce, is substituted for nitrate of silver at 5*s.* per ounce. Secondly, photographs in this way can be produced with greater rapidity than by the old mode. Thirdly, the pictures being composed of the same materials which form the constituent parts of writing-ink, it may be fairly inferred that they will last as long as the paper upon which they are printed."

Gaudin's mode of preserving Photographs on paper. — I dissolve a certain quantity of the gutta-percha of commerce in the *Colas* benzine. I decant in a few days, so as to have only the clear portion. I plunge my paper, sheet by sheet, in this solution, and withdraw it almost immediately; then, hanging it by a corner, I let it dry. I then present these sheets which contain the gutta-percha as a powder, but not as a varnish, to a good fire. The grains of gutta-percha unite, and cover the fibres, forming an interior varnish which is nearly impermeable.

I albuminize this paper which has lost none of its transparence (albumen, 100; rain-water, 25; chloride of sodium, 6). I dry it, and render it sensitive by a solution of 15 per cent. of nitrate of silver. I allow it to drip, and dry it by a gentle fire; I produce a positive in the usual way, and fix it by hyposulphite of soda at 10 or 15 per cent.; but this operation is so much abridged, that in a few minutes the proof is fixed like one on glass, and of a beautiful sepia tint. Nothing prevents the use of chloride of gold, if that is desired. The washing may be done in a quarter of an hour, in place of lasting from twelve to twenty-four hours, and the proof is of admirable transparence, the paper also keeping all its whiteness. — *Comptes Rendus.*

Photo-Lithography. — This name has been applied by the discoverers, Messrs. Cutting & Bradford, of Boston, to a new and beautiful application of the photographic art, which is evidently destined to revolutionize, in a great measure, the ordinary processes of lithography. The requisites for the production of the effect are, in the first instance, a well-ground lithographic stone, and a glass negative photograph. The stone, by a peculiar treatment, which is really the discovery itself, and is yet a secret, is prepared in a few minutes to receive the photographic impression, through the glass negative, by exposure to sun-light. The stone-picture can then be washed, inked, and any number of photo-lithographs printed from it upon paper, by means of the usual lithographic press. Images magnified by the microscope, and thrown by means of the camera upon a prepared stone surface, are depicted with wonderful accuracy. The value of this discovery, as applied merely to the illustration of books, especially scientific treatises, is obvious.

The French Academy have recently received some interesting memoirs on subjects connected with photography. In the camera obscura, which is usually employed in photographic operations, the objects daguerreotyped are traced by the sun on a plane surface, whose extent (being necessarily restrained) corresponds to a field of vision which cannot well cover more than thirty or thirty-five degrees of angular space. So, too, in the art of drawing, when the draughtsman would represent a scene on the plane, he takes care to restrict it to the same limits, as they include all the objects our eye is able to take in at the same time, without changing its direction. But if he desires to depict a larger portion, or rather all the objects which may be seen in every different direction from a given station, it is evident that instead of taking a plane surface, he must take the interior surface of a cylinder; for the observer, being placed upon the axis, will discover a suitable element, on whatever side he may look, for tracing the object supposed to be placed in the same direction. This sort of cylindrical picture, developed in a circular manner around the spectator, is called a panorama. In 1845, a photographer named Martens, published a method for obtaining, upon the daguerreotype plate, and in one single operation, a demi-panorama of exterior views. The plate was curved in a demi-cylindrical form around the optical centre of the objective, which being placed on a vertical pivot, was directed in succession to the different points of the horizon, carrying along with it a diaphragm, having an aperture with vertical edges; in this way the different portions of the plate were affected in succession, so that when once the proof was completed, it represented all objects visible within an angular field of 180 degrees. The scenes obtained in this way by M. Martens, were beautiful little panoramas; but the objects were represented in it in an inverse order to that they occupied in nature, and, unfortunately, the geometrical combination on which the method was founded, prevented the operator from restoring the objects to their original position. Had the operator attempted to place a reversing mirror before the objective, he would instantly have discovered that, during the evolution of the optical system, the clouds would have assumed a relative motion on the plate, which would have rendered it impossible to take a daguerreotype. Besides, as more daguerreotypes are taken on glass than on metal at the present day, and as it is inconvenient to bend a plate of glass, M. Martens' ingenious method gradually fell out of use. We now have a new method, devised by M. Garella, an engineer of the mining school, which completely solves the problem.

When the operator takes a view on paper, the view is reversed naturally, by the simple transfer to another sheet. Therefore, imagine an apparatus constructed so as to turn on the horizontal plane which supports it, and be directed at pleasure towards any point of the horizon. While this motion is taking place, the exterior objects represented on the ground glass change places in the same direction with the head of the objective, and if the screen is suitably arranged, the operator will be able to move it in such a manner as that these points remain at the same points of the image during the whole time they take to cross the field of vision. As this movement is indefinite, the operator may in this way sweep the whole horizon. To step from this simple observation to the practical realization of a panoramic apparatus, it is necessary to find the mechanical contrivance which coördinates in the required ratio the motion of the apparatus which sustains the glass plate on which the view is to be secured, and the motion of the whole apparatus on its plane. Now, it is easily demonstrated that the motion of the screen must be such that its plane moves without slipping on the ideal cylinder which describes the mean vertical line of the focal plane by its motion round the optical centre of the objective. If this motion of the screen could be effected, each of its points would describe in space an evolvent circle; if, therefore, the evolvent is materially contracted, and secured on the base of the apparatus, and the apparatus which contains the glass plate is fastened to its edges, so as to follow its contour, it would necessarily constantly occupy a position which would assure its relative steadiness to the image. Such is the elegant solution proposed and practised by M. Garella to obtain, on a rigid plate, as extensive panoramic views as the operator may desire. If the operator does not reverse the view obtained directly in the camera obscura, the image is engendered progressively, band by band, on the inner surface of a supposed cylinder, which corresponds exactly with M. Marten's real cylinder. But if he desires to reverse the objects at once, the image must be traced on the exterior surface of a cylinder of the same radius. He will then see that the centre of rotation of the apparatus must be moved back of the screen, and to a distance equal to the principal focal length of the objective. This change (which is the geometrical translation of a change of sign) enables the evolvent of circle to continue to guide the apparatus which supports the plate and secures the clearness of the images. M. Garella's plates exhibit by their clearness the perfection of this mechanical process. They cover a surface of great length, and when they are seen lying flat it is easy to detect that they represent impossible scenes. Each part may be examined by itself; but when the spectator attempts to take in the whole, he at once perceives very great deformities in the horizontal lines, which are caused by the difference existing between the position in which it is seen and that in which it was formed, part by part. As soon, however, as the curve of the ideal cylinder is reëstablished where it was formed, and the observer places his eye at the point occupied by the objective of the camera obscura, all the objects appear at their real angles.

THE "ELLIOTYPE" PROCESS.

A new application of photography is now a matter of such frequent announcement, that the reader's patience is sorely tried by an invitation to examine anything which promises novelty in this direction. And yet we believe a thought has at length been struck out, at once so simple and so practicable, that it cannot fail to produce a great extension of the art, both in the number and style of its products. In distinguishing the method we are about to describe, it may be premised at once, that this operation does not, as in the case of the great majority of other photographic processes, profess to give an *immediate* transcript of some scene or figure existing in nature. In ordinary cases the sun transfers to the photographic paper a picture of some object or group which has already a complete and independent existence, — whether it be a human figure, a building, a piece of sculpture, an engraving, or a landscape. It records established facts, or repeats works which were designed without reference to it. But the Elliotype photographs are always the results of paintings made expressly for the purpose. This involves, of course, a principle, namely, that the peculiar skill of the living painter, with all its powers and resources, is transferred at once, fresh from his hand, to the very sheet of paper which is to spread through the world the example of his style and composition. The other important point is the simplicity of the operation, which encourages attempts, obviates failures, and insures an unusual freedom, etc., in the result. A simple idea may generally be simply expressed; and to the reader familiar with these subjects, the whole process will be rendered clear at once by the explanation, that the artist who wishes to produce an Elliotype picture, paints the subject himself in white body colors upon one side of a piece of glass. It is evident when a sheet of sensitive paper, stretched on the other side of the glass, is exposed to the light — that where the glass is transparent the paper will come out black, and where the light is blocked out by a layer of paint, it will remain white. But the paint itself is slightly translucent, and thus every variety of shade may be produced by duly graduating the thickness of the layers. This is in substance the whole of the invention — for such it is believed to be, notwithstanding its simplicity. But there are several other points which deserve attention. As the oil painting is on one side of the glass, and the photographic paper on the other, the rays of light have to pass, not only around and partially through the paint, but also through the glass, before they reach the paper. The consequence of the light passing through the glass is, from refraction, or some other cause, to give a soft, melting outline on the paper, even where there is a sharp line drawn on the glass. The effect of this, in many instances, is very welcome and desirable, as in the case of clouds, feathers, or the rounded outlines of the face, limbs, etc. Where, however, sharpness in the photograph is absolutely required, this property becomes objectionable; and a remedy was long sought for by the inventor in vain. It was discovered at length by accident. A fragment of cotton thread, or some small foreign substance, had, much to the operator's annoyance, lodged *between* the glass and the paper, *i. e.*, on the opposite side to the painting. The effect was a blur on the photograph; but it had such remarkably clear, sharp edges, that the idea instantly occurred — Why not paint the sharp lines of the picture on the *other side* of the glass, next to the iodized paper? The result turned out exactly such as was expected and required; and sharpness of outline can thus be secured, as well as roundness.

The painter's part of the proceeding is an extremely simple one; he has only to lay a piece of black paper under the glass upon which he paints; everything then is as it will finally appear in the picture. His ground is dark, and he paints in the lights, heightening his color where they are highest, and leaving it blank where they are low. The first and most obvious application of this method is to the copying of paintings and engravings. A sheet of glass can be laid over the work to be copied, and thus every line and boundary of shadow can be accurately traced, and the very handling and manner of the original imitated, until the copyist's work begins to conceal what is beneath. The glass can then be removed, the black paper laid behind the glass, and the work proceeded with in the ordinary way. When it is complete, or, if desired, at any stage of the proceedings, an endless number of photographic proofs can be taken. Engravings and lithographic stones wear out, but there is literally no end to the *fac-similes* that may be taken of an oil painting on glass. When the number of good copyists who can be found of works in oil is considered, and that their work is so much more rapid than that of an engraver, the cost of production being consequently less, it really seems as though the engraver's art, except in its very highest efforts, must be to a great extent superseded by this method. Another application of the Elliotype process is the painting of subjects from nature. The artist who paints upon glass instead of canvas, not only prepares a work which may be multiplied indefinitely in the way we have described, but which also remains a finished work of art, distinguishable only, as we are told, by practised eyes from an ordinary drawing or painting on paper or canvas. It will be remarked at once that no colors can be employed except black and white. This is undoubtedly the case, so far as painting for the copying sake is concerned; but, after that is done with, it appears to be quite possible to paint in colors upon the reverse or back of the glass, so as to give the effect of a finished painting when hung in a room. It remains only to add, that the inventor and patentee of this ingenious process, from whom it takes its name, is Mr. Robinson Elliott, an artist who has himself practised the method to a considerable extent, and with a success that is very remarkable, even at this, which he considers an early, stage of the discovery. The roundness and accuracy of outline, and the depth and transparency of the shadows, are remarkably conspicuous, whilst it is in bringing out the high lights that the skill of the artist is chiefly exercised. The adaptability of the method to subjects requiring a multiplicity of minute and sharply-defined detail has not yet been so well established as to others where the forms are simple, and the effect of the picture depends mainly upon its light and shade. That this process will recommend itself at once and extensively to original painters, for the sake of imparting to their pictures the capability of being reproduced in a cheap, rapid, and direct manner, can hardly, perhaps be expected; but that it will be resorted to for the purpose of reproduction, we cannot doubt, particularly when the results are so satisfactory in themselves, and so flattering to the hand and pencil of the painter. — *London Literary Gazette.*

PHOTOGLYPHIC ENGRAVING.

The following is a summary of Mr. Fox Tablot's recently patented invention of photographic etching, which he terms "Photoglyphic Engraving."

Mr. Talbot uses the steel, copper, or zinc plates, ordinarily employed by engravers. The plate to be operated on is covered with a thin film of a solution of the common gelatine of the shops (in the proportion of a quarter of an ounce of gelatine to eight or ten ounces of water), to which has been added about an ounce of a saturated solution of bichromate of potash. The object to be engraved, which "may be either material substances, as lace, the leaves of plants, etc., engravings, writings, or photographs, etc.," is then placed on the prepared plate, and both are screwed in a photographic printing-frame. After exposure to sun-light, or for a longer period to the common daylight, as is usual in photographic printing, the plate is removed from the frame, when "a faint image is seen upon it — the yellow color of the gelatine having turned brown wherever the light has acted." Thus far, the process is precisely the same as that of Mr. Talbot's invention, patented in October, 1852. But in that process the next stage was to wash the plate in water, or water and alcohol, in order to dissolve that portion of the gelatine on which the sun had not acted, and in so doing the image has almost invariably been found to be injured. In the new method the plate is not washed at all, but the operator proceeds at once — and in this it is that the novelty of the new method mainly consists — to cover the surface of the plate evenly with a little finely-powdered gum copal, which is then melted by holding the plate over the flame of a spirit lamp. A uniform aquatint ground is thus formed, and as soon as it is cold the plate is ready for etching. If Mr. Talbot's specification be sufficient, the etching process is the simplest of any yet practised. The etching liquid is a solution of perchloride of iron — five or six parts of the saturated solution to one of water. A small quantity of this is poured upon the plate, and with a camel's hair brush spread equally all over it. "The liquid penetrates the gelatine wherever the light has not acted on it, but it refuses to penetrate those parts upon which the light has sufficiently acted. It is upon this remarkable fact that the art of photoglyphic engraving is mainly founded. In about a minute the etching is seen to begin, which is known by the parts etched turning dark-brown or black, and then it spreads over the whole plate — the details of the picture appearing with great rapidity in every part of it." If all proceeds well, the details of the picture will present a satisfactory appearance to the eye of the operator in two or three minutes; the operator stirring the liquid all the time with a camel's hair brush, and thus slightly rubbing the surface of the gelatine, which has a good effect. When it seems likely that the etching will improve no further, it must be stopped, and the plate cleaned, when the etching is found to be completed.

The etching process, as will have been seen, is finished at once. No "stopping out" even of the more delicate parts, as in ordinary engraver's etching, would seem to be necessary; at least none is mentioned. In order to bring out faint parts, or to deepen others, we are told, however, that the operator may "touch with a camel's hair brush, dipped in liquid (No. 3), those points of the picture where he wishes for an increased effect." The No. 3 liquid is merely a weaker solution (equal parts of water and the saturated solution) of the perchloride of iron — for it is note-worthy that the weaker solution is the most rapid in its effect. A simpler process of photographic etching is inconceivable. If it answer as perfectly, and if its range be as comprehensive as is here stated, the great question of sun-engraving is in a fair way of settlement,— not settled, however, as has been too hastily assumed. The process here described is etching simply; and we fear, from the description, is too superficial to produce many perfect impressions. However, if it go no further, it is an extremely beautiful extension of heliography, and to us it seems to lay the foundation for a more satisfactory result than has yet been achieved.—*London Literary Gazette.*

The process of Herr Pretsch, which excited so much attention a year ago (see Annual Scientific Discovery, 1858), has not worked as well as was expected, inasmuch as the prepared plates were found to require too much assistance from the engraver's scraper in order to perfect them. A company formed in England to carry out the process upon a large scale, has proved, commercially, a failure. Several processes of photographic engraving are, however, now used to some extent in Europe; the best of which is that of Niépce St. Victor, of Paris, whose results resemble an aquatint engraving, and are very beautiful. The great drawback encountered by all who have worked in this field thus far, appears to be, the small number of perfect impressions which the photographically engraved plates are capable of printing.

BINOCULAR VISION.

Of the thousands who gaze with delight upon the magical effects produced by that small instrument known as the stereoscope, how few there are who comprehend, or attempt to assign reasons for, the extraordinary optical illusions experienced through its instrumentality!

It is with the view of, in some degree, elucidating the principles of vision upon which these are founded, that the following article is written.

It will, in the first place, be well to consider the difference between monocular and binocular vision. Nature has furnished us with several means of determining the distance of objects which may happen to come within reach of our visual organs. One is, that of distinctness; a greater or less degree of which — other things being equal — gives an idea of greater or lesser distance in the object viewed. The second is through the change of focus required in the lens of the eye in refracting to a point on the retina, rays of light entering it with a greater or lesser degree of parallelism, thus producing in the brain a consciousness of unequal distances in the objects from which they emanate.

The means above alluded to, it is evident, are enjoyed in almost the same degree when viewing with one eye as where both are used. By far, however, the greatest power with which nature has endowed us of discriminating distances, is through the agency of binocular vision; or, in other words, in the sensation produced in the brain by the different degrees of convergency of the optic axes required in obtaining distinct vision of the differently distant points of objects upon which they are directed. It is to this faculty that we are indebted for our most palpable evidence of differential distances, and for that consciousness of solidity and relief so remarkably experienced in the stereoscope. It is evident, for example, when we are looking at a house or other object that has depth as well as breadth, from such a point of view as to enable us to see two sides of it at once, that we receive a differently perspective image upon the retina of either eye, or that we must see more of one side and less of the other with the right eye than with the left, or *vice versa*, — thus accomplishing with one view what a person with but one eye would require two views at positions 2 1-2 inches apart — the distance between the eyes — to accomplish. These are the differently perspective views of the stereoscopic cards, and it is the effort to reconcile these dissimilar pictures by converging the optic axes at points differently distant from the eyes which produces the wonderful effect above alluded to, and which enables us to experience all the sensations of delight which would be produced by the contemplation of the landscape itself. The stereoscopic pictures will, of course, never quite correspond. They are taken simultaneously with a camera constructed with two lenses, or consecutively with a camera with one moveable lens. The lenses of the stereoscope, besides magnifying the pictures, are so placed as to unite certain similar points of them, thus relieving the eyes of too great effort in uniting them entirely by convergency of the axes.

The means above alluded to, by which we are enabled to judge of differential distances, are of course much diminished by the distance that the objects viewed are removed from us. Our consciousness of different distances by distinctness is diminished through decrease of light. Our judgment, through change of focus, is diminished in consequence of the parallelism of rays from distant objects being so nearly the same as to require but little change in refracting them to a point on the retina. And lastly, the binocular effect is in a great degree impaired through the identity of distant views when seen from positions only separated by a base of 2 1-2 inches. Nature has thus observed her usual economy in providing for our necessities alone, — it being of little comparative importance to us generally to be acquainted with the relative positions of distant objects; whereas our personal convenience, and even safety, depend greatly upon our knowledge of those near at hand. We are therefore provided with much more ample means of determining the latter than the former. — *Jour. Franklin Institute.*

TELESTEREOSCOPE.

Prof. Helmholtz has recently published a description of an instrument which he calls a Telestereoscope (telescopic stereoscope), the object of which is to present, stereoscopically united, two pictures of a landscape corresponding to two points of view, whose distances considerably exceed the distance between the two eyes. The stereoscopic power of the eyes is small, because the distance between them is small; by the instrument it is widened, and the effect which a stereoscope produces in a picture of a landscape is thrown over the landscape itself. The instrument is made up of four mirrors and two eye-glasses. Two mirrors placed, alike, at an angle of 45°, one to the right and the other to the left, receive the rays of light from the landscape. These mirrors throw the rays horizontally towards one another, to two oblique mirrors, which throw the rays through the eye-glasses to the eyes. In a window, place on either side, say three or four feet, or the width of the window apart, a mirror, at the angle stated, to receive the rays from outside, the planes of the two mirrors converging, of course, to a point in the room. The mirrors will have the position of the half-opened shutters of the window. The rays from the scene outside, on reaching them, will be thrown parallel to the window, those of one mirror towards the other. Now, by placing at the middle of the window two smaller mirrors, meeting like the legs of a V, but at an angle of 90°, and facing in the room, the rays will be thrown into the room; and if these two mirrors are not too large, or are properly placed, the rays will have just the distance apart required to pass into the eyes. A box or frame may enclose the mirrors, and a couple of lenses be inserted as eye-pieces, and the effect thereby be improved, — though the lenses should have a focal length of thirty or forty inches. The mirrors should be made of the best plate glass. The size may be much larger than the breadth of a window, although not to any very great advantage.

To see near objects in the telestereoscope, the reflectors must be turned round their vertical axes so that the angle between their surfaces and the long edge of the box is somewhat greater than 45°. The objects then appear greatly reduced in size, but in surprisingly prominent relief. When the large mirrors only are turned, the small ones being left at the angle of 45°, an exaggerated relief is obtained. If the dimensions in the direction of the depth of the field of view, to those on the surface, are to retain their right relations, the large mirrors must always remain parallel to the small ones. The aspects of near objects, particularly of the human figure, are strikingly beautiful in the telestereoscope. The impression differs from the reduction produced by concave glasses, in the circumstance that it is not reduced pictures that the observer imagines he sees, but actually reduced bodies.

Magnifying power may easily be connected with the telestereoscope: it is only necessary to place a double opera-glass between the eyes of the observer and the small reflectors; it is still preferable, for the field of view, to separate the eye-glass from the object-glass of the instrument, and so fix them in the telestereoscope that the light at each side first strike the large mirror, then the object-glass, then the small reflector, and finally, the eye-glass; so that in this arrangement the optic axis of the telescope itself is broken at a right angle. The greater the magnifying power, the greater, of course, must be the perfection of the plane reflectors; but then it is not necessary to choose them larger than the object-glass of the telescope.

These views, at the same time telescopic and stereoscopic, also exceed, to an extraordinary degree, the common image of the telescope in vividness. In the simple telescopic images, difference of distance disappears totally: the objects look exactly as if they were painted on a plane surface. By the ordinary combination of the two Galileo's telescopes, the appearance of relief for nearer objects is in some degree obtained; and hence it is that a double opera-glass gives a much livelier impression of relief than a single one. But in the usual construction of the instrument the relief is false; the objects appear as if they were squeezed together in the direction of depth. In the case of human faces, on which, for the most part, opera-glasses are directed, this is very striking. When they are regarded from the front, they appear much flatter than they really are; and when looked at in profile, they appear too narrow and sharp. In both cases the expression of the countenance is essentially altered.

When a double opera-glass is turned round, and the observer looks through the object-glass, the deep dimensions of objects are magnified out of proportion. While, therefore, through a simple telescope all objects appear as paintings, through a double opera-glass complete objects are seen as *bas-reliefs;* while, by reversing the opera-glass, objects appear in high relief.

ON THE PHENOMENA OF RELIEF OF THE IMAGE FORMED ON THE GROUND GLASS OF THE CAMERA OBSCURA.

The following paper was recently read before the Royal Society, by M. Claudet:

The author having observed that the image formed on the ground glass of the camera obscura appears as much in relief as the natural object, when seen with the two eyes, has endeavored to discover the cause of that phenomenon; and his experiments and researches have disclosed the singular and unexpected fact, that although only one image *seems* depicted on the ground glass, still each eye perceives a different image; that in reality there exist on the ground glass two images, the one visible only to the right eye, and the other visible only to the left eye, — that the image seen by the right eye is the representation of the object refracted by the left side of the lens, and the image seen by the left eye is the representation of the object refracted by the right side of the lens. Consequently, these two images presenting two different perspectives, the result is a stereoscopic perception, as when we look through the stereoscope at two images of different perspectives.

It appears that all the different images refracted separately by every part of the lens, are each only visible on the line of their refraction when it corresponds with the optic axes; so that while we examine the image on the ground glass, if we move the head we lose the perception of all the rays which are not corresponding with the optic axes, and have only the perception of those which, according to the position of the eyes, gradually happen to coincide with the optic axes. Consequently, when we look on the ground glass perfectly in the middle, the two eyes being equally distant from the centre, the right eye sees only the rays refracted from the left of the lens, and the left eye only those refracted from the right of the lens.

If we move the head horizontally, as soon as we have deviated about 6° from the centre on the right or on the left, in the first position the right eye sees no image, and the left eye sees the image which before was seen by the right eye; in the second position the inverse takes place, and of course in both cases there cannot exist any stereoscopic illusion.

When we examine on the ground glass the image of a solid produced by the whole aperture of the lens, if we have taken the focus on the nearest point of the solid, we remark, in looking with the two eyes, that the image is stereoscopic, and as soon as we shut one eye the illusion of relief disappears instantly.

The stereoscopic effect is beautifully brought out by the image of a group of trees; and when experimenting in an operating room, it is rendered quite conspicuous if we take the image of an object having several planes very distinct, such as the *focimeter*, which the author has described in a former memoir (see *Pihl. Mag.* for June, 1851).

If, without altering the focus, we examine the same image with the pseudoscope, the effect is pseudoscopic. But if the focus has been set on the most distant plane of the focimeter, the effect is pseudoscopic, and it becomes stereoscopic in looking with the pseudoscope.

The image loses its relief when it is produced only by the centre of the lens. The stereoscopic and pseudoscopic effects are therefore as much less apparent as the aperture of the lens has been more reduced, and they are the more evident if the image is produced by two apertures on both extremities of the horizontal diameter of the lens. This mode of conducting the experiments presents the most decided manifestation of the whole phenomenon.

But it must be remarked, that if the image is received on a transparent paper instead of ground glass, it does not in any case present the least illusion of relief. The surface of the paper has the property of preserving to both eyes the same intensity of image, from whatever direction the rays are refracted on that surface, and at whatever angle the eyes recede from the centre to examine the image. In fact, all the various images refracted through every part of the lens and coinciding on the surface of the paper, are visible at whatever angle they are examined.

The reason of this difference between the effect of the ground glass and that of the paper is, that through the surface of the ground glass, composed of innumerable molecules of the greatest transparency, only deprived of their original parallelism by the operation of grinding, but acting as *lenses* or *prisms* disposed at all kinds of angles, the rays refracted by the various parts of the lens continue their course in straight lines in passing through these transparent molecules, and are visible only when they coincide with the optic axes, being invisible in all other directions; that, in short, they are not stopped by the surface of the ground glass; while the paper, being perfectly opaque, stops all the rays on their passage, by which the image of the object remains fixed on the surface. Each molecule of the paper, becoming luminous, sends new rays in all directions; and from whatever direction we look on the paper, we always perceive at once all the images superposed; so that each eye seeing the two perspectives mingled, the process of convergence, according to the horizontal distances of the same points of the various planes, cannot have its play, and no stereoscopic effect can take place, as is the case with the ground glass, which presents to each ray an image of a different perspective.

The author explains that he has ascertained these facts by several experiments, the most decisive of which consists in placing before one of the marginal openings of the lens a blue glass, and a yellow glass before the other. The object of these colored glasses is to give on the ground glass two images, each of the color of the glass through which it is refracted.

The result is two images, superposed on the ground glass, one yellow and the other blue, forming only one image of a gray tint, being the mixture of yellow and blue, when we look with the two eyes at an equal distance from the centre. But when shutting alternately, now the right and then the left eye, in the first case the image appears yellow, and in the second it appears blue.

If, while looking with the two eyes (the opening on the right of the lens being covered with the yellow glass, and the opening on the left with the blue glass), we move the head on the right of about 6°, the mixture of the two colors disappears, and the image retains only the blue color; on the other hand, if after having resumed the middle position, which show again the mixture of the two colors, we move the head on the left of 6°, the mixture disappears again, and the image retains only the yellow color.

This proves evidently that each eye sees only the rays which, when after having been refracted by any part of the lens, and continuing their course in a direct line through the ground glass, coincide with the optic axes, while all the other rays are invisible.

The consideration of these singular facts has led the author to think that it would be possible to construct a new stereoscope, in which the two eyes, looking at a single image, could see it in perfect relief; such a single image being composed of two images, of different perspectives superposed, one visible only to the right eye and the other to the left. This would be easily done by refracting a stereoscopic slide on a ground glass, through two semi-lenses separated enough to make the right picture of the slide coincide with the left picture at the focus of the semi-lenses. The whole arrangement may be easily understood; we have only to suppose that we look through a ground glass placed before an ordinary stereoscope at the distance of the focus of its semi-lenses, the slide being strongly lighted, and the eye seeing no other light than that of the picture on the ground glass. The whole being nothing more than a camera having had its lens cut in two parts, and the two halves sufficiently separated to produce at the focus the coincidence of the two opposite sides of the stereoscopic slide placed before the camera.

The Stereomonoscope. — At a subsequent meeting of the Royal Society, M. Claudet presented a new optical instrument of his invention, called the Stereomonoscope, by which, as its name implies, a single picture produces the stereoscopic illusion. In the centre of a large black screen there is a space filled with a square of ground glass, upon which, by some light managed behind the screen, is thrown a magnified photographic image representing a landscape, a portrait, or any other object. When we look naturally at that picture, with the two eyes, without the help of any optical instrument, an extraordinary phenomenon takes place: we see the picture in perfect relief, as when we look at two different pictures through a stereoscope. It is not necessary to be at a fixed distance from the picture; it may be examined as well at ten feet as at one foot, as an ordinary picture, without the least fatigue to the eyes. Although considerably enlarged by the instrument itself, we may magnify the picture still more by using large convex lenses; and two or three persons at once can examine it with the greatest ease, being able, while looking, to exchange any remarks, or express the sensations suggested by the picture, — an advantage which is denied by the use of the common stereoscope. By this remarkable discovery, M. Claudet has solved a problem which has always been considered as an impossibility by scientific men, — for the stereomonoscope, by its very name, must sound like a paradox to the ears of all those who are versed in the knowledge of the principles of binocular vision, until they have had the opportunity of repeating the experiments by which the author has found a new fact which they had not noticed or explained before. This new fact is, that the image on the ground glass of the camera-obscura produces the illusion of relief. But the phenomenon does not take place if the image is received on paper. When the medium is ground glass, the rays refracted by the various points of the lens upon that surface are only visible when they are incident in a line coinciding with the optic axes. So that the rays emerging from the ground glass, and entering the right eye, are only those which have been refracted obliquely in the same direction, by the left side of the object-glass; and those entering the left eye are only those which are refracted by the right side of the object-glass; consequently, both eyes have a different view and perspective of the object represented on the ground glass, and the single image is, in point of fact, the result of two images, each only visible to one eye, and invisible to the other. This is the main point of M. Claudet's discovery, which cannot be fully understood without reading the preceding paper, and without repeating the experiments described in that paper. The stereomonoscope is founded on the same principles; it is nothing more than a camera-obscura, before which are placed two images of a stereoscopic slide, and by means of two object-glasses, sufficiently separated, the two images are refracted on the same space, at the focus of the camera-obscura on the ground glass, where they coincide. By the same laws we have alluded to before, the right picture is seen only by the left eye, and the left picture by the right eye; so that, although only one picture appears represented on the ground glass, each eye sees on the same spot a different picture, having its particular perspective; and, consequently, in order to obtain a single vision, the eyes have to converge differently, to bring consecutively in the centre of both retinas the different similar points of the two pictures according to their horizontal separation on the ground glass, the criterion of their respective distances. This alteration of the convergence of the optic axis, according to the distances of the various planes, gives the same sensation of relief we obtain when we look at the natural objects, or at their photographic representations. There is a charm in the effects of this instrument peculiarly agreeable to the senses; and we may congratulate M. Claudet upon having made a very remarkable and pleasing discovery.

ALMEIDA'S STEREOSCOPE.

An important modification of the Stereoscope has recently been communicated to the French Academy by M. d'Almeida. With the common instrument, only one observer at a time can view the relief. M. d'Almeida renders it visible to several at a time, and at a distance of several metres. For this purpose he causes two stereoscopic images to be reflected simultaneously on a screen; as they are not identical, but only similar, the outlines of the one will intersect those of the other, and generate a confusion which can only be obviated by making each eye see only one of the images. For this purpose the inventor causes the luminous rays from each image to pass through a glass of a different color, one red and the other green; whereby one of the images will be reflected on the screen in red, the other in green. Now, if the observer's eyes be provided with glasses of the above-mentioned colors, the eye covered with a green glass will only see the green image, while the other will only be visible to the eye protected by a red glass. The moment this is effected, the relief appears; and if the observer shift his position laterally, the figure will appear to move in a contrary direction, which adds to the illusion. M. d'Almeida proposes another plan, in which both images are uncolored, and each eye is made to perceive one image only by rapidly intercepting the other from view by means of a revolving piece of pasteboard, cut so as only to cover one of the images at a time by each half revolution. As soon as the rotary motion acquires sufficient rapidity, the figures appear in relief.

ON THE CONSTRUCTION OF A HAND HELIOSTAT, FOR FLASHING SUN-SIGNALS.

The following paper was presented to the British Association, 1858, by Mr. F. Galton. A flash of sunlight from a looking-glass of a few inches in the side, can be seen further than any terrestrial object whatever; and the instrument about to be described shows how this remarkable power may be utilized for the purpose of telegraphy. Heliostats are used in all government surveys, and their power is well known in penetrating haze, and their utility in requiring no "sky line." They were also habitually employed by the Russians, for telegraphy, during the Crimean war. But all heliostats that have been hitherto used have been fixtures of large dimensions; commonly, a shaded screen, with an aperture in it, was placed at many yards from the signaller, who stationed himself in such a way, that, when he could see the play of his flash about the hole in the screen, he might be sure that some of the rays which passed through the aperture would be visible at the distant station. At other times a polished ring was used for the same purpose as the screen, but the principle was the same. The present instrument dispenses with all fixture — it is more portable than a ship's telescope, and as manageable as a ship's quadrant, and may be made by a carpenter for 4s., if he possesses a convex spectacle lens of short focus, and a piece of good looking-glass. The looking-glass attached to the heliostat is about three inches by four and a half inches, and therefore capable of being seen at distances, which may be calculated from the fact, that a mirror one inch square is perfectly visible, in average sunny weather, at a distance of eight miles, and that it shows as a brilliant glistening star at two miles. Before describing its principle and action, it will be necessary to explain clearly the peculiar characteristic of the reflection of the sun's rays from a mirror. If, for instance, we take a small square looking-glass, and throw its flash upon a wall two or three feet off, the shape of the flash will be little different from that of the mirror itself, seen in perspective; but, if we direct it on an object three or four yards off, the angles of the flash will appear decidedly rounded; at twenty or thirty paces, it will appear fairly circular; and, if we can manage to see it at fifty or one hundred yards (which can only be effected by selecting some object to throw it on that is naturally of a light color, but lying under a dark shade), it will appear like a mock sun, of identically the same shape and size as the sun itself; and for all greater distances, the appearance remains the same. That is to say, whatever may be the shape or size of the mirror, and whatever the irregularity of the distant objects on which the flash happens to be thrown, the shape and size of that flash, if it could be seen by the signaller, would always appear to him as exactly that of sun. In fact, the flash forms a cone of light, at the blunted apex of which are the mirror and the signaller's eye, and whose vertical angle equals that of the sun's angular diameter. Whoever is covered by the flash, sees the mirror, like a small fragment of the sun itself, held in the hand of the observer,— and the larger the mirror, compared to the distance, the larger and the more dazzling does it appear. Now, the hand heliostat provides a bright appearance of the sun, which, when the instrument is adjusted and looked through, overlays the exact area which is covered by the flash of the mirror, which is attached to its side. It is a perfect substitute for that mock sun which we can see at fifty or one hundred paces distant, but which becomes too faint to be traced much further. All we have to do, when we wish to send a flash to a distant object, is to make that image of the sun overlay the object, just as may be done in rough sextant observations. The principle of the instrument is extremely simple. A convex lens, of any focal distance (five inches is convenient), has a small screen attached to it, whose surface is at its focal distance. The mirror is so placed that a small portion of its flash impinges upon one end of the lens. The signaller's eye looks partly through the other end of the lens, and partly free of it. Now the rays from any one point of the sun's surface are converged by the upper part of the lens to a bright point on the screen; and those rays which radiate from that point and impinge on the lower end of the lens, are brought back by means of it to a state of parallelism with the rays that originally left the mirror. Consequently, the signaller's eye sees the bright spot in the precise direction of the vanishing point of the mirror's flash, and he can, by looking partly to the side of the lens, refer it to some particular spot in the distant landscape. But what is true for any one point on the sun's disk, is true for every point; and, accordingly, we obtain a bright disk upon the screen, which appears of exactly the same shape and size as the sun itself, and necessarily overlays the exact area covered by the flash of the mirror. It is scarcely possible to describe the instruments that were submitted to the Association, without drawings. They consisted of a tube of wood fifteen inches long, and with an eye-hole at one end; a mirror turned on an axis at right angles to the tube; and, in front of the mirror, a slip was cut away from the side of the tube, and the lens was inserted athwart the cut-out part. Part of the lens projected within the tube, and part outside of it, and in front of the mirror. The screen was placed at the further end of the cut-out part, and an envelop protected the whole from injury. A slide in front of the lens regulated the amount of light thrown on it, and toned the image to the required degree of brightness. The addition of a telescope was not found practically useful. Neither was that of a second mirror, for double reflection, to meet the difficulty of sending signals when the sun was behind the back of the signaller. It is not difficult to signal within twelve degrees of the point opposite to the sun, and it is possible to do so within seven degrees. The looking-glass should be of the very best plate-glass, and it ought to have its sides truly parallel, else there will be a confusion of images and an irregularity in the flash. Letters are conveyed by treble groups of flashes, each of which groups consists of one, two, or three flashes, as the case may be.

TELESCOPIC MIRRORS.

Foucault has communicated to the Academy of Sciences a memoir on the substitution of silvered glass for metallic alloys in the construction of reflecting telescopes, and on the possibility of producing surfaces of revolution which reflect parallel rays to a single focus. He remarks, in the first place, that the spherical surface itself is difficult to obtain with absolute accuracy. When a luminous point is placed in the centre of curvature of a concave mirror, the image of this point is usually surrounded by an aureole, the appearance of which indicates defects in the surface. The author remedies these defects by retouching the mirror in different parts, until the image is free from faults. The spherical mirror is then converted into an ellipsoidal, and finally into a paraboloidal mirror, by successive processes of approximation. A luminous point, placed at first in the centre of curvature, is made gradually to approach the principal focus; the image, of course, recedes in the opposite direction. By means of an appropriate polisher, the figure of the mirror is corrected for each successive portion of the luminous point, until finally the aberration becomes invisible for parallel rays. With a telescope constructed in this manner, with a mirror thirty-three centimetres in diameter, and having a focal length of $2^{m}\cdot25$ the author succeeded in resolving the blue star of γ Andromedæ into two distinct points. This result had hitherto been obtained only by Struve with the large Pulkowa instrument.—*Comptes Rendus*, xlvii. 205.

ON THE DANGER ATTENDING THE USE OF RED AND GREEN LIGHTS AT SEA.

The following is an abstract of a most valuable practical paper submitted to the British Association, 1858, by Prof. G. Wilson. It commenced by stating the Admiralty regulations, that "1. All sea-going vessels, when under way, or being towed, shall, between sunset and sunrise, exhibit a *green light* on the starboard side, and a red light upon the portside of the vessel. 2. The colored lights shall be fixed wherever it is practicable, so as to exhibit them, and shall be fitted with in-board screens, projecting at least three feet forward from the light, so as to prevent the lights being seen across the bow." The author then went on to show that these regulations, which would effectually secure the object intended in most cases, would be most dangerous, should a seaman be put to steer or look out, who had that peculiar kind of blindness, of which he had encountered many instances, of not being able to distinguish red light from green. Statistics of color-blindness are defective, not including females; but there is reason to think that not less than one in twenty is defective in this respect; and of the markedly color-blind, not less than one in fifty males are so. Out of 1,154 persons, including students, soldiers, and policemen, examined by the author, one in fifty-five were markedly color-blind, — *i. e.* entirely unable to distinguish the colors red, brown, green, and blue. The author suggests two remedies: — 1. A change of the system itself, which in its details must be left to nautical men. 2. An examination of all masters, mates, and pilots in the merchant service, as to their power of distinguishing colored lights within the limits of vision, and rigorously excluding those who could not.

ON THE DURATION OF LUMINOUS IMPRESSIONS ON CERTAIN POINTS OF THE RETINA.

At the last meeting of the British Association, 1858, Sir David Brewster stated that it was well known that the direction of luminous impressions upon the retina is one third of a second for white light of ordinary intensity. At a former meeting he had shown that the small circular area at the end of the axis, whether it be the retina or the choroid, retains light longer than the general retina, after the eye has been exposed to light; and he had recently observed that certain points of that membrane, situated, apparently, near its termination at the ciliary process, have even a greater retentive power. In order to observe this curious phenomenon, we must extinguish, suddenly, a gas-flame, to the light of which the eye has been for some time exposed. We shall then observe a number of bright luminous points arranged in a circle, the diameter of which is about 72°. These bright points or stars, apparently placed at equal distances, vanish so quickly, that he had found it very difficult to determine their number. They may amount to fifteen or twenty. He had sometimes observed them upon extinguishing a candle, and also upon quickly shutting the eyes. The parts of the retina from which these points of light emanate, are probably places where the retina is attached to the ciliary ring, or other parts in the interior of the eye, and may, therefore, be detected by the anatomist.

ON THE LAWS OF COLOR.

The following is an abstract of a lecture on the above oject, before the Royal Institution, London, by Mr. Grace Calvert. The lecturer stated that he had three objects in view in his discourse. The first was to make known the laws of color, as discovered by his learned master, M. Chevreul; secondly, to explain their importance in a scientific point of view; and, thirdly, their value to arts and manufactures.

To understand the laws of colors, it is necessary to know the composition of light. Newton was the first person who gave to the world any statement relative to the components of light, which he said consisted of seven colors, —red, orange, yellow, green, blue, indigo, and violet. It is now distinctly proved that four of these seven colors of the spectrum are the result of the combination of the three colors now known as the primitive colors, viz., red, blue, and yellow. Thus, blue and red combined, produce purple or indigo; blue and yellow, green; while red and yellow produce orange: these facts being known, it is easy to prove that there are not seven, but three primitive, and four secondary, called complementary, colors.

Several proofs can be given that light is composed of three colors only. One of the most simple consists in placing pieces of blue, red, and yellow papers on a circular disk, and rotating it rapidly,— the effect of the eye being to produce a disk of white light. If, therefore, the eye can be deceived so readily while the disk travels at so slow a rate, what must necessarily be the case when it is remembered that light proceeds at the rate of 190,000 miles per second?

The rapidity with which light travels is such that the eye is not able to perceive either the blue, red, or yellow, — the nerves of the retina not being sensitive enough to receive and convey successively to the mind the three or seven colors of which the light is composed.

Before entering into the laws of color, Mr. Grace Calvert stated that it might be interesting to know what scientific minds had devoted attention to the laws of colors.

Buffon followed Newton, and his researches had special reference to what M. Chevreul had called the "successive contrasts" of colors.

Father Scherffer, a monk, also wrote on the laws of color. Goethe, the poet, also brought his mind to bear upon the subject, and studied it to a great extent. Count Rumford, about the end of the eighteenth century, published several memoirs on the laws of colors. He explained very satisfactorily the "successive" contrast, and arrived at some insight into the "simultaneous" one; still he did not lay down its real laws.

Prieur, Leblanc, Harris, and Field, were also writers of most interesting works on this subject. The reason that they did not arrive at the definite laws of color was because they had not divided those laws into successive, simultaneous, and mixed contrasts. These form the basis of the practical laws of color, and the honor of their discovery is due to M. Chevreul.

The reason why a surface appears white or brilliant, is, that a large portion of the light which falls on its surface is reflected on the retina, and in such a quantity as gives to the surface a brilliant aspect; whilst in plain white surfaces, the rays of light being diffused in all directions, and a small portion only arriving to the eye, the surface does not appear brilliant. The influence of colors on these two kinds of surfaces is very different, as may be perceived by the examples round the room, showing the influence of different colors on gold ornaments. When rays of light, instead of being reflected, are absorbed by a surface or substance, it appears black; therefore white and black are not colors, as they are due to the reflection or absorption of undecomposed light. It is easy to understand why a surface appears blue: it is due to the property which the surface has to reflect only blue rays, whilst it absorbs the yellow and red rays; and if a certain portion of light is reflected with one of the colored rays, it will decrease its intensity; thus, red rays with white ones produce pink. On the contrary, if a quantity of undecomposed light is absorbed, black is produced, which, by tarnishing the color and making it appear darker, generates dark reds, blues, or yellows. The secondary colors are produced by one of the primitive colors being absorbed, and the two others reflected; for example, if red be absorbed, and blue and yellow reflected, the surface appears green. There are two reasons why a perfect blue, yellow, red, cannot be seen, etc. The first is, that surfaces cannot entirely absorb one or two rays, and reflect the others. The second is, that when the retina receives the impression of one color, immediately its complementary color is generated. Thus, if a blue circle is placed on a perfectly gray surface, an orange hue will be perceived round it; if an orange circle, round it will be noticed a bluish tint; if a red circle, a green; if a greenish-yellow circle, a violet; if an orange-yellow circle, an indigo; and so on.

The "successive" contrast has long been known; and it consists in the fact, that on looking steadfastly for a few minutes on a red surface fixed on a white sheet of paper, and then carrying the eye to another white sheet of paper, there will be perceived on it not a red, but a *green* one; if green, *red;* if purple, *yellow;* if blue, *orange.*

The "simultaneous" contrast is the most interesting and useful to be acquainted with. When two colored surfaces are in juxtaposition, they mutually influence each other,—favorably, if harmonizing colors, or in a contrary manner, if discordant; and in such proportion, in either case, as to be in exact ratio with the quantity of complementary color which is generated in the eye. For example, if two half-sheets of plain tinted paper, one dark-green, the other of a brilliant red, are placed side by side on a gray piece of cloth, the colors will be mutually improved, in consequence of the green generated by the red surface adding itself to the green of the juxtaposed surface, thus increasing its intensity; the green in its turn augmenting the beauty of the red. This effect can easily be appreciated if two other pieces of paper of the same colors are placed at a short distance from the corresponding influenced ones, as below:

Red. Red-green. Green.

It is not sufficient merely to place complementary colors side by side to produce harmony of color, since the respective intensities have a most decided influence. Thus, pink and light-green agree; red and dark-green, also; but light-green and dark-red, pink and dark-green, do not: and thus, to obtain the maximum of effect and perfect harmony, the following colors must be placed side by side, taking into account their exact intensity of shade and tint.

HARMONIZING COLORS.

Primitive Colors.	Complementary Colors.		
Red, - - - - - - -	Green, - - -	Light-blue, Yellow, Red,	White light.
Blue, - - - - - - -	Orange, - - -	Red, Yellow, Blue,	White light.
Yellow-orange, - -	Indigo, - - -	Blue, Red, Yellow,	White light.
Greenish-yellow, -	Violet, - - -	Red, Blue, Yellow,	White light.
Black, - - - - - -	White, - - -	Yellow, Blue, Red,	White light.

If attention is not paid to the arrangement of colors according to the above diagram, instead of their mutually improving each other, they will, on the contrary, lose in beauty. Thus, if blue and purple are placed side by side, the blue, throwing its complementary color, orange, upon the purple, will give it a faded appearance; and the blue, receiving the orange-yellow of the purple, will assume a greenish tinge. The same may be said of yellow and red, if placed in juxtaposition. The red, by throwing its complementary color, green, on the yellow, communicates to it a greenish tinge; the yellow, by throwing its purple hue, imparts to the red a disagreeable purple appearance. The very great importance of these principles to every one who intends to display or arrange colored goods or fabrics, was convincingly shown from a great variety of embroidered silks, calicoes, and paper-hangings, which demonstrated that if these laws are neglected, not only will the labor and talent expended by the manufacturer to produce on a given piece of goods the greatest effect possible, be neutralized, but perhaps lost. It was clearly demonstrated that these effects are not only produced by highly-colored surfaces, but also by those whose colors are exceedingly pale, as, for example, light greens, or light blues with buffs; and that even in gray surfaces, as pencil drawings, the contrast of tone between two shades was distinctly visible. The contrast of tone, or tint, was most marked when two tints of the same color were juxtaposed, and it was therefore the interest of an artist to pay attention to this principle when employing two tints of the same scale of color. From the "mixed contrast," arises the rule that a brilliant color should never be looked at for any length of time, if its true tint or brilliancy is to be appreciated; for if a piece of red cloth is looked at for a few minutes, green, its complementary color, is generated in the eye, and adding itself to a portion of the red, produces black, which tarnishes the beauty of the red. This contrast explains, too, why the tone of a color is modified, either favorably or otherwise, according to the color which the eye has previously looked at. Favorably, when, for instance, the eye first looks to a yellow surface, and then to a purple one; and unfavorably, when it looks at a blue and then at a purple.

Mr. Calvert also showed that black and white surfaces assume different hues according to the colors placed in juxtaposition with them; for example, black acquires an orange, or purple tint, if the colors placed beside it are blue or orange; but these effects can be overcome, in the case of these, or any other colors, by giving to the influenced color a tint similar to that influencing it. Thus, to prevent black from becoming orange by its contact with blue, it is merely necessary that the black should be blued, and in such proportion that the amount of blue will neutralize the orange thrown on by influence, thus producing black. As an instance, to prevent a gray design acquiring a pinkish shade through working it with green, give the gray a greenish hue, which, by neutralizing the pink, will generate white light, and thus preserve the gray. Mr. Calvert, after explaining the chromatic table of M. Chevreul, which enabled any person at a glance to ascertain what was the complementary of any of the 13,480 colors which M. Chevreul had distinctly classed in his table, stated that it was of the highest importance to artists to be acquainted with these, in order to know at once the exact color, shade, and tint, which would produce the greatest effect, when placed beside another color; and that they could save great waste of time—which, no doubt, the great masters lost in ascertaining by experiment those laws—by consulting M. Chevreul's work.

ON SOME APPARATUS FOR EXHIBITING OPTICAL ILLUSIONS OF SPECTRAL PHENOMENA.

Mr. H. Dircks, in a paper before the British Association on the above subject, after quoting some passages in Sir David Brewster's "Natural Magic," in which the author had intimated that reflection by concave specula must form the basis of all spectral illusions by reflection, and pointing out the inconvenience of using these for producing images of living and moving persons, in consequence of their inverting objects, stated that he had contrived a means by which living actors, some the real persons, others the images of persons concealed from the direct view of the spectators, might be formed by a large plate of glass dividing the room in which the exhibition was made; the spectators being in a darkened portion above, but at one side of the glass plate, while the living persons on the other side of it could be seen quite clearly through the glass; and the images of other persons walking about in the room under them, seen by reflection, would appear in the same place as the living persons seen directly, and could be thus made to appear to perform most amusing spectral feats, such as passing through walls, into and coming out of the living actors, and so on.

NEW AND GIGANTIC TELESCOPE.

Mr. Lassell, the eminent English astronomer, has been for some time engaged upon the construction of a new and gigantic reflecting telescope. The diameter of the mirror (not yet worked) is to be four feet, the same size as Sir William Herschel's largest mirrors. Its weight is a ton and a half. The tube is a skeleton. The mounting is equatorial, similar in its general form to that which Mr. Lassell has used with smaller telescopes. A part of the apparatus, of which the plan is perfectly novel, is the observing-box. It is something like a very tall sentry-box, between thirty and forty feet high, in which slides upwards and downwards a cage like the teagle of a cotton-mill; in this cage the observer is stationed. The tall box stands upon a ring-turn-table which surrounds the telescope's polar axis. The ring appears to have a motion in azimuth, and the tall box has a radial motion on the ring: and the combination of these two motions with vertical motion of the teagle, gives command of the telescope's mouth in all positions. The tall box is so arranged that, when the telescope is turned to a proper azimuth, and is depressed to a nearly horizontal position, the tall box, turning upon a hinge at its base, can be lowered over the telescope tube to protect it from the weather. In addition to the particular motions described, the mounting for the observer was hinged, so as to cover the telescope; and it also turned upon an axis, so as to present the observer in the more favorable position with regard to the eye-piece.

ON THE INFLUENCE OF TEMPERATURE UPON PHOSPHORESCENCE.

E. Becquerel has found that the color of the light emitted by phosphorescent bodies depends upon the temperature. Thus, sulphid of strontium, obtained by the action of sulphur upon caustic strontia at 700° C. or 800° C., is luminous with a violet light at ordinary temperatures, but changes its tints when the temperature varies, and returns again to its original tint when the original temperature is restored. In the case of sulphid of strontium, the colors diminish in refrangibility as the temperature rises; but the reverse is the case with some other substances. The different effects depend upon the particular molecular condition of each substance. — *Comptes Rendus*, xlvii. 104.

EXPERIMENTS ON RADIANT HEAT.

The following account of some experiments on radiant heat, involving an extension of Prevost's theory of exchanges, was presented to the British Association, at the last meeting, by Mr. B. Stewart. The experiments in question were performed with the aid of the thermomultiplier, the source of heat being for the most part bodies heated to 212°. Four groups of experiments were considered. Group the first contains those experiments in which the quantities of heat radiated from polished plates of different substances at a given temperature, are compared with the quantity radiated from a similar surface of lampblack at the same temperature. The result of this group of experiments is, that glass, alum, and selenite radiate about 98 per cent. of what lampblack does; thick mica, 92; thin mica, 81; and rock-salt only 15 per cent. The second group of experiments was designed to compare together the quantities of heat radiated at the same temperature from polished plates of the same substance, but of different thicknesses. The result of this group was, that, while the difference between the radiating power of thick and thin glass is so small as not to be capable of being directly observed, there is a perceptible difference between the radiation from thick and thin mica, and a still more marked difference between the radiation from plates of rock-salt of unequal thickness. The third group of experiments was made with the view of comparing the radiations from various polished plates with that from lampblack, as regards the quality of the heat, — its quality being tested by its capability of transmission through a screen of the same material as the radiating plate. From this group of experiments it appears that heat emitted by glass, mica, or rock-salt, is less transmissible through a screen of the same material as the heated plate than heat from lampblack, — this difference being very marked in the case of rock-salt, which only transmits about one-third of the rays from heated rock-salt. The common opinion that rock-salt is equally diathermanous for all descriptions of heat, is therefore untenable. The fourth group of experiments shows that heat from thick plates of glass, mica, or rock-salt, is more easily transmitted by screens of the same nature as the heated plate than heat from thin plates of these materials. It was shown that all these experiments may be explained by Prevost's theory of exchanges, somewhat extended. This extension consists of the following laws: — 1. Each particle of a substance has an independent radiation of its own, equal in all directions, and without regard to the distance of the particle from the surface of the body. 2. The radiation of a particle equals its absorption, and that for every description of heat. 3. The flow of heat from within upon the interior surface of a polished plate of indefinite thickness, is proportional to the index of refraction of the body, and that for every description of heat. The bearing of these experiments on Dulong and Petit's law of radiation was then attempted to be traced. It was shown that unless bodies from simply being heated change their transmissibility for the same description of heat (which there is no reason to suppose), the radiation of thin plates or particles at a high temperature will bear a less proportion to the total radiation of that temperature than at a low, — the consequence will be, that the radiation of single particles will increase with the temperature in a less degree than Dulong and Petit's law would indicate. It may even be that the radiation of a particle of very thin plate may be proportional to the absolute temperature of that particle. Taking a piece of glass or mica, therefore, at a low temperature, as it is very opaque with regard to the heat radiated by itself, we may suppose that the total radiation consists of that of the outer layer of particles only, that from the inner layers being all stopped by the outer. At high temperatures, however, we may suppose there is not only the radiation of the outer layer, but also part of that of the inner layer which has been able to pass, swelling up the total radiation to what it appears in Dulong and Petit's experiments. This way of looking at radiation may possibly bring the radiative power of particles to obey the same laws with the conducting power of particles, which Professor Forbes has shown decreases with an increase of temperature.

ON THE HEATING OF THE ATMOSPHERE BY CONTACT WITH THE EARTH'S SURFACE.

The following is an abstract of a paper on the above subject, read before the British Association for 1858, by Prof. Hennessy:

The temperature of the atmosphere depends principally on the heat which it receives from the sun and on what it loses by radiation. A portion of the solar heat is absorbed in passing through the air, while another portion penetrates to the earth's surface. The ground becomes thus heated, and the lower strata of the atmosphere acquire the greater part of their heat from contact with the warmed surface. It is admitted that the mode in which the air becomes heated by contact with the ground must be a kind of circulation analogous to that seen in the movements of a heated mass of liquid, such as boiling water. When studying the vertical movements of the atmosphere, he had been led to consider the connection between such movements and the influence of the heated ground. In order to experimentally study the question, thermometers were suspended at different heights above the ground, and under different circumstances of exposure to the influence of the supposed currents. Observations were made every minute, and sometimes every half minute, during short intervals, about the middle of the month of May, on days when the sky was clear, and during which there was consequently a great deal of solar radiation. In general the thermometers exhibited fluctuations of temperature, the intensity of which diminished the more they were protected from the influence of circulating currents in the air. The greatest fluctuations were presented by thermometers with blackened bulbs exposed in the sun. This arose from the circumstance that the blackened bulbs, by acquiring a high temperature, became themselves disturbing agents in the calorific conditions of the surrounding air. Evidence of similar phenomena appears to be presented by the curves of temperature obtained by the aid of photographical registration at the Radcliffe Observatory in Oxford. Attention has been called by Mr. Johnson to a remarkable serration in the temperature curves during the day. This serration is found only when there is a considerable amount of solar radiation; it disappears during sunless and cloudy weather. While it is explained by referring it to the influence of the solar heat upon the ground, and the consequent circulation of small atmospheric currents, it affords a very satisfactory confirmation of the trustworthiness of the photographical method of registration.

Prof. H. also showed, that the decrease of temperature in ascending through the atmosphere depended not only on height above the sea level, but also upon the absolute height above the nearest surface of solid land. In this way the decrease of temperature over plains, mountains, and plateaus, would be necessarily very different; and we cannot immediately infer the state of the phenomena in the two latter instances from what may exist in a former.

Admiral FitzRoy thought that one circumstance was too much overlooked by Prof. Hennessy in these researches, namely, that along with these ascending currents the whole body of the air was carried along by horizontal currents, so that it could not be assumed that it was the very same air which gave some of the indications which afforded the others. Again, it had been clearly shown that a thermometer placed upon the ground, or close to it, frequently fell 17° or 18° below one placed a few feet or inches above it, while somewhat higher up still, the indications of the thermometer again fell; thus clearly indicating a spot at which there was a maximum temperature. As to the latter part of what he stated, it was so commonly observed, that if you placed a thermometer in the lower window of a house, and another in the window immediately above it, in nine cases out of ten you would find the latter indicate a lower temperature than the former. Prof. Stevelly said that, besides what Admiral FitzRoy had pointed out, there were two other circumstances of much importance to be attended to in such observations as Prof. Hennessy had been making. First, that evaporation was going on more or less rapidly according to the circumstances of the locality where the observations were conducted. Secondly, that the air, when having — either gradually, as in some cases, or abruptly, as in others — to ascend in its course very elevated ground, was compelled to contract in volume, become condensed, and yet, in some cases, part with a portion of its vapor, and thus form the cloud which we so often saw capping the hills, as well as giving origin to the high winds and storms which so frequently prevailed there. Dr. Tyndall said that in Switzerland, on the tops of high mountains, he had a full opportunity of witnessing these phenomena on a scale of grandeur truly sublime. The snow in these regions was naturally as dry as dust, and he had frequently an opportunity of witnessing columns of it whirled up to an immense height by the ascending currents of air, into regions where it was soon dissipated or melted, and dispersed into vapor. It was also to be observed that the sun's heat had a power of penetrating water and other screens, such as the clouds formed, far surpassing that possessed by heat derived from less intensely ignited or heated sources, as, for instance, from bodies heated red-hot, or from vessels filled with hot water and the like. Hence the sun's rays, though they penetrated the clouds and the earth, yet there they totally lost their former powers, and when radiated back possessed no such power as before of penetrating clouds or other screens; and thus the earth and its atmosphere became a kind of trap for the solar rays.

ON THE DISTRIBUTION OF HEAT IN THE INTERIOR OF THE EARTH.

In a paper submitted to the British Association, 1858, by Dr. Siljeström, of Stockholm, on the distribution of heat in the interior of the globe, the author stated as his belief, that the interior of the earth was occupied by currents of various degrees of heat, which mixed with each other and attained a certain degree of temperature in the same manner as substances subject to all the physical influences of the earth's exterior.

Prof. Hennessy remarked that the views of Dr. Siljeström seemed to state in other words the well-known fact, that a mass of fluid possessing different temperatures in different parts of its interior must be subjected to a process of convection. The result is usually a change of volume in the entire mass of circulating fluid. This change is capable of being observed in ordinary experiments, and may also affect the volume of the fluid matter in the interior of the earth, provided the changes of temperature of the fluid are sufficiently great. But it is clearly proved that the refrigeration of the earth is now so extremely slow, that it is not likely that any considerable changes of volumes arising from this cause could have arisen within recent periods. If such changes have arisen, they must have occurred during remote geological epochs.

EXPERIMENT SHOWING THE CONTRACTION OF WATER THROUGH TEMPERATURE.

At a recent meeting of the Edinburgh Philosophical Society, an experiment showing the contraction of water above the freezing point, was exhibited. The water operated on was contained in a glass jar, about 4 inches in diameter and 18 inches high; and the changes in its density were shown by the ascent and descent of colored glass balls, about an inch in diameter. When the water was ice-cold the balls were all at the bottom; but gradually, as the warmth of the room was communicated to the water, its contraction and consequent increase of density caused the balls to rise. As the water approached the state of greatest density, the heavier balls were seen to move irregularly about, in consequence of the current caused by the changes of temperature. In the course of an hour, the point of maximum density having been passed, the balls began to descend in reverse order, and at last all again reached the bottom.

ON SOME PHYSICAL PROPERTIES OF ICE.

In a recent lecture by Professor Tyndall before the Royal Institution on the above subject, the author, after referring to the force by which crystalline architecture is accomplished, exhibited some phenomena of crystallization by means of the photo-electric microscope. The manner in which the molecular aggregation was affected when a beam of radiant heat was sent into the interior of a mass of ice, was examined. The track of such a beam presented a beautiful appearance — flattened spheroids were observed, which at certain incidences of the light shone with more than metallic brilliancy, and around each a liquid flower, consisting invariably of six petals, was formed. The spot at the centre of each flower was proved to be a vacuum, and the formation of the flowers in a piece of ice through which a beam of electric light was transmitted was rendered visible to the audience. The air and water cavities, which, in the case of glacier ice, have caused so much discussion, were next examined. It was proved that the water was due to the melting of the ice round the air cavities. The hypothesis pronounced by M. Agassiz and the Messrs. Schlagintweit to account for this water, and which has hitherto been universally accepted, is, that the ice permits the radiant heat to pass, the heat warms the air, and it in its turn melts the ice. It was proved by the speaker that this view is wholly untenable. One of its consequences would be that a bubble of air would be capable of absorbing in a few minutes a quantity of heat which would raise its temperature upwards of 400,000 degrees, or more than 160 times that of fused cast iron. The melting of the ice was shown to be a simple consequence of the dynamical theory of heat; molecular motion is transmitted through the solid ice, without prejudice to its solidity, and detaches the particles at the surface of the internal cavity, as the last of a series of elastic balls is detached by force which has traversed a row of them without producing visible separation. The passage of snow into glacier ice was next considered. It was referred to the enormous pressure of the moist *neve* upon its own mass. That moisture was necessary was shown by moulding ice at thirty-two degrees into cups; while, when it was rendered perfectly dry by immersion in a bath of solid carbonic acid and ether, the ice on being crushed became a powder as white as snow. Crushed glass or quartz could not have been whiter or more opaque.

ON THE EXISTENCE OF AN ETHEREAL MEDIUM.

At a recent meeting of the French Academy, M. Le Verrier was asked (on the occasion of announcing the return of Encke's comet in almost exactly the position assigned to it by calculation) the very delicate and difficult question, whether or not the present rate of return of this important little body confirms the opinion which the study of its orbits originated, namely, that the space through which the heavenly masses move, is full of a resisting ethereal medium, tending always to retard their advances, diminish their orbits, and bring them finally into the sun. The reply was entirely noncommittal, and, in brief, was that, even if Encke's first suspicions of it should not be confirmed, his long and admirable calculations of the subject of this resistance shows the handling of a master. Mr. Babinet, however, boldly entered upon the subject, and asserted that the continued observations of this comet teach us, as yet, nothing definite upon the subject.

PERSONAL EQUATION.

At a recent meeting of the Royal Astronomical Society, England, the Astronomer Royal read the following communication from Professor Mitchel of Cincinnati, on "Personal Equation."

"At the meeting of the American Association of 1856, I announced the fact that I had contrived a simple apparatus for the investigation of the subject of '*absolute* personal equation.' Sickness in my family, and other causes, have combined to prevent a full investigation of this subject until the beginning of the present year. In case a star could be made to record the moment of its own transit, the difference between the star's record and that of any observer would be the observer's *absolute* personal equation, or what I shall term hereafter the 'personality of the eye.' In like manner, in case a sound could be made to record the moment of its occurrence, the difference between the record and that of an observer would give what I term 'absolute personality of the ear.' The same of the sense of touch, which as a matter of physiological curiosity, has somewhat engaged my attention. As the stars cannot at present be made to record their own transits, I have substituted what I call 'artificial stars,' moving uniformly with a velocity somewhat greater than that of an equatorial star observed with a power of two hundred, the power of the eye-piece now used in the transit telescope. By means of an electro-magnet, these artificial stars (ten in number) attached to my revolving disk, are made to record the exact moment at which they transit an artificial meridian line. The observer, by the aid of another magnet, records the moment of his observed transit, and the difference of these two records, corrected for difference of armature and gross time of the two magnets, gives me the 'absolute personality of the eye.' This same quantity has been obtained also from a record of the moment at which the eye perceives a white 'stripe' on a dark ground, thrown into view by the sudden action of the electro-magnetic armature, which records the moment of transit of the artificial stars. These quantities, as will be seen hereafter, are almost identical. To obtain 'absolute personality of the ear,' the observer, with his magnetic key, attempts to record the moment he perceives the sound produced by the fall of the 'time-pen' on the disk, as driven by the armature of the time electro-magnet, it falls, and makes its 'true' dot. The interval between the dot struck by the time-pen and that struck by the observer's recording-pen, gives the time required for the ear to execute its office, and for the nerves obedient to the will to execute the record. A like process, which is quite unnecessary to describe, gives 'personality of the touch.' To the practical astronomer the personality of eye and ear are alone important; to those who have adopted the American method of transits, the 'personality of the eye' remains as the quantity whose value and variations it is required to determine. Our regular observations have been continued daily, with few exceptions, through some sixty or seventy days, — my assistant, Mr. H. Twitchell, and myself making an equal number of observations to determine the following quantities: — 1. Absolute personality of eye. 2. Absolute personality of ear. 3. Observed moment of transit. 4. Observed moment of emersion. 5. Observed moment of immersion. These constitute the regular observations. Besides these, I have obtained the 'eye and ear personality' of about thirty persons (not observers) of each sex, and of ages from fourteen to seventy-five years. Among these individuals I find thus far no law which seems to govern the personality. The range is small, as the personality of eye varies between the lowest limit 137 thousandths of a second, and the highest limit 214 thousandths of one second of time. The personality of ear has for its least amount 137 thousandths of a second, and for its greatest limit, 223 thousandths of one second of time; and each of these limits belongs to the same two observers. The mean personality of my own eye, as obtained from 255 observations, is 161 thousandths of a second. The mean personality of my ear, as obtained from an equal number of measures, is 164 thousandths of a second. These same quantities for Mr. Twitchell, as given by the same number of observations on the same days with my own, are for the eye 144, and for the ear, 153 thousandths of one second of time. My minimum 'eye personality' is 139 thousandths of a second, the maximum reaches to 191 thousandths. My minimum 'ear personality' is 143, my maximum 'ear personality' is 193 thousandths of one second of time. The same quantities for Mr. Twitchell are for

	s		s
The eye, minimum	0·118	maximum	0·184
ear "	0·129	"	0·201

Having reached the above results, I was now curious to learn whether the eye and ear were steady for very short periods of time. For this purpose my assistant and myself each made five series of ten observations, each on alternate minutes, which being continued several days, showed that we were liable to a variation of 'eye personality' amounting to about two hundredths of a second on the mean of ten observations. I also found that the difference already established between Mr. Twitchell and myself was confirmed in in these observations. I will simply remark that the sense of touch gave results almost identical with those of the eye; and this fact being soon discovered, the observations for personality of touch were discontinued. Thus far we have presented results obtained by the eye, in seizing an almost instantaneous movement, the sudden darting of a white line from behind a black screen. When a comparison was instituted between the absolute and observed moments of transits of the artificial stars, I found, much to my surprise, that both my assistant any myself largely *anticipated* the true time; and that in every instance, without one exception, the same fact was noticed in other persons, who were ignorant of what they were doing, while recording their transits. After learning the fact of this unconscious anticipation, efforts were made to cure the evil by special attention. To some extent this was done, but the tendency was to an immediate relapse the moment special attention was discontinued. I find (on a mean of ten observations) my own anticipations amounting to the tenth of a second of time in more than one instance, while Mr. Twitchell's error is nearly as large. The variations from day to day, and from observation to observation in the same set were far larger than I had anticipated. This gave rise to the observation of 'emersions' of the artificial stars from behind a dark screen. Here I found a steadiness in the results precisely equal to the performance of the eye as already determined, which for my assistant and myself seems to be the highest limit of attainable accuracy. The experiments of observed immersion exhibited the fact of a strong tendency to anticipation, and a less degree of steadiness in the work. I now became anxious to apply the discoveries thus made in some practical manner to our star transits. For this purpose I have constructed a diaphragm, consisting of eight occulting bars, four on each side of a central spider's line. We observe the emersion from the first bar, both immersion and emersion from the second, third, and fourth bars, the transit of the central wire, the immersion and emersion of the fifth, sixth, and seventh bars, and the immersion on the eighth bar; in this way we make fifteen observations; these bars are about two seconds of time in width, and their intervals about four seconds at the equator. By observing emersion and immersion, we hope to avoid any error arising from stars of different magnitudes, as the larger stars will emerge sooner and disappear later, a mean of the two observations giving us the place of an imaginary wire between the two bars correctly."

ON ROTATORY STABILITY AND ITS APPLICATIONS TO ASTRONOMICAL OBSERVATIONS ON BOARD SHIPS.

The following paper has been read before the Royal Society, by Professor Baden Powell. The subject of rotatory motion, especially when taking place under those combinations which are presented in the gyroscope, or free-balanced revolver, has attracted much attention at the present day; and though the primary mechanical principles bearing upon it had long since been understood, and acknowledged in theory, yet the practical results, to which they might lead, had been so little considered, that when first tangibly exhibited, they created unbounded surprise. In a former lecture before the Royal Institution (1854), he had discussed the principle of "composition of rotations," and those applications of it which had been found in certain rotatory phenomena of projectiles, illustrated the same by the gyroscope in its several earlier forms as successively modified by Bonenberger, Atkinson, Fessel, and Wheatstone, showing the identity of these results on a small scale with the grand cosmical phenomenon of the procession of the equinoxes. Since the date of that lecture, the striking results produced by merely carrying out the same principles, and applying the gyroscope to demonstrate directly the fact of the earth's rotation, as well as under other conditions to point to the poles, by M. Foucault, have become familiarly known.

In recurring to the subject on the present occasion, the object is to explain another application of the same principles, like the former, very obvious *when once disclosed*; but which, nevertheless, remained unknown and unthought of until it was pointed out and actually effected by the inventions of Professor C. P. Smyth; the use of *rotatory apparatus for giving an invariable plane or platform for astronomical instruments used at sea.* To render the subject intelligible it is necessary to recur to two simple first principles in dynamics, which, when distinctly apprehended, give the clue to the whole of the applications. The first of these is the tendency of a body in rotation to retain that rotation in the same plane, when perfectly balanced, irrespective of the motion of external objects, which is termed "the fixity of the plane of rotation." The second is "the composition of rotatory motion;" or that when a force is impressed on a body in rotation, it does not show itself directly, but is *compounded* with the first motion, so that the rotation takes an intermediate direction, or the axis shifts its position in space; this being the cause of the motion of the earth's axis, giving rise to the precession of equinoxes, it is called generally a "precessional motion." This first principle is that chiefly referred to in the inventions about to be described, where the effect depends essentially on the great amount of resistance thus offered to any angular motion impressed by an extraneous cause on a perfectly-balanced revolving heavy disk. The most important observations requisite to be made at sea are those of the *altitudes* of the heavenly bodies, on which depend both the determination of the *latitude*, and the correction of *time* essential to finding the *longitude*; and for this purpose there is a necessity for a well-defined horizon, which it is often impossible to obtain from the atmosphere in its lower parts, when the sun or star can be distinctly seen above, and this more especially at night; yet the safety of the ship may essentially depend on such an observation. Hence various plans have been resorted to for obtaining an *artificial* horizon. Simple reflection from the surface of a liquid can hardly ever be practicable, on account of the motion of the ship, though it is the usual substitute on land. The most celebrated attempt to substitute some other principle was an application of *rotatory motion*, devised by the late Mr. Troughton, in 1820. It consists in causing a disk, truly balanced on a fixed pivot, to spin round with great velocity, so as to keep up its motion during the time required for an observation, known by the name of "Troughton's top." The disk carried a plane reflector on its upper surface; and being a cylinder hollowed out at its lower end, and the point of support within, the centre of gravity is thrown below, so that it is in stable equilibrium when at rest. The velocity is communicated by a separate train of wheels, from which it can be instantly detached. Thus, from the principle of *fixity of the plane of rotation*, it was expected that the reflecting surface would preserve its level, notwithstanding the motion of the ship. The method was, however, found practically to fail; and the failure has been since traced to another mechanical principle. The pivot partakes in the irregular motion of the ship. When the disk is *not revolving*, this motion is in turn communicated to the disk, and the centre of gravity being below,—the very circumstance which gives it stability on land,—causes it to acquire an oscillatory movement. When in *rotation* this does not show itself directly, but is compounded with the rotation, and causes a *processional motion*, which is fatal to its use as a *horizontal* reflector. Hence, if the centre of gravity *coincided* with the point of support, as would be most readily done by suspending the revolving disks in gymbals in the manner of Bonenberger's machine, this cause of irregularity would be avoided. By this means it would preserve *its original level;* but this would not necessarily, or usually, be the *true horizontal* level. To obtain the *true horizontal point* another contrivance has been made by the same inventor, which has been fully described in the "Notices of the Royal Astronomical Society," vol. xviii., p. 65, January, 1858. But for other classes of observation on board ship which involve the use of the *telescope*, especially those requiring one of considerable power, the same requisite of invariable stability of direction is yet more indispensable, but hitherto unattained. One of the most important desiderata of nautical astronomy has always been the means of observing at sea the eclipses of Jupiter's satellites, so frequently recurring, and affording so simple and direct a means of obtaining the longitude. In general, to procure stability on ship-board, it seemed an obvious recourse simply to *suspend* any object which it was desired to keep steady by cords from a fixed point in the vessel. But a body thus suspended is like a *plumb line:* when the point of support is itself set in motion, it acquires a part of that motion and becomes a *pendulum;* and it oscillates more irregularly and violently from the accumulation of motions impressed upon it continually by every fresh motion of the ship. The case is the same as that just considered in Troughton's top. Nairne's or Irwin's "Marine Chair," for carrying the observer and his telescope, was simply an application of this principle. It was tried on board ship, especially in a voyage to the West Indies, by the late Dr. Maskelyne: and though somewhat prematurely rewarded by the government, was found not to answer; though no one seemed fully aware of the cause of its failure, till Sir J. Herschel (in the "Admiralty Manual,") pointed out the principle just stated, and showed that this free suspension must tend to perpetuate disturbances rather than to destroy them. Thus, to produce the desired stability for a plane or stand on which the telescope is to be rested, we must have recourse to the *free revolving disk accurately balanced within gymbals, on its centre of gravity.* The balancing must be perfected by means of adjustable plugs both in the disk and in the gymbal frames; the pivots of the gymbals must be of perfect workmanship, to turn with the least possible friction, yet without looseness or displacement. An immense rotatory velocity must be communicated to the disk by machinery, of which its suspension must be quite independent, so that the moving power can be instantaneously withdrawn. All these conditions are fulfilled in the form of the machine, which, after repeated trials, has been adopted by Prof. Smyth, exhibited by him at the Paris Exhibition, 1855, and successfully tried on board Mr. Stephenson's yacht *Titania*, on his voyage to Teneriffe, in 1856. The grand principle of *fixity in the plane of free rotation,* is that which enables the revolving disk to retain parallelism to its original plane, however the external plane or pivots supporting the whole be moved.

From this principle the revolving disk resists all angular change of position *in directions perpendicular to its plane;* but it offers no resistance to any motion *in that plane.* Thus a free revolving disk in gymbals externally turning on pivots horizontally resting on supports fixed to the deck, will suffice to preserve the telescope from all deviation due to pitching and rolling. The addition of another disk, freely revolving in a *vertical plane,* whose external pivots turn vertically in a frame attached to the top of the former internal frame, the upper pivot projecting through it, and carrying a small platform for the telescope, and the whole, of course, balanced below, — will preserve the telescope from any *lateral* deviations of the ship. And the combination of the two will give a plane retaining its parallelism against all three causes of disturbances. But under favorable circumstances this last cause of disturbance is but small; so that this addition may be often of little importance. The invariable platform of this revolving apparatus may then equally be applied to support either a *telescope,* or the *artificial horizon* before mentioned, whether simply, or in conjunction with the sextant or reflecting circle. By a mere enlargement of the *scale* of the machine, the same stand which carries the telescope might be made to carry the observer also, which would be a material convenience for any nice observations. But the essential condition is the preservation of *perfect equilibrium* about the centre of motion. Now, if this were secured by proper compensation for the observer in *one* position, the slightest change of position on his part would vitiate that arrangement. The observer, instead of being an *extraneous* source of disturbance incapable of producing any impression on the balanced and revolving system, now becomes *a part of it,* and thus impresses upon it a fresh motion arising from every slight change of posture, which alters the exact position of the centre of gravity of the whole. This effect, however, would not manifest itself *directly;* but being *compounded* with the *rotation,* would show itself in a *precessional* motion, fatal to the stability of the direction of the telescope. A modified arrangement to meet this contingency has (which after all is not one of great importance practically, though interesting theoretically) been devised by Prof. Smyth. To complete the whole, Prof. Smyth has carefully investigated the best form of a train of wheels for communicating the rotatory motion, and has also considered the question of the best moving power to be used; which he finds, after many trials, to be that of *water:* which is but brought to bear by a peculiarly beautiful and simple form of the *turbine.* Thus, taking a summary view of the whole subject: — by direct consequence, from the simplest acknowledged mechanical principles, the gyroscope, when its equilibrium is slightly disturbed, demonstrates the precession of the equinoxes; explains the boomerang; and sustains itself in the air against gravitation. When its equilibrium is undisturbed it exhibits to the eye the actual rotation of the earth; and when restricted to one plane it acts as a magnetic needle without magnetism, or spontaneously rotates in parallelism with the earth. To these remarkable, diversified, and somewhat paradoxial applications, we have now to add another of far higher utility, — that it gives perfect stability for the nicest astronomical observations on board a ship pitching and tossing with every way and gust of wind.

ON A NEW ARTIFICIAL HORIZON.

At a recent meeting of the Royal Astronomical Society, Professor Baden Powell read a paper by Prof. C. Piazzi Smyth, on the above subject. After adverting to the use of the spirit-level for adjustment of astronomical instruments on land, Prof. Smyth observes: — "On shore nothing can be more convenient than this use of the level; but its adaptation to sea service is not easy. For, first, on account of the constant movement of the vessel, — never for the one-thousandth of a second stationary, — we cannot first adjust the level and then look at the star, but must see both the star and the level simultaneously in the field of the telescope. Let us accomplish this before going into further and residual difficulties. The sextant or angular measurer at sea is made to allow of two objects being seen at once, the star, and the referring point at a great or infinite distance, usually the horizon of the sea; but in the present case the level-bubble is to take place of the horizon. We must, therefore, 1st, put it at an infinite distance by means of a collimator; 2nd, have it to be looked at in a horizontal direction; and 3d, have the bubble something very notable. Here a level is illuminated by artificial light thrown through it from below; above is a diagonal mirror, and in front a lens, in whose solar focus the bubble is. An eye therefore in front sees the bubble in the horizontal direction, at an infinite distance, and very visibly, because where the fluid curls up to form the edge of the bubble, it scatters the upward light so completely that none reaches the eye, and the bubble looks as if it had a painted black margin. If we place the eye partly in front of the collimator, and partly off, we may of course see both the level-bubble and something else; and in sextant observations may, by moving the proper mirror, see a star and the level-bubble in the field together. To make a practical use of the level so arranged, we may either have it attached to the frame of the sextant, or have it on a stand in front of the sextant. But in either case the bubble must be brought to its own fiducial mark at the instant of any observation. Herein is the first of our practical difficulties; for if the level be on a stand, and that on the ship's deck, the bubble is rolling from end to end of the tube with every roll of the ship; and if the apparatus be fastened to the sextant and held by hand, no man has power or tact to hold it level. Some natural principle must be brought in then to assist man's hand. And such a principle is found in the level-bubble itself, which will always be at the summit of the arc. If, therefore, we can refer our observations, not to the fiducial mark, but to the bubble itself as it moves apparently with the rolling of the ship, we shall be able to measure altitudes accurately at sea; and herein are the chief of my novelties. If we make the radius of curvature of the level, and the solar focus of the collimator equal, then if the whole is tilted one degree the bubble viewed in the diagonal mirror runs in the opposite direction to the same angular extent, by reason of the equality of the level radius, and the focal length of the collimator, or radius of the sphere on which the optical picture is formed. The result of these compensations is, that tilt the case of the level up or down by any quantity within the angular extent of the level tube, you will always see the bubble in the same true horizontal direction. . . . . When carrying out the principle in practice, if the level is of 12 feet radius, a collimator 12 feet long would be an inconvenient appendage to a sextant; but the focus may then be lengthened virtually by inserting one or more concave lenses with a convex of much shorter focus. Now put everything together; and for an observation, in place of the ordinary sextant, use my reflecting circle, the level being attached to the same stand. Looking into the telescope, you see the star in the upper mirror, which moves with the circle, and gives the angle moved through on the vernier. The lower mirror, fixed on the frame of the instrument, shows the bubble of the level, along with the star, in the telescope. The observer sees the bubble in the middle of the field, and the star in its centre. If the stand be tilted by a small angle, the star ascends or descends in the field, but so also by the same angle does the peculiar level-bubble. . . . . In other words, though the field of view, and all fiducial marks of the telescope and the level-tube vary and err, yet the level-bubble always continues to show the true horizontal direction; it preserves the same angular distance from the star, and all that we have to do in making an observation (no matter whether the base be quite level or not — and herein is the difference from the ordinary use of a level on land), is so to move the upper mirror of the circle as to make the star come on to the bubble, and then read off the verniers. Once on the bubble, the star will remain there as long as the support, or ship's deck, does not roll through a greater angle than the level-tube comprises, or does not roll in a quick, jerking manner. For these larger deviations the principle of the stability of the free revolver must be applied, as elsewhere described. (See 'Ast. Soc. Notices,' xvii. 40.) On looking into the collimator, all the transmitted light is blocked out, except the central line. This is to keep the field dark and appropriate for star observations. The self-adjusting motions of the bubble then take place along the luminous lane, and laterally an approximate correction is shown by the approach of the bubble to one or the other side of the said lane." The inventor also adds, with reference to the practical construction of the level: — "Some practical men would exclaim, on hearing of a level of one foot radius, that it must be so sluggish as to be good for nothing; yet it is less sluggish; and the nature of sluggishness in levels has been much misunderstood, viz., mixed up with amount of motion of the bubble as depending on length of radius of the tube. Make treacle the fluid of levels, and they will all be sluggish; long or short radii, *i. e.*, after a certain angular tilt, the bubbles will be moving slowly for a long time before coming to their new point of rest. Put ether into a tube of long radius: it moves quickly in the above case from want of sluggish nature, and moves far, by reason of the long radius. Put ether into a tube of very short radius: men say, How sluggish, because it does not move far, measured in the linear way. But they should rather ascertain which bubble comes to its new place of rest soonest; and if by reason of short radius they cannot easily see what arc one bubble moves through, they can increase the radius optically by magnifying. Now, by mere optical magnifying, the bubble has no mechanical friction added to its motion; but if the magnifying be by increasing the radius of the level, the bubble has a greater mechanical task of walking along so much more in length of glass surface. Hence it may be shown that these new short-radius levels, viewed through magnifiers, are as accurate, and quicker in their motions of angle, than the usual ones of long radii looked at with the naked eye. All fluids have more or less of the sluggishness or stickiness so greatly developed in treacle: alcohol has less than water; ether the Germans found to have less than alcohol; and chloroform I have found to have less than ether, while it has the great advantage (seeing that the end of the tube must be hermetically sealed with a blow-pipe flame) over both ether and alcohol, of not being inflammable." He also mentions that practically a good range of arc is 5°, and a good radius, 9 feet.

At the close of this communication Prof. Powell presented a detailed account of the principle of the construction of Prof. Smyth's new instrument. To render such a description complete, Prof. P. stated, that it appeared to him, that a more precise elucidation of the optical principle involved was desirable: this may be most distinctly stated as follows: — 1. The tube of the spirit-level being a small arc of a circle of considerable radius, within the limits of this arc, the true horizontal line is the tangent to that point of the arc at which the bubble appears; and in any successive positions, the change of inclination is measured by the angle of intersection of the tangents, which is the same as the arc of the tube traversed by the bubble, or the angle at the centre. 2. Taking the chord of this arc as the base, a plane mirror inclined at 45° to it above, gives for the reflected image of the level-tube a similar arc convex towards it. 3. A lens placed in front of the mirror, having its principal focal length equal to the distance of the image of the bubble when at the middle point of the arc, and adjusted to receive centrically the reflected diverging pencil from the bubble will cause it to emerge in a parallel pencil, thus placing the image at an infinite distance, and enabling us to view it through a telescope. 4. In other positions of the bubble (since the convexity is towards the lens) it will not be accurately in focus; though it will be approximately so if the focal length be considerable, so that the small difference in the length of the rays from the middle point, and from the two extremities, may be neglected. 5. If the base be accurately horizontal, and consequently the bubble in the middle of the arc to which the index is adjusted, the axis of the reflected pencil from its image passing through the centre of the lens will be accurately horizontal, or the image seen through the lens gives the true horizontal point; this adjustment is supposed accurately made in the first instance once for all. 6. If the base (the lens and mirror being firmly attached to it) be inclined either way within the limits of the arc, any change in the position of the bubble measured on the arc of the tube will be exactly equal to the change measured on the arc of the reflected image. 7. If the focal length of the lens be equal to the radius of the arc of the level, and if the distance were accurately the same from the lens to all points of the reflected arc (as it would be if the image were concave towards the lens), then for any change of arc there would be an exactly equal change of angle in the rays; and as in the middle position the reflected ray is exactly horizontal, so in every other position would the reflected rays for those positions respectively give the true horizontal point. 8. If the radius or focal length be large and the arc small, the conditions will be so approximately fulfilled, that without sensible error the change of angle will be equal to the change of arc, or of inclination to the horizon, and consequently the reflected ray will give the true horizontal point without sensible error at all inclinations within the limits. Professor C. P. Smyth's invention becomes of peculiar value (in combination with his free-revolver stand), since the attainment of an artificial horizon on board ship has been in vain sought in any applications of the ordinary spirit-level, of liquid reflecting surfaces, by simple suspension in equilibrio, or by the rotating plane reflector of Troughton's top, the failures of which are demonstrable on mechanical grounds. But the invention may become of not less importance to observers on land, and especially to scientific travellers, from its portability and exemption from the disturbances incident to liquid reflectors.

ON THE COMPOSITION OF ROTARY MOTION.

At a recent meeting of the Liverpool Philosophical Society, Prof. Hamilton read a paper on the "composition of rotary motion," of which the following is a *résumé:*

The principle of compound rotations, he observed, was discovered by Paul

Frisi, in 1760. It was discussed in an abstract form by Ponsot in a Mémoire read before the Academy of Sciences at Paris, and by Poisson, in the *Journal de l'Ecole Polytechnique.* The same author had treated the subject at great length in his *Traité de Méchanique.* Professor Airey also had handled it in a very elegant manner in his "Tract on Precession." As the subject was not capable of being discussed without the use of the higher mathematics, it had not been alluded to by any of the popular writers on mechanics. The method of experimentally illustrating compound rotations was accidentally discovered by Fessel, of Cologne. The instrument which he invented was called polytrope, or gyroscope. It was modified in form and greatly improved by Plucker, Magnus, Wheatstone, and Foucault. M. Foucault, after making the beautiful pendulum experiment, which afforded the first *positive* demonstration of the earth's rotation, considered that the gyroscope might be employed for the same purpose, as it possessed the property of "maintaining the plane of its rotation unchanged." In experimenting with this instrument, Professor Hamilton referred particularly to its alleged properties of "maintaining the plane of rotation unchanged," and the "fixity of the plane of rotation." He remarked upon the loose sense in which these terms had been employed in descriptions of the gyroscope, and pointed out the restricted and qualified sense in which they ought to be used. The plane of rotation was not fixed in an absolute sense. It held a fixed relation to the plane of a great circle of the earth, and participated in the earth's diurnal motion, which was illustrated by a mathematical diagram. The time of its making an apparent revolution round a circle at the pole was twenty-four hours. The time at any other place varied inversely as the sine of the latitude. This was also submitted to a mathematical proof, and the point, in common with every other embraced in the lecture, rendered beautifully simple and clear. The gyroscope was then considered as a mechanical compass. Since the free axis of the gyroscope tended to become parallel to any other axis about which the disk was constrained to revolve, and since it was constrained to revolve about the axis of the earth, the free axis tended to become parallel to the axis of the earth. Hence, at any given station, the four parts of the horizontal ring indicated the cardinal points of the horizon, the extremity of the axis coincided with the celestial pole, and the plane of the disk with the plane of the celestial equator. And since the elevation of the pole above the horizon was equal to the latitude of the place, the elevation of the axis above the horizontal plane was equal to the latitude of the station. The gyroscope was therefore theoretically a mechanical compass, but practically it was of no value, since friction on the pivots produced a horizontal motion in one direction or the other, if the ring deviated in the slightest degree from the true horizontal position. For the same reason the gyroscope afforded no satisfactory proof of the earth's diurnal rotation. It would indeed be a valuable discovery if a method could be devised of making it, with certainty, deviate from the truth equally in opposite directions, and thus, by a mutual destruction of errors, indicate the true north. Mr. Hamilton then described Professor Elliot's apparatus for illustrating the precession of the equinox and the mutation of the earth's axis.

The Rev. H. H. Higgins put the following question: If the gyroscope were revolving in a vertical plane, the axis being horizontal, and force were applied to incline the axis, where did that force go to? Was it expended in producing an increased friction on the pivot? And if so, would the ultimate result be to arrest the motion of the disk?

Some conversation ensued upon the point thus raised, Mr. Hamilton giving it as his opinion that the effect would be as Mr. Higgins supposed. It would be, he observed, an interesting question whether the force thus applied would produce heat. In every instance where action was arrested heat was developed. M. Foucault arrested the motion of the disk by a magnet, and it became hot. He ascribed it to a mechanical action in arresting the speed of the instrument. The test suggested would be an *experimentum crucis* on the theory of an arrested mechanical action always giving rise to an equivalent amount of heat.

Professor Elliot thanked Professor Hamilton for the more than ample justice he had dealt out to him in noticing his instruments. He begged leave to make some additional statements regarding his own researches on the subject, as well as those of others. Professor Hamilton had correctly described his (Professor Elliot's) instruments for illustrating, both in regard to fact and principle, the precession of the equinoxes, the mutation of the earth's axis, the retrogradation of the moon's nodes, the equilibrium of Saturn's rings, and the peculiar effect of a magnet upon an iron disk in rotation. He had been led to the construction of the first of these instruments as long ago as 1835, from the difficulty he experienced, while teaching astronomy, in explaining the subject of precession by mere verbal description. He conceived that the motion of the common spinning-top might be converted into an exact imitation of those of the earth, by altering the position of the centre of gravity in reference to the point of support. In the conception of such an instrument he was not aware at the time that he had been anticipated by others; but he now found that the records of the Royal Astronomical Society contained a short and obscure notice of a similar instrument, which had previously been constructed by Mr. Atkinson. M. Bohnenberger's gyroscope had also been previously known on the Continent, and partially in this country, which, among other purposes, exhibited correctly the precession of the equinoxes; and a member of the society, now present, had, it appeared, constructed an instrument of the same kind. The effect of a magnet on a rotating iron disk had been shown to him (Professor Elliot) by a friend, but almost accidentally, and without reference to its theory or astronomical application. Its use for that purpose was to exhibit the effect of one planet in disturbing the plane of another planet's orbit, in eight different positions of the disturbing body (as described in Newton's Principia), in producing a forward or a backward movement of the nodes, or an increase or diminution of the obliquity or the plane of its orbit to the ecliptic. Professor Elliot stated that although his experiment with the iron ring and magnet were admited to be an exact imitation of the peculiar motion of Saturn's rings, yet the coincidence of the two in regard to principle had been disputed by some of our best mathematicians. The objection advanced to it by Professor Thomson, of Glasgow, that the one was, to a certain extent, a constrained, and the other a perfectly free motion, was certainly a valid objection; but there were some difficulties attending the subject which required to be removed by further experiments. He (Professor Elliot) had shown the greater part of these experiments to the Liverpool Polytechnic Society in the year 1839. Since that time the subject of rotary motion had become a fashionable study among mathematicians, having been taken up successively by Professors Magnus, Wheatstone, Powell, Foucault, Smythe, and Maxwell. Foucault's experiment for showing the stability (perfect or partial) of a rotating disk during the earth's rotation was not new in theory, as it had been described more than twenty years before by Mr. Sang, nor was it in his (Professor Elliot's) opinion practically successful in showing the earth's rotation, since its success depended altogether upon the adjustment of the apparatus; and the only mode, he believed, of making that very delicate adjustment was by trying it if it produced the very motion it was intended to demonstrate. It could be made to show the rotation of the earth, or not show it, just as it was balanced. Professor Smythe's apparatus he had previously described. Professor Maxwell, of Aberdeen, was the last who had produced anything new on the subject of rotary motion, and in a set of beautiful experiments and some refined calculations, he had advanced further into the subject than any of his predecessors. Professor Maxwell's principal object was to throw light on the theory of Saturn's ring, for his essay on which he lately had the high honor of gaining the Adams Prize offered by St. John's College, Cambridge, for the best dissertation on that subject.

Professor Maxwell objected to the applicability of his (Professor Elliot's) experiment on the iron ring to that of Saturn, but agreed with him in maintaining that Laplace's hypothesis of a load on the ring was untenable.

Professor Elliot briefly described two other experiments which he had himself made in regard to rotation. One of these consisted in magnetizing the axis of a light rotating disk, carefully depriving the disk of all precessional movement, inclining its axis a little from the vertical line, placing over its centre an electro-magnet, and then giving to the disk a rapid rotation on its axis. The axis remained at rest in its oblique position till an electric current was passed along the wire of the electro-magnet. As soon as that was done the axis of the disk began to revolve around the magnet. When the current was transmitted in the opposite direction, the direction of revolution immediately changed, producing a singularly close resemblance to that mysterious phenomenon, electro-magnetic rotation. The other experiment consisted in taking a very large cylindrical vessel of water, making a small orifice in the centre of the base, with a straight brass pipe extending a few inches downward, the outer end of the pipe being closed with a plug, and the inner end being made perfectly smooth. The vessel was filled with water, and as soon as that had attained perfect repose the plug was withdrawn. On this being done, as the level of the water sunk, it gradually acquired a rotary movement, at first very slow, but becoming more rapid as the evacuation proceeded, and always in the direction of the earth's rotation, provided that perfect stillness of the fluid had been secured. The *rationale* of the process was this: That the vessel of water participated in the earth's diurnal rotation, its own motions consisting of a revolution round the earth's axis, and a slow rotation on its own axis, that the portions near the circumference had a more rapid motion in rotation than those near the centre. In approaching the centre they retained their actual velocity, and, consequently, increased in angular velocity, and in escaping at the central orifice, produced a vortex in the direction of the earth's rotation, affording another experimental proof that such rotation existed.

Mr. Higginson apprehended that when the iron pole of the instrument was not itself a magnet, the action of the magnet was the same as that produced by suspending a weight to the equatorial part of the instrument. When a movable magnet, turned upside down, was supplied, there would be repulsion. It would act as if the instrument were made topheavy, so as to give it a larger and larger eccentricity. Would the fact of the different position of the centre of gravity make the difference?

ON THE VIBRATIONS IN WATER FALLING OVER DAMS.

The continuance of the discussion on this subject has been kept up during the past year, in the Boston Society of Natural History, and at a recent meeting the following communication respecting experiments made at South Natick, Mass., by Mr. William Edwards, was presented. The dam at this place across Charles River, is nine feet high and 200 feet long, and at certain stages of the water, the vibrations are so powerful as to agitate bodies three-quarters of a mile distant. During the month of December, 1857, the water being at its most favorable point for the production of vibratory movement, Mr. E. undertook a series of experiments by erecting a flashboard, three feet in length, on the top of the dam; the water was shut off, and a dry entrance obtained behind the falling sheet. Ample room was found to walk back and forth. At the place of entrance, the flame of a lamp was unagitated, a fact unexpected by them, as they had favored the hypothesis of Prof. Snell, and had supposed that air sufficient to cause the vibrations would have found an exit at this opening.

Twenty-five or thirty feet from the entrance, the flame was slightly agitated simultaneously with the vibratory motions. A falling feather descended as quietly as in a close room. The discharge of a pistol produced no perceptible effect upon the water, although the report nearly stunned them. It having been suggested that the vibrations are produced at the bottom of the sheet and continued upward to the top of the dam, they endeavored to produce such an effect by placing obstructions at various points, but without success. Whilst holding the ends of the fingers in the current, two or three inches from the corner of the timber over which the water breaks, it was evident that the water passed the fingers in ridges, apparently one-half or three-quarters of an inch apart. By following with the eye, in their descent, these ridges or vibrations, it was found that the interval between them increased, with the velocity of the water, to the extent of fourteen inches. An aperture in the water, made by the passage of a stone of the size of a hen's egg, at the top of the fall, increased in size just in proportion to the increasing distance of the vibrations, retaining its circular form, and finally expanded to fourteen inches in diameter. Upon the top of the dam they could see distinctly through the current to the edge of the timber over which the water breaks, and they found that *the water at this point acquires a tremulous motion, giving origin to the ridges or vibrations* alluded to above, which here follow each other at intervals not exceeding one quarter of an inch. The sheet of water at the top of the dam is six inches in thickness; at the bottom two and a half inches. The ridges are evident on the inside the whole length of the fall, but upon the outside they do not make their appearance for the space of ten or twelve inches. They increase in size relatively with the distance.

At a subsequent meeting, Mr. E. stated that he had counted, as nearly as possible, the number of vibrations, at some distance from the dam, and the number of the waves, and, although their rapidity made it very difficult to count them, ranging as they did from 280 to 325 per minute, he found that they coincided. This fact was rendered still more conclusive by assuming a position at one end of the dam where the vibrations could be seen, heard, and felt, all at the same time. Every portion of the timber over which the water flows, produces vibrations of greater or less distinctness, and, occasionally, the waves of a certain portion of the dam fall in the wave intervals of the other end of the dam, and then the vibrations of the earth cease. Standing in front of the dam, and placing a pole on the bed of the river, directly under the fall, the pole was violently agitated, although there are two feet of back-water through which the water must pass before it reaches the pole. In the falling sheet, the wavelets are concavo-convex, and not double convex, that is, the internal surface corresponding with an external convexity is concave.

Prof. W. B. Rogers remarked, that the wave-like divisions of the descending sheet of water were probably referable to the same general law which has been shown by Savart and Plateau to obtain in the case of a stream flowing from an aperture in the bottom or side of a vessel. These philosophers have proved that, at a certain distance from the point of discharge, the stream, although seemingly continuous, is in reality divided into separate parts; and Plateau explains this subdivision by the preponderance of transverse cohesive forces in the column, when its length exceeds its thickness by more than a determinate amount. In this case the sides of the stream are drawn together at intervals, and the mass is thus broken up in separate sections, which, by further cohesive action, are moulded into drops. Thus every such stream, at some distance below the aperture, loses its straight outline, and assumes the form of a series of enlargements and contractions, which, at a still greater distance, become visible as a succession of drops.

It is interesting to remark that a regular system of vibrations occurs in the streams thus affected by wave-like subdivisions, enabling them to produce strong musical tones, when suffered to strike upon an elastic surface, and to communicate similar musical vibrations even to the reservoir itself. In this action we discern somewhat analogous conditions to those of powerful vibratory movements attending the descent of large masses of water over dams.

ON THE MATHEMATICAL THEORY OF SOUND.

In a paper on the above subject read before the British Association, 1858, by the Rev. S. Earnshaw, the author adverted to the circumstance, that the only impediment to the complete development of the mathematical theory of sound has hitherto been the difficulty of integrating the partial differential equation $\left(\frac{dy}{dx}\right)^2 \frac{d2y}{dt2} = \mu \frac{d2y}{dx2}$. As an approximative mode of surmounting this difficulty, it has been usual to assume $\left(\frac{dy}{dx}\right)^2 = 1$. But the author suggested that the legitimacy of that step is by no means evident; and that the true test of the allowableness of it is a knowledge of the change, which must take place in the constitution of the atmosphere, in order that $\frac{d2y}{dt2} = \mu \frac{d2y}{dx2}$ may be the *exact* equation of motion. In this way it will be seen whether the physical change, represented by assuming $\left(\frac{dy}{dx}\right)^2 = 1$, be of such a minute character as to be allowable. But the author stated that he had found the requisite change in the constitution of the atmosphere must be such that it must *increase* in volume with an increase of pressure, — a constitution the very *opposite* of that of the natural atmosphere. From this it was inferred that the equation which represents the properties of sound does not admit of the assumption $\left(\frac{dy}{dx}\right)^2 = 1$. The reason why this assumption, though analytically allowable, is not allowable in the problem of sound is, that in that assumption quantities are neglected which (in the case of sound) are of the same order as those which are retained, so that the equation $\frac{d^2y}{dt^2} = \mu \frac{d^2y}{dx^2}$ is not an approximation, but is reduced to the form $0 = 0$; from which nothing can be inferred but that the assumption $\left(\frac{dy}{dx}\right)^2 = 1$ is not admissible. The result of this reasoning is, that the equation $y = \mathrm{F}(x + at) + f(x - at)$, which has hitherto been the basis of explanation in Treatises on Sound, has nothing whatever to do with sound, but represents the motion of a wave in an imaginary elastic medium of a constitution the very opposite of that of the atmosphere and of all known gaseous media. The mathematical theory of sound is consequently put back to its differential equations, beyond which not an inch of ground can be maintained. Till the differential equation is integrated accurately, without assuming $\left(\frac{dy}{dx}\right) = 1$, no advance can be made, and science remains *without* a mathematical theory of sound. The author then announced that he has succeeded in integrating the differential equation of sound without approximative assumptions; that he has, in fact, obtained its *exact* integral; and in the result has possessed himself of the key to the various properties of sound. Among several others, it was stated that the exact integral accounts for the great difficulty which experimenters have found in obtaining accordant velocities of sounds, — for the sweetness of musical sounds, — for the rapid decay of violent sounds as they progress, — and proves that the velocity with which a sound is transmitted through the atmosphere depends on the degree of violence with which it was produced, and not (as in light) on the length of the wave; so that sounds of every pitch will travel at the same rate, if their genesis do not differ much in violence; but a violent sound, as the report of fire-arms, will travel sensibly faster than a gentle sound, such as the human voice. This last property the author stated to have caused him much trouble, in consequence of its being directly opposed to the testimony of almost every experimenter. For many affirmed, as the direct result of their observations, and others assumed, that all sounds travel at the *same* rate. Fortunately, it transpired at the meeting that in Capt. Sir J. Franklin's Expedition to the North, whilst making experiments on sound, during which it was necessary to fire a cannon at the word of command given by an officer, it was found that the persons, stationed at the distance of some miles to mark the arrival of the report of the gun, always heard the report of the gun before they heard the command to fire; thus proving that the sound of the gun's report had outstripped the sound of the officer's voice; and confirming in a remarkable manner the result of the author's mathematical investigations.

The Astronomer Royal said, that while he had no doubt whatever of the general accuracy of the conclusions at which Mr. Earnshaw had arrived, and while he fully admitted their importance, he could not subscribe to all that he had said. In his historical sketch of the steps by which we had arrived at our present knowledge of the subject, he could not admit that the method of Newton was wrong, the fact being, that it was a strictly correct solution of one case of what was a very general problem. The method of Newton was the very basis of all our modern methods; and he looked upon that portion of the second book of the Principia as a monument of the genius of Newton, which he was very sorry to see was beginning to be much less attended to in our Universities than it deserved. He could not also admit that so little had been done by the methods heretofore in use: and although he considered a vigorous integration of the equation to be very important, yet he considered much had been done even by himself by using the method of successive approximations, similar to that adopted in the Lunar and Planetary Theories. Of this he adduced several examples, such as those in his article on Waves and Tides in the Encyclopædia Metropolitana, and the non-reflection of breaking waves, while at the same time, like the whisper in the gallery of St. Paul's, they were conducted along a smooth wall up to which they moved very obliquely; also bores and quiet tide-waves, and some others. He also could not subscribe to the objection that assuming the differential coëfficient to be unity required that the air should be so constituted that pressure in a given direction should be accompanied by a motion of the particle in the opposite direction, for this frequently happened where the particle was already in motion. Mr. Earnshaw explained that what he meant to convey was, that the pressure should be the originator or cause of motion in the opposite direction.

NATURAL DIAPASON.

M. Cagniard de la Tour, a French physicist, has satisfied himself that he hears the sound *la* of the musical scale sounding within his head when he agitates it from side to side; and Mr. Jobard has confirmed this observation in himself, and asserts that any one can verify it, if he will disembarrass his neck of the cravat and collar, and place himself apart from all noise.

Mr. Jobard thus explains the fact: This natural *la* is caused by the contact of the *malleus* against the *incus* in the ear — a contact easily made by a rapid movement of the head. Mr. Jobard goes still farther: for according to him, those persons who, on shaking the head, hear two *la* in perfect unison are born musicians; they have the voice and ear perfectly correct. But those that hear *la* only in one ear, have an imperfect appreciation of sounds; and those who perceive two different sounds, the *la* and another note, not only do not love music, but detest and avoid it; and he proposes by this method to discover among young people those who may become musicians.

Mr. Jobard has still under inquiry, whether the note which is produced in the head by a quick motion is the same or not in all persons. The question will be of difficult solution; for the *la* of the opera of Paris is different from that of the theatre of Strasbourg, Lille, Vienna and London, and that this *la* is becoming more and more elevated, so that in twenty years it will correspond to many more vibrations than now, and be therefore still more acute.

ON THE SONOROUS ACTION OF JETS OF BURNING GAS.

At a recent meeting of the Boston Society of Natural History, Prof. Rogers called attention to the curious phenomena connected with the sonorous action of jets of burning gas, which have lately been observed by Prof. Tyndall and others.

The production of a musical sound by a small flame of hydrogen gas, burning within a tube, has long been one of the most familiar of lecture-room experiments. Prof. Faraday, in one of his earliest investigations, showed that this musical effect was not confined to hydrogen, but could be produced with flames of carbonic oxide, common illuminating gas, and several other gases and vapors. He was, moreover, the first to give a philosophical theory of the sound, by showing that, in the conditions of the experiment, the flame resolved itself into a series of little explosions, which, succeeding each other at very small and equal intervals of time, gave rise to regular, and therefore musical vibrations in the tube. In the recent experiments the further fact has been observed, that the flame, both when singing and when silent in the tube, is strongly acted on by external sounds, having a certain musical relation to the tone of the pipe or flame.

In studying experimentally the conditions under which these sounds are produced, Prof. Rogers had lately ascertained that the usual absence of the sonorous effect in the case of lamps or candles burned under the same conditions as the gas, is not due, as might be supposed, to a mechanical interference of the wick with a vibrating motion. He found that wicks of cotton thread, and of asbestus, introduced into a jet-pipe of gas, even so as to project far into the flame, did not prevent its singing, although they impaired the purity of the musical tone. The difficulty of obtaining continuous musical sounds from a common flame with a wick, must rather be ascribed to the nature of the combustible matter, which, requiring a very large supply of air to produce the explosions, is liable to be extinguished before the musical sound is developed.

To obtain an *ether flame* suitable for these experiments, Prof. Rogers uses a glass tube about eight inches long and one quarter of an inch in diameter, open below, and drawn somewhat bluntly to a point, with a small aperture at the top; some loose cotton twine or thread of asbestus being introduced so as to terminate at or very near the pointed opening, the tube is half filled with sulphuric ether; the larger end is closed with a tight cork, and the little ether candle is fixed vertically in the centre of a wooden block. On applying a light to the apex, the ethereal vapor burns in a steady bluish jet, which with proper tubes enables us readily to repeat all the experiments on the singing and the silent flame. This simple apparatus acts freely at ordinary temperatures, and may be used from time to time for several days without replenishing.

In regard to the agency of the flame in giving rise to the musical tone, Prof. Rogers thought it might be compared to that of the reed of various wind instruments, the vibration of which, by giving motion to the column of air in the pipe, causes the sound to begin, although the vibrating column, by its reäction on the reed, controls more or less the rate of its oscillations, and determines the pitch of the sound produced. A similar reäction between the aërial column and the flame quickly establishes a synchronous motion of the two, corresponding to the fundamental note, or to one of the natural harmonics of the tube.

At a subsequent meeting, Prof. Rogers stated that by employing hollow circular wicks and using tubes but slightly exceeding them in diameter, he had been able to produce these musical effects with the flames of sulphuric ether, common alcohol, and the mixture of the latter with spirits of turpentine, which is known as burning-fluid. By using an iron tube at a high temperature he had obtained, though less perfectly, a similar result with the flame of spermaceti oil.

As the effect in these experiments depends on the access to the flame of a current of air of definite amount and velocity, and in a proper direction, it is necessary to adjust the diameter of the wick and size of the flame to the dimensions of the tube employed, and to hold the tube with its lower edge a little below that of the wick. The flame will then be seen to contract, to lose much of its brilliancy, and, after more or less of a rattling sound, to give forth a musical tone, which, with a little care, may be rendered quite smooth and continuous.

These results are readily obtained with the flame of the small circular wick lamp now in use for burning the mixture of alcohol and turpentine. In this form of lamp the wick-tubes rise about two inches above the reservoir, and an internal movable tube is provided, which, being raised or lowered, serves to vary the depth of the wick and to adjust the flame with great nicety. The body of the lamp should be removed from its pedestal, and placed on a ring support, to secure a free current of air upward through the central wick-tube.

A simple way of making the experiment with an alcohol wick, is to enclose between two glass tubes a strip of cotton cloth or thick paper, so that it may project a little beyond the upper end of the tubes. When charged with alcohol and lighted, it will furnish a hollow circular flame well suited for the temporary production of the musical effect.

These results favor the conclusion that *flames of every kind are capable of exciting sonorous vibrations,* provided the air and combustible vapors are brought together in such proportions as to form more or less of an explosive mixture, and they therefore confirm the explanation of Faraday, which refers the musical sounds produced in such cases to a rapid and uniform succession of small explosions.

The following experiments illustrate, in a very simple way, the origin and some of the conditions of these musical vibrations:

(1.) When a jet of burning coal-gas is introduced into the resonant tube in a position in which it does not sing spontaneously, we may cause it to commence its musical performance by simply vibrating the jet-pipe rapidly from side to side. In this experiment the pipe should be covered with soft buckskin for two or three inches near its upper end, to prevent the sharp jarring sound caused by its striking the glass. This movement of the jet is so efficient in bringing on the sonorous state, that it will compel the flame to sing, even in a tube in which it would not do so spontaneously in any position in which we could place it. Indeed, it will often excite the musical vibrations in cases where, from the unsuitable proportions between the tube and flame, the external sounds used in Tyndal's experiment entirely fail to bring on the sonorous state.

Although the singing is induced more promptly when we allow the jet-pipe in its vibrations to strike the sides of the tube, this action is not at all necessary; for we obtain the same result when the pipe is merely shaken to and fro within such limits as to prevent its touching the glass. The effect here described can hardly fail, when first observed, to excite surprise, especially if from previous trials we have found that the flame refuses either to sing spontaneously or under the action of external sound.

So far as the impulse of the jet-pipe against the sides of the tube is influential in exciting the sound, we must ascribe its action to the feeble musical resonance produced by it within the tube, which, although composed of several sounds, may always be observed to include the fundamental note of the tube. This action is therefore like that of a unison note sounded by the voice or an external instrument, or that of any other mechanical agency giving rise to a vibration of the included column of air. But the other and far more remarkable effect, the excitement of the musical condition in the flame by simply moving it to and fro without striking the tube, cannot be thus explained, since the gentle impulse given to the air by the vibrating pipe produces no audible effect, and would seem quite inadequate to excite in the column any but the very feeblest vibration. Admitting, however, that these extremely faint vibrations of the air may contribute somewhat to the result, it can hardly be doubted that the main influence by which the movement of the jet produces the effect in question is by causing so rapid a mixture of the adjoining air with the gas that the latter, before being inflamed, is brought into a condition to produce those small explosions, which, by their quick succession, give origin to the sound. The effect of motion in bringing about this explosive condition of the flame is well exemplified by the following experiment:

(2.) Fastening a jet-pipe, some ten inches in length into the end of the flexible tube through which the gas is supplied, and holding it erect by a point a little below the insertion so that we can readily cause it to vibrate, we ignite the gas and adjust it to form a slender flame about an inch long. If now the flame be moved from side to side at a moderately rapid rate, it will assume, according to a well-known visual law, the appearance of a continuous arch of whitish light, retaining at the extremities the whole height of the stationary flame, but growing narrower from either side towards the middle. In these conditions the flame is *entirely noiseless.* As we gradually increase the speed of the vibrations, the arch, at a certain stage, suddenly breaks in the middle, where a faint bluish flame takes the place of the usual whitish light, and at the same instant *a sharp noise is heard,* due to the inflammation of the explosive mixture at this part of the vibration. It is hardly necessary to say that as the vibration is quickest at the midway point, coming to a pause at each end of the arch, the gas becomes more largely mixed with air at the middle than towards the extremities of the motion, and is, therefore, at this point earliest reduced to the state of an explosive mixture.

As we increase the velocity of the vibrations, the sonorous part of the arch extends towards the ends, until the path of the flame presents the aspect of a narrow, bluish band, irregularly serrated at the top, and flanked at the extremities by a tall flame of the usual whitish color. As might be expected, when the jet is revolved rapidly in a circle, the white light entirely disappears, and the ring of bluish flame which results gives forth a continuous but not distinctly musical sound. When made in a dark room these simple experiments were found to be unexpectedly interesting and beautiful.

(3.) As in the above cases the action is mainly traceable to the more rapid mingling of atmospheric air with the flame, it is natural to conclude that a like effect would be produced by passing *a current of air* upwards through the tube, and on trial this anticipation was strikingly verified. In order that the current may be evenly distributed, it is convenient to employ an argand burner, having the supply-pipe at the side, and the central opening entirely free, so that the jet-pipe may rise through the centre, and the burner be adjusted to the proper distance below the flame and the bottom of the glass tube. The air conveyed to the argand burner through a flexible pipe, may be supplied either from the lungs of the operator or from an adjacent gasometer. In most cases the action of the current is more easily managed when the apertures from which it flows are some two or three inches below the bottom of the resonant glass tube.

With this arrangement, and a proper graduation of the current of air impelled into the tube, we can cause the flame to sing when the other methods above described have failed to produce any effect. When the flame is not far from the position in which it would spontaneously sing, the *lightest breathing through the argand pipe is sufficient to bring it to the sounding state*, and to maintain a clear, smooth tone. Even when the flame is large and otherwise not readily susceptible of the sonorous action, a stronger current of air applied nearer to the bottom of the resonant tube will rarely fail to bring on the musical vibrations. It should be mentioned that these effects can be produced in a simpler, but less satisfactory manner, by using, instead of the argand burner, to conduct the current, a common glass tube, bent suitably, and held near the jet-pipe below the opening of the resonant tube.

The sound familiarly observed when a flame of any kind is blown upon, and especially when the air is forced into or through the flame, in the case of the jet of a blow-pipe, was long ago referred by Faraday to the combustion of an explosive mixture formed by the air and burning matter. The sound produced by a blazing fire of wood or bituminous coal, as contrasted with the silence of a flameless mass of ignited anthracite, is an obvious illustration of the same principle. But the experiments above described show the operation of this law under conditions which enable us more satisfactorily to mark the origin of the sound and the gradations by which it accompanies the formation of the explosive mixture.

(4.) The *intermitting character* of the combustion in a singing flame has been beautifully shown by Prof. Tyndal, by causing the light of the flame to fall upon a revolving mirror, from which it is reflected so as to form a series of images on a distant screen. A similar resolution of the flame into successive explosions is more simply exhibited by *moving it rapidly* to and fro, or better still, by giving it a steady *revolving motion* within the tube. In using the former method, the jet-pipe may be attached to the common gas stand by a short piece of flexible hose, and passed through a ring so placed as to restrict the vibrations to a range less than the diameter of the tube. A sufficiently regular movement may then be given by the hand. If, now, we adjust the flame in the tube, so that it will not begin to sing for some time after the vibration has commenced, or until the tube is further lowered, we observe at first merely the continuous band of light due to the permanence of the visual impression; but, as soon as the singing commences, this band becomes waved or serrated at the top, and with a proper velocity divides into nearly separate columns of flame, with obscure spaces between.

The effect is, however, far more striking *when the flame is made to revolve* at a uniform rate in the tube. In this case, so long as it remains silent, it presents the appearance of a hollow cylinder or short tube of whitish light; but the moment the singing begins, the cylinder assumes a toothed form on the top, resembling a brilliant crown, and *divides itself into a number of narrow luminous columns, separated by bands nearly or quite deprived of light*. It is hardly necessary to say that the obscure spaces mark the moments in the rotation when the explosions occur, and the bright ones the successive intervals between them. With a given rate of rotation, as might be expected, the number of these subdivisions is greater in a short tube than in a long one, and is greater when the tube is yielding one of its harmonic notes than when giving its fundamental sound. In the same tube the number of subdivisions diminishes as we increase the velocity of rotation, a less number of vibrations or explosions in this case corresponding to one revolution of the jet.

(5.) To render the effect visible at a distance, it is, of course, necessary to use a large tube and flame. It is, however, beautifully distinct when the tube is some six feet long, by one and a half inches in diameter, and the flame three quarters of an inch in height. The mechanism employed to give rotation to the jet consists of a grooved wheel connected by a band with a small pulley. Into the latter the supply-pipe enters from below by a smooth gas-light joint, which allows the pulley freely to revolve. The jet-pipe, secured to the middle of the upper face of the pulley, tapers to the extremity, and rising to the height of six or eight inches, is elbowed near the top, so as to give the flame, when revolving, an orbit of nearly an inch in diameter. When the experiment is in progress, the appearance of the horizontal portion of the jet-pipe affords incidentally a very pretty proof of the intermittence of the singing flame. As each successive explosion makes this part strongly visible, it assumes the aspect of *a number of brilliant spokes* corresponding to the subdivisions of the crown of flame; and if, to vary this effect, we blacken the horizontal part of the pipe, and fasten near its outer end, or where it resumes a vertical direction, a brilliant bead of glass or metal, we are presented with *a circle of starry points,* each of which, by a proper adjustment of the motion, appears to be at rest.

(6.) The following proof of the intermitting nature of the singing flame, suggested by the effects just described, is at once so simple, and so readily seen at a distance, as perhaps to merit a place among useful lecture-room illustrations. In this experiment the jet-pipe bearing the flame is held at rest in the tube, and the required effect is produced by receiving the light on a circular disk of thick pasteboard or of metal, some six inches in diameter, supported near the tube on a horizontal axis, around which it may be revolved by the impulse of the hand. The face of the disk next the tube, colored of a dead black with paint or a covering of cloth, should have a narrow strip of white paper fastened upon it in a radial direction, or a small circular bit of the same placed near the edge. If both faces are used alternately, we may affix the white bar to one and the dot to the other.

On bringing the six feet tube down over the flame, and giving rapid motion to the disk,we remark that so long as the flame continues silent, the bar or dot is quite invisible; but, as soon as the sound commences, the black disk becomes diversified by a series of whitish images of one or other of these objects arranged at equal intervals around the central point. It is scarcely necessary to say, that the number of these images, as well as their apparent motion or rest, will depend on the time of rotation, as compared with the intervals of the explosions of the flame. Should it happen that the period of one revolution of the wheel is precisely that of a certain number of the explosions, neither more nor less, or that of any multiple or sub-multiple of this number, the images of the bar or dot will continue in each successive rotation to present themselves at the same points; but, should this relation not subsist, these images will be seen to shift their places on the disk,— sometimes appearing to advance, and at others to retreat.

EXPERIMENTS ON THE QUALITY OF SOUND.

The *Cosmos* (Paris), December 25, 1857, describes a very curious apparatus, devised by M. Scott, by means of which some very interesting experiments were made in reference to the different qualities of sounds, and the cause of these differences. The apparatus consists of a tube spreading out widely at one extremity like a trumpet, and closed at the other end by a thin stretched membrane, to the middle of which is attached a very light pencil. The tube concentrates the sounds which enter by its base, and the vibrations of the membrane thus produced are written by the pencil upon a paper coated with lamp-black, which is uniformly passed under the pencil by clock-work. The traces thus produced may be copied and preserved (magnified if necessary) by photography.

When the common accord was sounded on different instruments, the figures formed were very different both in form and dimensions, according as wind instruments, stringed instruments, or the human voice were used. The same differences were seen when the record of singing was compared with that of unmusical noises. M. Scott established this curious fact, that the series of vibrations formed by the sound of an instrument or voice was more regular, even, and consequently more nearly isochromous, in proportion as it is more pure and agreeable to the ear. In shrill cries, and harsh sounds of instruments, the waves of condensation are irregular, unequal, and not isochromous. In one experiment, it was shown that, in the impure sounds of the voice, two, and sometimes three, secondary sets of vibrations could be detected, combined with the principal.

THE AIR-BALANCE: A NEW FORM OF BAROMETER FOR WEIGHING ATMOSPHERIC FLUCTUATIONS. BY J. B. JAMES, M. D.

Reflections upon the action of a barometer said to have been in use for some time in the observatories of Liverpool and Rome, which has that portion of the tube not immediately immersed balanced upon a lever, has suggested an arrangement, which, it is believed, is new, and by which minute changes in the pressure of the atmosphere may be weighed with great accuracy. Let the barometer tube be fixed over a platform, with its lower end freely exposed; let the platform also support a balance, one arm of which, being placed directly under the lower end of the tube, supports the reservoir containing the surplus mercury; in this the lower end of the tube is immersed in the usual manner. If, now, the other arm of the balance be weighed to counterpoise the reservoir and the mercury not supported by the atmosphere, the alteration which change of atmospheric pressure renders necessary in the counterpoise, indicates the change of pressure which has occurred.

Several circumstances require attention, besides delicacy in the balance and accuracy in the weights, to secure accuracy in the results. 1st. The tube must be of uniform capacity throughout its length, excepting so much as is immersed in the mercury; varying capacity in the different portions of the tube will be productive of error by the alterations which change of temperature produces, even if the vacuum chamber be uniform. 2d. An alteration of either the temperature of the mercury or the atmospheric pressure, will require a reädjustment at each successive observation, by elevating or depressing the support of the balance or the tube in such manner as to cause the immersion of the tube in the mercury to the same point to which the first adjustment was made; the immersion of more or less of the tube will cause an error in the result. For this purpose two balances may be used; upon one the tube may be balanced in the manner first alluded to, by which the point of immersion is determined, the reservoir-balance being fixed in its normal position for that purpose. The tube-balance being now fixed, the other is freed, and brought to the same position by weights; thus the mercury need not be seen in any part of the operation. This adjustment would probably be facilitated if a short piece of iron or platinum tube of small diameter were attached, which would displace less mercury, and prevent, to some extent, a fluctuation of the column of mercury caused by the motion of the balance. 3d. If very great accuracy is desired, it may be requisite that the arms of the balance be symmetrical and bear equal and similar surfaces, so that change of temperature or atmospheric pressure, the accumulation of dust, oxidation of mercury, etc., may affect both sides equally. Variation of temperature will cause an alteration in the diameter of the tube, and be productive of a small error, for which no remedy is suggested.

This instrument, it is obvious, may be so adjusted as to indicate the pressure of the atmosphere in pounds and its parts on the square inch of surface. But, as it will often be desirable to compare observations made by it with those of other instruments, it will generally be required that it express the height of the column of mercury; to this end the instrument may be adjusted to a pressure equal, say, to thirty inches of mercury at a temperature of 32° Fahr. If, now, a tube has been used having a capacity of 1000 grs. of mercury at 32° F. for an inch of its length, which would have a diameter of about ·6 of an inch, each grain will represent the thousandth part of an inch; and if a correction has been made for the capillarity of the tube, all its indications, at whatever temperature, will represent the height of the column at 32° F., and require correction only for the altered diameter of the tube.

A rude instrument has been constructed on the principle here indicated. A barometer tube having a capacity of 100 grs. to the inch, or a diameter of ·18 inch, was suspended over the dish of a U. S. Post-office balance, upon a peg, by turning which the tube was adjusted to the surface of the mercury in the reservoir; and this rested upon the dish. Its operation was such as to justify the foregoing conclusions. Of course a scale may be attached, which will show, in the usual manner, the height of the column. — *Silliman's Journal.*

MODE OF PREPARING LIQUIDS OF GIVEN SPECIFIC GRAVITY, WITHOUT CALCULATION OR PREVIOUS TRIALS.

In the laboratory and in the arts, we are often required to prepare a definite mixture of two liquids, such as sulphuric acid and water, alcohol and water, etc. One of two modes is generally employed. 1st. Given the quantity and specific gravity of one of the liquids, the quantity of the other liquid is calculated. This mode is not always practicable, requires time, and for alcoholic liquids especially, the concentration or mixture gives rise to difficulties frequently insurmountable; or secondly, areometers are floated in the liquors; but this means, which is very practicable and very much used, presents great difficulties in manufacture, owing to the various temperature of the mixtures.

A densimeter of a new form, constructed by M. Spacowsky, of St. Petersburg, allows the preparation of a liquid mixture with great ease and precision, and without a thermometer. The apparatus consists of a vessel or areometer of platina. This areometer is closed above by a very thin partition or metallic plate, such as that employed in the aneroid barometers, and yielding to the fullest pressure. At its lower end the areometer is terminated by a tube furnished with a stop-cock. It is suspended by a platina wire from one arm of a delicate balance, and equilibriated by a weight suspended also by a platina wire from the other arm. The equilibrium thus established will evidently be destroyed if the areometer be filled with any liquid, but will be restored if the areometer and the counter-balancing weight be plunged in a liquid of the same specific gravity as that which it contains; and, as the thin partition allows the liquid contained to expand in accordance with the temperature to which it may be subjected, a very simple calculation will show that the reëstablishment of the equilibrium is independent of the temperature. As, moreover, the metal of which the instrument is made, is very thin, and a good conductor of heat, the equilibrium of temperature will soon be established between the interior and exterior liquid.

Now, to reproduce in any quantity a liquid of given specific gravity: fill the areometer with the given liquid, and plunge it and the equilibriating weight into the heavier of the liquids to be mixed, and add the other until the equilibrium is restored. The liquids will be rigorously of the same specific gravity.

ÆTHESIOMETER.

Considerable interest has been excited by an instrument, which it is proposed to call an æthesiometer, exhibited at a recent meeting of the Harveian Society, London, by Dr. Sieveking. It is constructed for the purpose of measuring the comparative sensibility of different parts of the surface, and consists of a rod of bell-metal, graduated into inches and tenths of an inch, upon which two movable points slide. The distance at which a person is able to distinguish the points as two separate impressions is a test of the sensibility of a given part. Thus, a person in health is able to recognize two points at the tips of the fingers which are less than one-tenth of an inch apart; in paralytic conditions this space would widen in proportions to the amount of insensibility; and the instrument, by measuring this space, becomes a physical test of considerable accuracy of the existence and extent of paralysis of sensation. Dr. Sieveking stated that the ordinary mode of determining the amount of sensation in such cases, by pinching or pricking the patient, did not afford sufficiently satisfactory results; but that he found the instrument which he exhibited useful as an aid to the physical diagnosis of some nervous affections, and determining by actual measurement the progress of the disease. Generally speaking, the purposes of diagnosis would be met by comparing the two corresponding points of the two sides of the body; but where an absolute standard of comparison was required, Webster's table, showing the sensibility of different parts of the body, and given in *Müller's Physiology,* would afford this. — *Med. Times and Gaz.*

SOLAR ECLIPSE OF MARCH, 1858.

M. Quetelet, at Brussels, carefully observed two compensating pendulums, in comparison with a chronometer, during the eclipse. The object was to see whether their vibrations were slower, as Professor Zantedeschi thought would be the case. The two pendulums were arranged so as to vibrate, the one parallel, the other perpendicular to the meridian. The one parallel to the meridian showed no change, but the other showed a loss of more than a second and a quarter per hour, during the eclipse. The record of observations made several times a day for several days, both before and after the eclipse, show that this was no accidental coincidence; but many more observations will be necessary to establish the connection of effect and cause between it and the eclipse.

WIND OF A SHOT.

The following extract from an Indian letter confirms the doubts entertained as to deaths attributed to the "wind of a shot": — "Brigadier Russell is also about to leave the army, under the advice of a medical board. Never, perhaps, in all the chances of war, has there been such an escape as his. A cannon ball cut the gold watch-chain at the back of his neck as cleanly as if it had been a pair of nippers, and did him no further injury, except inflicting a shock to his nervous system." — *Medical Times and Gazette.*

SOLAR REFRACTION.

Professor Thomson has applied the term "solar refraction" to characterize an effect deduced theoretically from the dynamical theory of heat, which, if proved to exist, will result in important consequences to every department of astronomy; for it at once infers the necessity of the existence of a medium pervading space of similar constitution to our own atmosphere, and undergoing, by necessity, a condensation in the neighborhood of the sun. Hence, also, there cannot but arise a refraction of objects beyond the sun, when this body crosses their line of direction. When Professor Piazzi Smith tested this "solar refraction" by the observation of stars transiting the meridian near the sun, the results, as far as they could be deduced, showed the existence of this "solar refraction," and, with it, of a resisting medium filling space, and forming a material connection still between the sun and all the planets.

PROF. FORBES ON SOME PROPERTIES OF ICE NEAR ITS MELTING-POINT.

Prof. Forbes has communicated to the Royal Society of Edinburgh the results of some experiments made by him on the properties of ice near its melting-point, with particular reference to those of Mr. Faraday, published in the *Athenæum* for June, 1850, to which attention has been more lately called by Dr. Tyndall and Mr. Huxley, in relation to the phenomena of glaciers. The substance of Prof. Forbes's statement is as follows:

"Mr. Faraday's chief fact, to which the term 'regelation' has been more lately applied, is this: that pieces of ice, in a medium above 32°, when closely applied, freeze together, and flannel adheres apparently by congelation to ice under the same circumstances.

"1. These observations I have confirmed. But I have also found that metals become frozen to ice when they are surrounded by it, or when they are otherwise prevented from transmitting heat too abundantly. Thus, a pile of shillings being laid on a piece of ice in a warm room, the lowest shilling, after becoming sunk in the ice, was found firmly attached to it.

"2. Mere *contact*, without *pressure*, is sufficient to produce these effects. Two slabs of ice, having their corresponding surfaces ground tolerably flat, were suspended in an inhabited room upon a horizontal glass rod passing through two holes in the plates of ice, so that the plane of the plates was vertical. Contact of the even surfaces was obtained by means of two very weak pieces of watch-spring. In an hour and a half the cohesion was so complete, that, when violently broken in pieces, many portions of the plates (which had each a surface of 20 or more square inches) continued united. In fact, it appeared as complete as in another experiment where similar surfaces were pressed together by weights. I conclude that the effect of pressure in assisting 'regelation' is principally or solely due to the larger surfaces of contact obtained by the moulding of the surfaces to one another.

"3. Masses of strong ice, which had already for a long time been floating in unfrozen water-casks, or kept for days in a thawing state, being rapidly pounded, showed a temperature of 0°·3 Fahr. below the true freezing-point, shown by delicate thermometers (both of mercury and alcohol), carefully tested by long immersion in a considerable mass of pounded ice or snow in a thawing state.

"4. Water being carefully frozen into a cylinder several inches long, with the bulb of a thermometer in its axis, and the cylinder being then gradually thawed, or allowed to lie for a considerable time in pounded ice at a thawing temperature, showed also a temperature decidedly inferior to 32°, not less, I think, than 0°·35 Fahr.

"I think that the preceding results are all explicable on the one admission, that Person's view of the gradual liquefaction of ice is correct (*Comptes Rendus*, 1850, vol. xxx. p. 526), or that ice gradually absorbs latent heat from a point very sensibly lower than the zero of the centigrade scale.

"I. This explains the permanent lower temperature of the interior of ice, and the formation, when ice is immersed in water, of a sort of plastic ice, or viscid water, having a most rapid variation of temperature.

"II. Such a state of temperature, though it is in one sense permanent, is so by compensation of effects. Bodies of different temperatures cannot continue so without interaction. The water *must* give off heat to the ice, but it spends it in an insignificant thaw at the surface, *which therefore wastes, even though the water be what is called ice-cold*, or having the temperature of a body of water inclosed in a cavity of ice.

"This waste has yet to be proved; but I have little doubt of it; and it is confirmed by the wasting action of superficial streams on the ice of glaciers, though other circumstances may also contribute to this effect.

"III. The theory explains 'regelation.' For when a plane surface of ice immersed in water is brought up to nearly physical contact with another plane surface, there will be a double film of 'viscid water' isolated between two ice surfaces colder than itself. So long as the surfaces immersed in water were kept apart, the films of water investing them were kept in a liquid or semi-liquid state by the heat communicated to them by the perfect water beyond. That is now removed, and the film in question has ice colder than itself on both sides. Part of the sensible heat it possesses is given to the neighboring strata which have less heat than itself, and the intercepted film of water in the transition state becomes more or less perfect ice.

"Even if the second surface be not of ice, provided it be a bad conductor, the effect is practically the same. For the film of water is robbed of its heat on one hand by the colder ice, and the other badly-conducting surface cannot afford warmth enough to keep the water liquid.

"This effect is well seen by the instant freezing of a piece of ice to a worsted glove even when on a warm hand. But metals may act so, provided they are prevented from conveying heat by surrounding them with ice. Thus, as has been shown, metals adhere to melting ice."

NEW METHOD OF INCREASING THE DURABILITY OF ENGRAVED COPPER PLATES.

At a recent meeting of the London Society of Arts, Mr. F. Joubert gave an account of a new method of rendering engraved copper plates capable of producing a greatly increased number of impressions, which consists of covering the printing surface with a very thin and uniform coating or film of iron. This is effected as follows: — At the positive pole of a galvanic battery a plate of iron is placed, and immersed in a proper iron solution, and a copper plate being placed at the opposite pole, and likewise immersed, if the solution be properly saturated, a deposit of iron, bright and perfectly smooth, is thrown upon the copper plate. This coating may be removed and renewed as often as is found necessary, and thus it is stated that 12,000 impressions have been produced from one copper plate.

ON THE REFINEMENT OF MECHANICAL AND ARTISTIC WORK.

The editor of the London *Literary Gazette*, having recently charged Ruskin, the well-known art-critic, with extravagant hyperbole, in using in one of his lectures the following expression: "Turner's pencil did not move over the thousandth of an inch without meaning," the lecturer defends himself in the following reply:

So far from being an hyperbole, it is much within the truth, being merely a mathematical accurate description of fairly good execution in either drawing or engraving. It is only necessary to measure a piece of any ordinarily good work to ascertain this. Take, for instance, Finden's engraving at the 180th page of Rogers's poems — in which the face of the figure, from the chin to the top of the brow, occupies just a quarter of an inch, and the space between the upper lip and chin as nearly as possible $\frac{1}{17}$ of an inch. The whole mouth occupies one-third of this space, say $\frac{1}{50}$ of an inch, and within that space both the lips and the much more difficult inner corner of the mouth are perfectly drawn and rounded, with quite successful and sufficiently subtle expression. Any artist will assure you that, in order to draw a mouth as well as this, there must be more than twenty gradations of shade in the touches; that is to say, in this case, gradations changing, with meaning, within less than the thousandth of an inch.

But this is mere child's play compared to the refinement of any first-rate mechanical work — much more of brush or pencil drawing by a master's hand. In order at once to furnish you with authoritative evidence on this point, I wrote to Mr. Kingsley, tutor of Sidney-Sussex College, a friend to whom I always have recourse when I want to be precisely right in any matter; for his great knowledge both of mathematics and of natural science is joined, not only with singular powers of delicate experimental manipulation, but with a keen sensitiveness to beauty in art. His answer, in its final statement respecting Turner's work, is amazing even to me, and will, I should think, be more so to your readers. Observe the successions of measured and tested refinement. Here is No. 1:

"The finest mechanical work that I know which is not optical is that done by Nobert in the way of ruling lines. I have a series ruled by him on glass, giving actual scales from ·000024 and ·000016 of an inch, perfectly correct to these places of decimals*, and he has executed others as fine

* That is to say, accurate in measures estimated in *millionths* of inches.

as ·000012, though I do not know how far he could repeat these last with accuracy."

This is No. 1 of precision. Mr. Kingsley proceeds to No. 2:

"But this is rude work compared to the accuracy necessary for the construction of the object-glass of a microscope such as Rosse turns out."

I am sorry to omit the explanation which follows of the ten lenses composing such a glass, "each of which must be exact in radius and in surface, and all have their axes coincident;" but it would not be intelligible without the figure by which it is illustrated, so I pass to Mr. Kingsley's No. 3:

"I am tolerably familiar," he proceeds, "with the actual grinding and polishing of lenses and specula, and have produced by my own hand some by no means bad optical work, and I have copied no small amount of Turner's work, and I still look with awe at the combined delicacy and precision of his hand; *it beats optical work out of sight.* In optical work, as in refined drawing, the hand goes beyond the eye,* and one has to depend upon the feel; and when one has once learned what a delicate affair touch is, one gets a horror of all coarse work, and is ready to forgive any amount of feebleness sooner than that boldness which is akin to impudence. In optics the distinction is easily seen when the work is put to trial; but here too, as in drawing, it requires an educated eye to tell the difference when the work is only moderately bad; but with 'bold' work nothing can be seen but distortion and fog, and I heartily wish the same result would follow the same kind of handling in drawing; but here, the boldness cheats the unlearned by looking like the precision of the true man. It is very strange how much better our ears are than our eyes in this country: if an ignorant man were to be 'bold' with a violin, he would not get many admirers, though his boldness was far below that of ninety-nine out of a hundred drawings one sees."

The words which I have italicised in the above extract are those which were surprising to me. I knew that Turner's was as refined as any optical work, but had no idea of its going beyond it. Mr. Kingsley's word "awe," occurring just before, is, however, as I have often felt, precisely the right one. When once we begin at all to understand the work of any truly great executor, such as that of the three great Venetians (Tintoret, Titian, and Veronese), Coreggio, or Turner, the awe of it is something greater than can be felt from the most stupendous natural scenery. For the creation of such a system as a high human intelligence endowed with its ineffably perfect instruments of eye and hand, is a far more appalling manifestation of Infinite Power, than the making either of seas or mountains.

NEW THEOREMS AND TABLES FOR THE CALCULATION OF EARTH-WORK.

An interesting mathematical investigation of some problems in the calculation of earthwork, together with tables adapted to such calculations, has been recently communicated to the Pottsville (Pa.) Scientific Association,

* In case any of your readers should question the use, in drawing, of work too fine for the touches to be individually seen, I quote a sentence from my "Elements of Drawing." "*All* fine coloring, like fine drawing, is delicate; so delicate, that if at last you see the color you are putting on, you are putting on too much. You ought to feel a change wrought in the general tone by touches which are individually too pale to be seen."

by Mr. Warner. The communication presents a theoretical discussion, with practical illustrations of the method known to engineers, as the method of *transverse ground slopes*, which treats the surface of the ground as a plane, the position of which is given, in respect to the roadbed or formation level, by the centre height at each end of the work, and the transverse slope of the ground. The tables may also be used in calculating the solidity under a warped surface.

Mr. Warner's formulæ admit of various transformations and applications. We shall notice briefly some which pertain to those cases of the method of transverse slopes, wherein the cross section of the work is a quadrilateral figure, and the side slopes alike. To find the solidity, it is necessary to find an expression for the area of the cross section at any point, which is bounded by the traces of the two side planes, and the traces of the roadbed and ground surface. If the traces of the side planes be prolonged to meet, and the point of their intersection assumed as the pole to which the polar equations of the roadbed and ground-plane traces are referred, then the area sought may be derived from a known formula for the area included between two radii vectores and two lines given by their polar equations. This being done, the solidity between two given cross sections may be found. For this solidity Mr. Warner gives the following formula:

Let L denote the length of the work perpendicular to the parallel end sections, B the width of base, σ and γ the inclinations of the side and transverse slopes respectively to the base or horizon. Let also S denote the sum of the end heights measured from the intersection of the side slopes, and D the difference of these heights; then the solidity will be

$$\tfrac{1}{4}L\left(S^2-B^2\tan^2\sigma+B^2\tan^2\gamma+\frac{D^2}{3}\right)\frac{\tan\sigma}{\tan^2\sigma-\tan^2\gamma} \quad - - - - \quad 1$$

If B and D be both $=0$, the solid in question becomes a triangular prism whose end height is $\frac{1}{2}S$, and whose solidity is

$$\tfrac{1}{4}\frac{L\,S^2\tan\sigma}{\tan^2\sigma-\tan^2\gamma} \quad - - - - - - - - - - - - - - - - - - - \quad 2$$

If, in formula 1, the square root of the quantity within the parenthesis be denoted by $\overset{1}{S}$, the solidity expressed by that formula may be put under the

form $$\tfrac{1}{4}\frac{L\,\overset{1}{S}{}^2\tan\sigma}{\tan^2\sigma-\tan^2\gamma} \quad - - - - - - - - - - - - - - - - \quad 3$$

which, by its similarity with 2, shows that $\frac{1}{2}\overset{1}{S}$ is the end height of a triangular prism, whose solidity is equal to that of the work. We may also consider the solidity of the work as made up of the solidities of two or more prisms, with similar bases, and of equal length.

Hence it is evident that tables containing the solidities of such prisms may be employed in the computation of earthwork. Of this description were some of Mr. Warner's tables, adapted to several of the most usual side slopes. They may also be used by finding the whole content included between the side slopes and the surface, and deducting therefrom the redundant prism lying above or below the roadbed. This process has been partially developed by previous writers.

Other tables exhibited by Mr. Warner, based on the same general formulæ and adapted to logarithmic computation, were directly applicable to any one of thirteen different side slopes, combined with any one of forty different transverse slopes, and with end heights whose sum does not exceed 100 feet. Both sets of tables embrace directly all ordinary widths of roadbed, and are extended by proper formulæ to side hill work, or work of triangular cross sections.

The quantity $\overset{1}{S}$ of formula 3 may be found by construction, and then employed to enter the table of prisms. The operation and advantages of this process were shown by the solution of practical examples.

Mr. Warner further gave some rules for the computation by centre and side heights. This part of the subject, he said, had been more fully discussed than the method of transverse slopes,—several American writers having treated it with ability, especially Colonel Ellwood Morris.

Under such circumstances, it was prudent to say little concerning the value of one's own work. He would, however, remark, that he had exhibited nothing which did not appear to him as original, at least in its whole scope; and that, as far as his rules were applicable, he had given practical proof of their expedition and accuracy. In regard to the method of transverse slopes, he would observe, that he had as yet met with no other treatment of it as full and satisfactory to him as his own; but, independently of what belonged to the theory, the labor of computation had been great,—having engaged his attention, at intervals, for several years.

CHEMICAL SCIENCE.

THOUGHTS ON THE PROGRESS OF CHEMICAL SCIENCE.

THE following is an abstract of an address delivered by Sir John Herschel, on assuming the chair of the chemical section of the British Association, at its last meeting, 1858. After briefly alluding to the rapid progress of chemistry in his own time, he desired "to put in a word of reclamation against the system of notation into which chemists, who for the most part are not algebraists, have fallen, in expresing their atomic formulæ. These formulæ have been gradually taking on a character more and more repulsive to the algebraical eye. There is a principle which I think ought to be borne in mind in framing the conventional notations as well as nomenclatures of every science, at every new step in its progress, — viz., that as sciences do not stand alone, but exist in mutual relations to each other — as it is for their common interest that there should exist among them a system of free communication on their frontier points — the language they use and the signs they employ should be framed in such a way as at least not to contradict each other. As the atomic formulæ used by the chemist are not merely symbolic of the mode in which atoms are grouped, but are intended also to express numerical relations, indicative of the aggregate weights of the several atoms in each group, and the several groups in each compound, it is distressing to the algebraist to find that he cannot interpret a chemical formula (I mean in its numerical application) according to the received rules of arithmetical computation. In a paper which I published a long time ago on the Hyposulphites, I was particularly careful to use a mode of notation which, while perfectly clear in its chemical sense, and fully expressing the relations of the groupings I allude to, accommodated itself at the same time perfectly well to numerical computation — no symbol being in any case juxtaposed, or in any way inter-combined with one another, so as to violate the strict algebraical meaning of the formula. This system seemed for a while likely to be generally adopted; but it has been more and more departed from, and I think with a manifest corresponding departure from intelligibility. The time is, perhaps, not so very far distant, when, from a knowledge of the family to which a chemical element belongs, and its order in that family, we may be able to predict with confidence the system of groups into which it is capable of entering, and the part it will play in the combination. A great step in this direction seems to me to have been lately made by Prof. Cooke, of the United States (in a memoir which forms part of the 5th volume of the Memoirs of the American Academy of Arts and Sciences), to extend and carry out the classification of chemical elements into families of the kind I allude to, in a system of grouping, in which the first idea, or rather the first germ of the idea, may be traced to a remark made by M. Dumas, in one of his reports to this Association, and which is founded on the principle of arranging them in a series, in each of which the atomic weight of the elements it comprises are found among the terms of an arithmetical progression, the common difference of which in the several series are 3, 4, 5, 6, 8, and 9 times the atomic weight of hydrogen respectively. So arranged, they form six groups, which are fairly entitled to be considered natural families, each group having common properties in the highest degree characteristic; and what is more remarkable, the initial member in each group possessing in every case the characteristic property of the group in its most eminent degree, while the others exhibit that property in a less and less degree, according to their rank in the progression, or according to the increased numerical value of the atomic equivalent. Generally speaking, I am a little slow to give full credence to numerical generalizations of this sort, because we are apt to find their authors either taking some liberties with the numbers themselves, or demanding a wider margin of error in the application of their principles, than the precision of the experimental data renders it possible to accord; so that the result is more or less wanting in that close appliance to nature which makes all the difference between a loose analogy and a physical law. But in this instance it certainly does appear that the groups so arising not only do correspond remarkably well in their theoretical numbers with those which the best authorities assign to their elements, but that it really would be difficult to distinguish the elements themselves into more distinctly characteristic classes by a consideration of their qualities alone, without reference to their atomic numbers. When we find, for instance, that the principle affords us such family groups as oxygen, fluorine, chlorine, bromine, and iodine, self-arranged in that very order; or again, nitrogen, phosphorus, arsenic, antimony, or bismuth; when we find that it packs together in one group all the more active and soluble electro-positive elements, hydrogen, lithium, sodium, and potassium, and in another the more inert and less soluble ones, calcium, strontium, barium, and lead, and *that* without outraging any other system of relations, — it certainly does seem that we have here something very like a valid generalization: and I shall be very glad to learn, in the course of any discussions which may arise on such matters as may be brought before us in the regular conduct of our business, from those more competent to judge than myself, whether I have been forming an overweening estimate of the value and importance of such generalizations. I will only add on this point, in reference to what fell from our excellent President in his address to the assembled Association last night, that this kind of speculation, followed out, would seem to me likely to terminate in a point very far from that which would regard all the members of each of these family groups as allotropes of one fundamental one; inasmuch as the common difference of the several progressions which their atomic weights go to make up, are neither equal to, nor in all cases commensurate with, the first terms of these progressions. For instance, in the chlorine group, the first term being 8, the common difference is 9. Something very different from allotropism is surely suggested by such a relation. It would rather seem to point to a dilution of energy of one primary element by the super-addition of dose after dose of some other modifying element; and this the more strikingly, since we find oxygen standing at the head of very distinct groups having very striking correspondences in some respects, and very striking differences in others. But all these speculations take for granted a principle, with which I must confess I think chemists have allowed themselves to be far too easily satisfied, viz., that all the atomic numbers are multiples of that of hydrogen. Not until these numbers are determined with a precision approaching that of the elements of the planetary orbits, — a precision which can leave no possible question of a tenth or a hundredth of a per cent., and in the presence of which such errors as are at present regarded as tolerable in the atomic numbers of even the best determined elements shall be considered utterly inadmissible, — I think, can this question be settled; and when such gigantic consequences — so entire a system of nature is to be based on a principle — nothing short of such evidence ought, I think, to be held conclusive, however seductive the theory may appear. I do not think such precision unattainable, and I think I perceive a way in which it might be attained, but one that would involve an expenditure of time, labor, and money, such as no private individual could bestow on it. If the phenomena of chemistry are ever destined to be reduced under the dominion of mathematical analysis, it will, no doubt, be by a very circuitous and intricate route, and in which at present we see no glimpse of light. We should, therefore, be all the more carefully on the watch in making the most of those classes of facts which seem to place us, not indeed within view of daylight, but at what seems an opening that may possibly lead to it. Such are those in which the agency of light is concerned in modifying or subverting the ordinary affinities of material elements, those to which the name of actino-chemistry has been affixed. Hitherto the more attractive applications of photography have had too much the effect of distracting the attention from the purely chemical question which it raises; but the more we consider them in the abstract, the more strongly they force themselves on our notice: and I look forward to their occupying a much larger space in the domain of chemical inquiry than is the case at present. That light consists in the undulations of an ethereal medium, or at all events agrees better in the characters of its phenomena with such undulations, than with any other kind of motion which it has yet been possible to imagine, is a proposition on which I suppose the minds of physicists are pretty well made up. The recent researches of Prof. Thomson and Mr. Joule, moreover, have gone a great way towards bringing into vogue, if not yet fully unto acceptation, the doctrine of a more or less analogous conception of heat. When we consider now the marked influence which the different calorific states of bodies have on their affinities — the change of crystalline form effected in some by a change in temperature — the allotropic states taken on by some on exposure to heat — or the heat given out by others on their restoration from the allotropic to the ordinary form (for though I am aware that Mr. Gore considers his electro-deposited antimony to be a compound, I cannot help fancying that at all events the state in which the antimony exists in it is an allotropic one), — when, I say, we consider these facts in which heat is concerned, and compare them with the facts of photography, and with the ozonization of oxygen by the chemical rays of the electric spark, and with the striking alterations in the chemical habitudes of bodies pointed out by Draper, Hunt, and Becquerel; and when, again, we find these carried so far that, as in the experiments of Bunsen and Roscoe, we find the amount of chemical action numerically measuring the quantity of light absorbed — it seems hardly possible not to indulge a hope that the pursuit of these strange phenomena may by degrees conduct us to a mechanical theory of chemical action itself. Even should this hope remain unrealized, the field itself is too wide to remain unexplored; and, to say nothing of discovery, the use of photography merely as a chemical test may prove very valuable, as I have myself quite recently experienced, in the evidence it has afforded me of the presence in certain solutions of a peculiar metal having many of the characters of arsenic, but differing from it in others, and strikingly contrasted with it in its powerful photographic qualities, which are of singular intensity, surpassing iodine, and almost equalling bromine. There is another class of phenomena which, though usually considered as belonging peculiarly to the domain of general physics, and so out of our department, seems to me to want some attention in a chemical point of view. It is that of capillary attraction. The coëfficient of capillarity differs very remarkably in different liquids, and no doubt also in their contacts with different solids, a fact which can hardly be separated from the idea of some community of nature between the capillary force and those of elective attraction. I hardly dare to hint at the existence of some slight misgiving I have always felt as to the validity of the received statical theory of capillary action, which carries with it the authority of such names as those of Laplace and Poisson. Any discussion of this point would be matter for another Section of this Association; and if I here touch upon it, it is only to observe, that my impression of the requisiteness of a force *so far allied to chemical affinity as to be capable of saturation*, rests on other grounds besides that of the mere diversity of action above alluded to. But I must remember that you are not met here to listen to generalities, of whatever nature, but that we have plenty of real and special business before us.

EQUIVALENTS OF THE ELEMENTS.

The following is a *résumé* of results of recent researches on the above subject, recently communicated to the French Academy, by M. Dumas:

Among the simple bodies or elements studied by him, twenty-two have equivalents that are multiples of hydrogen by a whole number, viz.:

O	8	N	14	Bi	214	C	6	Li	7	Cd	56
S	16	Ph	31	Fl	19	Si	14	Na	23	Sn	59
Se	40	As	75	Br	80	Mo	48	Ca	20		
Te	64	Sb	122	I	127	Au	92	Fe	28		

Seven have equivalents which are multiples of *half* an equivalent of hydrogen.

Cl	35·5	Mn	27·5	Ni	29·5	Pb	103·5
Mg	12·5	Ba	68·5	Co	29·5		

Three have equivalents which are multiples of *one-fourth* of an equivalent of hydrogen.

Al	13·75	Sr	43·75	Zn	32·75

Among the comparisons that may be made there are the following:

N	14	Ph	31	As	75	Sb	122
Fl	19	Cl	35·5	Br	80	I	127

It is seen that on adding 108 to the number for nitrogen we have the number for antimony $(14 + 108 = 122)$; and adding it to the number for fluorine, we have the number for iodine $(19 + 108 = 127$.

So also on adding sixty-one to the equivalent of nitrogen we obtain that of arsenic ($14+61=75$); and the same to that of fluorine, gives that of bromine ($19+61=80$).

In a word, if the numbers be arranged on two parallel lines, the ordinates of one series lengthened by five, become the ordinates of the other, with the single exception of phosphorus and chlorine, which are separated by 4·5 instead of 5. These facts teach the propriety of arranging the metals in series, that shall show a double parallelism; for such a classification brings out to view the various analogies existing among them. In fact, when arranged by natural families, each of the elements is in proximity to two others, belonging to two related families; and these related families occupy the two lines next to that containing the metal selected for comparison. Finally, each metal is surrounded in such a table by four others, which are united to it by analogies of different kinds and more or less close. — *Correspondence of Silliman's Journal.*

From a report of Dumas' paper in the *Comptes Rendus,* the following additional details are derived.

In order to exhibit the numerical relations between the equivalents of the different elements, the author, after referring to the previous investigations of Professsor Cooke, takes up, in the first place, the examination of certain groups and series presented by organic chemistry. If we consider the homologous series C_2H_3, C_4H_5, C_6H_7, etc., we remark at once that there is a common point of departure for and a common difference between the equivalents of the successive terms. The formula $a+nd$ represents the generation of all these radicals, a being the equivalent of the first, and d the difference between the first and second term. The author remarks, that if we did not know the law of progression, we might easily be led to think that the ratio between the numbers 141 and 281, 127 and 253, 113 and 225, is the simple ratio of 1 : 2, especially as chemistry can hardly decide with absolute certainty whether an element has, for example, the equivalent 225 or 226. The formula deduced from the simple progression above mentioned would not account for the generation of the elements, as Professor Cooke supposed. But the organic radicals are not always produced by addition, but sometimes by substitution, as we see in the compound ammoniums. We may have, for instance, the following ammoniums:

$$\begin{array}{llll}
a+d & a+2d & a+3d & a+4d \\
a+d' & a+d+d' & a+2d+d' & a+3d+d' \\
 & a+2d' & a+d+2d' & a+2d+2d' \\
 & & a+3d' & a+d+3d'
\end{array}$$

$$a+d+d'+d'+d''',$$

where a represents ammonia NH_4, and d, d', etc. represent the equivalent of hydrocarbons of the series $C_n H^n$.

In the next place, there are certain radicals in organic chemistry where the fundamental molecule itself changes, as well as the bodies added to or substituted in it. Thus, tin and ethyl form six molecular groups, possessing all the properties of organic radicals. If we represent tin by a, and ethyl by d', we have for the six species of stannethyl the formulas

$$\begin{array}{lll}
a+d' & 2a+d' & 4a+d' \\
 & 2a+3d' & 4a+3d' \\
 & & 4a+5d'
\end{array}$$

($na+nd'$) being the general formula. With these premises the author proceeds to compare the equivalents of the elements. The elements F, Cl, Br, I, do not form a single progression. The relation between their equivalents is, however, exhibited by the scheme a, $a+d$, $a+2d+d'$, $2a+2d+2d'$, or in numbers,

Fluorine,	19
Chlorine,	$19+16{\cdot}5=35{\cdot}5$
Bromine,	$19+33+28=80$
Iodine,	$38+33+56=127$.

Nitrogen, phosphorus, arsenic, antimony and bismuth, form another natural group; and for their equivalents we have the scheme, a, $a+d$, $a+d+d$, $a+d'+2d'$, and $a+d+4d'$, or in numbers,

Nitrogen,	14
Phosphorus,	$14+17=31$
Arsenic,	$14+17+44=75$
Antimony,	$14+17+88=119$
Bismuth,	$14+17+176=207$.

The author gives similar series for carbon, boron, silicon, and zirconium, as well as for tin, titanium and tantalum, which we omit. For oxygen, sulphur, selenium and tellurium, we have either of the series a, $2a$, $5a$, $8a$, or a, $a+d$, $a+4d$, $a+7d$. Analogy points out the latter as preferable, and we have in numbers,

Oxygen,	8
Sulphur,	$8+8=16$
Selenium,	$8+32=40$
Tellurium,	$8+56=64$.

A common difference of 8 also connects Mg, Ca, Si, Ba, Pb; thus we have

Magnesium,	12
Calcium,	$12+8=20$
Strontium,	$12+32=44$
Barium,	$12+56=68$
Lead,	$24+80=104$.

Lithium, sodium and potassium, belong to a similar series, with a common difference of 16.

Lithium,	7
Sodium,	$7+16=23$
Potassium,	$7+32=39$.

Molybdenum, tungsten, chromium, and vanadium form a similar series, of which the common difference is 22; the progression being 26, 48, 70, 92. The author considers his results as favorable to the idea of Dr. Prout, who supposed the equivalents of all the elements multiples, by a whole number, of that of hydrogen. In the case, however, of chlorine, and perhaps of some other elements, the unit of reference is less than the equivalent of hydrogen, and is probably 0·5. In all the series the first member determines the chemical character of all the other terms. These considerations, the author remarks, will have more weight when he presents the study of a natural family of which hydrogen is the first term, and exhibits the connection between the physical properties of them and the position which each occupies in the series of which it forms a member.

ON THE PRODUCTION OF ORGANIC BODIES WITHOUT THE AGENCY OF VITALITY.

The following is an abstract of a lecture on this most interesting subject, recently read before the Royal Institution of Great Britain, by Prof. E. Frankland:

The earlier researches of chemists brought them into contact with two classes of bodies, distinguished from each other by well-marked and obvious peculiarities. One of them was met with in the inanimate or mineral kingdom, the various materials of which were distinguished by their comparative stability or resistance to change, and by the facility with which the greater number of them could be artificially produced from the elementary bodies composing them. The other class of bodies was found exclusively in the animate portion of creation, or was directly derived from the productions of the organs of plants and of animals; these compounds were distinguished by their proneness to undergo change, and by the impossibility of producing them by artificial means. By no processes then known to chemists could the elements composing these latter bodies be made to unite so as to produce compounds, either identical with or analogous to the substances generated by the organs of plants and of animals. These substances were consequently, from their origin, termed *organic bodies*, or *organic compounds*. They were regarded as dependent for their origin upon the influence of that aggregate of conditions sometimes called the *vital force*; and it was generally believed that we should never succeed in producing these bodies artificially, until we could form and endow with vitality the organs from which they were derived. Such was the state of knowledge and opinion until the year 1828, when Wöhler succeeded in artificially producing *urea*, a body which had, up to that time, been known only as a product of the animal organism.* This discovery was followed, many years later, by the artificial formation of acetic acid, which was produced by Kolbe from a mixture of protochloride of carbon, water, and chlorine exposed to sunlight; the chloracetic acid thus obtained being afterwards converted into acetic acid by an amalgam of potassium. The subsequent production of methyl, by the same chemist, from acetic acid, added one of the organic radicals to the list of compounds producible from their elements. Although little further progress was made for several years in this department of chemical research, yet the artificial production of urea and acetic acid, together with their derivatives, completely broke down the barrier between so-called "organic" and "inorganic" bodies; and although the name "organic" was still retained for the class of bodies to which it had previously been assigned, it was now obviously no longer strictly applicable. The recent ingenious researches of M. Berthelot have greatly extended this branch of chemical inquiry, and have in a most important degree increased the number of bodies capable of artificial formation. The production of chloride of methyl and the members of the olefiant gas family up to amylene ($C_{10}H_{10}$) furnish us with the whole series of alcohols and their derivatives, from amylic alcohol downwards. Phenylic alcohol and napthaline, both artificially produced by Berthelot, yield a host of interesting bodies; whilst phenylcarbamic acid enables us to step from the phenylic to the salicylic group, since, when treated with hyponitrous

* The artificial formation of urea from cyanate of ammonia was exhibited under the influence of polarized electric light.

acid, it yields salicylic acid. Lastly, M. Berthelot has succeeded in artificially forming glycerine, the basis of animal and vegetable oils and fats, and also in forming grape sugar; the latter, however, is obtained by the contact of glycerine with putrifying animal matter, and consequently cannot be said to be produced altogether without the agency of vitality, — although the putrifying organic matter contributes none of its constituents to the new compound, and does not undergo any appreciable change in weight or appearance during the process. These substances yield such a numerous class of derivatives that upwards of 700 distinct organic compounds can now be produced from their elements without the agency of vitality. The processes employed for the artificial production of these bodies, though deeply interesting, present, with one or two exceptions, little or no analogy to the natural mode by which organic compounds are formed in the tissues of plants; but the speaker endeavored to show that a close attention to the nature of the inorganic materials assimilated by the vegetable kingdom, and their relations to the more important organic compounds derived from plants, leads to the belief, that such compounds can be successfully produced by processes strictly analogous to those employed by nature. He contended that the constitution of the so-called organo-metallic bodies, in which the production of complex organic compounds from inorganic ones, by the replacement of elements by organic groups, can be so clearly traced, afforded a valuable clue to the formation of organic bodies in general, and led directly to the conclusion, that, if the organic compounds of the metals be formed upon the model of the oxides of the respective metals, the organic compounds of carbon (that is, all organic compounds) are formed upon the model of the oxides of carbon. It has long been known that, with slight and unimportant exceptions, the only materials employed by nature in the construction of the most complex organic compounds, are carbonic acid, water, ammonia, and nitric acid. The fact that a vast number of organic compounds are cast in the molecular mould of water, has been proved by the ingenious researches of Williamson and Gerhardt; whilst the wonderful fertility of the ammonia model has been amply demonstrated by the labors of Hofman and Wurtz. It would also not be difficult to prove the claim of nitric acid to be considered as a third model, upon which a number of other organic compounds are built up; but it was necessary to confine attention on the present occasion to the consideration of carbonic acid only, as a model upon which a very large number of organic bodies are formed. Guided by the constitution above referred to, of the organo-metallic bodies, and bearing in mind the replacability of the oxygen in water and binoxide of nitrogen, and the chlorine in terchloride of phosphorus, by organic radicals, Professor Kolbe and the speaker were led to the following hypothesis regarding the constitution of several important classes of organic compounds.

1. The replacement of one atom of oxygen in carbonic acid, by hydrogen or its homologues, produces an organic acid, either of the fatty or of the aromatic series, thus:

Carbonic Acid.	Acetic Acid.	Benzoic Acid.
$C_2 \left\{ \begin{matrix} O \\ O \\ O \\ O \end{matrix} \right.$	$C_2 \left\{ \begin{matrix} (C_2 H_3) \\ O \\ O \\ O \end{matrix} \right.$	$C_2 \left\{ \begin{matrix} (C_{12} H_5) \\ O \\ O \\ O \end{matrix} \right.$

2. The like replacement of two atoms of oxygen in carbonic acid, produces either a ketone or an aldehyde, thus:

Carbonic Acid	Acetone.	Aldehyde.	Oil of Bitter Almonds.
$C_2 \left\{ \begin{array}{c} O \\ O \\ O \\ O \end{array} \right.$	$C_2 \left\{ \begin{array}{c} (C_2 H_3) \\ (C_2 H_3) \\ O \\ O \end{array} \right.$	$C_2 \left\{ \begin{array}{c} C_2 H_3 \\ H \\ O \\ O \end{array} \right.$	$C_2 \left\{ \begin{array}{c} C_{12} H_5 \\ H \\ O \\ O \end{array} \right.$

3. The like replacement of three atoms of oxygen in carbonic acid produces an ether, thus:

Carbonic Acid	Vinic Ether.
$C_2 \left\{ \begin{array}{c} O \\ O \\ O \\ O \end{array} \right.$	$C_2 \left\{ \begin{array}{c} C_2 H_3 \\ H \\ H \\ O \end{array} \right.$

4. The like replacement of all the atoms of oxygen in carbonic acid produces a radical, a hydride of a radical, or a double radical, thus:

Carbonic Acid.	Ethyl.	Hydride of Methyl. (Fire damp.)	Methyl-thyl.
$C_2 \left\{ \begin{array}{c} O \\ O \\ O \\ O \end{array} \right.$	$C_2 \left\{ \begin{array}{c} C_2 H_3 \\ H \\ H \\ C_4 H_5 \end{array} \right.$	$C_2 \left\{ \begin{array}{c} H \\ H \\ H \\ H \end{array} \right.$	$C_2 \left\{ \begin{array}{c} C_2 H_3 \\ H \\ H \\ C_2 H_3 \end{array} \right.$

The authors of this hypothesis now attempted to verify it by direct experiment. They endeavored to avail themselves of the powerful affinities of zincethyl, in order to effect the substitution of oxygen in carbonic acid, and sulphur in bisulphide of carbon, by ethyl; these attempts were, however, at best only partially successful; the reägent, the zincethyl, was not sufficiently powerful to rival the action of plants in the decomposition of carbonic acid; and its effects upon bisulphide of carbon resulted in the production of a number of organic bodies containing sulphur; and although one of these appeared to have the formula of sulphropropionic acid ($C_6 H_5 S_3 +$ $H S$), yet its complete separation and purification presented such difficulties that it would have been hazardous to rely upon it as a proof of the correctness of their hypothesis. In short, the verification of these views was not permitted to their authors, but was reserved for Mr. Wanklyn, who, in his newly-discovered sodium and potassium compounds of the organic radicals, came into possession of reägents, which enabled him at once to effect the desired substitutions. His memoir on the production of propionic acid by the action of sodium-ethyl upon carbonic acid,* which has just been communicated to the Chemical Society, proves the first proposition of a hypothesis, which considerably simplifies our views of the molecular structure of organic bodies, and which, if proved to be throughout correct, cannot fail to enable us greatly to increase the number of organic compounds capable of being procured from their elements without the intervention of vitality. The speaker then referred to the following list of important organic bodies, selected from the large number above spoken of, as being capable of artificial formation from their elements:

Name.	Formula.
Oxalic Acid..	$(C_2 O_3, H\ O)_2$
Hydrocyanic Acid..	$C_2 N, H$

* This conversion of carbonic acid into propionic acid was experimentally demonstrated, and the remarkable properties of sodium-ethyl and potassium-ethyl were also exhibited.

Name.	Formula.
Light Carburetted Hydrogen	$C_2 H_4$
Urea	$C_2 N_2 H_4 O_2$
Formic Acid (Acid of Ants).	$C_2 H O_3, H O$.
Chloroform	$C_2 H Cl_3$.
Acetic Acid	$C_4 H_3O_3, H O$.
Alcohol	$C_4 H_5O, H O$.
Ether	$(C_4 H_5 O)_2$.
Olefiant Gas	$C_4 H_4$.
Acetic Ether	$C_4 H_5 O, C_4 H_3 O_3$.
Oil of Garlic	$(C_6 H_5 S)_2$.
Oil of Mustard	$C_6 H_5 S, C_2 N S$.
Glycerine	$C_6 H_8 O_6$.
Butyric Acid	$C_8 H_7 O_3, H O$.
Pine Apple flavor (Butyric Ether)	$C_8 H_7 O_3, C_4 H_5 O$.
Succinic Acid	$C_8 H_4 O_6, 2H O$.
Valerianic Acid	$C_{10} H_9 O_3, H O$.
Pear flavor (Acetate of Amyl)	$C_4 H_3 O_3, C_{10} O_{11} O$.
Apple flavor (Valerianate of Amyl)	$C_{10} H_9 O_3, C_{10} H_{11} O$.
Lactic Acid	$C_{12} H_{12} O_{12}$.
Grape Sugar?	$C_{12} H_{12} O_{12}$.
Caproic Acid	$C_{12} H_{11} O_3, H O$.
Benzole	$C_{12} H_6$.
Nitrobenzole	$C_{12} H_5 N O_4$.
Aniline	$N (C_{12} H_5) H_2$.
Phenyl Alcohol (Creosote)	$C_{12} H_5 O, H O$.
Picric Acid	$C_{12} H_2 (N O_4)_3 O, H O$.
Salicylic Acid	$C_{14}, H_5 O_5, H O$.
Salicylate of Methyl (Oil of Wintergreen)	$C_{14} H_5 O_5, C^2 H^3 O$.
Napthaline	$C_{20} H_8$.

The artificial formation of urea, lactic acid, and caproic acid, is interesting in connection with certain functions of the animal economy. Pine-apple oil, pear oil, and apple oil, are instances of the artificial production of the delicate flavors of fruit, whilst oil of wintergreen and nitrobenzole are like examples of the formation of esteemed perfumes. But of all the bodies hitherto thus produced, alcohol, glycerine, and sugar, are undoubtedly the most deeply interesting, owing to the part they take in the nutrition of animals; they prove to us the possibility of producing, without vegetation or any vital intervention, an important part of the food of man. Should the chemist also succeed in forming artificially the nitrogenous constituents of food, without which life cannot be maintained, it would then be possible for a man, placed upon a barren rock, and furnished with the necessary apparatus and inorganic materials, to support life entirely without either animal or vegetable food. No one of these nitrogenous constituents has however yet been artificially produced, and the absence of all clue to their rational constitution forms at present a formidable barrier to their non-vital formation. It would be difficult to conclude a subject like the present without any notice of the considerations which naturally suggest themselves, regarding the possibility of economically replacing natural processes by artificial ones in the formation of organic compounds. At present, the possibility of doing this only attains to probability in the case of rare and exceptional products of animal and vegetable life. Thus valerianic acid, which, for a long time was extracted from the root of the *Valeriana officinalis,* could now probably be more cheaply prepared from its elements; but a still cheaper source of this acid has been in the meantime discovered, viz., the oxidation of amylic alcohol, a waste product formed in the manufacture of spirit of wine, and obtainable at such a moderate cost as to prevent, in an economical point of view, the successful production either of amylic alcohol or valerianic acid by any artificial and exclusively non-vital processes at present known. It is also highly probable, that if we could produce artificially such bodies as quinine and the rare alkaloids, or alizarine and similar powerful and valuable organic coloring matters, we should be able to compete with organic life in the formation of these bodies; nevertheless, the discovery of the processes of artificial formation would doubtless be preceded by a knowledge of methods, by which such rare bodies could be produced from more abundant, and consequently cheaper, forms of vegetable or animal matter; and it is therefore exceedingly improbable that any purely non-vital process will be successfully, and at the same time economically employed for the manufacture even of such rare and valuable vital products. Such being the economical bearings of the case with regard to the rare and exceptional educts of vitality, when we turn to consider the great staple products of the animal and vegetable kingdoms, the hope of rivalling natural processes becomes faint indeed. By no processes at present known could we produce sugar, glycerine, or alcohol from their elements, at one hundred times their present cost, as obtained through the agency of vitality. But, although our present prospects of rivalling vital processes in the economical production of staple organic compounds, such as those constituting the food of man, are so exceedingly slight, yet it would be rash to pronounce their ultimate realization impossible. It must be remembered that this branch of chemistry is as yet in its merest infancy, and that it has hitherto attracted the attention of few minds; and further, that many analogous substitutions of artificial for natural processes have been achieved. Thus, under certain circumstances, we find it less economical to propel our ships by the force of the wind, and our carriages by animal power, than to employ steam power for these purposes. We do not find it desirable to wait for the bleaching of our calicoes by the sun's rays; and even the grinding of corn is no longer entirely confined to wind and water power. In such cases, where contemporaneous natural agencies have been superseded, we have almost invariably drawn upon that grand store of force collected by the plants of bygone ages, and conserved in our coal fields. It is the solar heat of a past epoch that furnishes the power which we now utilize in our steam-engines. One important element in cheap production is *time*, and it is precisely in regard to this element, that we economically supersede, in the above instances, the contemporary resources of Nature. Now time is also an important element in the natural production of food; and although it is true that the amount of labor required for the production of a given weight of food is not considerable, yet it is nevertheless true that this weight requires a whole year for its production. By the vital process of producing food we can only have one harvest in each year. But if we were able to form that food from its elements without vital agency, there would be nothing to prevent us from obtaining a harvest every week; and thus we might, in the production of food, supersede the present vital agencies of nature, as we have already done in other cases, by laying under contribution the accumulated forces of past ages, which would thus enable us to obtain in a small manufactory, and in a few days, effects which can be realized from present natural agencies, only when they are exerted upon vast areas of land, and through considerable periods of time.

GRIFFIN'S THEORY OF CHEMICAL RADICALS.

Mr. Griffin, the well-known chemical writer of England, has published, during the past year, a new chemical theory of radicals, upon the elaboration of which, he states, he has spent thirty years of his life. The following is a general abstract of its principal points.

"Every salt is composed of two radicals, simple and compound, oxidized or not oxidized.

"Every element can act as a radical except oxygen. Oxygen never acts as a simple radical, nor forms part of a compound radical. Some of the metallic elements form two radicals, which differ in weight and properties.

"Compound radicals consist of (1) carbon and hydrogen, or (2) carbon and nitrogen, or (3) combinations of other elements with the foregoing.

"The quantity of an element which constitutes a radical is an atom, or as much as forms a single volume of gas.

"The quantity of a compound which constitutes a radical is as much as forms a single volume of gas. When the compound is not gaseous, the radical quantity is as much as is equivalent to a single volume of hydrogen or chlorine.

"Every gaseous salt measures two volumes, which is the measure of its two radicals. When salts contain oxygen, that element adds to their weight, but not to the measure of their gas.

"Though a compound radical, that measures one volume in the state of gas, still measures one volume when combined with one or more atoms of oxygen, the oxygen is not to be considered as a constituent part of the radical, but only as an addition to it.

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"Since all gaseous salts that contain two radicals form two volumes of gas, whether the radicals are simple or compound, oxidized or not oxidized, it is assumed that every compound radical, if isolated and brought into the gaseous state, would measure one volume.

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"Salts combine with one another, so as to form double, triple, quadruple, and other forms of compound salts."

ON THE DETERMINATION OF THE VALUE OF PRECIPITATES.

M. Mené, of France, has communicated to the Academy a new mode of determining the numerical value of precipitates in chemical analysis. The common practice, when proportional solutions cannot be made, is to drive off the water with which the precipitate is impregnated by calcination, sand-baths, or other contrivances which take much time. M. Mené simply washes his precipitate by decantation, and then introduces it into a graduated phial, which he afterwards weighs. The precipitate is then taken out, and the phial, filled with water to the same degree, is again weighed; when the difference of the two results gives the exact weight of the precipitate.

ARTIFICIAL PRODUCTION OF GEMS AND OTHER MINERALS.

The late M. Ebelman suggested the ingenious method of producing the gems of the corundum family (sapphire, ruby, hyacinth, etc.,) by fusing the alumina with an excess of boracic acid, and then suffering the solvent to evaporate gradually at a high heat constantly maintained. The process is doubtless applicable to all minerals which are formed by the union of their components at a high heat; but, although successful in practice, it gave only microscopic crystals, and the other occupations and subsequent death of the distinguished experimenter prevented him from prosecuting the subject. Prof. Sainte-Claire Deville, in conjunction with Captain Caron, has recently presented to the Academy of Sciences, at Paris, a new mode of operating, by which, it appears, fine crystals of practical size may be obtained. This method, which was probably suggested to the Professor during his investigations on aluminum, consists (when the Corundum minerals are sought) in establishing the reäction at a high heat between the fluoride of aluminum and boracic acid. The fluoride is introduced into a black-lead crucible, and above it is adjusted a small cupel containing the boracic acid; the crucible is then tightly covered and protected from the action of the air, and heated to a white heat for an hour. The two vapors decompose each other, giving rise to alumina (Corundum), and the fluoride of Boron. The crystals are rhombohedric with the faces of the regular hexagonal prism: they have but one axis, and are negative, and possess all the optical and crystallographic properties of natural Corundums, as well as their hardness. The crystals produced were sometimes more than a centimetre (0·4 inch) long, and very broad, but are wanting in thickness.

When the materials are used pure, the resulting crystals are of course colorless; but by adding a little of the fluoride of chromium to the fluoride of aluminum, the colored gems, the ruby, the sapphire, or oriental emerald may be produced, the colors depending solely upon the proportions of the chrome used, which, in all cases, must be very small (except in the green gem, M. Damour having detected twenty-five per cent. of oxide of chrome in ouvarowite). The colors produced are identical with those found in nature, and the gems retain their perfect transparency. In some cases rubies and sapphires were produced alongside of each other. The zircons and other analogous minerals were produced in a similar way. Chrysoberyl, with its characteristic crystallization, was produced by mixed fluorides of aluminum and glucinum treated as above; zahnite, from the fluorides of aluminum and zinc; staurotide by substituting silica for the boracic acid, or by heating alumina to a high temperature in a current of gaseous fluoride of silicium. But all the silicates thus prepared are very basic, containing a very small portion of silica. Rutile was obtained by the decomposition of a fusible titonate, especially titanate of protoxide of tin by silica.

"In making these experiments we often obtained in solution in the tin, a brilliant substance, crystallizing in large metallic plates, separable from the tin by hydro-chloric acid, which scarcely attacks them. This curious material is an alloy of equal number of equivalents of iron and tin. This appearance and chemical properties give it considerable interest."

These researches are valuable not only from their applicability to the arts, — in which the artificial production of the hard minerals will greatly add to our facilities, — but also by their important bearing on the theory of the formation of gems and the production of minerals in nature.

ON THE FUSION OF MOLYBDENUM.

Debray finds that pure metallic molybdenum completely withstands the temperature at which platinum, quartz, etc., become liquid. The metal melts in a crucible of carbon before the oxyhydrogen blowpipe at a temperature at which rhodium fuses; but the fused mass contains 4·5 per cent. of carbon. It has a silvery lustre, and scratches glass and topaz with ease. Tungsten appears to be even less fusible. — *Comptes Rendus*, xlvi, 1098

ON THE PREPARATION OF CALCIUM.

Liês Bodart and Gobin have communicated to the Academy of Sciences a note on the preparation of calcium, which is of much interest. The author found it impossible to obtain the metal by the action of sodium upon chlorid of calcium at a high temperature, but the reduction succeeds extremely well when the iodid is employed instead of the chlorid. The iodid of calcium was obtained by the action of iodhydric acid upon white marble, evaporating the solution and fusing the salt out of contact of air. As thus prepared it resembles chlorid of magnesium. Equal equivalents of sodium and iodid of calcium are to be mixed and gradually heated in a covered iron crucible to a strong red but not to a white-red heat. After an hour the crucible is to be removed from the fire and allowed to cool. In this manner the author obtained a button of calcium weighing about three grammes, by employing four grammes of sodium. The metal was pale yellow with a reddish reflection, and proved, on analysis, to be pure. The authors promise more ample details with respect to the properties of calcium, as well as a notice of their results in reducing barium and strontium by a similar process. — *Comptes Rendus*, xlvii, 23.

NEW FACTS RESPECTING ALUMINUM.

It has generally been stated that aluminum could resist the highest temperature without absorbing oxygen; but we now learn, that if the temperature be raised from a white to a welding heat, aluminum will burn with great intensity until a stratum of alumina is formed upon its surface sufficiently thick to exclude the atmosphere. As regards alloys, that made with iron is not malleable, but will crystallize. An alloy of 100 parts of aluminum and 3 of nickel is more fusible and harder than the pure metal. Bismuth forms with aluminum in the proportion of one to three, an alloy which is very fusible, but also very subject to oxidation when in a state of fusion. If two equivalents of aluminum and one of oxide of lead be exposed to a white heat, a violent detonation ensues, the crucible breaks into pieces, and even the doors of the furnace are driven to a distance. Similar effects occur with oxide of copper, or the sulphates of potash or soda. Aluminum is now much used for jewelry, especially bracelets, pins, and combs; in cabinet-making, it is excellent for inlaid work; its lightness renders it extremely convenient for pencil-holders, thimbles, seals, small statues, medallions, vases, and the like; for spectacles, as it does not blacken the skin like silver. But one of its most useful applications consists in using it for reflectors of gas-lamps, since it resists the effects of sulphurous emanations, which silver and brass do not.

Mr. Gerhard, of London, has recently patented a simple and economic process for obtaining the metal, whereby it is produced at a considerably less expense than by the means heretofore practised. In this process hydrogen gas combines in an oven with the fluoride of aluminum, and forms hydrofluoric acid, which acid is taken up by iron, and is thereby converted into fluoride of iron, whilst the resulting aluminum thus obtained remains in the metallic state in the bottom of trays containing the fluoride.

Dr. McAdam, in a communication to the British Association, 1858, stated that in striking medals from aluminum, he had noticed a peculiar gray appearance on their surface, which it was supposed arose from the uncleanness of the die. Close examination, however, showed that this was not the case. Some of these medals were subjected to the action of hydrochloric acid and nitric acid separately, without producing much effect on their surfaces. When some of them were put in a solution of caustic potash they were acted on very violently, hydrogen being evolved, and the surface of the metal becoming beautifully frosted. This phenomenon of an alkali comporting itself to a metal as acids do, was worthy the attention of chemists. After aluminum has been frosted in this manner, it does not become tarnished on exposure to the action of the air.

ON THE MANUFACTURE OF STEEL.

The following paper, read before the Society of Arts, London, by Mr. C. Binks, on the manufacture of steel, is one of the most practical and valuable contributions to science made during the past year. Although of great length, its importance seems to warrant a publication in the pages of the present volume:

The existing and generally received theory of the formation and the alleged actual composition of steel have ever appeared to have in them points that are not quite satisfactory. But it is probably owing to the fact that chemistry, throughout the whole range, is so replete with instances in which extraordinary effects or phenomena follow from insignificant causes, or from causes apparently inadequate to produce them, that this instance of the alleged composition of steel has been allowed hitherto to pass unquestioned generally.

The magical effects (as seen in its assumption of properties so singular and distinctive) of the addition to pure iron of some apparently insignificant proportion of carbon, is a conspicuous instance of this kind of chemical anomaly. That simple combination is, and has ever been alleged to be, the sole cause of the conversion of iron into steel. Carbon has been the only tangible or apparent element brought in contact with the iron in the act of its conversion; and after analysis of steel has detected in it, or assigned to it as essential, the existence of iron and carbon only. Therefore has this explanation ever been generally accepted without misgivings; and, solely to this simple combination, has ever been, and still is, attributed the conversion and consequent assumption by the iron, when it becomes steel, of properties so distinctive and peculiar. Still the broad distinctions that exist in their mechanical or physical properties between steel on the one hand and malleable iron, or cast-iron, on the other, would seem to leave room for great doubt that the cause of these distinctions is due solely to the absence in the one, and to presence in the other, of the element carbon, or to the merely minute differences in the relative proportions of that element that are found in steel and in cast-iron. Yet everywhere is this the received formula of the composition of steel: namely, that it consists solely of about ninety-nine parts of pure iron combined with one part of carbon; and any other matters that, in extremely minute proportions, analysis may have, from time to time, or occasionally, detected in it, have been considered as foreign and accidental only, and as being in no way essential to, but rather as interfering with, its true chemical composition and character. In this light, for example, have been looked upon the minute proportions of manganese found in some descriptions of steel, and also the appearance of nitrogen developed during analytical operations. The former (howsoever its presence may be considered to modify some mechanical property of the steel) has never been deemed essential to its chemical composition when the steel is in its normal or pure state; whilst the latter has ever been held (when recognized or detected at all) as the result of some merely mechanical adherence of that element to the metal, or to have been derived from the reägents present on analysis. The same reasoning has applied to some other (so-called) foreign or accidentally present matters; and steel has, consequently, ever since the doctrines of modern chemistry began to be applied in reasoning upon it, been looked upon as simply a compound of iron and of carbon, and as such, and such only, it would appear to be held to be even up to the present hour.

The same chemical doctrine of composition has always influenced, and still continues to influence, the selection of materials to be used as reägents in the formation of steel, or for the conversion of iron into steel. Hence, to effect this conversion, it has ever been deemed needful only to bring heated iron in contact with carbon, or with some carbon compound, in order that the iron shall take up the one per cent. or thereabouts, considered as essential to steel; and hence, also, the selection of charcoal as this reägent principally; and whenever other reägents may have been taken and used as aids or substitutes — leather shavings, for example — this selection also has always been made on the same general principle that it was the carbon alone that was to be absorbed by the metal. It will be seen, however, that, notwithstanding this guiding idea of the steel-makers, either accident alone, or some theory of the quality of the carbon in these specially selected materials, has undesignedly led to the employment of the very elements along with the carbon that the production or the chemical composition of steel demands, and which other elements existing theory would have either altogether rejected, or certainly never have especially sought for.

Bearing in mind the broad facts as seen in the distinctive physical properties of steel and of iron, and the unsatisfactory character of the carbon-percentage explanation of these remarkable distinctions, is it not possible that a careful examination of the daily operations used to produce steel may exhibit the existence of some other phenomena, or the action of some other elements playing as important a part, either in the operations or in the ultimate composition of the steel, as that hitherto supposed to be fulfilled by carbon alone?

Before proceeding with my examination of already known facts, under which iron is converted into steel, or steel converted into iron, or before instituting any special line of research for developing new facts upon which to reason as to the actual conditions and results, mechanical or chemical, of conversion, it is needful to define what steel really is, physically; that is, what distinguishing properties it possesses, which, taken independently of its chemical composition, shall constitute an easy and incontrovertible test; or, in other words, shall enable us clearly to distinguish steel proper from iron compounds, or alloys of iron with other metals, or mixtures of iron with non-metallic elements, which in some respects resemble but are not real steel.

Steel is contradistinguished from all other compounds by its capability of receiving different degrees of hardness, and a degree of hardness comparatively superior to any other metal; by its elasticity under certain kinds of treatment, its capability of receiving a fine and a peculiar polish, by its development of certain different colors under different degrees of heat, and by the permanency of the action upon it of induced magnetism. It is distinguished from pure iron by the complete absence in the latter of any one, or degrees of any one of the properties just enumerated. But there are compounds of iron that exhibit some, but not the whole of the special properties of steel. The outer coating of common cast-iron when "chilled," or when the casting has been made in sand, is often as hard and as untouchable by the file as the best tempered steel itself. There exist, also, alloys of iron (as of iron with manganese and other metals) such as those that were investigated by Stoddart and Faraday, that in the property of hardness alone are scarcely inferior to the finest steel. But in none of these special compounds are there associated the whole of the peculiar physical properties, the collection or series of which distinguish steel from any other substance. The peculiar effects in modifying its normal properties of an admixture with steel, or with pure iron, of phosphorus, sulphur, silicium, etc., are pretty well understood; but it is the varieties of steel, the results of admixtures with steel proper, of non-converted iron, in various proportions, that constitute the real difficulties of discrimination, and for these there exists no special test.

Place in the hands of an experienced steel-worker (a filemaker, a razor, a watch-spring, a needle, or a surgical-instrument maker, for example), a piece of rough, black, and unworked steel, — part of a bar of cast-steel, for instance, — and ask him what metal it is. He will not judge of it by its specific gravity, nor by application to it of nitric acid, or of any other chemical test, but will proceed most probably as follows: — He will balance it on his hand, and, tapping it with a hammer, will bring out his "ring," as he calls it, the peculiar intonation of which, when steel, as compared with the tone of iron, is, to his practised ear, a specific and infallible test of kind, and almost exactly of quality. He will next make it red hot, and try how it "draws;" that is, by repeated blows, will elongate the bar, watching as he proceeds the texture of the metal, its adhesiveness, its flexibility, its indisposition to scale, and the character of the marks inflicted upon it by his hammer. When it is good steel upon which he is working, the sharp-edged, well-defined impressions of his hammer's face (so finely developed, indeed, as to reproduce even the grindstone lines that are left on the face of a recently-ground hammer), but when it is with iron or bad steel that he is working, then the shapeless and ill-defined impressions that result give to his practised eye all the information he seeks for as to the real quality of the metal he is handling. Next he will try the temper of his forged specimen — heat it to some known degree, and, after dipping it in cold water, test its degree of hardness by the file. Still, further, in proof, he will next fracture the forged and tempered specimen, and, through its grain, find another evidence of its character. If the fracture be a clean one, close-grained, compact and silvery, it is steel; if ragged, fibrous, and leaden-hued, it is iron: or it will be one or other of these, with such intermediate gradations as correspond to all the differences in quality that lie between these two extremes. Next he may polish its surface, and, gradually heating the specimen, he will, as the temperature rises, watch that peculiar arrangement of colors, that, in their special brilliancy, are peculiar to real steel alone: the assumption, first, of various shades of yellow, deepening, as the heat increases, almost into brown, then successively into greenish blue, with pure blue, and into purple — upon which there follows another kind of change — a disruption of the constitution of the metal, to which is due those play of colors and the oxidation of the surface. Finally, as the *ne plus ultra* of his testings, he will proceed, probably, to forge out of his specimen a turning-tool, or preferably a cold-chisel, and then with the latter, cutting for awhile at a piece of cold cast-iron, will at once pronounce upon the kind and the quality of the metal, and the exact purposes to which it is best applicable. Now, from the nature of the case, the chemist must imitate or select from these practices of the handicraftsmen (for there exists no special test); and the file test, after tempering, together with the color test under high and different temperatures, affords sufficiently accurate tests for most of the purposes of the laboratory.

The searcher for information among the steel-makers and steel-workers will speedily find abundance of instructive and suggestive facts, the careful study of any one of which may possibly give him the clue he seeks for. Let him, for example, in the first instance, carefully examine the phenomena involved in the very old practice of using ferrocyanide of potassium as an agent of conversion. It is well known that the application of this compound to heated iron instantaneously converts that portion of the metal that is brought into actual contact with it into steel; and that under a continued contact, the entire mass, as well as merely the surface of any piece of iron, equally undergoes this transmutation. Thus, this agent is used to improve the quality of inferior steel, that is, more completely to effect the conversion of iron into steel; is also sometimes resorted to to renew or to restore the steel quality of steel tools — for example, of chisels, the repeated heatings and forgings of which have decomposed the steel externally, or to a greater or less depth reconverted it into common iron It is used more especially to case-harden iron; that is, to give to iron an external coating of steel, or to improve soft steel by its more complete or perfect conversion superficially. This ferrocyanide of potassium is a *carbon* compound, containing, in its anhydrous form, no oxygen; and, doubtless, it would be on some theory of its carbon-giving agency, that its application (could we possibly trace the origin of it) to iron was first made. But, besides carbon, it contains also iron, nitrogen, and potassium. Its formula is, K_2, Fe Cy_3 (or 3 NC_2).

Now, the specific action of this reägent, or the cause of its producing this singular effect — the instantaneous conversion, at the points of contact, of iron into steel — might, *à priori*, be held to be due to one or other of the following kinds of reäctions.

1. To the reduction of some portion of the carbon of the reägent, and its being taken up by the hot metal with the usual result of such a combination, as viewed on the old theory of what steel is.

2. To a deposition upon the surface of the hot metal of a thin film of the pure iron, combined in some peculiar proportion or manner with the pure carbon, both of which exist in this reägent itself.

3. To some peculiar action of the potassium present in the reäction; or,

4. To some peculiar action of the nitrogen of the reägent, or of that element and its associated carbon existing there in the form of cyanogen.

For merely preliminary trials or indications, let there be selected some ready method of determining which of these elements, or combination of them, plays this part of conversion; and for this purpose, let there be taken as the test the formation or the nonformation upon the surface of soft iron of a case-hardened surface, or of a superficial coating of steel as the result of an application to the iron of one or other of the following reägents; the relative hardness of the surface being determined by the file test; and after tempering by dipping the hot metal in water in the usual manner, and this, together with the color test,—that is, the development of the series of colors under different degrees of heat peculiar to steel,—being taken as the test of its formation.

Let the kind of iron that is selected be the best, and, commercially, the purest malleable iron, such as would be chosen for conversion into the best steel.

The manner in which the writer proceeded in these trials was as follows: Little bars of this iron were made red hot in a porcelain tube, and then the reägent washed over or sprinkled upon its clean surface, or the gaseous or volatile matter (when such was used) was passed through the tube holding the red-hot bars. When the charcoal experiment was made, freshly made and pulverized boxwood charcoal was selected, which was made red hot to expel all adhering azotized or other gaseous matter; then quickly transferred to the tube, the rod of iron imbedded in it, and the two ends of the tube closed. When, to this last arrangement, atmospheric air was added, the ends of the tube, placed horizontally, were left open, and the air, by diffusion, or by a quiet interfusion, found its way into and within the body of the charcoal, and, of course, into contact with the heated iron.

It is needless to point out that this line of experimenting is calculated to obtain, and aims at obtaining, only very broad indications of reäctions and effects; for the iron used is only approximately and not absolutely pure. But the indications of the special action of each reäction on its application to the iron, are so marked and distinctive, and develop themselves so broadly under the above system of testing, that this method of detecting the reactions, though not absolutely unqualified in its accuracy, is sufficiently tangible for its intended purpose, and lies within reach of every one. It will be seen how, in following up this investigation, for these comparatively rude methods there are substituted others aiming at greater precision. The temperature under which the following several reägents were applied to the iron was that of a full red heat, or that usually employed in case-hardening, or in the cementation process of conversion. Let any experimentalist proceed in this manner to apply to heated iron the following special reägents and he will find:

1. That heated iron exposed to the action of pure carbon, and kept out of reach of contact with any other element, is not converted into steel. A small rod of the malleable iron packed in boxwood charcoal in the closed porcelain tube, and kept at a full red heat for twelve hours, did not, after being tempered, show a hard steel surface; nor did it exhibit the play, under high and different degrees of heat, peculiar to real steel colors. It still remained malleable iron.

2. But that when atmospheric air is admitted to such an arrangement in such quantity only as still to keep the carbon in excess, then, in the first instance, the surface of the iron, and, finally (if the time of contact be long enough), the whole of the iron, is converted into steel.

3. That the application to the iron of gaseous nitrogen does not produce steel.

4. That neither does the application of carbonic oxide give steel.

5. That the application to the iron of a hydro-carbon (as when olefiant gas is passed through the tube, or when the red-hot rod is dipped into oil containing no nitrogen) does not produce steel.

6. But that the application of olefiant gas mixed with ammonia, or the application of gaseous cyanogen, produces steel, as does also the dipping of the hot metal into a nitrogenized oil or fat.

7. That the application of ferrocyanide of potassium (as has been so long known) gives steel.

8. And that equally with the ferrocyanide does the application of the simple cyanide of potassium result in the production of steel; therefore, it is not to the iron contained in the ferrocyanide that the steel-making property of the latter salt is due.

9. That potash applied to the hot iron, or keeping the hot iron in contact with the vapor of potassium, does not yield steel.

10. That with iron of the kind that has so far been referred to and used (*i. e.*, commercially pure wrought iron, containing no material proportion of carbon), the application to it of ammonia, or of nitrate of ammonia, fails to produce steel.

11. But that the application of ammonia, or its muriate, to iron containing a considerable proportion of carbon, results in its conversion into steel.

These results tabulated, and the composition of the reägents expressed in formulæ, will better exhibit the inevitable deductions to which they lead.

(1.)	$Fe + C$ (in excess), every other element excluded	Leaves iron.
(2.)	$Fe + C$ (in excess) + (atmospheric air)	Gives steel.
(3.)	$Fe + N$ (gaseous nitrogen)	Leaves iron.
(4.)	$Fe + C\,O$ (gaseous carbonic oxide)	Leaves iron.
(5.)	$Fe + H_4\,C_4$ (olefiant gas)	Leaves iron.
(6.)	$Fe + H_4\,C_4$ (in excess) — $N\,H_3$ (ammonia)	Gives steel.
(7.)	$Fe + N\,C_2$, (cyanogen)	Gives steel.
(8.)	$Fe + K_2$, $Fe\,Cy_3$ (ferrocyanide of potassium)	Gives steel.
(9.)	$Fe + K$, Cy (cyanide of potassium)	Gives steel.
(10.)	$Fe + KO$ (potash)	Leaves iron.
(11.)	$Fe + K$ (potassium)	Leaves iron.
(12.)	$Fe + NH_3$ (ammonia)	Leaves iron.
(13.)	$Fe + NH_3\,Cl$ (sal ammoniac)	Leaves iron.
(14.)	$\dfrac{Fe + C}{95 \quad 5} + NH_3$ (ammonia)	Gives steel.
(15.)	$\dfrac{Fe + C}{95 \quad 5} + NH_3$ (sal ammoniac)	Gives steel.

Now, out of a consideration of these preliminary and merely guiding trials, besides the other deductions they lead to, as those have been already stated, there is made apparent one significant fact, namely, the invariable coöperation, so far as these trials extend, of both nitrogen and of carbon in the production of steel; but these coöperating in some manner yet to be defined and ascertained. It still remains to be determined if this coöperation of nitrogen be a *necessity* in steel-making; or, if the apparent invariableness of its presence and coöperation will, on a more extended examination, be borne out by the evidence of every other process; and if so, is it that the nitrogen conjointly with the carbon, forms some combination with the iron, and remains there? Or, that the nitrogen acts merely as an intermediate agent, and that it still remains a chemical fact that steel is merely iron combined with carbon only, though nitrogen plays an essential part in effecting that combination.

But whatsoever may be the functions in steel-making that are exercised by nitrogen — if its office be functional at all, and its presence be not a mere coincidence in every case — the fact of its invariable coëxistence with carbon, wherever steel is produced, is incontrovertible.

We have it on the old ordinary cementation-boxes, which, filled with charcoal and the imbedded iron, are closed, but not hermetically sealed, and still sufficiently open to the inevitable permeation, through the excess of carbon, of atmospheric air, yielding, by its oxgen, carbonic oxide, and to the steel, nitrogen. We have it still more especially and obviously when, in this cementation process, there is superadded to the charcoal some horn or leather shavings, or animal charcoal, along, sometimes, with an alkali — a very old but not a generally used modification. We have it when the iron for conversion is exposed in close vessels to the action of coal-gas, but in which coal-gas, to a greater or less extent, there is always present either cyanogen or ammmonia, or both. We have it also on all the multifarious expedients of the steel-workers and steel tool-makers, resorted to to give increased hardness to the metal; that is, to effect its most complete conversion into steel; as, when the file-maker coats his file, before tempering it, with a composition of cow-dung or with pig-flour — two favorite specifics, and both highly azotized substances, which he thinks useful merely for protecting the sharp angles of his cuttings from the action of the fire, but which act, in reality, as well in more completely steelifying his finished work. We have it in the use, in so many cases, of horn shavings, of horn dust, of leather shavings, and of other animal, and consequently azotized, matters of various kinds, in the use of other vegetable substances (besides that just mentioned) containing large proportions of gluten, and consequently of nitrogen; in the use of the ammoniacal salts, to say nothing of the prussiates, the recognition of the potency and of the great value of which for steel-making in bulk, as well as for merely hardening it superficially, as heretofore, is now becoming general.

We have a conspicuous instance of the effect of the presence of this element in a well-known fact, that, whilst the dipping of the hot metal into olive-oil fails, the use of beef suet (an azotized fat) succeeds in giving to the iron a coating of steel.

It was the presence (but, to him, the unconscious one) of this same element that gave to the celebrated expedient of Mr. Heath its chief potency, in improving the quality of inferior steel, and not solely to any purifying or alloying action (if any) of his manganese; for latterly Mr. Heath used *coal-tar*, placed in contact with the steel, to reduce his manganese oxide; and this coal-tar is a highly nitrogenized as well as a carbonized compound. In short, in whatever practice the various and continual trials of the steel artisan may result, in his searchings after the best hardening agents (and he resorts to the most extraordinary things), that practice will be found invariably, when successful, to involve the employment of some material in which nitrogen is an essential element.

A review of the above facts and phenomena is provokingly suggestive that the existing theory of the composition of steel is a wrong one. And the first suspicion is that this nitrogen element does actually enter into and exist in that compound; and that not to errors in analysis, but to misconceptions when nitrogen was found in steel, or to the influence of preconceived notions, are to be attributed the fact of this element exercising any kind of agency in steel-making, having hitherto been overlooked; or that its presence, when found, has either been disputed, or attempted to be accounted for on other grounds than that of chemical combination.

The attention of the chemical world was first prominently called to the fact of the existence of nitrogen in iron and steel by Professor Schafhäutl, a translation of whose paper appears in the *Philosophical Magazine* for 1840. Now, about this period there had arisen some of those new methods in analysis for finding the quantity of nitrogen, that, added to subsequent discoveries in the same direction, have given so happy an impulse to analytical chemistry. The method of Dumas — of Schafhäutl himself — of Will and Warrentrap, and of Lassigne, were about this time brought into notice; and Schafhäutl, without appearing to have in his mind any theory as to the part played by nitrogen on the composition of steel as distinguished from iron, but knowing that nitrogen was ever present in the manufacture of iron, would appear to have tried his hand at its analytical detection. His results are given with a broadness and an absence of specific details that suggested a repetition of them by the next investigator, Professor R. Marchand, whose essay will be found in the *Chemical Gazette* for 1850; the latter chemist applying, with considerable ingenuity, the resources of a still more advanced chemistry in refutation of some of the results of the former. To give a still greater zest to the investigations, it had just been discovered by Wohler that those beautiful copper-colored cubic crystals, found in the slags of the blast-furnaces, which we had been accustomed to call titanium, were none other than a mixture of a cyanide and of a nitruet of that metal — a fact suggestive enough that in iron too there might, not improbably, be discovered some analogous combinations with nitrogen. Schafhäutl gives as his results that malleable cast-iron contains 0·532 of nitrogen, close-grained cast-iron 0·927, coarse-grained cast-iron 0·740, white pig-iron 1·200, and in Beinhauer's razors 0·532 of nitrogen. Marchand asserts that these proportions are too high, but admits the invariable presence of nitrogen in cast-iron, and its equally invariable absence in malleable iron. Speaking of that method under which the existence of the nitrogen is proved by the formation of Prussian blue, he says that "with steel powder (as compared with cast-iron) it was still more striking, and with soft iron it was never apparent." But it is to be observed that it is with cast-iron chiefly, and not with steel, that Marchand operated; still he detects in this cast-iron the invariable presence of nitrogen, and in pure iron its invariable absence. Neither of these chemists speculated as to the meaning or the effect of the presence of the nitrogen in steel; wherever, in the exercise of their manipulatory skill, it is found, the fact is left without comment or consideration. Had the comparisons been made with pure steel, as with pure iron, there can be no doubt that Marchand would have recognized those marked distinctions which it is the object of the writer to point out.

Now, suspecting less from such evidences or suggestions of these than from the facts to be observed on conversion (such as that of the singular influence of cyanogen compounds), the substantial and invariable existence of nitrogen in steel, the writer proceeded to arrive at that point as follows: The best malleable iron on the one hand, and by way of comparison with this, the same kind of iron fully converted by the usual process, were taken on trial; the steel was dissolved in very dilute and pure hydrochloric acid; and after many trials, it was found best to place the bar of steel or iron in single voltaic arrangement with platinum, and to effect the solution in the cold with the usual precaution of expelling air from the water employed. In this way, slowly, the steel was dissolved, and the carbonaceous flocculent matter that was left, collected, carefully dried, and analyzed. The iron was treated in the same manner, and the comparatively very small proportion of carbonaceous residue given by it also examined. And these were compared with the residue also obtained from cast-iron. If the acid be strong, and heat be used, and the voltaic arrangement be not used, the results are very different. Gaseous nitrogen, in very minute quantity, is given off along with the hydrogen, some muriate of ammonia is formed in the solution, and but little nitrogen left in the residue.

Effecting the combustion of each of these residues by aid of the soda lime process, in the usual manner, the following results were obtained: — 1. The residue of the malleable iron contained no nitrogen whatever. 2. That from the cast-iron always showed the presence of nitrogen, but in very minute and invariable quantities; an average of results would seem to confirm the analysis of Marchand. 3. In the steel residue there was invariably detected a considerable quantity of nitrogen.

The analysis of this carbonaceous residue gave — $C = 0{\cdot}63$, $N = 0{\cdot}24$; impurities, $0{\cdot}13 = 100$. The direct analysis of this sample of steel, using the soda lime process, gave, in 100 parts of steel, $C = 0{\cdot}68$, $N = 0{\cdot}19$; that is, that any 100 parts of steel contained about one-fifth per cent. of nitrogen associated with about three times its weight of carbon. The proportion of nitrogen in the residue was greater than in the steel itself — a result proved afterwards to be due to the absorption of nitrogen or formation of ammonia in the act of drying the residue. The direct analysis of the sample of iron gave no nitrogen whatever — that of the cast-iron only a trace. This steel contains, therefore, about one-fifth per cent. of nitrogen, and by other trials good steel always gives about this quantity, but inferior steel much less. It is obvious that the residue is an azotized carbon, out of which fact arises some important considerations. But confining these, for the present, to the cases of the malleable iron and the steel, it would appear that the difference in chemical composition between these two is not less remarkable than the difference between their respective physical properties; but in what precise manner these two elements produce these differences, or the form in which they exist together in the steel, we can only as yet conjecture theoretically.

When malleable iron is placed in a crucible along with some of this azotized carbon, the intractability as to fusion of the metal is soon overcome, and it melts at a white heat, and cast-steel is the product.

When there is prepared a mixed precipitate of oxide of iron and of oxide of manganese, and this is reduced to the metallic state by passing over it, at a high temperature, hydrogen gas — that is, in the usual manner, made into spongy iron, plus manganese; this, placed in a covered crucible, readily melts, and gives an exceedingly hard alloy, but one that does not, possess all the properties of real steel.

But when with spongy iron itself there is mixed some ferrocyanide of manganese, and then it is exposed to a full heat in a covered crucible, then the button that is produced has all the properties of steel.

The same follows on substituting ferrocyanide of iron, or any of the achydrous alkaline ferrocyanides.

The addition in the hot and closed crucible of bitartrate of potash to the pure iron does not give steel; but the addition of the double salt of tartrate of potash and ammonia (which is a kind of improvised mode of making cyanogen), gives a button of steel.

Another series of instructive facts, in proof and in illustration of these reäctions, is obtained through the use of the voltaic battery — to heat the iron whilst it is exposed to the action of the atmosphere of certain gaseous or volatile matters.

Let the malleable iron, upon which to operate for conversion, or proof of non-conversion, be drawn into rods of about one-fourth of an inch in diameter (at each end), but drawn out between these — that is, in the middle, into a thin wire tapering gradually towards the thin ends. Fix such a rod in the centre of a glass tube or globe, so shaped and contrived that gaseous matters can be passed into and through it; connect the obtruding thick ends of the rods with the opposite poles of a voltaic battery — powerful enough in its action to raise the thinner portion of the rod to a red or white heat *ad libitum*, whilst the thicker ends never become so hot as to interfere with the means used to keep them in their position. Fill the tube successively with the following gases, and watch the results:

Gaseous cyanogen, after a few hours, gives rise to the formation of steel in those portions of the rod that have been fully heated, and this is accompanied by a deposition of carbon upon the face of the metal.

Gaseous ammonia *per se* does not give steel, but produces a curious disintegration of the face of the hottest portion of the rod.

Olefiant gas *per se* does not give steel, but gives a deposition of carbon on the hotter portions of the wire. But olefiant gas mixed with ammonia or with nitrogen, does give steel, and so on.

But without further multiplying examples (and this mode of experimenting admits of a great variety), and without attempting at this time to inquire into some of the complicated phenomena they present, one result is ever apparent, namely, the invariable coöperation of both nitrogen and carbon wherever the result is the production of steel.

The conclusions that, to the writer, appear to be warranted by the previous evidences, are:

That the substances whose application to pure iron convert it into steel, all contain nitrogen and carbon, or nitrogen has access to the iron during the operation.

That carbon alone, added or applied to pure iron, does not convert it into steel.

That nitrogen alone, so added or applied, does not produce steel; but that —

It is essential that both nitrogen and carbon should be present, and that no case can be adduced of conversion in which both these elements are not present and in contact with the iron.

That nitrogen as well as carbon exists substantially in steel after its conversion; and such presence is the real cause of the distinctively physical properties of steel and of iron, in which latter these elements do not exist.

That presumptively, but not demonstratively, the form of combination is not that of cyanogen (though that compound plays an important part in conversion), but is that of a triple alloy of iron, carbon, and nitrogen.

But that experimental research is yet required to determine the relative proportion of elements when their union gives pure steel.

What in the chemical history of nitrogen is there that is incompatible with its substantial existence in steel — in some form analogous to other combinations we know it to assume under similar conditions with other metals? Is it under a temperature as high as that needed to melt steel that it combines with carbon to form cyanogen, and then with potassium to form cyanide of potassium, and under this combination it is permanent so long as kept out of contact with decomposing agents, as with oxygen and the elements of water, etc.

In the presence of our atmosphere, with its affluence in nitrogen, why should we ever ascribe to that element some merely negative attributes, or properties serving only to control or modify the more vivid action of some other element? Why dwell only on its ozotic action, or on its assumed mere modifying action among the phenomena of animal or of vegetable life? An element existing everywhere, touching everything, penetrating, permeating, and by diffusion intermingling itself with every gaseous body it comes in contact with, might be supposed *â priori* to exercise other functions (and many) besides the merely negative ones usually assigned to it. And, among other speculations that naturally arise out of these questions, is it quite impossible that the play of colors peculiar to heated steel, the assumption, for example, of the pure blue and the purple, may not, in reality, be due to some phase of development of some of the forms of ferrocyanide of iron?

We possess another evidence of the use of nitrogen, from another and unexpected quarter. It is on record, as a practice of the Indian "Wootz" steel-maker, that, along with his iron or imperfect steel in his melting crucible, he places, as his carbon-giving material, the wood of the *Cassia auriculata*, and covers the whole earth with the leaves of the *Convolvolus laurifolia*, both vegetable productions, rich in azotized matters. These, placed in his closed crucible, will give an azotized carbon in contact with the metal. And what may have been the origin of this far-back practice of the East — this, to us, apparently empirical handicraft of some Indian artificer? Has it originally been the fruition of some mere accident, or of some induction or deduction — or is it a relic of some state of civilization and of science superior to those of the West? The Sheffield artisan seeks, even up to the present day, that which the Indian artificer had found out ages ago.

But howsoever all this may be, whether the nitrogen exist as an essential constituent in steel, or its office be one of agency only, the practical applications for manufacturing purposes that flow out of the above collection of facts, are in no way affected by the tenability or the contrary of any theory of combination. The fact of the important part, in the conversion of iron into steel, that is played by nitrogen and carbon conjoined, and particularly, when in the form of cyanogen compounds, is incontestible, howsoever may be explained their mode of action.

It is the experience of the writer, in his examinations of iron that is deficient in malleability, that this deficiency is due as well, and even more frequently, to the presence in such iron of unreduced oxide disseminated throughout the mass, as to the presence and action of sulphur, phosphorus, and other matters to which this deficiency is most generally attributed. This fact seems also to have attracted the attention of Mr. Bessemer, who alludes to it in one of his recent specifications. Now, the reducing action in metallurgic operations of alkaline cyanogen compounds is well known, and hence is suggested the possibility of employing them as well to remove from impure iron the sulphur, phosphorus, and silicium, as to effect the complete reduction to the metallic state of any oxide of iron disseminated through the ores. But these alkaline cyanogen compounds, ferrocyanide of potassium for example, when added to molten, impure iron, whilst exercising extraordinary purifying effects, leave the metal finally in the condition of steel. Here, then, is another problem, how to take advantage of these peculiar reäctions, in order to produce, *ad libitum*, either steel or malleable iron; in other words, how best, after steel is produced, to effect its reconversion into iron.

It is impossible, within the limits of this paper, fully to discuss these reäctions, or even such, for example, as those between that admirable converting agent, the ferrocyanide of manganese and iron, or those between iron containing a large quantity of carbon, when such iron is converted into steel, on the application to it of muriate of ammonia.

The value of combinations of carbon and nitrogen in steel-making being acknowledged, then, of all such combinations or of elements containing these, it is undoubtedly to the use of the cyanogen compounds that we should resort for all manufacturing purposes; and the time seems not very far distant when these compounds will become some of the most readily obtained and cheapest of chemically-manufactured products.

The operations of the blast-furnace suggest methods for the production of those compounds that are of the highest practical value. There are at play here all the elements for the production of cyanogen of certain cyanides, and thence of other compounds, and the requisite conditions can be superadded for securing these for commercial purposes. That cyanogen was formed in certain zones of the furnace, was proved by Bunsen and Playfair. Dr. Clark, of Aberdeen, many years ago, examined a saline product that was found to ooze out of the tuyere-holes of a blast-furnace in Scotland, and discovered it to be cyanide of potassium. In several places on the Continent, as at Mariazoll, in Styria, for example, we are told by Gmelin, that this product is so abundant as to be sold, commercially, for galvanic gilding purposes. It is, of course, the product of cyanogen, when combined with the accumulated proportion of potash contained in the flux lime-stone. But why not specially add the alkaline element, and combine in the furnace simultaneously the peculiar reducing and converting actions of these compounds with their special manufacture for other and equally valuable industrial applications of them that are springing up? And this is undoubtedly one of the most important of the directions that the iron manufacture of this country will in future be found to take.

PREPARATION OF FERRUM REDUCTUM.

Hr. Zängerle recommends igniting a mixture of five parts protoxalate of iron with six parts anhydrous ferrocyanide of potassium and one and three-quarter parts anhydrous carbonate of potash. The ignition is maintained until the evolution of gas from the melted mass ceases. This, when cold, is thoroughly washed with pure water, and the residue dried. The product appears as a dark-gray powder, consisting of metallic iron, so finely divided that, when touched with a lighted match, the whole mass burns gradually.

NEW LIGHT ON THE BESSEMER PROCESS.

Since the public experiments, with the so-called Bessemer's process, on refining iron and producing steel, most practical iron manufacturers have believed that nothing of any value could come of it. All agreed that the product was not malleable iron — that it was red-short, and had no weld. The discovery, therefore, has taken the character of an interesting phenomenon, having little or no commercial value. It might have been regarded, perhaps, as the germ from which some really successful process was yet to spring, to revolutionize at once the iron manufacture of the world. It has been, however, all along suspected by many thinking men, that the inherent impurities of the ores operated upon, and of the fuel, were the cause of the want of success, which the philosophical and simple nature of the new process so fully promised. That the air-boiling effectually decarbonized the iron, and that to any required degree, was clearly established; and, according to all our knowledge of the constitution of iron, the product therefore should have been a malleable bloom, or at least, a steel ingot. If the product was neither of these, what was it? Calling it refined iron, did not answer the question; for, beyond the commercial application of that term, steel itself is merely iron in a certain stage of refinement.

While public attention was subsiding in Europe and the United States, and Mr. Bessemer was being left to disappointment, a Swedish iron-master, who had examined the process, ordered the requisite apparatus to be sent to his works at Edsken, and, after considerable delay in experimenting, has, within a recent period, succeeded in establishing the manufacture of good steel, on a practical scale, by the Bessemer process; and, in short, devotes his whole establishment to this one process. This steel has been made into engineers' tools, boiler-plates, and cutlery; and the improvement must now be regarded as an accomplished commercial fact, which can no longer admit of question of theoretical grounds.

Mr. Goransson, the Swedish iron-master in question, states that he has carried out Bessemer's invention to the fullest extent, without ever having had recourse to any one of the numerous plans which have been patented by others, under the idea of improving the original simple process. The converting vessel is erected near the tap-hole of the blast-furnace, so that about one ton of fluid pig-iron can be run into the apparatus at a time. The pressure of the blast is from seven to eight pounds to the square inch; and when continued for six or seven minutes, the whole charge is converted into steel. The fluid steel is discharged into a loam-lined ladle, where it is well stirred, and considerable carbonic oxide disengaged and inflamed. After a short interval of repose, which is probably necessary for the steel to condense from the aërated condition in which it leaves the converting vessel, it is run off from the bottom of the ladle, in a vertical stream, into the ingot moulds.

The whole time occupied, from the moment the fluid pig-iron leaves the furnace until it is cast into the mould, does not exceed twelve minutes. The loss in weight, including the impurities thrown off, does not exceed fifteen per cent., which is only about one-half the waste incurred in the manufacture of bar-iron by the old system in Sweden. By this improvement Mr. Goransson states, in a letter to the London *Engineer*, that more than one thousand tons annually of cast-steel can be made with the same quantity of fuel as is now required for making five hundred tons of bar-iron. He says: "So completely have we accomplished the object, that we now make several tons of large ingots of cast-steel, in succession, without a single mishap or failure of any kind. The steel can be made either hard, medium, or soft, at pleasure. It draws under the hammer perfectly sound and free from cracks or faults of any kind, and has the property of welding in a most remarkable degree."

ON SOME POINTS OF CHEMICAL INTEREST CONNECTED WITH THE BESSEMER PROCESS.

The following abstract of a communication, presented to the American Academy by Dr. A. A. Hayes, on the above subject, embraces many points of interest.

It is well known that Mr. Bessemer has based his improvements on the startling novelty of making crude iron nearly pure, without the aid of fire, from carbonaceous matter. In considering the ordinary mode of refining crude iron, the final operations being performed on crude pig, or on partially refined pig-iron, we have, as one of the conditions of success, the application of an intense heat, and the presence of more or less atmospheric oxygen, necessary to maintain the required degree of fluidity in the mass of iron, and to burn out the carbon and other impurities present. As the iron loses its carbon and other extraneous substances, it becomes less fusible, and the workman, stirring the mass as it begins to lose its fluidity, gathers into rough masses the aggregated particles, which are always spherical in general form. From the masses, which are very porous and unequal, a bar of regular form is obtained by the usual means of pressing, or hammering and rolling.

"There is in this process strictly a segregation of particles of pure iron from the crude mass, which, under the agitation of stirring, unite to form rounded aggregations; and the heat of the furnace being increased, the separation of pure iron continues, until the melted impurities alone remain. The change of crude to pure iron is accompanied by the *production* in part of the impurities which remain; they are not educts. Aside from the loss of carbon in the form of carbonic oxide, the phosphorus and sulphur, — which my experiments have proved are always united to the metallic bases of the earths or alkalis, — with these bases, burn into oxidized products; while the silicium and a portion of the iron, also oxidized, form the melted slag, or cinder, as an additional foreign matter. To the loss of impurities we must also add the weight of iron burned in forming secondary products; so that, if the operations were performed on crude iron containing ninety-two per cent. of pure iron no more than eighty-two per cent. of malleable iron will be obtained. By the method of Bessemer, crude iron in a melted state is exposed, in a nearly closed receptacle, to jets of air forced into and under the fluid; and it is alleged that such an excess of heat is produced in the process, 'that the metal continues to boil even after the blast has ceased.' The direct statement is made, that 'air, dividing into globules, and diffusing itself among the particles of fluid iron, and thus coming in contact at numerous points with the carbon contained in the crude iron, and producing thereby a vivid combustion,' and the same action is implied in other parts of Mr. Bessemer's patent-specification.

"Now, it is well known to chemists, that the combustion of the carbon of crude iron *cannot take place under the conditions*. This carbon exists in gray iron in the allotropic state of graphite, and is not combustible even alone, when exposed highly heated to a current of atmospheric air. We burn it in the laboratory by the application of oxygen in some condensed state only. The proper chemical explanation of this point is a very simple one. Iron, which is a highly combustible body at ordinary temperatures, has its attraction for oxygen enormously increased by the heat of fluidity, and in combining with this element, the heat disengaged is ample for carrying the temperature of the mass still higher. A portion of oxide of iron being formed, the mechanical motion imparted by the jets of air favors the contact of the oxide with the carbon, *which then burns with the condensed oxygen of the oxide of iron.* The products of this combustion, arising from the mingling of oxide of iron and graphitic carbon and pure graphite, are two, — pure iron, and carbonic oxide; the former uniting with the mass, the latter escaping as gas, and burning in the atmosphere, or even with any oxide of iron it meets with in the mass. A moment's consideration of the operation shows that the combustion of the iron at the first stage leads to the separation of the carbon as carbonic oxide, and a reduction of the oxide formed to pure iron. Silicium, phosphorus, cyanogen, and sulphur, the bases of the alkaline earths, and interposed slags, are oxidized, and removed as fusible compounds in the same way, *while the pure iron assumes the crystallized state.* The combustion of the iron raises the temperature of the acting bodies far above the initial point, while the reduction of the oxide of iron formed diminishes in a corresponding degree this temperature. Were the conditions of the experiment such that the oxide formed from the iron burned was equivalent to converting the carbon into carbonic oxide only, at the moment the oxide of iron became pure iron, then no increase of temperature would be noted, and the cooling influences of the surrounding medium would cool the acting bodies below the initial temperature. Hence, it is essential that more than an equivalent of iron should be burned, and a loss of this substance must take place, so that the operation of purification by the new process is carried on by substituting *iron as fuel* for carbon consumed in the ordinary process. Assuming six pounds of carbon to exist in a sample of crude iron containing ninety-two pounds of pure iron in one hundred pounds, then *twenty-eight pounds of iron* must be burned to oxide, and the six pounds of carbon will exactly reproduce the twenty-eight pounds of iron, leaving ninety-four parts of iron deprived of carbon. But the practical result differs from this statement, inasmuch as a positive loss of at least ten pounds of iron occurs; and in explaining the increased elevation of temperature, we neglect that portion of the iron which, having been burned and again reduced, adds to the mass, and keep in view the effect of the combustion of ten pounds of iron lost in the operation at the high temperature attained. Accurately, some addition to the temperature is made by the combustion of other bodies present besides carbonic oxide, but there are also sources of expenditure; leaving as useful effect the amount of heat generated by ten pounds of iron burned, from every one hundred pounds of melted iron taken.

"I believe this combustion, going on momentarily with the reduction of the oxide, is sufficient to afford the excess of heat required to maintain the temperature of the mass of iron above the initial temperature for the short time of thirty or forty minutes, during which the conversion takes place in a nearly closed vessel.

"The other point in this connection is the condition of the pure iron at the moment of its conversion. As this is the most important to a correct conception of the practical bearing of the method, it was deemed necessary to describe briefly the ordinary mode of puddling iron, and reference is now made to that part where it is stated, that, as the iron becomes pure, it is less fusible.

"In ordinary, this less fusible part is 'gathered,' and forms 'puddle-balls;' if not thus removed, and time sufficient were allowed, the whole charge would become consolidated, and could not be removed. In the new method the jets of air agitate or 'boil' the fluid iron, and yet this solidification does not proceed, and it has been assumed that the acting temperature is so high that the pure iron becomes fluid. No evidence has been presented to sustain this assumption, and it has been shown above, that there is no source of heat present adequate to cause such fluidity. All the specimens of a suite illustrating the manufacture, prepared under the eye of Mr. Bessemer, show that such heat of fluidity is unnecessary.

"These specimens prove that the molten mass of pure iron is not a *liquid iron*, but a semi-fluid, composed of crystals of pure iron, which, in accordance with the laws of crystallization, have withdrawn from the fluid, merely wet by the fluid iron present, and rendered pultaceous by the carbonic oxide gas entangled. This physical condition of the iron is represented by particles of hail mixed with a small proportion of water, or more exactly by the mixture of crystals of sugar and concentrated syrup, as it is filled into the forms; such a mass will flow and take sharp impressions in the moulds, while its texture, on cooling, is highly crystalline and porous. Although the iron in this state is as pure, chemically, as any bar-iron, its mechanical state does not assimilate it to malleable iron, and the ingots rarely present the compactness of cast-iron of the coarser qualities. A careful examination of the specimens suggests the conclusion, that much of the character of fluidity is also due to the presence of the engaged carbonic oxide, which, like any gas disengaging from a dough-like semi-solid, causes it to flow.

"This mechanical constitution of the pure iron removes the difficulty which every iron-master must have conceived to exist in the descriptions of the new method heretofore published, and it will be seen that the effects produced in the old and new process are strikingly similar; while the fuel in the one case is iron, in the other the ordinary coke or coal. In removing the iron from the furnace, the puddler depends on forming a rude porous aggregate, while Mr. Bessemer, by a refined mechanical agitation, converts the whole into a semi-solid, crystalline mass, full of gas-bubbles, which flows from an inverted vessel, and takes the form of the moulds."

ON THE COMPOSITION AND PREPARATION OF BRONZE POWDERS.

Hr. König has made some experiments, with the view of ascertaining the mode of preparing bronze powder, which is generally kept secret. The specimens he examined were those known commercially as—

1. Pale yellow. 2. Yellow. 3. Reddish-yellow. 4. Orange. 5. Copper-red. 6. Violet. 7. Green. 8. White.

The bronze powders, 1, 2, 3, 4, 6, and 7, were found to consist of copper and zinc, with traces of iron. 3, 4, 6, and 7, contained a small amount of oxidized copper, the surface of the particles being covered with suboxide of copper. This is shown by the action of acids; for when the powder was covered with dilute sulphuric or hydrochloric acid, the color immediately disappears, owing to the solution of the thin layer of suboxide, and the proper color of the alloy appears. The amount of oxygen could not be estimated in any of these bronzes, but it did not amount to one thousandth. The bronze powder called white, contains zinc and tin. The bronzes 3—7 were also found to contain a minute quantity of fat, which, on dissolving the powder in dilute acid, separated upon the surface of the liquid in the form of a thin film.

The quantitative analysis of the alloys gave the following per-centage results:

	Copper.	Zinc.	Iron.	Tin.	Remarks.
1. Pale yellow.......	82.33	16.69	0.16	—	
2. Yellow............	84.50	15.30	0.07	—	Fine golden-yellow.
3. Reddish-yellow....	90.00	2 60	0.20	—	Brass-yellow, with a shade of red.
4. Orange............	98.93	0.73	0.08	—	The color of clean copper that has been heated.
5. Copper-red........	99.90	—	trace	—	Copper-color, with a shade of purple.
6. Violet............	98.22	0.50	0.30	trace	Purple-violet.
7. Green.............	84.32	15.02	0.03	trace	Bright bluish-green.
8. White.............	—	2.39	0.56	96.46	Between tin-white and lead-gray.

These results show that bronzes of the most different colors possess nearly the same composition. The action of acid shows likewise that their color is owing to different degrees of oxidation. It appears therefore probable, that in the preparation of bronze powder, a certain alloy is used for all, and that the colors are produced by heating to different temperatures. This Hr. König found to be the case. In his experiments with the above-named powders, No. 1, when heated, presented successively all the prismatic colors, and acquired a fine dark violet. Most of the other bronzes presented the same character, and when the heat was maintained, lastly became black, from complete oxidation.

Hr. König is of opinion that the fat in these bronze powders originates from the use of fat in their preparation, for the purpose of insuring uniform temperatures. Tallow or fat oils would not answer for this purpose, since they would, in course of time, cause an oxidation of the copper. Wax, and especially paraffine, appear better adapted for this purpose. He states that 0.5 per cent. of either substance, mixed with the bronze powder in a shallow pan, is sufficient.

The mechanical subdivision of the alloy is affected by rolling and hammering, as in the preparation of goldleaf, and then grinding the leaves of metal, with water, between stones.

Hr. König has endeavored to prepare these bronzes in the wet way, by reducing the metal; but has not succeeded in obtaining any satisfactory results. The only substance of this kind which he was able to prepare in the wet way, is that called "iron black," which is used for coating gypsum figures, in order to give an appearance similar to gray cast-iron. This consists of very finely divided antimony precipitated from solution by means of zinc.

ON TWO NEW METALS IN SWEDISH MAGNETIC IRON ORE.

Professor Ullgren, in a recent communication to Liebig's Annalen, states that he believes he has detected in a magnetic iron ore from near Askersund, Sweden, two metals — one of an electro-negative, and the other of an electro-positive nature — both possessing properties which justify the assumption that they have not hitherto been known. The ore in question, when added to good ores in smelting, is said to cause a high degree of deterioration in the iron obtained.

The electro-negative metal has the following properties: — it is thrown down, with a brown color, by sulphuretted hydrogen from an acid solution, and the precipitate is soluble with a brown color in ammonia and sulphuret of ammonium. Its solution in nitro-muriatic acid, when slowly evaporated, deposits a solid body of a brownish-yellow color. Before the blowpipe, this gives colorless globules, with salt of phosphorus, and furnishes no metal with soda upon charcoal.

The properties of the electro-positive metal are as follow: From a solution of iron, mixed with a sufficient quantity of acetate of soda, it is thrown down by a sulphuretted hydrogen, together with iron and a small amount of zinc which is contained in the ore. After the precipitate has been partially dried upon the filter, iron and zinc may be removed by dilute muriatic acid, and afterwards nitric acid. The residue, calcined with access of air, and afterwards fused with carbonate of soda, yields a grayish-yellow substance, which, when calcined in hydrogen gas, furnishes a black powder, which burns in the air to a grayish-yellow body. The black powder obtained by reduction with hydrogen gas, is dissolved with extreme difficulty by nitric acid, but more readly nitro-muriatic acid; in this solution alkalies form a yellowish-brown, flocculent precipitate — ferrocyanide of potassium a blue or green one. Before the blowpipe, it gives a colorless globule, with salt of phosphorus; this becomes opalescent in the inner flame, and when a large quantity is present, gray. It is not in the least attracted by the magnet.

IODINE IN ATMOSPHERIC WATERS.

M. Marchant, in the *Comptes Rendus*, No. 17, 1858, publishes the details of a great number of analyses, which seem to prove beyond a doubt that iodine and bromine (traces) "are constantly and normally present in atmospheric waters."

A NEW METHOD FOR DETECTING THE PRESENCE OF IODINE AND BROMINE IN MINERAL WATERS.

The following paper, by M. M. Henri and Humbert, is copied from the *Comptes Rendus:*

The mineral water, or the residue from its evaporation, more or less concentrated, is treated with an acid solution of nitrate of silver. The precipitate which takes place ought to contain, in the state of silver salts, all the chlorine, bromine, and iodine which was in the water. The precipitate is washed and carefully dried, and in this state it is mixed intimately with a small quantity of cyanide of silver, then introduced into a tube, at one of the extremities of which it is fixed between two small plugs of wadding or flax. A current of chlorine gas, perfectly dry, is made to pass over the mixture, whilst the corresponding part of the tube is gently heated. The iodine, bromine, and cyanogen are displaced, and, combining together, condense in the coldest part of the tube in beautiful white and crystalline rings of iodine and bromide of cyanogen. The tube is afterwards closed at both its extremities, and reserved for testing.

The iodide and bromide of cyanogen possess physical and chemical properties which prevent their being confounded with other compounds. The iodide sublimes at a temperature of 113°, and the bromide at a temperature of 59° Fahrenheit. This difference in their volatility permits of their being mechanically separated; by plunging the tube containing these compounds into water of 86° the bromide alone gains the upper part of the tube, care having been taken to keep it conveniently cool.

We do not insist upon the chemical composition of these combinations; it suffices us to say that it is easy to determine with certainty their nature in obtaining with them the principal reäctions which characterize iodine and bromine. Proper care should be taken in preparing the chlorine for this process, that the substances should be pure, and, above all, contain neither iodine, nor bromine; besides this precaution, after having mixed the cyanide of silver with the other silver salts, the whole should be heated for some time, and if there are no traces of iodide or bromide of cyanogen, the process is proceeded with in the ordinary way.

IMPROVED METHOD OF PURIFYING WATER.

The following plan of purifying water has recently been patented by Henry Medlock, of England, the specification reading as follows:

I suspend, in a tank or reservoir containing the water to be purified, by means of iron rods passing across it, iron wire, of about one-sixteenth of an inch in diameter, loosely packed in bundles or coils, and in the proportion of about one pound weight of such wire to every one hundred gallons of water. I allow the water to remain in contact with the iron wire from twenty-four to forty-eight hours, according to the rapidity with which the precipitation of organic matter occasioned by such contact takes place, and I then pass the water through any kind of filtering medium now in use which is capable of retaining the precipitate formed. For the filtration of water in large bulk, I have found the ordinary sand-filter sufficient. The effect of the contact of the water with the solid bodies above described, when the water contains nitrogen in any form, is to decompose or oxidize the organic matter and the ammonia contained in the water, whereby a certain part of the organic matter and ammonia is converted into nitrous or nitric acids, or both of them, by which the rest of the organic matter is rendered insoluble. The nitrous and nitric acids finally combine with the iron, or with some of the inorganic bases, if any, contained in the water, and the organic matter rendered insoluble is precipitated, together with some part of the inorganic matter, if any, contained in the water.

FILTRATION THROUGH SAND.

Experiments by Mr. H. M. Witt, at the Chelsea waterworks (England), have proved that by simple filtration of water through sand, soluble salts, as well as suspended matters, are separated. Out of 65·527 of solid residue dissolved and suspended, 24·237 were separated, including 7·559 of soluble salts (1·820 of these chlorid of sodium). From water containing 55·60 of solid residue, 32·75 were thus separated, 3·404 of which were dissolved salts. Hence mere percolation through sandy strata for long periods may separate dissolved saline ingredients from waters. Mr. Witt applies the facts to explain the occurrence of fresh-water springs on coral islands, that ebb with the tide. But here Mr. Darwin's explanation seems most reasonable, — that the fresh waters are from the rains; and that, being detained upon the coral rock, and at the same time being lighter than the salt water of the ocean, the setting in of the tides pushes the fresh waters before them. — *Silliman's Journal.*

PERMANGANATE OF POTASH AS A DEODORIZER.

Dr. Girwood, in a communication to the London *Lancet*, highly recommends permanganate of potash as a powerful deodorant, and also as an escharotic and stimulant when applied to sores, ulcers, etc. Dr. G. says:

A teaspoonful of the substance powdered, added to a tablespoonful or two of water, just enough to moisten it well, and sufficient to cover the surface of a flat dish — a dinner-plate for example, being used for the purpose — giving a broad surface for the absorption, and this plate placed under the bed, or anywhere most convenient in the sick-chamber, all odor disappears: and it has an advantage above those in general use in the sick-chamber, that it has no odor of its own. Vinegar and chlorine and nitrous acid gas are often of themselves a nuisance; whilst destroying one odor they create another: but the permanganic acid has none. It only destroys; it does not create. I have employed the solution successfully in my stables, and in other places engendering odors. It does not require frequent change. Has it lost its original beautiful purple color? Has it become black and slimy? If so, renew it; but not till then.

USE OF WOOD CHARCOAL IN SEWERS.

The last report of the Commissioners of the London sewers contains the following testimony respecting the value of wood charcoal as a deodorizer:

"We have," says the Report, "in common wood charcoal a powerful means of destroying the foul gases of sewers. How it is to be applied, is a question of but little embarrassment. Ventilate the sewers as you will, either by the open gratings in the streets, or by the rain-water pipes of the houses, or by the pillars of the gas-lamps, or by tubes carried up at the landlord's expense from the drains of every house, or by special shafts in the public streets — in fact, let the gases go out of the sewers how they will and where they will, you have but to place a small box containing a few penniesworth of charcoal in the course of the draught, and the purification of the air will be complete. As far as we know, the strength and the endurance of this power are almost unlimited; so that, when once the air-filter has been set up, it will last continuously for years. Its action, also, upon the draught, is not particularly injurious. The temperature of the sewers, and the agencies which are now at work in circulating the air and ventilating them, will be sufficient to keep up a current of foul air through the filters; and if these were multiplied to a large extent, the friction of the gases upon the charcoal would be reduced to an insignificant amount."

EXTEMPORANEOUS PREPARATION OF CHLORINE AS A DISINFECTANT.

The chloride of lime, usually employed as a means of disengaging chlorine, has, besides its price, the inconvenience of being rather rapidly exhausted. M. Lambossy substitutes for it a cheap and simple preparation, consisting of common salt, red lead, sulphuric acid, and cold water. The red lead is mixed with the salt, and introduced into a bottle full of water. The sulphuric acid is added gradually, and shaken at intervals. By this process sulphate of lead is formed and precipitated, and sulphate of soda and chlorine remain dissolved in the water. The chlorine, which gives the liquid a yellow color, is disengaged as soon as the bottle is opened. To produce a more rapid disengagement, the liquid is poured into flat plates, so as to offer a large surface for evaporation.

ON COLORS OBTAINED FROM COAL-TAR PRODUCTS.

The following paper was recently read before the London Society of Arts, by Mr. Grace Calvert:

In November 1854, in a paper read to this society, I stated that, ere long, besides carbo-azotic acid, some valuable dyeing substances would be prepared from coal-tar. This expectation has now been fulfilled. Messrs. Perkins & Church have obtained several blue coloring substances from the alkaloids of coal-tar, and one from napthaline, named by them Nitroso-phenyline and Nitroso-naphthyline, etc.

Mr. Perkins has lately taken a patent for the commercial application of some of these beautiful purple-blue colors, which he has succeeded in fixing on silk, a sample of which I have the pleasure to lay before you. This fine color, which rivals the delicate and admired color of orchil, has this great advantage over it, that it is not destroyed by light; Mr. Perkins has, therefore, solved one of the problems of the art of dyeing, viz., the production of a fast color similar to the fugitive one of orchil. Mr. Perkins's process consists in dissolving in water the sulphates of aniline, of cuminine, and of toluidine, and adding a quantity of bichromate of potash sufficient to neutralize the sulphuric acid in these sulphates. The whole is left to stand for twelve hours, when a brown substance is precipitated, which is washed with coal-tar naptha, and then dissolved in methylated spirits. This solution, with the addition of a little tartaric oxalic acid, forms the dyeing liquor of Mr. Perkins.

Mr. Charles Lowe and myself have lately been fortunate enough to obtain from coal-tar products having a most extraordinary dyeing power, and yielding colors nearly as beautiful as safflower-pinks and cochineal-crimsons; and what increases the interest of this coal-tar product is, that, by the process we have discovered, we can obtain with it, on a piece of calico mordanted for madder colors, all the various colors and shades given by this valuable root — violet, purple, chocolate, pink, and red. The only thing which prevented us from introducing into the market the crown-red, inodorous product which we prepare, has been, that it is as yet too expensive to compete with this extraordinary color-giving root; but we intend pursuing our researches in the hope of employing it as a substitute for safflower or cochineal, two coloring matters, the price of which is sufficiently high to induce us to continue our investigations. We may add, that our imitation of safflower stands soap and light, whilst safflower colors do not.

I shall now draw the attention of the meeting to the preparation, dyeing, and printing of a magnificent crimson color, called murexide, obtained from guano, a substance which, until lately, has been entirely imported for agricultural purposes. The interesting application of this color to calico-printing has been, like many valuable chemical discoveries, progressive, and has only been brought to successful commercial application by successive discoveries, made by various persons.

Prout was the first chemist to remark that if the fæces of serpents were heated with nitric acid, and a little ammonia added, a beautiful purple color was produced. He named it purpurate of ammonia. This substance, when dry, has the appearance of a dark-red powder, soluble in water, to which it communicates a magnificent red color. This solution not only gives a precipitate with metallic salts, but, when evaporated, yields beautiful crystals, having the irridescent appearance of the wings of cantharides.

This discovery has also been useful to medical men, by enabling them to distinguish the uric acid calculi.

Messrs. Liebig and Wöhler had also investigated the subject, and succeeded in obtaining from the uric acid contained in the fæces of serpents this substance, which they called murexide, and a new class of organic substances, the knowledge of which has much facilitated the application of murexide to dyeing and printing. Mr. Saac was the first to apply the products of uric acid to the dyeing of fabrics; his process consisted in dipping woollen fabrics, prepared with a salt of tin, into a weak solution of alloxan, a product discovered by Liebig and Wöhler, in heating urea with nitric acid. The fabic so prepared was then dried, and when submitted to heat, a fine crimson was generated, the intensity of which was increased by the fumes of ammonia. But, owing to the difficulty of obtaining a color of uniform shade, Mr. Saac's process required improvements, and these have been effected by Mr. Schlumberger.

The process followed by Messrs. Saac and Schlumberger, could not be applied to silk or cotton fabrics. The method of dyeing silk with murexide was discovered by M. de Pouilly, who adopted the following processes, viz., dipping the silk in a concentrated solution of bichloride of mercury mixed with murexide, squeezing the silk well, and hanging in the air, when a magnificent crimson insoluble compound is fixed on the silk. This effect is produced from the fact, that, when solutions of bichloride of mercury and murexide are mixed together, an insoluble compound is only formed after the lapse of an hour or two.

The process for dyeing cotton is due to Messrs. Lauth and Schlumberger, and consists in producing on cotton a purpurate of lead by mordanting with nitrate of lead, passing into an alkali, and then dyeing in a solution of murexide; in order to give full brilliancy to the color, it is lastly passed through a weak solution of bichloride of mercury. This process was further improved by Messrs. Dolfus, Meig & Co., in France, and Mr. Lightfoot, in Lancashire, by printing murexide with an excess of nitrate of lead, and subjecting the cloth so printed to the action of ammoniacal fumes, or passing through a solution of caustic soda mixed with sal ammoniac. In order to render this substance more generally useful, it remained to find a method for obtaining fast colors with it on mixed fabrics, such as mousseline de laine, and this has also been effected by Mr. Schlumberger. The cloth is first prepared by uniting binoxide of tin with the wool. This object is attained by using a salt, known to calico-printers as pink salt, the double chloride of ammonium and tin, and then printing on the prepared fabric the following mixture:

1 part of murexide.
6 parts of nitrate of lead.
2 parts of nitrate of soda.

The pieces are then allowed to age for two or three days, when, to fix the purpurate of lead on the cotton, and the purpurate of ammonia on the wool, it is necessary to pass the cloth into a bath of bichloride of mercury, composed as follows:

Water, ...	100 gallons.
Bichloride of Mercury, .	6 pounds.
Acetate of Soda, . .	12 "
Acetic Acid, . . .	2 quarts.

Until recently, all the green colors produced on fabrics were the results of blue and yellow mixed together; but of late public attention has been drawn to a green matter discovered by the Chinese, and fixed by them on cotton. It has been ascertained that they prepare it, by a long and tedious process, from two plants called Pa-bi-lo-za (*Rhamnus chlorophorus*) and Hom-bi-lo-za (*Rhamnus utilis*), and sell it in small square cakes, under the name of Luh-kaou, or Luh-chaou. The first commercial importation of this color, new to us, is quite recent, as the first public sale of it in England took place during the present year (1858), under the name of China-green indigo. No sooner had a foreign green substance been brought to our notice, than in Europe we had succeeded in obtaining also a green dyeing substance from the plants which surround us; and Mr. Schlumberger has been fortunate enough to fix on woollen fabrics the green chlorophylle, or coloring matter of leaves and grass. This discovery will, in time, prove of great service to dyers and calico-printers. Mr. Schlumberger's process consists in boiling 60 lbs. of grass with 25 gallons of water. This operation is repeated, and the grass then treated with 25 gallons of soda lye, with addition of 2 to 4 lbs. of Mercer's dung substitute (phosphate of soda and lime). Boil half an hour, and then add excess of hydrochloric acid; a green precipitate falls, which is separated by filtration. The precipitate is dissolved in very dilute soda lye, adding a little of the substitute, and the silk or wool to be dyed is dipped in until, the desired shade is obtained. Stannate of soda is the only mordant which gives any beneficial result.

ROSOLIC ACID.

At a recent meeting of the London Chemical Society, Dr. Hugo Müller read a paper "On Rosolic Acid." Since the discovery of this body by Runge, no mention has been made of its reöccurrence, while even its existence has been called in question. The author met with it accidentally, as a result of the slow action of caustic lime upon the crude carbolic acid of coal-tar. After long exposure to the air, the mass assumed a red color, and when acted upon by water, yielded a solution of crude rosolate of lime, having a fine red color. From this salt rosolic acid was obtained and purified. Its empirical formula was found to be $C_{46}\,H_{22}\,O_8$. It is a very feeble acid, uniting only with caustic alkalies and earths. The solutions of these compounds are of a most magnificent crimson color, but are very unstable. The carbonic acid of the air liberates the rosolic acid, which is eventually destroyed by continued exposure to air and light.

IMPROVEMENT IN THE MANUFACTURE OF COAL OILS.

An invention recently patented in England, relates to a method of distilling coal, shale, and bituminous substances, whereby a pure oil, suitable for illumination and other purposes, is obtained at the first distillation.

It has long been known that many varieties of coal, shale, and bituminous substances were capable of affording oil and oily matters, when subjected to dry distillation at a low temperature; but in general the oil obtained at the first distillation comes over in a crude, coarse condition, totally unfit for use. Several processes have been invented for purifying this crude oil, and some of them are attended with great success; but nearly all of them are expensive. They involve the use of large quantities of sulphuric and other acids, salts, repeated distillations, heatings, boilings, agitations, decantations, and other labors. The bituminous substances before referred to, yield, on distillation at a low temperature, a gas, which, if passed through a worm or other suitable refrigerator, condenses into what is known as crude oil, requiring purification, as described.

The present invention consists in straining the gas which produces the oil, by passing it through a stratum or strata of sand, or other suitable medium, so that, when condensed, it forms a clear and valuable oil, ready for immediate use. This result is obtained by the following process, viz.: The coal, shale, or whatever bituminous substance is to be distilled, is broken up into very small pieces, and deposited upon the bottom of the retort. Upon the coal is thrown a quantity of common sand, about four times greater in weight than the weight of coal. The sand should be made to cover the coal evenly, so that the gas in escaping from the coal will pass through the sand. A condensing tube leads from the upper part of the retort to the refrigerating worm. The retort, thus prepared, is submitted to a low fire, the heat of which is very gradually and carefully increased, until the coal and sand having reached a temperature of about 212° Fahr., the moisture contained in the coal and sand begins to rise into the condensing tube in the form of steam, and on passing into the worm, is condensed into water, and escapes: the water thus brought over is loaded with black carbonaceous impurities. The same temperature being continued, the condensed water gradually becomes clearer, and the oil begins to form: both oil and water escaping together from the worm, the oil rising to the surface in the receiving vessel. The oil, as it thus exudes, is beautifully clear and pure, and when burned in an argand lamp, with a deflecting button over the wick, gives a most brilliant light, totally free from smoke. As the distillation proceeds, the quantity of water that comes over lessens. The temperature before named should be steadily maintained until no more pure oil is produced. With some varieties of bituminous substances, however, the oil ceases to come over before it has all been exhausted from the material, although exposed to the above heat for a time, as described; in such cases a higher temperature is then required. Such additional heat should be applied very gradually, and with the utmost care. The distillation may proceed, adding degree of heat by degree, so long as the distilled substance yields pure oil. When the heat has passed a certain point, which is determined by the nature of the substance under distillation, no more pure oil can be had, and crude oily and tarry matter comes over. Owing to the great variety of bituminous substances existing, it is impossible to lay down the exact degree of heat required for the distillation of each by the process; but as a general rule, the following method should be observed: — Commence with a low temperature, and carry it up very gradually until the pure oil begins to condense; continue the same temperature so long as the oil exudes. If the oil ceases, increase the heat very gradually, as before described, until no more pure oil can be obtained. The gas out of which the oil is formed should be set free, and have an opportunity of passing slowly through the filtering or straining substances, so as to deposit its impurities. When too much heat is applied, the filtering or straining operation will be imperfectly accomplished. Instead of using sand as the straining or filtering medium for the gas, clay and earths of most kinds may be employed, as also chalk, gypsum, lime, black oxide of manganese, some salts, plumbago, charcoal, etc.

ON THE PRESENT STATE OF CHEMICAL KNOWLEDGE IN RELATION TO ALCOHOLIC BEVERAGES AND THEIR FALSIFICATIONS.

At a recent meeting of the Boston Society of Natural History, Dr. A. A. Hayes, by request, addressed the Society on the above subject.

He stated that the products of fermentation — whether resulting from "worts," in which the altered starch of grains furnishes the material, or the "must" of expressed juices of grapes, fruits and plants — might be, for convenience, classed as wines. Thus ale, which is the product of the first fermentation of malted barley, may be considered as a wine of malt; the addition of hop extract being a matter of taste, which replaces the fruit extract in wines. If we include also the mixtures of cane products, sugar and water, honey and water, and, finally, skimmed milk, we have sources of various resorts for producing exhilaration or intoxication, adopted by all nations.

The first chemical change in many of the mixtures used is produced by the remarkable body called diastase, which has the power of converting nearly two thousand times its own weight of starch into dextrine, or about half that quantity into grape sugar. This body is developed in the act of germination in grains and seeds; and probably in this principle of malted barley we have the type of a class of these substances, which in different ways act to produce remarkable transformations in organic bodies.

The second change, which demands attention, is that of the conversion of dextrine and glucose — the sugar of fruits — into *alcohol*, which remains dissolved in the fluid. This breaking up and reärranging of the elements of sugar, is effected, as is well known, through the aid of the *beer yeast plant*, usually. Like diastase, the beer yeast plant is endowed with life, and has the power of communicating motion to organic and organized matter, resulting in change of composition. In so simple an expression of the phenomena of beer, or wine production, we have omitted some most important substances, which participate in the changes. These are the natural fixed oils and fats, and the volatile odorous bodies in grains, but more especially in grapes, fruits, cane products, etc. Thus the hop extract, added before fermentation to ale, beer, or porter, becomes altered, as do the grape oils; and the remark applies to all known cases of fermentation of mixed solutions. Mature grapes contain a natural ferment, in its appropriate cells, requiring, to bring its affinity in view, only that the cell walls and tissues of the fruit be broken, so as to provide fluid sugar, on which it reäcts. Fermentation, which at a temperature of 70° F. commences immediately in grape juice, develops, besides alcohol, a whole class of new bodies. So in the fermentation of grains, the fixed bland oils of the seeds, slightly altered, remain, while more volatile odorous oils are directly produced from materials present. Now these oils deserve special attention, because on the production of these, and their subsequent further change, the money value of the beverages depends. A very large consumption of beverages is supplied by the first fermentation of the materials named, and these alcoholic mixtures also form the basis of the *spirit* manufacture.

The second fermentation of wines is perhaps more important than the first. In this, the alcohol remaining almost unaltered, the greatest changes take place in the fruit extracts and oils. Fruit extracts lose their acidity and much saline matter, while the presence of acids leads to the production of *ethers*, from oily bases present, which then give their odors to the wine. It is apparent that the proportions, and even kinds of ethers present, may vary; but a general predominance of one designates a wine. Closely connected with the presence and kind of ether, are the effects of wine on the human system; those wines having much ether and little alcohol proving exhilarating, while the alcoholic and oily wines may be distinguished as intoxicating beverages.

In the spirit manufacture, after the first fermentation has ceased, distillation follows, for the separation of alcohol from the non-volatile organic matters. Here again the volatile oils are characteristic bodies, and impart their odors to the spirits in a strongly marked degree. Brandy, whiskey, rums, corn and potato spirits, have their values based on the kind and quality of oil each contains. Their other constituents, in pure samples, are simply *odorless* water and *odorless* alcohol.

In all pure distilled spirits, with the oils, certain acids are connected, and the etherification of the oils slowly proceeds, as in the case of wines. The distinction between new, or raw spirits, and old, or matured spirits, arises from this etherification of the oils commonly called *fusel* oils. The highly aromatic brandies of the last century, like the "Nantes" brandy, were distilled from partly matured wines, and contained all their ethers.

The falsifications of beers, wines, and spirits, so far as our commerce is concerned, are extensive, and will be alluded to briefly.

In the early manufacture of brandy, wines of low cost were distilled; but the more general demand for wines, with occasional short crops of grapes, have led to another mode of producing the enormous quantity of raw spirits now consumed.

After the grapes have been pressed and fermented, the residue, consisting of skins, kernels, and pulpy parts, is mixed with solutions of glucose, obtained from starch; odorless spirit is added, and the artificial must thus formed is fermented cautiously. Distillation separates a spirit, loaded with the oils of grape fermentation to such an extent that an excess of grape oil, besides brandy, is obtained in rectification. This is the brandy of the present day, and forms the large bulk of our importation. The oil of grapes, under the name of "*Cognac Oil*," is vended for a moderate price, and becomes a large *producer* of brandy, by a very short process.

As the oils known as fusel oils are less volatile than alcohol, a managed distillation permits us to separate the oil from alcohol, which then becomes odorless or neutral. Cognac oil, mixed with neutral spirit, instantly produces factitious brandy, sugar and coloring matter being adjuncts. Such a base mixture is largely made in this country; and as it has no principle capable of maturing, it should take a place below the raw spirits. Falsified wines are made here from mixtures of spirit, water, sugar, and low-priced wines, as imitations of wines of well-known names. Sparkling wines are made on a large scale, from fruit wines or sweet wines, with sugar and carbonic acid mechanically introduced; and such wines are probably often imported. Sherry and Madeira, as imported, have spirits mixed with the wine, and often forty per cent. of proof spirit is contained in them. Both ale and beer are sold which have been mixed with low-priced spirits, to increase their intoxicating effects. But the most demoralizing intoxicating beverages are the new, or raw spirits, so common. In many of these the fusel oils exist to the point of saturation. These oils have specific actions on the system, and the ethers are not present.

LIQUORS AND THEIR ADULTERATIONS.

Mr. F. Stearns, Pharmaceutist, of Detroit, communicates to the *Medical Independent* the following popular account of the modern processes for manufacturing and adulterating liquors, the most of which are well known to chemists and dealers, but not to the public generally.

Modern researches in organic chemistry have enabled the chemist to isolate from fermented grape juice those peculiar principles which give it aroma, bouquet, color, etc., and which serve to distinguish it from other liquids. They have also enabled him to procure, from the refuse of wine-vats, and by artificial means, the materials necessary for the successful imitation of every wine and distilled liquor known.

Formerly, *pure* liquors were *mixed* — that is, they were increased in bulk by additions of distilled spirit, and then brought up to the required standard by means of foreign substances. Now, however, they are *manufactured* entirely. The art of the "*mixer*" has become scientific by the aid of the chemist, and he is a *producer*. Now, the "jobber" who is skilled in the mysteries of the trade, can stand beside a cask of pure spirit (highwine), and imitate perfectly, with chemical flavors, essential oils, etc., Otard or Schnapps, Johannisberg or St. Peray.

In our own country this art of *manufacturing* liquors is carried to its greatest perfection — and it is the purpose of the writer to show, in this article, some of the methods employed.

The high ruling prices of imported wines and liquors, caused by the scant vintages of late years, has been the great incentive to the artificial production of them here. It is even asserted that foreign liquor-makers, not satisfied with sophisticating their own growths and distillations, import, from this country, large quantities of alcohol, for the purpose of making brandy, and reshipping it to the United States.

The substances in the grape which impart color, bouquet, taste and flavor to wine, are tartar, tannin, essential oil, and the coloring matter of the husk. These form but one per cent. of it — the balance being alcohol, water and sugar. It has been ascertained, that, in the process of fermentation, not over a fourth part of these substances are taken up by the wine, and that the most valuable one — the essential oil — can be obtained from the lees. This flavoring oil — the product of the grape — is mixed with an artificially-produced œnanthic ether, and constitutes what is known in commercial parlance as "Oil Cognac," and is used in manufacturing brandy.

This is found in market, varying much in quality of aroma and appearance. That which bears the highest price (about twenty-five dollars for a fluid ounce) is of a pale amber-color, and consists almost entirely of œnanthic ether, as produced from the grape, and is used for imitating the finer kinds of brandy. There is another variety — a mixture of essential oil and œnanthic ether — of a light-green color, due to the presence of copper; another is colored pink by cobalt; others are white, yellow, or brown, — the greatest difference being in the quality of the œnanthic ether.

The properties of *pure* brandy are subject to some variation, arising from different growths of the vine. An experienced dealer and judge can always recognize the products of different provinces. Brandy is colorless when distilled, but acquires a slight amber-color from the cask in which it is kept, or is colored by caramel (burnt sugar), which is said to render the spirit mellow and more palatable. Analysis shows pure brandy to consist of alcohol, water, volatile oil, œnanthic ether, coloring matter, and sugar. Some varieties also contain a small portion of acetic ether and tannin.

Now, to show how closely modern manufacturers follow the guide thus given by analysis, in producing a *domestic* article, I will state some of the methods employed.

The highest proof-spirit is employed for imitating fine brandy, because less contaminated with the grain or fusel oil present in the whiskey, — the most of it being removed by distillation with the permanganate of potash. This spirit is reduced to the standard required, — being that of equal parts, by volume, of absolute alcohol and water, called "neutral spirits," though this term indicates a spirit of any proof, — and is converted into brandy as follows:

A mixture (a little over six ounces) of amber cognac oil, oil of bitter almonds and ethereal oil of wine, U. S. P., put into one hundred and seventy-five gallons of spirit, prepared as above, produces, with the assistance of a little simple spirit and more or less caramel, the finest varieties of pale or dark brandy. Sometimes a gallon of Malaga wine (a made-up wine) is added.

A mixture, in all one and a half gallons, of acetic ether, tamarinds, sour cherry-juice, and a little *white* oil cognac, forms, when put into one hundred and thirty gallons of "neutral spirits," pale or dark cognac, as may be desired, with caramel and simple syrup.

The *green* oil of cognac, with ethereal oil of bitter almonds and tannin, — in all, nearly six ounces, — converts one hundred and fifty gallons of neutral spirits into brandy. A few pounds of elder-flowers give it *mellowness*, and the tannin imparts *roughness* and *age*.

The *pink* or *brown* oils of cognac make "Rochelle" brandy, when mixed in due proportion with acetic and peach ethers. It only requires of them seven ounces, to *doctor* one hundred and thirty gallons of neutral spirits — caramel as before.

It is, perhaps, well to remark, that manufacturers also require, in order to produce the *variations* of different *pure* brandies, bruised French plums, for their *acid and flavor;* wild cherry juice, for its *astringency and bouquet;* oak shavings, for their *astringency, color, and odor;* catechu, for its *tannin and color;* powdered charcoal, black tea, ground rice, peach ether (a compound of oil of bitter almonds and diluted butyric ether), and grape oil — a compound of the organic, radical amyle, possessing a strong, vinous odor. In connection with this substance, it is a singular fact that amyle is carefully separated, in one of its forms of combination, — that of fusel oil (hydrated oxide of amyle), — from the spirit, in first preparing it for the manufacturer's use, only to reënter it, in another form, for the purpose of forming "Brandy."

Holland gin, when *pure*, is a spirit obtained by first fermenting barley and rye with hops, allowing effervescence to cease; then distilling, and repeating distillation from juniper berries, which last gives a peculiar aroma and diuretic properties. Age improves gin, imparting to it a smooth, oily flavor, much admired by many.

Three-quarters of the gin sold is entirely innocent of any knowledge of Holland. The *English cordial* gins, so much used as medical agents for their diuretic and carminative qualities, are all made-up liquors.

Manufactured gin is made as follows: A mixture of oils, of juniper berry (freshly distilled) and angelica seed, in equal proportions, together with rum ether, oil of lemon, common salt, and simple syrup, is added to spirit of the proper proof, *neutralized* by means of *spirits of nitre*. Sometimes oils of caraway seed and fennel seed, with peach ether, are added to the above, to create an "Aromatic Gin," or to give *smoothness, richness*, and *creaminess* to the liquor.

Creosote is used to give to gin a certain degree of smokiness; *caustic potash* is added to make it strong and biting on the palate.

British "cordial" gin is sometimes made of oil of bitter almonds, oil of vitriol (sulphuric acid), spirits of turpentine, oil of juniper, coriander seed, orris root, elder flowers, acetic ether, and sugar, in proper proportions, macerated in proof spirit.

Common gin is a crude distillation of whiskey, from a mixture of spirits of turpentine and common salt, with a *dash* of juniper berries.

Rum, when *pure*, is the distilled product of fermented refuse saccharine matter which accumulates where sugar and molasses are made. The best varieties are obtained from the West Indies, and termed Jamaica, Santa Croix, and Kingston — the Jamaica being considered the best. The peculiar flavor of rum is due to the presence of a portion of empyreumatic oil, which forms and passes over it during the process of distillation.

Now, by distilling a mixture of black oxide of manganese, sulphuric acid, alcohol and acetic acid, a peculiar ethereal liquid is obtained, possessing the odor of rum in a remarkable degree; this, when mixed with essential oils, and colored brown, is termed, commercially, "Oil Jamaica Rum;" colored pink, and entitled "Oil Kingston Rum;" or left of its natural color, and it is "Oil St. Croix Rum."

A single ounce of the oil Jamaica rum, with a little essential oil of pimento (allspice), some acetic ether, a few pounds of sugar, and ten gallons of water, converts one hundred gallons of fourth-proof spirit into "Jamaica;" or, by using white or pink oils, with some essence of lemon and spirits of nitre, the same quantity of spirit is converted into Kingston or St. Croix rum. Simple syrup, molasses, and caramel, are also used to sweeten, give smoothness and color to this liquor.

It is often remarked, that if habitual drinkers would confine themselves to whiskey — *that pure drink* — the probabilities are that they would average longer lives, and be less subject to the horrors. This might be true if high proof alcohol was drunk by them, properly diluted, but not when the common grain whiskey of the still is used.

Common whiskey contains a large per-centage of fusel oil (an oxide of the organic radical amyle), which passes over in distillation in considerable quantities. Repeated rectification only partially removes it. It is, in a pure state, highly poisonous.

I am credibly informed by an extensive alcohol distiller, that, in running highwines enough to produce three hundred barrels of alcohol, he separated at least a barrel of crude fusel oil, one tablespoonfull of which, if swallowed, would produce fatal effects.

In order to cheapen the production of whiskey, and of course, at the same time, lessen its wholesomeness, manufacturers are accustomed to *cover up* the flavor of the grain oil which the raw spirit contains — not *remove* it partially by rectification — by adding strong flavoring materials.

Peach whiskey is fashionable. Where does the *peach* come from? When nitric acid is distilled with benzole, a peculiar substance is produced, which resembles in odor the oil of bitter almonds — this is the *peach* part of the whiskey. Raw grain whiskey is readily converted into *pineapple* whiskey by adding a portion of an ethereal liquid obtained by the action of oil of vitriol upon butyric acid, butyric acid being the result of a fermented mixture of putrid cheese.

Artificial flavors are sold for flavoring the spirit in imitation of Monongahela and Scotch whiskey; and all that a barrel of raw whiskey requires to convert it into "Bourbon," is two gallons of Jamaica rum, with a little oil of caraway and bitter almonds.

So much for distilled liquors.

Wines, generally, are so doctored by foreign growers, to suit the vitiated taste of habitual drinkers of them, that, after the addition of liquor tonic (brandy), sugar, and coloring matter, the best of them would hardly be recognized when compared with samples in their original state of purity.

All the best wines *imported* into the country contain a fair share of brandy; it is added to them previous to exportation, for the ostensible purpose of making them stand the voyage without fermenting, but, in reality, because the market demands *strong wines.*

The *manufacture* of wines has reached the greatest perfection.

Port wine, which, in a pure state, is a sweet, rich, aromatic wine, of deep color and mild taste, is required, by those who use it habitually, to be *strong.* The English, especially, prefer the strong port, and it constitutes the variety termed London dock, and is made by adding brandy, elderberry juice, and sugar, to pure juice port.

Of the other varieties of port, though some may reach us in a pure state, yet they are generally of a poor quality, being mixtures of wines of different growers, good, bad and indifferent.

Neutral spirit is the base of manufactured ports in this country, containing, usually, 25 per cent., by volume, of alcohol, flavored with "May wine ether," colored with elderberry juice, beet juice, or cochineal and caramel. The other qualifications of pure port wine are obtained by suitable additions of red tartar, catechu, tannin, sugar, honey, and spices.

Madeira wine, in a *pure* state, it is asserted, is exported to our shores in greater quantities than to any other country, because Young America likes this wine for its flavor, rather than strength. In a pure state, and when old, it has a pungent, bitter-sweet taste, and nutty flavor; it is very fragrant, and is generally admired by wine-drinkers. It is considered one of the most valuable of medicinal wines. It is *manufactured* from neutral spirit, to which a portion of good Maderia or sherry is added, with sugar, coloring matter, and flavoring, denominated "Ether of Madeira Wine," sold by importers of those flavors. It is also *made* by fermenting a mixture of malt and sugar, and adding Cape wine, brandy, sherry and port.

Sherry, it is believed, we also get a fair share of, in a pure state — at least with nothing more than brandy in it, varying in amount from five to ten per cent. It is pale or dark, according to the amount of coloring matter it is allowed to take from the husk of the grape. This is the wine directed to be used in the manufacture of the medicated wines of the United States Pharmacopœia, from the fact that it contains no acidity.

As a made-up wine it is manufactured similarly to Madeira, and materials are used which will impart to raw spirit its peculiar dry, nutty flavor. British sherry is a wine obtained by fermenting sugar, ale-wort, raisins, and yeast; then biter almonds and orris powder is added, and the wine fined; or a spirit, obtained by fermenting parsnip-juice with water and sugar, is mixed with a poor grade of Madeira, cassia, cloves, and bitter almonds added, and the whole fined with isinglass.

Clarets are abundantly produced, and consequently cheap, though some of the favorite varieties rank among the highest priced wines. They are as fully as abundantly manufactured from rough cider, a red wine from the Cape of Good Hope, catechu, spirits, bitter almonds, etc.

The less said about champagne wines the better, for there is more — several times over — exported to America and Russia alone then the whole champagne district in France produces; and in the immediate champagne districts are large establishments for the manufacture of artificial champagne.

The Rhine wines, or "Hocks," as they are called, are nearly all made-up wines, and by the same means that the other wines mentioned in this article are manufactured. Some of the brands of Rhine wines bear fabulous prices in districts where they are produced, and are never exported. It is the same with all wines grown: all are subject to sophistication in the hands of dishonest producers; or the demand being greater than the supply, tempts those skilled in the manufacture to so make imitations of them.

ON AMYLIC ALCOHOL.

The following research on Amylic Alcohol has been submitted to the French Academy, by M. Pasteur:

The fusel oil of commerce consists chiefly of two kinds of amylic alcohol — one active, and the other inactive, with regard to polarized light. These two varieties are exactly similar in their chemical properties, differ but slightly in density and boiling point, and give rise, under similar circumstances, to products which resemble each other in all respects, excepting in their relation to polarized light, — those which are derived from the active alcohol being themselves active, and those which result from the inactive alcohol being themselves also inactive. The proportion of the active and inactive alcohols in fusel-oil varies according to its origin: thus the fusel oil obtained by fermentation of the juice of mangold wurtzel, contains about one-third of the active and two-thirds of the inactive amylic alcohol; whereas, that which is produced by the fermentation of the molasses, contains about equal parts of the two alcohols. The two alcohols cannot be separated by fractional distillation, but only by fractional crystallization of the active and inactive sulphamylates of baryta. For this purpose it is necessary to prepare a large quantity of sulphamylate of baryta from crude amylic alcohol, rectified by a single distillation, in order to free it from water and vinic alcohol. The amylic alcohol, thus far purified, is mixed, as usual, with an equal weight of sulphuric acid, the mixture treated with carbonate of baryta, then filtered, and left to crystallize. The crystals have all the same aspect, lustre, form, and angles; and, as in the case of a perfectly constant and homogeneous substance, the salt may be crystallized either wholly or partially any number of times, without the slightest change in the aspect of the crystals. Nevertheless, the mass is really composed of two kinds of crystals differing in solubility, and in their action on polarized light, — one being, indeed, active, and the other inactive. They are very difficult to separate, in consequence of their complete isomorphism. Nevertheless, the active salt is 2½ times more soluble than the inactive; and if the first crystals which separate be recrystallized about twenty times, a product will at length be obtained which has no action on polarized light; and by repeatedly crystallizing the mother-liquor, a solution will ultimately be left containing nothing but the active salt. Lastly, on extracting from these two varieties of the sulphamylate, the amylic alcohol of which they contain the elements, it is found that the more soluble salt yields an amylic alcohol which rotates the plane of polarization to the left, and to the amount of 20° in a tube fifty centimetres long, while the less soluble salt yields an amylic alcohol which has no perceptible action on polarized light.

The comparative study of these two alcohols exhibits many points of interest. Every reäction that can be performed with the one, may likewise be produced with the other, under the same circumstances; and the resemblance of the resulting products often approaches nearly to identity, without ever actually attaining it. Moreover, the active alcohol always gives active products, and the inactive alcohol inactive products, provided we do not go as far as the radical $C_{10}H_{11}$, in which reside the dissymmetry of the molecules and the action on polarized light. One of the most remarkable differences exhibited by the two alcohols is in their densities. The active alcohol is heavier than the other, and the difference amounts to nearly $\frac{1}{100}$. Consequently, equal volumes of the two alcohols do not contain equal numbers of molecules,— those of the active alcohol being more crowded than those of the other; and the difference is considerable for a phenomenon of such a nature. The active alcohol boils at 127° to 128° C., under the ordinary pressure, and the inactive alcohol at 129°. The mixture of the two boils at intermediate temperatures, and not at 132°, as is commonly stated.

THE CHEMISTRY OF CAUSTICS. BY WILLIAM BASTICK.

Of all the applications of chemistry to the sciences of medicine and surgery, there is not one which has been so little studied or written upon as the Chemistry of Caustics. Having recently had my attention called to this fact, while making some investigations into the nature of caustics, and especially their mode of action, I propose to lay briefly before those interested in this subject the conclusions arrived at, however fallacious the labors of future and abler investigators may prove them to be.

It seems to me that caustics, with reference to their action, may be divided into two great classes, namely, one which comprises those which merely kill or destroy the vitality of the living tissue; and the other, which includes those which not only destroy the vitality of the living tissue, but decompose or dissolve the tissue, whether dead or living.

As examples of the former class, may be enumerated chloride of zinc, sulphates of copper and zinc, bichloride of mercury, etc.; and as examples of the latter class, may be mentioned caustic potash, nitrate of silver, manganese cum potassa, chromic acid, etc.

Another distinctive feature of these two classses is, that, while the latter destroys and decomposes the living or dead tissue, the former, having killed the living tissue, acts afterwards as a powerful antiseptic or preservative of it.

It is not within my province to point out to those extensively employing caustics, to whom these facts may be new, the importance of bearing in mind this distinctive feature between the two classes of caustics, when selecting the description of caustic to be employed in any given case.

Although caustics may be conveniently divided in the manner described into two principal classes, these classes can be further subdivided into many others, because the mode of action is frequently distinct in each individual case, whatever the final result may be on the living tissue.

To illustrate this point, the modes of action of caustic potash and chromic acid may be cited. When the living tissue is placed in contact with caustic potash, the destruction of its vitality ensues by the potash dissolving its albuminous and fibrinous components; — in fact, acting in the manner described by chemists for obtaining the various protein substances from organic matter. Of course I only allude to the leading features of the action of caustics in this instance as well as in others. When the same tissue is treated, chromic acid, instead of obtaining a solution of the protein compounds of the tissue, and thus destroying its organized structure, the tissue is destroyed by a slow process of combustion; or, in other words, it is oxidized at the expense of the oxygen of chromic acid, by reason of the facility with which that acid parts with its abundant oxygen when in contact with organic bodies. The manganese with potash acts in a similar way as a caustic to chromic acid, but in consequence of the permanganic and manganic acids which it contains being in combination with the base potash, its action is more controllable and persistent. It may not be here out of place to mention, what appears to me to be a practical advantage, that the destructive caustics, if I may so term them, possess over the conservative ones. In doing so I beg to state, once for all, that I offer my opinions on such points with great diffidence, knowing that chemistry is not medicine or surgery, but only one of their instruments. The practical advantage is this: When the surgeon desires the removal of the diseased tissue by caustics, if he uses a conservative caustic he kills the tissue, but has to effect its separation by a further process of suppuration, etc.; whereas, if he employs a destructive caustic, the processes are in simultaneous action, and the desired result is, consequently, more speedily accomplished.

Nitrate of silver is essentially an oxidizing caustic, but its action is much slower than that of chromic acid or manganese with potassa, from the circumstance that it does not so readily part with its oxygen; and it forms an insoluble compound with organic structures, which acts as a preventative to its continuous power as a caustic, by forming a sort of impermeable coating on the tissue to be removed. I am aware that this action is an advantage where hemorrhage is to be feared.

The exsiccated sulphate of zinc and copper, when employed as caustics, act like chloride of zinc by their powerful affinity for water. But when the vis vitæ is destroyed by such affinity, their further action is that of strong antiseptics, thereby greatly, if not entirely, retarding the natural disruption of tissues which have ceased to possess vitality. Bichloride of mercury, and, in fact, all mercurial caustics, possess a conservative action, by their strong affinity for the albuminous components of organic structures, with which they form compounds of definite character.

Nitric and sulphuric acids belong to the class of destructive caustics; the action of the former is that of the oxidation of the tissues, while the latter owes its power as a caustic to its power of extracting the elements of water from organized bodies, — behaving like the exsiccated salts previously mentioned, with which it is sometimes judiciously combined to prevent the spreading of the acid beyond the parts to be destroyed by reason of its fluidity when uncombined.

Chloride of gold has been extensively employed, generally in combination with other caustics, in some of the continental hospitals. When placed in contact with organic matter, this salt is reduced to a metallic state similar to the action of nitrate of silver; but, as far as my experience goes, it is inferior as a caustic to the silver salt, because of the large quantity of oxidizing material which is set free when the organic matter is treated with nitrate of silver. Among the conservative caustics, arsenic and its compounds will find its proper class; for although arsenic is poisonous to living tissue, it is a powerful antiseptic agent. It forms no combinations with dead or living tissue, and only a feeble one with albuminous matter: and from this cause it must be regarded, in a chemical point of view, as a very inefficient caustic.

Chlorides of antimony and iron, which have been used as caustics, exhibit a mode of action similar to chloride of zinc. The very feeble action of the latter must, in some cases, be its principal recommendation.

It will be evident from the previous statements that chemistry will supply us with an indefinite number of caustics; for it is clear that whatever decomposes or combines with living tissue sufficiently to kill it, is, to all intents and purposes, a caustic. It is equally manifest that, while it is the essential condition of every substance professing to be a caustic, that it should kill the living tissue, it by no means follows that all caustics performing this condition should destroy or dissolve away, as it were, the tissue when no longer possessing life, for this latter property belongs to a distinct class of caustics.

I am aware that I have not noticed the so-called irritant action of caustics; but, in explanation, I reply, that the consideration of this action is foreign to the purpose of this communication, and, moreover, a subject not within the province of the chemist. — *Med. Times and Gazette.*

THE CHEMISTRY OF GUNPOWDER.

Although the processes involved in the combustion of gunpowder, and determining its mechanical action, would appear to be very simple, according to what is at present known of this subject, still our acquaintance with the precise nature of these processes is exceedingly slender and imperfect. The consideration of the subject, since its first, and still most important experimental investigation, undertaken by Gay-Lussac more than thirty years ago, has led to such discordant results, that, at the present time, there is no chemical view of it that is at all consistent with experience. The normal composition of gunpowder may be represented in chemical proportions, by

$$C_3 + S + KO, NO_5,$$

or one equivalent of nitrate of potash, one equivalent of sulphur, and three equivalents of carbon (charcoal). Supposing the whole of the carbon to be converted into carbonic acid, and the nitrogen to be liberated, $C_3 + S + K\,O$

$N\,O_5 = 3\,C\,O_2 + N + K\,S$, 100 grains would furnish 130 86 cubic inches of gas at the temperature of 32° Fahr., and under an atmospheric pressure of twenty-nine of mercury.

10 grains of gunpowder.			Yields by Combustion.	By weight.		By measure 13·086 cubic ins.
Carbon	0·1332	=	Carbonic acid	0·4885	=	9·823
Sulphur	0·1184		Sulphide of potassium	0·4078	=	—
Nitre	0·7484		Nitrogen	0·1037	=	3 263

The volume of the gas would be the same if the carbon were converted into carbonic oxide instead of carbonic acid; and if, instead of nitrogen being liberated, binoxide of nitrogen were formed; and since the gas produced by the explosion of gunpowder consists, as a general rule, solely of carbonic acid, carbonic oxide, nitrogen, and nitric oxide, with mere traces of hydrogen and sulphuretted hydrogen, it would appear that 131 cubic inches is the largest volume of gas that 100 grains of normal gunpowder could possibly furnish. However, the experiments of Gay-Lussac, and those of most subsequent investigation, indicate, on the contrary, that the volume of gas furnished by the explosion of gunpowder under the ordinary atmospheric pressure is much greater than that which could be furnished according to the above view. Messrs. Bunsen and Schischoff, of Heidelberg, have, therefore, directed their attention to this important point in theoretic artillery, and have published a memoir on the subject, which not only furnishes the first complete solution of the problems involved in the theory of the explosion of gunpowder, but which is particularly rich in new methods of investigation and analysis. The authors, from want of time, were obliged to confine their researches to one kind of powder, and to its combustion under the ordinary atmospheric pressure. The methods employed, however, will apply, with slight modifications, to other cases. The powder gave, on analysis —

Saltpetre		78·99
Sulphur		9·84
Carbon,	Carbon	7·69
	Hydrogen	0·41
	Oxygen	3·07
	Ash	*trace.*
		100.00

A preliminary qualitative analysis gave the following substances as solid products of the combustion of the powder employed: 1. sulphate of potash, 2. carbonate of potash, 3. hyposulphite of potash, 4. sulphid of potassium, 5. hydrate of potash, 6. sulphocyanid of potassium, 7. nitrate of potash, 8. carbon, 9. sulphur, 10. carbonate of ammonia. The gaseous products were 1. nitrogen, 2. carbonic acid, 3. carbonic oxide, 4. hydrogen, 5. sulphid of hydrogen, and occasionally 6. nitric and nitrous oxide. By means of a particular apparatus, the authors were able to prepare the solid residue of the explosion, the smoke or vapor which accompanies it, and the gaseous products, and to analyze each separately. For the elaborate details of the analysis we must refer to the original memoir. The solid residue was found to contain —

Sulphate of potash	56·62
Carbonate of potash	27·02
Hyposulphite of potash	7·57
Sulphid of potassium	1·06

Hydrate of potash	1·26
Sulphocyanid of potassium	0·86
Saltpetre	5·19
Carbon	0·97
Sulphur	*trace.*
	100·52

The condensed smoke, or powder-vapor, was found to contain —

Sulphate of potash	65·29
Carbonate of potash	23·48
Hyposulphite of potash	4·90
Hydrate of potash	1·33
Sulphocyanid of potassium	0·55
Nitrate of potash	2·48
Carbon	1·86
2-3 Carbonate of ammonia	0·11
	100·00

These analyses show clearly that the residues of the explosion of gunpowder consist essentially of sulphate and carbonate of potash, and not of sulphid of potassium, as is erroneously assumed in military books. The gaseous products of the explosion were found to be —

Carbonic acid	52·67
Nitrogen	41·12
Carbonic oxide	3·88
Hydrogen	1·21
Sulphid of hydrogen	0·60
Oxygen	0·52
	100·00

From this analysis it also appears that the old theory of the explosion is incorrect, since, according to it, nitrogen and carbonic acid should be present in the ratio of 1 : 3; whereas the actual ratio is not even as 1 : 1·5. The authors found, further, that one gram of the powder used by them gave 0·6806 gr. of solid residue, and 193·1 c. c. of gas, or one-third less than should be given according to the old theory. To determine theoretically the work done by the explosion, the authors first determined the quantity of heat evolved in the combustion, which was found to be in heat units 643·9 C. for one gram of powder. This number requires, however, to be corrected for the heat evolved by the combustion of the gaseous products in the combustion-tube filled with air, amounting to 24·4 units, so that the true heat of combustion is 619·5 C. The calculated quantity of heat, supposing that the combustible ingredients of the powder burn freely in oxygen, is 1039·1 C.: the difference is easily explained by the absorption of heat on the part of nitrogen set free. The temperature of the flame of the powder is found by dividing the number 619·5 by the specific heat of the products of the explosion, namely 0·206, and is therefore equal to 2993° C. When, however, powder burns in a space in which it cannot expand, the number 619·5 must be divided by the specific heat of the products of the combustion under a constant volume, which is 0·18547, and the temperature of the flame is therefore 3340° C. These values, 2993° C. and 3340° C., are approximations which cannot be far from the truth. The authors, in the next place, show that the received theory that the solid residue of the explosion of the powder exert a sensible tension, is wholly unfounded; since this residue does not boil at the temperature of hydrogen burning in air, which is 3259° C., and its tension even at that temperature cannot, therefore, equal a single atmosphere. The authors next calculate the pressure in atmospheres which the powder exerts at the moment of explosion, and find it 4373·6, of which about 1000 are due to the expansion of the solid residue. The pressure which the powder employed could exert upon the sides of a cannon, would, as these results show, never exceed 4 1-2 thousand atmospheres. Hence it appears that the statements of the best authorities on artillery, that this pressure amounts to 50,000 or 100,000 atmospheres, are entirely without foundation in fact. One kilogram of the powder employed could exert in its explosion a theoretical work of 67410 meterkilograms. — *Pogg. Ann.*, cii, 321.

ALCOHOL FROM SORGHUM.

M. Leplay, of France, has recently published the results of a protracted research upon the sorghum. He has recognized: 1st, that the quantity of solid matter which the stems of sorghum give in drying augments gradually and quite regularly, from the formation of the panicle (flowering) to the maturity of the grain, whatever the soil; 2d, this solid matter accumulates in the juice and not in the insoluble part of the plant; 3d, when the stem is still green, and the panicle scarcely formed, it contains only a little sugar; the sugar is developed as the plant advances and the grain approaches maturity, and hence the composition of the stem and the production of the saccharine matter depend entirely on the state of the plant, and not on the epoch of its cultivation; 4th, in the juice of the sorghum, before its maturity, in which the saccharimeter can discover little or no sugar, fermentation indicates between thirty-two and one hundred grammes of sugar per litre; but as the grain advances towards maturity, there is a gradually increasing deviation to the right in the polarizing apparatus, caused by the cane sugar present; and when the grain is ripe, the quantity of cane sugar indicated by the polarizing apparatus is but little less than the sugar indicated by the fermentation.

On desiccation, the sorghum loses seventy per cent. of its weight. In this state it can be preserved, and so used in the manufacture throughout the year. Mr. Leplay carries on the fabrication with little cost, and observes that it is easily done with apparatus that may be carried from place to place.

EXAMINATION OF ALCOHOLIC LIQUIDS TO ASCERTAIN THEIR ORIGIN. BY M. MOLNAR.

According to this author, this process is applicable to alcoholic liquids which have apparently no foreign odor. It consists in introducing sixty grammes of the spirit to be examined into a flask containing two or three decigrammes of caustic potassa dissolved in water. It is well agitated, and the whole is subjected to evaporation until only five or six grammes remain. Then the residue is put into a bottle with a glass stopper, and about five grammes of dilute sulphuric acid are added; the characteristic odor is immediately diffused; this is especially true with regard to spirit from grain and from beet-root.

M. Molnar mentions incidentally that he has always succeeded in purifying spirituous liquors, and freeing them from their essential oils, by using caustic potassa concurrently with recently-calcined wood charcoal.—*Dingler's Polytechn. Journal.*

ON THE CHEMICAL CHANGES OF THE GLUCOSE OF THE SORGHUM.

At a recent meeting of the Boston Society of Natural History, Dr. A. A. Hayes read the following paper "On a chemical change which takes place in the Glucose of the Sorghum:"

In a paper communicated some months since, I alluded to the fact, that the glucose of the sorghum cultivated in New England, like fluid fruit sugar, passes to the condition of dry, or crystalline fruit sugar. The subsequent more careful investigation of this change, proceeding indeed during many months, has resulted finally in the *production of pure glucose of sugar, having the higher grade of a variety of beet-root, or cane sugar.*

In the account which follows, the experiments were made on the glucose of that variety of sorghum which has dark purple seed coverings, — the variety generally cultivated in the Northern States.

When we extract the saccharine matter of the stalk of the sorghum, either by expression, or through the aid of water, and purify the solution by means of animal charcoal, we obtain glucose, holding in solution some salts of potash, lime, and soda. This glucose does not afford crystals by evaporation in desiccated air, nor does alcohol, saturated with cane sugar, leave undissolved any sugar.

The perfectly formed cells of the plant, triturated with animal charcoal, afford to boiling alcohol the same substance. The dry glucose is abundantly soluble in alcohol of 86 per cent., and the dense syrup of the same dissolves without limit in it. After exposure in warm air, crystalline concretions, resembling dry grape sugar, form in isolated masses. Analysis shows a large proportion of saline matter, composed of phosphoric acid, chlorine, sulphuric acid, acetic acid and potash, soda, lime, and oxide of iron. This saline matter forms a compound with the glucose, and thus makes up the crystalline grains, which first appear in the dense syrup. These are constant results, in treating the plant which has been cultivated the two past seasons, and they present no remarkable feature, in comparison with those obtained on glucose from other sources.

After the lapse of several weeks, however, the pure glucose which has been withdrawn from the foreign aggregates exhibits the production of crystalline points, which, becoming numerous, soon assume the forms of regular crystals. These crystals increase in volume, but while forming in the glucose they present skeletons, rather than solid crystals, of a pure substance, and are often grouped. Crude syrup, remaining after the concentration of the juice by rapid boiling, undergoes the same modification, and crystallized sugar slowly separates from samples which did not originally contain any.

Slips of the pith of the plant, which had been carefully examined under the microscope, without any traces of crystals being found, after some months, show their cells filled with voluminous, dry crystals. Repeated trials prove that the chemical change, resulting in the production of the crystals, from glucose, is not dependent on exposure to air and loss of water, but it takes place when the syrup is kept closed in bottles.

As the glucose is abundantly soluble in alcohol of 90 per cent., this agent enables us to learn at any moment the production of sugar in a sample, — the sugar, when formed, being nearly insoluble in cold alcohol. Thus, when a certain number of crystals have formed, if we withdraw by solution in alcohol the unchanged glucose, and after dissipating the alcohol, allow it to repose, crystallization recommences in the portion removed, and repetitions of this experiment may be made, until after about ten months, small portions only of unaltered glucose remain.

Although the evidence of the conversion of the glucose, step by step, into sugar, afforded by the action of alcohol, is important, the observations here recorded are based upon experiments made in a similar manner, with the alkaline solution of tartrate of copper, and acidulated alcohol saturated with cane sugar; they leave no doubt that the normal saccharine juice of the plant becomes, *per se*, converted into sugar, forming regular crystals of large size. These crystals, by solution in water, are easily purified, losing their porous structure and becoming solid, transparent, and colorless modifications of the rhombic prism from an aqueous solution. They are always apparently more voluminous than the crystal of cane sugar, formed under like circumstances, but they have all the brilliancy of cane sugar. In chemical characters, the most pure crystals yet obtained show a diversity when compared with our cane or palm sugar. They are less soluble in water; in sulphuric acid they do not exhibit the same depth of coloration that sugar-cane does. With the copper test, a partial reduction takes place, under the same conditions, where cane sugar does not produce change on this agent.

The conclusion reached is, that this sugar, wholly unlike any variety of glucose or fruit sugar, belongs to a higher class, and probably will rank with beet sugar, in most of its characters.

The present is the first instance, within my knowledge, of the conversion of any variety of glucose into sugar of high grade, after its extraction artificially.

Dr. Hayes further remarked, that neither the crystalline form, nor the action of the polariscope, are to be relied upon in distinguishing the varieties of sugar. He considered the crystalline form of the sorghum sugars, and the honey sugars to be identical; the chemical properties of the sugars he regarded as the only distinct proof of the several varieties.

ON THE ACTION OF ANÆSTHESIA.

At a recent meeting of the N. Y. Academy of Medicine, Dr. Detmold remarked that members would recollect that, about the year 1847, he called the attention of the academy to certain propositions, which he then made, proving quite conclusively that carbonic acid gas is the efficient agent in causing anæsthesia. The carbonic acid may be given as such, or one of its chemical ingredients may be so administered, that, finding in the blood the other constituents of this compound, carbonic acid gas is generated, and anæsthesia, to a certain extent, is the result. Thus we may administer oxygen in large quantities, in the form of nitrous oxide (protoxide of nitrogen, or laughing-gas), which has all the chemical reäctions of oxygen, but is much more soluble in water and the serum of the blood than pure oxygen, and, therefore, is much more readily taken up. This compound, meeting with the carbon of the blood, carbonic acid gas is formed in large quantities, with the production of anæsthesia to a certain extent. Or we may, on the contrary, administer the carbon, as the oxide of carbon or any of the hydro-carbons, alcohol, the ethers, etc.; in this case the blood again furnishes the other constituent of carbonic acid, oxygen, and anæsthesia is again the result.

The stage of excitement corresponds to the period of combination of these elements and the formation of carbonic acid gas. If the gas is administered as such, there will be no stage of excitement; but if the constituents combine slowly, and the gas is generated in limited quantities, there will be a corresponding state of excitement. Thus, in the stupor of drunkenness, carbonic acid is exhaled in normal quantities; but as the stupor passes off, large quantities of that gas are exhaled. The venous state of the arterial blood, during anæsthesia, is another proof that carbonic acid is being generated in large quantities. If it is true that in *post-mortem* examinations of those dying while under the influence of chloroform, bubbles of air are found in the heart and blood-vessels, it is highly probable that this air is carbonic acid gas, unless, perchance, it has entered the circulation by some mechanical lesion.

The only means, in his opinion, of any avail in restoring a patient from profound or fatal anæsthesia, is artificial respiration, or such other means as, by exciting reflex action, will restore respiration, and thus hasten the elimination of the carbonic acid gas. It has been recommended in threatened and apparent death from anæsthesia, to resort to the inhalation of oxygen or nitrous oxide. Reasoning from the premises which he had given, such remedies would be in the highest degree dangerous. To satisfy himself in regard to this fact, he had made numerous experiments upon animals, and invariably found a fatal issue hastened by administering oxygen. — *New York Journal of Medicine.*

At a subsequent meeting, Dr. Detmold favored the academy with a written exposition of his views of the rationale of the action of chloroform, sulph. ether, and nitrous oxide, the three agents employed for the purpose of producing anæsthesia. He attributes the action of all of them to the production of carbonic acid gas *in the system*. The first two supply the carbon, which, absorbing oxygen from the blood, and the last supplying oxygen, which, absorbing carbon, — in either case carbonic acid is the result; which, by its action on the living organism, produces anæsthesia. This theory, though not absolutely susceptible of demonstration, is yet, apparently, based on a logical foundation, and finds a seeming confirmation in a number of well-known facts.

ANESTHESIS BY CARBONIC ACID.

In the Annual of Scientific Discovery for 1858, page 320, some account of experiments instituted by Drs. Follin and Simpson, on the use of carbonic acid for local anesthesis, was given. We have now to report on the effects of its inhalation when mixed with a certain quantity of air, as observed by M. Ozanam, who proposes its use for man. He states, that the effects of carbonic acid are similar to those of ether, but more fugitive; and while, with the latter, the inhalation should be interrupted at intervals, with carbonic acid, the reverse is true. He affirms, that as long as it is wished to prolong the sleep, the inhalations should be continued; that they may be continued ten, twenty, thirty minutes or more, without danger to life. On ceasing the inhalation, waking is almost immediate. In these experiments no case of sudden death has been observed, as in chloroform. When death is in prospect, it comes slowly, and may be foreseen for a long time in advance, and its progress noted, by the state of the head and that of the pupils of the eyes. The following experiment, reported by M. Ozanam, is highly interesting:

"I prepared a gas-bag, containing 100 litres of carbonic acid, and resolved to continue the anesthesis as long as was possible. The animal was asleep at the end of three minutes, without convulsions, and so remained, extended on its side. The inhalations were continued for eighty-seven minutes, and then the apparatus was withdrawn. The sleep was prolonged afterwards for five minutes; towards the tenth minute the legs commenced to shake; at the fifteenth the animal was up again; and thus 102 minutes were used in the whole experiment — a length of time much beyond what the longest operations would require."

Messrs. Ferre and Ozanam have respired the gas several times — if not to the production of sleep, at least to feeling its first effects. They say its taste is slightly pungent, and as agreeable as that of ether; it excites salivation. They propose its adoption, in surgical practice, as the least dangerous method, and as sufficiently efficacious. — *Correspondence of Silliman's Journal.*

INQUIRIES INTO THE QUANTITY OF AIR INSPIRED THROUGHOUT THE DAY AND NIGHT, AND UNDER THE INFLUENCE OF EXERCISE, FOOD, MEDICINE, TEMPERATURE, ETC.

A communication on the above subject, to the Royal Society, by Edward Smith, M. D., contains the results of 1200 series of observations. The author himself was the subject of all the investigations. He is 38 years of age, 6 feet in height, healthy and strong, and with a vital capacity of the lungs of 280 cubic inches. From this communication the following facts are derived:

The total quantity of air inspired in 24 hours (allowance being made for intervals, amounting, altogether to 40 minutes, during which it was not recorded), was 711,060 cubic inches; or an average of 29,627 cubic inches per hour, and 493·6 per minute. The quantity was much less during the night than during the day. There was an increase as the morning advanced, and a decrease at about 8^h 30^m P. M., but most suddenly at 11 P. M. During the day, the quantity increased immediately after a meal, and then subsided before the next meal; but in every instance it rose again immediately before a meal. The rate of frequency of respiration generally corresponded with the quantity, but the extremes of the day and night rates were greater. The period of greatest parallelism was between tea and supper. An increase was occasioned by one meal only, namely, breakfast. The average depth of respiration was 26·5 cubic inches, with a minimum of 18·1 cubic inches in the night, and a maximum of 32·2 cubic inches at 1^h 30^m P. M. The mean rate of the pulse was 76 per minute; the minimum at 3^h 30^m A. M., the maximum at 8^h 45^m A. M.; — the difference being more than one-third of the minimum rate.

Sleep came on in two of the series of continuous observations, and the time of its occurrence was also that of the lowest quantities of air inspired.

The amount of breathing was greater in the standing than in the sitting posture, and greater sitting than lying. It was increased by riding on horseback, according to the pace, also by riding in or upon an omnibus. In railway travelling, the increase was greater in a second than in a first-class carriage, and greatest in a third-class and on the engine. An increase was also produced by rowing, swimming, walking, running, carrying weights, ascending and descending steps, and the labor of the tread-wheel; and in several of these cases the rate of increase was determined for different degrees of exertion used. Reading aloud and singing, and the movement recommended by Dr. Hall for restoring suspended respiration, increased the quantity; bending forward, whilst sitting, lessened it.

The quantity of inspired air was increased by exposure to the heat and light of the sun, and lessened in darkness. Increase and decrease of artificial heat produced corresponding effects; and the depth of respiration was greatly increased by great heat. An increase in quantity was caused also by cold bathing, and sponging, and the cold shower-bath; by breakfast, dinner, and tea — when tea actually was taken; but when coffee was substituted, there was a decrease. Supper of bread and milk also caused a decrease. Milk by itself, with suet, caused an increase.

An increase was obtained with the following articles of diet, viz.: eggs, beefsteak, jelly, white bread (home-made), oatmeal, potatoes, sugar, tea, rum (1 oz). The following caused a decrease, viz.: butter, fat of beef, olive-oil, cod-liver oil, arrow-root, brandy (1 oz. to $1\frac{3}{4}$ oz.), and kirchenwasser. Ether ($\frac{1}{2}$ drachm) increased the quantity and depth of inspiration. A decrease in quantity was caused by sp. ammon. co. (ʒiss), sp. ammon. fœt. (ʒiss), tincture of opium (20 ♏), morphia ($\frac{1}{6}$ and $\frac{1}{8}$gr.), tartarized antimony ($\frac{1}{2}$ gr.), and chlorid of sodium.

Carbonate of ammonia (15 grains) caused a small increase, at first, and then a small decrease; febrifuge medicines had a like effect. Chloroform (25 ♏ and ʒss), by the stomach, varied the quantity from an average increase of 28 cubic inches to an average decrease of 20 cubic inches per minute, with a maximum increase of 63 cubic inches per minute. Chloric ether (℥ss) also varied the quantity, but there was an average increase of 17 cubic inches per minute, and of 1·8 per minute, in the rate; whilst the pulse fell, on the average, 1·7 per minute. Chloroform, by inhalation (to just short of unconsciousness), lowered the quantity a little during the inhalation, and more so afterwards. The rate was unchanged, but the pulse fell, on an average, 1·7 per minute. Amylene, similarly administered, and to the same degree, increased the quantity, during inhalation, 60 cubic inches per minute, but afterwards decreased it to 100 cubic inches per minute less than during the inspiration.

DETECTION OF ARSENIC AND ANTIMONY.

Dr. Odling has ascertained that $\frac{1}{500}$ grain of arsenious acid may be detected with certainty by means of Reinsch's test, and that the metallic deposite, crystalline sublimate, and yellow sulphide, may be obtained successively. He gives the preference to fine copper gauze for the precipitation of the arsenic, and conducts the sublimation in a hard glass tube, two inches long, one-eight inch diameter, sealed at one end, and drawn out at the other end to about an inch, almost capillary. He finds that decisive results are obtained when the dilution amounts to 2,250,000 times the weight of arsenious acid. Protracted ebullition seems to be a necessary condition of the deposition of arsenic, particularly when the quantity is small or the degree of dilution great. It has been urged as an objection to Reinsch's test, that during the ebullition with hydrochloric acid, arsenic is volatilized as chloride; but Dr. Odling does not consider this fact is of any consequence in practice, as the loss is inappreciably small, and might be provided against by using a small retort for the operation.

It is generally believed that Reinsch's test is applicable only for the detection of arsenical compounds that are dissolved by dilute hydrochloric acid. In cases of poisoning, it is not unfrequent that the whole of the arsenic is converted, by the decomposition of the tissues, into tersulphide, which is generally represented as being insoluble in dilute hydrochloric acid, and consequently the arsenic would not be extracted from the organic substance and tissues by boiling with dilute hydrochloric acid; however, Dr. Odling has found that the precipitated tersulphide of arsenic is readily dissolved by very dilute hydrochloric acid, and even by boiling water, to a much greater extent than was observed by Dr. Christison.

He finds also that the deposits obtained from arsenic and antimony resemble each other very closely; but that this is not of any consequence in practice, owing to the ease with which the arsenical deposit is distinguished from that produced by arsenic.

With regard to the detection of antimony by means of Reinsch's test, he finds that bismuth, and even tin, will yield metallic deposits which cannot safely be distinguished from that obtained from antimony, by the appearance only.

The characters of the bismuth deposit are somewhat peculiar; when thin, it approximates closely in appearance to that obtained with antimony. The deposit obtained with tin differs much according to circumstances: when thin, it has a peculiar dotted appearance; sometimes it is almost black; sometimes steel-blue. When heated, it sometimes appears to diminish considerably. It would, moreover, be produced only when the amount of metal in solution was so large as not to present any difficulty in detecting it.

Dr. Odling suggests the following as a delicate method of confirming the indication of antimony:

The coated copper is covered with a solution of one grain permanganate of potash in fifteen ounces of water, a drop or two of potash solution added, and the whole boiled. In a few minutes the permanganate is decomposed, the antimony passes into solution, and may be precipitated by means of sulphuretted hydrogen from the solution acidulated with hydrochloric acid.

Excretion of Arsenic and Antimony in the Urine. — Dr. Kletzinsky, as the result of his investigations upon the expulsion of metals in the secretions, comes to the following conclusions: 1. The presence of a small quantity of albumen in arsenical urine is indubitable, but it is problematical in antimonial urine. The excretion of both metals may take place in the form of their alkaline salts. 2. The excretion takes place a short time after poisoning by arsenic, more quickly than in antimony poisoning, and continues uninterruptedly until death or recovery,— the excretion of antimony continuing usually longer than that of arsenic. 3. That in its forensic relation, the analysis of the urine in arsenic or antimony poisonings, providing the patient live for from twelve to twenty-four hours, is capable of furnishing a complete negative or positive conclusion.— *Wien Wochenschrift*, No. 8.

Source of Error in Marsh's Test for Arsenic. — The following is a *resumé* of a paper read before the French Academy of Medicine, by M. Blondlot, on a source of error in making use of Marsh's method of detecting arsenic.

It is known that Messrs. Flandin and Danger have proposed to modify the method of Marsh by applying sulphuric acid to carbonize the organic matters suspected of containing arsenic. M. Blondlot, while examining the stomachs of three persons who had been poisoned by arsenious acid, found on the mucous membrane small fragments of this acid, which superficially were of a fine yellow color. He thought that this yellow matter resulted from the formation of a sulphide of arsenic, resulting from the action of hydrosulphuric acid on the arsenious acid, and ulterior experiments proved the correctness of this view. As hydrosulphuric acid is formed in putrefied organic matters, if they contain arsenic, the sulphide of arsenic will be formed; and M. Blondlot shows that employing sulphuric acid to carbonize these matters, will leave with the carbonized parts the sulphide of arsenic, so that a portion of the poison will be lost. A committee of the Academy has ascertained the exactitude of the assertions of M. Blondlot; and to avoid this source of error, they propose to throw upon the carbonized remnant a great deal of concentrated and boiling nitric acid, so as to transform the sulphide of arsenic into arsenious acid. By distilled water the excess of nitric acid is then expelled, and the arsenic may be easily found by the method of Marsh.

SENSIBILITY OF THE PRINCIPAL REAGENTS ON STRYCHNINE.

Professors De Vry and Der Burg, of Rotterdam, have recently published the result of a series of experiments, showing the sensibility of the principal reägents used for the detection of strychnine, when administered as a poison. Their conclusions are as follows:

Chromate of Potash, or Ferricyanide of Potassium and Concentrated Sulphuric Acid. — By these reägents $\frac{1}{60000}$ of a grain of strychnine can be detected, if one drop of a solution, containing one grain of strychnine in 60,000 grains of water, is evaporated in a small porcelain dish on a water-bath, and the remaining substance moistened with the smallest possible quantity of pure concentrated sulphuric acid. By introducing in this solution a *very small* fragment of a crystal of bicromate of potash or ferricyanide of potassium, and moving this fragment with a glass rod in the solution, a beautiful dark purple color is produced on every part of the surface of the porcelain that has been in contact with the acid solution, and the fragment of one of the two salts.

Bin-iodide of Potassium, and Iodide of Mercury and Potassium: By a solution of one of these compounds, $\frac{1}{50000}$ of a grain of strychnine can be detected. These reägents, like the following, possess only the ascertained sensibility, provided the drop of liquor is contained in a capillary test-tube, in which the liquid, although only a drop, forms a small column, in which the formation of a precipitate can be observed by comparison with a similar capillary tube filled with pure water, and mixed with the reägent.

Tannic acid reveals $\frac{1}{50000}$ of a grain of strychnine.

Solution of chlorine in water, $\frac{1}{3000}$.

Sulphocyanide of potassium, $\frac{1}{3000}$.

Neutral chromate of potash, $\frac{1}{3000}$.

The precipitate formed by bin-iodide of potassium is brownish-red, and if dissolved in weak, warm spirit, acidulated by sulphuric acid, beautiful crystals are formed of sulphate of iodo-strychnine, which polarize the light, as has been discovered by Mr. Herapath. The precipitates formed by iodide of mercury and potassium, by tannic acid, and by solution of chlorine in water, are white. This last reägent must be used in relative large quantity, and the precipitate formed by it does not appear immediately.

The precipates formed by sulphocyanide of potassium and neutral chromate of potash are both crystalline. The color of the former is white, and the form of the crystals observed by the microscope is very characteristic. The color of the latter is a beautiful yellow. The formation of both these precipitates is accelerated by rubbing the surface of the tube with a glass rod.

The precipitate formed by chromate of potash gets immediately a dark purple color, if moistened by concentrated sulphuric acid. All the other precipitates get the same color, if they are dissolved in a small quantity of strong sulphuric acid, and the solution brought into contact with a fragment of a crystal of chromate of potash or ferricyanide of potassium.

The authors also give as their opinion, founded on experiments and investigation:

That if death has been caused by strychnine, this poison can be detected in the body, provided it has been administered in a quantity *more than sufficient* to cause death.

That if the poisoning by strychnine has been chronic, and has resulted from a quantity not greater than just necessary to cause death, the cause of this death *cannot be proved*, either by the *post-mortem* examination of the body, or by a chemical investigation of the intestines.

That it appears to be highly probable that that part of the strychnine which acts mortally is decomposed in the living body.

That the urine of patients who take strychnine or its salts as a medicine, contains not a trace of this poison.

Mr. Maxwell Simpson, in a communication published in the Dublin *Hospital Gazette*, states that strychnia is not the only substance that will produce a purple color when brought into contact with a mixture of bichromate of potash and oil of vitriol,—the test usually relied on for the detection of this poison. I have found that naphthalidam, an organic base derived from naphthalin, will produce, as might have been expected, an exactly similar color when tested with the same mixture. The color is best brought out by making a mixture of equal volumes of oil of vitriol and a cold saturated solution of bichromate of potash, and bringing it, while still warm, into momentary contact with a particle of the naphthalidam.

ANTIDOTE FOR STRYCHNINE.

The Rev. S. Haughton, in a communication recently made to the Royal Irish Academy, on the properties of nicotine and strychnine, stated:

"That he was induced to make the experiments by the consideration of the specific actions of strychnine and nicotine upon the muscular system, which appeared to be so opposite in their character as to lead him to a conviction that they might prove to be equally antidotes to each other's action. It is generally believed that strychnine exerts a specific action upon the lower or lumbar portions of the spinal column, exciting the muscular system (at least the voluntary muscles) into a state of tetanic contraction, and ultimately producing death indirectly, by rendering respiration mechanically impossible, by virtue of the permanent contraction of the pectoral muscles, and not, as was once supposed, by its action upon the heart. It is also well known that the most powerful agent we possess for relaxing the action of the muscles is nicotine, whether administered in the form of tobacco smoke or the infusion of the leaves. From these well-known facts he (Mr. Haughton) was led to believe that these powerful poisons might be used as antidotes to each other's action; and with the view of testing this conjecture, he made several experiments. Four of these went to show the effects of the two poisons separately. The fifth and sixth are important, as they appear conclusive to Mr. Haughton as to the action of nicotine in retarding, and in certain cases, completely counteracting the effects of strychnine. In the fifth experiment, a frog had lived for forty-seven minutes in a mixture of two solutions, of which one would have destroyed life in four minutes, and the other would have produced paralysis in one minute, and destroyed life in twenty-three minutes; and yet in the mixture the animal had lived for forty-seven minutes, and afterwards for twenty-four hours. In the sixth experiment, a frog immersed in a similar mixture of the poison for ten minutes, had ultimately recovered, — the effects of the strychnine being completely obviated by the action of the nicotine. Mr. Haughton expressed a hope that further inquiries would be instituted into the action of strychnine and nicotine upon some of the warm-blooded animals, as he believed that in nicotine, which was always easily procurable in the form of tobacco-leaf infusion, would be found a valuable antidote in at least some cases of strychnine poisoning, whether intentional or accidental.

ON THE DETECTION OF THE ORGANIC ALKALOID POISONS.

At a recent meeting of the Boston Society of Natural History, Mr. John Green stated that he had detected unequivocal signs of strychnine in the liver of a skunk which was said to have been killed by this poison. The process by which it was found is that proposed by Flandin.

As is well known, there is great difficulty in detecting minute quantities of vegetable poisons in the animal body; and the theory has been advanced, that they are destroyed before reaching the liver.

Dr. A. A. Hayes remarked that the communication of Mr. Green had interested him, and he deemed it highly important, on account of the positive fact of the detection of strychnia, after a moderate dose had caused death. From personal experience, he was led to add his testimony to the efficiency and simplicity of the method of M. Ch. Flandin, for detecting the poisonous alkaloids in the tissues of the organs, as well as in the contents of the stomach.

In some trials, in the way of testing this method, Dr. Hayes stated that, using simple extract of opium, and pursuing morphia and meconic acid, he had been led to the conclusion that the excess of lime might reäct on the alkaloids, at the temperature of the water-bath, and thus reduce the amount of any alkaloid it would be possible to detect. In some cases, too, the dry mass containing the alkaloid with a large excess of lime, even in the state of fine powder, was slowly acted on by alcohol: a great abundance of lime combinations with fatty acids being present. He had found it advantageous, especially when operating on partially digested food in stomach contents, to add a small portion of chloride of calcium solution, which has the power of dissolving lime in excess, when concentrated (Ca. Cl., CaO.), and thus, without diminishing its activity in breaking up protein compounds, allows the dry matter subjected to alcohol, or other solvents of alkaloids, to remain permeable. When baryta or lime is subsequently employed to develop the alkaloid, either with or without ammonia salts, the chloride of calcium does not interfere, and very pure alcoholic or ethereal solutions can at once be obtained.

He added, that he wished to express his accordance with the opinion of Mr. Green, that the chemical experiments should be so conducted in the search after poison which had caused death, as to eliminate from the tissues that portion which had entered them, as being distinct in its effects from the remainder found in the stomach or intestines, uncombined with protein compounds. In cases of poisoning by corrosive poisons, the parts destroyed or altered are easily observed and the results of many experiments had led him to form the opinion, that the organic poisons, in their actions, leave traces only little less strongly marked.

In proof that strychnine is a much more stable body than has been hitherto considered, Mr. Green cited from the *Journal Medicale* (1856) an account of toxicological experiments upon the bodies of various animals which had been poisoned with minute doses of strychnine, in the *tissues* of which that substance had been found. It was recognized after the lapse of months, and even for years; and, in one instance, was found in the bones, and in the material of a wooden box in which an animal had been buried.

COLORATION OF POISONS.

Dr. Moffat, an English chemist, recommends the coloration of poisonous substances with carbo-azotic acid, for the following reasons:

"1. Its coloring power is so great, that one grain is sufficient to impart a distinct yellow color to 70,000 grains, or one gallon of water.

"2. The taste is so intensely bitter, that, in the above proportions, it imparts a very decided bitterness.

"3. Carbo-azotic acid also possesses the valuable property, *which is peculiar to itself*, of giving a yellow color to the skin, as if the person were suffering from jaundice, when taken for three or four days in doses of one grain per diem. This coloration would be easily distinguished from jaundice by any medical man.

"4. The strong color which carbo-azotic acid imparts to food, urine, and fæces, would serve as a proof that poison had been administered, especially when detection of the particular poison employed might be doubtful, as in the case of strychnine and other organic poisons. [Carbo-azotic acid can also be detected in *minute quantities* by the appropriate tests.]

"5. A saturated solution of carbo-azotic acid in prussic acid does not appear to modify the therapeutic action of that potent remedy; and twenty drops of such solution impart a deep yellow color to one gallon of water. [Ten drops *tinge* one gallon.]

"6. The color imparted to water is permanent. A bottle of water taken from SIXTEEN *gallons of water*, to which ONE *grain* of carbo-azotic acid was added on the 21st of March, 1856, *upwards of fifteen months since*, still retains its yellow color.

"7. Carbo-azotic acid does not produce any deleterious effects upon the system. I have administered it until the skin, conjunctiva, urine, and saliva were quite yellow, and a bitter taste felt in the mouth, without perceiving an unfavorable symptom."

BASIC NITRATE OF BISMUTH AS A REAGENT FOR GRAPE SUGAR. BY PROF. BOTTGER.

In testing urine for example, for sugar, if one volume of urine be boiled with an equal volume of a solution of one part of crystallized carbonate of soda in three parts of water, and a very small quantity of basic nitrate of bismuth be added to it, the latter becomes gray by reduction, if grape sugar be present. Cane sugar does not possess this property, and none of the other bodies occurring in the urine blacken the bismuth salt. — *Journ. für Prakt. Chemie*, lxxi. p. 431.

NEW TEST FOR MANGANESE.

Böttger has given us a new reägent for manganese. He states that the minutest quantity may be detected by the chlorate of potash. In order to detect it, throw a small quantity of the material suspected to contain manganese into a test-tube, which already contains the chlorate of potash in a state of fusion. After the combustion has entirely ceased, and the tube is cold, a peach-blossom residue will be left if there has existed the smallest quantity of manganese. By means of this reäction, Böttger has discovered manganese in boxwood, beech, cork, in the iodine of commerce, tea-leaves, and several articles of food.

ON THE DETECTION OF ADULTERATIONS OF THE ETHEREAL OILS.

In 1854, an eminent drug house of Dresden offered a prize for the solution of the following problem: — To find a certain and easily applicable method for the detection of oil of turpentine in the most important ethereal oils. This problem was solved by, and the prize awarded to, Mr. G. S. Heppe, of Leipsic.

Though it was not the good fortune of Mr. Heppe to discover a substance which characterizes the oil of turpentine exclusively, yet we are enabled, by the test used by him, successfully to demonstrate the same in most of the oils containing oxygen, — those oils containing no oxygen, as ol. Juniperi, Citri, etc., therefore excepted. This reägent consists in nitro-prussid copper, and the mode of its application is as follows:

A so-called eprouvette (a small glass tube, open at one end), which ought to be perfectly clean and dry, is filled one-fourth, or at the most one-third, with the suspected oil, then 2-5 milligrammes (about as much as a small pin-head) of finely powdered and perfectly dry nitro-prussid copper is added, and well shaken up; then the tube is heated over a spirit-flame up to the boiling-point of the oil, and, after one ebullition, is drawn away, and set aside to settle. If the oil was free from turpentine, then the precipitate will be, according to the oil used, either black, brown, or gray, and the oil itself be more or less of a darker color than originally. But if the ethereal oil contained turpentine, then the precipitate will be either green or bluish-green, and the oil either colorless or of a pale-yellow hue. The longer the oil is left for settling, the more perceptible the color of the oil or of the precipitate will be.

The methods generally in use at the present time for the detection of oil of turpentine in other ethereal oils are as follows:

1. To rub the suspected oils between the hands, and to detect the adulteration by its peculiar smell; or to insert a strip of paper into the same, then to light it, and extinguish it a little while afterwards, when the peculiar smell of turpentine will be remarked, if it is present in any considerable quantity.

2. To mix the oil in question with an equal quantity of 80° alcohol, when, if it contained any oil of turpentine, fennel, or anise, no complete solution will ensue.

3. Oil of turpentine will not dissolve red sanders, though many other ethereal oils will, whereby this property will be diminished, if any oil of turpentine had been added.

4. Iodine, if brought into contact with oil of turpentine, will raise a slight explosion; but not so with many other oils. Yet, if the former had been added to the latter, and if even in only small proportion, a similar effect will result.

ON THE AMOUNT OF CAFFEINE IN COFFEE-BEANS. BY PROF. A. VOGEL, Jr.

The method hitherto employed for the extraction of caffeine from coffee-beans and tea-leaves, is both complicated and uncertain. It consists in extracting the coffee-beans with water, precipitating the tannic acid from the solution by lead salts, and evaporating only the solution freed from lead for crystallization. This method is exceedingly inconvenient, and this is probably the principal reason why the statements as to the amount of caffeine in coffee differ so much from each other.

The following method appears to the author to be much simpler, and to lead to more accurate results. It is founded on the treatment of powdered coffee-beans with commercial benzole. This extracts two constituents from the coffee — oil of coffee and caffeine. After the evaporation of the benzole, these two substances may be easily separated from each other by agitation with hot water, in which the caffeine dissolves, whilst the oil floats on the surface, and may be skimmed off. The caffeine is obtained by the evaporation of the aqueous solution, in very beautiful crystals, which may be sublimed.

The whole of the benzole may be recovered, by distilling it in a retort, after it has stood about a week upon the coffee-beans. The residue in the retort is the oil of coffee and caffeine, which may be separated as above by agitation with water, or by treatment with ether, which dissolves the oil and leaves the caffeine in crystals. By this method oil of coffee and caffeine might by obtained as subsidiary products in benzole manufactories. — *Kunst-und Gewerbeblatt für Bayern*, 1858.

ETECTION OF MINERAL ADULTERATION IN FLOUR BY CHLOROFORM.

A chemist at Charleville has contrived the following procedure, by which, according to M. Lassaigne, a ten thousandth portion of mineral matter may be detected in flour adulterated with the same. A glass tube, three centimetres in diameter, and from fifteen to twenty in length, closed at one end, is to be tightly corked at the other, so that fluid may be well shaken in it. From one to three drachms of the flour are to be introduced into this tube, which is next to be nearly filled with chloroform. The tube is now to be corked, well shaken, and left awhile at rest, until the separation has had time to take place. The flour rises to the surface of the chloroform, while the mineral adulteration falls to the bottom, and may be submitted to analysis in order to detect its exact nature. — *Moniteur des Hôp.*

ANALYSIS OF FISH SCALES.

At a recent meeting of the Boston Society of Natural History, Mr. John Green gave the results of an analysis of the scales of the striped bass (*Labrax Lineatus*), as follows, viz.:

In scales dried at 212° F., 45·9 per cent. of ash. 100 parts of ash contained of lime, 48·36; of magnesia, ·99; of phosphoric acid, 50·65. This result is identical with the composition of bone ash. The structure of bone is different in the Ganoid fishes from that of any Cycloid or Ctenoid, and confirms the differences already established from the appearances of the scales. The scales of the Amia, of the western waters, contain bone corpuscles of the same form and appearance as those of Megalops and Lepidosteus, showing a new analogy of Amia with Ganoid fishes.

ON THE DETECTION OF ORGANIC IMPURITIES IN THE AIR.

At a recent meeting of the London Chemical Society, Dr. Augustus Smith stated that he had for several years paid particular attention to the condition of the air of towns, and had, during that time, frequently endeavored to obtain a ready mode of estimating the organic matter present in it. Until recently, however, he had not found any satisfactory method of so doing: but now, by the adoption of a new process, results before requiring days or weeks of experimenting, could be obtained in a half an hour. The method devised consists in finding how much of a solution of permanganate of soda will be decomposed by a given amount of air. The indications given by this are very beautiful, and illustrate those truths which sanitary economy has long been teaching, with indifferent success, to the country. I find, says Dr. Smith, as much difference between the back streets of a town and the air of a hilly district, as from 1 to 22. In other words, there was found in the air of a close court 22 times more matter capable of decomposing the solution, than there was found in a free, hilly district. Some of the results obtained, when tabulated, are as follows:

A definite amount of a standard solution of the salt was decolorized by 22 measures of air from the high ground in the neighborhood of Preston, by 9 measures of air from an open street in Manchester, by 5½ measures of air from between some small houses on the banks of the Medlock river, by 2 measures of air from a closed carriage full of passengers, and by 1 measure of air from the back yard of a house in a low and closely-built neighborhood.

Mr. Smith terms the means thus employed the *sepometer*. Its delicacy is such that it registers many steps lower than the point of which the ordinary smell is capable of taking cognizance. It clearly and distinctly tells the state of ventilation in a room.

Mr. Smith has also observed a very noticeable difference when blood was agitated with different varieties of air. Contrary to expectation, the air of the town was found to exert a greater reddening effect than the air of the sea-shore.

REMARKS ON THE NATURE OF OZONE.

In the January number of the *Philosophical Magazine* there is a memoir by Schönbein, in which he ascribes to ozone the very remarkable character, that while it exercises a powerful oxidizing influence upon oxidizable substances, and even upon the noble metals, a moistened strip of paper colored with binoxide of lead and immersed in ozonized air, is bleached in consequence of the reduction of the binoxide of lead, — the ozone being at the same time destroyed, or rather converted into ordinary oxygen.

This statement has confirmed Hr. Clausius in the opinion which he had previously formed as to the nature of ozone, and consequently he has been induced to make it known.

In a recent memoir by Hr. Clausius on "the kind of motion called heat," he has endeavored to account for the relations of volume existing between the simple and compound gases, by the assumption that in simple gases several atoms are combined so as to form one molecule; that, for instance, the molecule of oxygen consists of two atoms of oxygen. He is of opinion, that under special circumstances, it may happen that among the number of molecules in a given quantity of oxygen, some may be decomposed into separate atoms. These oxygen atoms would differ in their behavior towards other substances from those combined into molecules, and Hr. Clausius is of opinion that those uncombined atoms are ozone.

In support of his peculiar views, Hr. Clausius brings forward the following data, in a communication to the *Phil. Magazine*:

If electricity passes out into oxygen or atmospheric air, or if electric sparks are discharged through either of these gases, ozone is formed; and this formation is independent of the nature of the electricity, that is, whether it be positive or negative. This action may probably be attributed simply to the repulsive power of the electricity, by virtue of which the two atoms of a molecule, being charged with the same kind of electricity, are driven apart in the same manner as is observed with larger bodies.

Oxygen, when separated from its combinations by electrolysis, under favorable circumstances, is obtained in an ozonified state. This is explained thus: — At the moment of disengagement, the atoms of oxygen are separate. Most of them combine immediately upon the electrode, two and two together, to form molecules, and here perhaps the electrode itself, when, for instance, it is formed of platinum, exerts an auxiliary action. A small portion of the atoms, however, remain in the separate condition, and this constitutes the ozone with which the oxygen is mixed.

Finally, a third mode of formation occurs when atmospheric air is in contact with moist phosphorus. This process may perhaps be imagined to proceed as follows: As the phosphorus combines with the surrounding oxygen, a number of oxygen molecules, in contact with the phosphorus, must be decomposed into their two constituent atoms; and it may happen that the phosphorus does not combine with both of such atoms, but that one of them, being removed from the sphere of activity of the phosphorus through the motion caused by the heat developed, remains in the separate condition. We know from electrolysis, that in the combination of *heterogeneous* atoms to a molecule, one part of the molecule is positively, and the other is negatively electrical. This may, perhaps, also be the case in the combination of two *homogeneous* atoms, as, for instance, of two oxygen atoms, one of them becoming positively, and the other negatively electrical. Inasmuch, now, as, by the oxidization of the phosphorus, the oxygen doubtless enters into combination as the electro-negative constituent, it may come to pass that, of the two oxygen atoms resulting from the splitting up of a molecule, the negative one is that especially retained by the phosphorus, and the positive one may be free to move away, or at least may be less hindered from doing so. Even after such an atom, in the course of its motions, and through contact with other molecules, or the walls of the vessel, has lost its electro-positive state, thus becoming more adapted for combination with the phosphorus, yet such combination cannot take place until its motions bring it again into the sphere of action of the phosphorus.

Certain remarkable phenomena in connection with ozonification by means of phosphorus, have been observed. Thus, rarefied oxygen is more easily ozonified than denser oxygen, and oxygen mixed with hydrogen or nitrogen is more easily ozonified than when it is in the pure state. I believe that probable, or at all events, possible, explanations may be given for many of the secondary phenomena. I shall not discuss these, however, in this place.

The circumstance which was mentioned before as being probable, that, namely, in the combination of two oxygen atoms to a molecule, the two atoms have opposite electrical states, may be made use of to explain certain other phenomena. The fact that the ozone, which is formed in a quantity of oxygen, does not disappear again in a short time of its own accord through the formation of molecules by the recombination of the separated atoms, may perhaps be due to the diminished tendency which such free atoms have to combine, through the loss of their electrical condition; just as oxygen, even when ozonified, may be mixed with hydrogen without combination resulting.

When ozonified oxygen is heated, the ozone is destroyed. This may perhaps be explained by supposing that the high temperature determines the combination of the separated oxygen atoms, just as it may that of oxygen with hydrogen and other oxidizable bodies.

Becquerel and Fremy have shown experimentally that the ozonification of a given quantity of oxygen by electric sparks can only be carried to a certain extent if the ozone remain mixed with the oxygen; but if the ozone be removed as soon as formed, for instance, by the oxidation of silver, the whole of the oxygen may be gradually converted into ozone. This points to the conclusion, that if too many separate atoms be contained in the gas, they recombine with one another; and perhaps the electric sparks themselves may have the power of exerting the inverse effect under altered circumstances; that is, they may assist the combination of separated atoms in the same manner as they can determine the combination of oxygen and hydrogen.

Let us now consider some of the effects of ozone.

The principal action, namely, the strongly oxidizing power, may be considered as self-evident, after the description of ozone given; for it is clear that separated oxygen atoms can more easily enter into combination with foreign bodies than such as are already combined with one another, two and two, and which must be first freed from such combination before they can be in a state fit for combination with other substances.

In this respect ozone is comparable with oxygen in the nascent state, with the exception that with the latter the electrical condition must be taken into account in addition; for when oxygen is evolved out of a compound in which it was electro-negative, it will for two reasons easily enter into another combination, in which it has also to play an electro-negative part: first, because the atoms are in the separate state; secondly, because they already are in the proper electrical condition. Hence oxygen in the nascent state may, in many cases, surpass ozone in activity.

The galvanic polarization of a plate of platinum, by immersion in ozonified oxygen, is related to the above action. It is known that the two electrodes, which serve for the galvanic electrolysis of water, become thereby polarized in such a manner as to be capable of giving rise by themselves to a current in the opposite direction. This is explained by supposing the one electrode to be covered by a layer of hydrogen, and the other with a layer of oxygen; and such explanation accords with the fact that a plate of platinum, when immersed in hydrogen, acquires thereby also a positive polarization. But if a platinum-plate be immersed in common oxygen, the corresponding phenomenon, which might, perhaps, be expected, — namely, the acquirement of negative polarity by the plate, — does not occur; and this appears to contradict the above-given explanation. I imagine, however, that this difference may be accounted for as follows: Inasmuch as a molecule of water consists of *two* atoms of hydrogen and *one* atom of oxygen, the atoms of hydrogen, which, like the atoms of oxygen, are also combined two and two to molecules, may enter into combination with oxygen without separating from one another. The atoms of oxygen, on the contrary, as long as they are combined together as molecules, are not in a suitable condition for combination with the hydrogen. Hence oxygen, in its ordinary state, is incapable of causing galvanic polarization, but acquires this power by ozonification.

Besides an oxidizing action, ozone may exert an opposite or deoxidizing one, as Schönbein has proved in the case of peroxide of lead, the ozone itself being converted thereby into ordinary oxygen. Now, as this transformation of ozone into oxygen occurs also when it is brought into contact with other peroxides, it immediately suggests itself that the deoxidation of the peroxide is also not confined exclusively to the peroxide of lead. This action may be explained without difficulty. If we imagine an oxide which readily gives up the whole, or a part of its oxygen, in contact with a gas in which separate oxygen atoms are moving about, seeking to combine with second atoms, these moving atoms, on coming into contact with the oxide, are able to withdraw the atoms of oxygen which are only feebly combined. This accounts at the same time for the double effect — the reduction of the oxide and the disappearance of the ozone.

The behavior of ozone is, in many respects, similar to that of the peroxides. Peroxide of hydrogen, for instance, has, as is well known, a strong oxidizing action, by reason of the facility with which it gives up its second atom of oxygen. If, on the contrary, peroxide of hydrogen be brought into contact with the oxides of the noble metals, or with certain metallic peroxides, a reciprocal reduction takes place. We may, in such a case, presume that the oxygen atoms liberated from the peroxide of hydrogen combine and form molecules with those which are given off from the metallic oxides or peroxides.

In considering the above-mentioned phenomenon, the question may arise, why the atoms of ozone, or the easily-separable oxygen atoms in a single oxide or peroxide should not unite with one another with as great a facility as the atoms of two heterogeneous substances combined together? Various secondary reasons, however, may be of influence. In the first place, the state of aggregation must be considered. In a solid metallic oxide or peroxide, the several parts are fixed in position with respect to one another; and we may therefore presume that the oxygen atoms do not come into that contact with one another which is necessary for combination. A fluid body, on the contrary, adapts itself better upon a solid one, and its particles possess, at the same time, the necessary mobility. The same is the case with a gaseous body; and such a one, in addition, undergoes a condensation on the surface of the solid body. It may, moreover, be the case, that the equally electrified condition of the oxygen atoms of a definite compound renders them less disposed to combine with one another than with the non-electric ozone, or with the oxygen atoms of another compound, whose electrical state may possibly be a different one. Moreover, the electrical conductibility of the substance may be of influence, inasmuch as those alterations of the electrical condition which are necessary for combination, may take place more easily in contact with metallic bodies than in the interior of badly-conducting bodies. Probably still further reasons might be given in answer to the question proposed; but those already advanced may suffice, at all events, to show how numerous the influencing circumstances may be, and how vain it must be to expect to find the phenomena following some simple law which holds good in all cases.

Finally, I must remark, that the density of ozone, as given by Andrews and Tait,* — according to whom it would appear to be nearly four times as great as that of ordinary oxygen,— is contradictory of my hypothesis concerning the nature of ozone. If, however, we reflect, that the experiments could only be tried with oxygen containing comparatively little ozone, and that, in order to convert the ozone into ordinary oxygen, the whole was heated to 230° C., or more, it is easy to perceive how extremely difficult it must have been to remove the disturbing influences so far as to attain the requisite degree of accuracy. For this reason, then, without in the least calling in question the skill and care of the experimenters, I have, nevertheless, hesitated to attach sufficient weight to the results they have found, to induce me to suppress my hypothesis. — *Philosophical Magazine.*

At the Leeds meeting of the British Association, 1858, Dr. Lankester exhibited an instrument for measuring the intensity of ozone, which consisted of two small rollers included in a box, which were moved by means of ordinary clockwork. Over the roller a strip of paper, prepared with iodide of potassium and starch, is allowed to revolve, — the paper being exposed to the air from an inch of open surface in the lid of the box. Twenty-four inches of paper pass over the rollers in the course of the twenty-four hours, and thus registers, by its color, the intensity of the action of ozone in the atmosphere. By this instrument the intensity of the ozone for every hour in the twenty-four could be registered, and *minima* and *maxima*, with an average, be ascertained. The register of ozone could also be compared with those of the anemometer, and the relation of ozone to the direction and force of the wind ascertained. Dr. Lankester pointed out the importance of ascertaining the presence of ozone, on account of its undoubted relation to health. He drew attention to a series of tables which had been drawn up from the registra-

* Proceedings of the Royal Society, vol. viii., p. 498, and *Phil. Mag.* for February, 1858, p. 146.

tions of the anemometer, made at London, Blackheath, and Felixstow, on the coast of Suffolk. From these it was seen that the relation of these three places was as 0, 22, and 55. The instrument acted also as a clock, and the time could be accurately marked upon the ozonized paper.

Dr. Marshall made some remarks on his own observations during the last twelve months, and stated that he had not been able to discover, though assisted in the investigation by medical gentlemen, that there was any obvious connection between ozone and the state of health.

UNWHOLESOMENESS OF LIGHTS.

Recent experiments have proved that lights of equal intensity, obtained from different materials, require very different lengths of time to generate the same quantity of carbonic acid. The following is the relative time required by the common materials: Olive-oil, 72 minutes; Russian tallow, 75; common (French) tallow, 76; whale-oil, 76; stearic acid, 77; wax candles, 79; spermaceti, 83; gas from common coal, 98; gas from fat or cannel coal, 152 minutes. Coal gas, therefore, and especially gas from cannel coal, is the least unhealthy of all ordinary lights; which is contrary to the usual opinion. — *Cosmos, June,* 1858.

DESTRUCTION OF INSECTS IN ORCHARDS, GARDENS, ETC.

M. Mellot Brulé, the distinguished horticulturalist (France), has recently demonstrated, by experiment, the efficacy of the powdered proto-sulphuret of iron (which has been before used for the preservation of timber), in destroying noxious and annoying insects.

The powder may be strewed over the ground around the roots of the tree, or fixed on the surface of a collar surrounding the stem. No insect will pass it; or, if they attempt it, they are immediately killed. The proto-sulphuret of iron (black pyrites,) occurs as a mineral in various parts of France and Germany, and is manufactured for the purpose of developing sulphuretted hydrogen, which is undoubtedly the effective agent in destroying the vermin. — *Cosmos.*

NITRO-GLYCERINE, OR GLONOINE.

Mr. A. G. Field, F. R. C., communicates to the London *Pharmaceutical Journal*, the following account of the preparation and properties of glonoïne, with its therapeutic effects:

" In 1847, when chemists were intent on the production of gun-cotton, M. Sobrero made known the fact that glycerine, when treated with a mixture of sulphuric and nitric acids, yielded a similar compound, which he described as an oily liquid, heavier than water, in which menstruum it was insoluble, although readily dissolved by alcohol and ether.

" According to this author, the smallest quantity of it was sufficient to produce a most violent headache, from which he concluded it would prove a most dangerous poison.

" This consideration induced me to try and ascertain the best mode of preparing this substance, and again examine its principal properties.

" *Preparation.* — After repeated experiments, I found the following the best mode of preparation: — One hundred grammes (1543.3 grs.) of glycerine, freed as much as possible from water, and having a sp. gr. 1.262, were cautiously, and in small quantities at a time, added to 200 cubic centim. (18 ounces) of monohydrated nitric acid, previously immersed in a freezing mixture. The temperature rises upon each addition. It is therefore necessary to allow the mixture to cool down again to —10° C. (14° Fahr.) before any fresh addition is made, as it is very necessary that the temperature should never rise above 0° C. (32° Fahr.) When the glycerine and nitric acid have formed a homogeneous fluid, which may be facilitated by stirring the mixture with a glass rod, 200 cubic centim. (18 ounces) of concentrated sulphuric acid are cautiously and slowly added.

"This operation is accompanied with the greatest danger, if the temperature is not continually watched. Experience, however, shows me that there is no reason for fear, provided the temperature be always kept below 0° C. (32 Fahr.)

"When these precautions have been taken the nitro-glycerine separates, after the addition of the sulphuric acid, in the form of an oily liquid floating on the surface, and may be collected by means of a separating funnel.

"The product thus obtained, which is still contaminated with a little acid, weighs about 200 grammes (3086.6 grs.). A still further portion, however, about 20 grammes (308.6 grs.), may be obtained from the acid liquor by diluting it with water.

"The products thus obtained are then dissolved in a small quantity of ether, and this solution repeatedly shaken with water, till all trace of acid is removed. The ethereal solution is then heated over a water-bath till nothing more is volatilized.

"*Properties.* — Nitro-glycerine is an oleagenous liquid of a clear yellow color, having a sp. gr. from 1.595 to 1.600. Heated to 160° C. (320° F.) it is decomposed, evolving red vapors; at a higher temperature it either explodes or inflames without any detonation.

"It is difficult to determine accurately the point at which explosion takes place; it is best observed by allowing the nitro-glycerine to drop, from time to time, upon a piece of heated porcelain. At first it burns away with a vivid flame; but as the temperature diminishes, it violently explodes, evolving red vapors, and frequently breaking the porcelain on which it falls.

"By placing a drop on an anvil, and striking it with a hammer, it instantly detonates. When properly prepared, and free from acid, it may be kept for any length of time. I have some in my possession, which has been kept for two years without undergoing the slightest change.

"Upon the addition of sulphuric acid to the ethereal solution, decomposition ensues, and a great quantity of sulphur is thrown down."

The following is Mr. Field's description of his experience, of the effects of glonoïne.

"On the evening of the 3rd of February, 1858, I was conversing with a homœopathic practitioner, when he mentioned a medicine which possessed peculiar and extraordinary qualities, some of which he described as having affected himself, though he had taken it in very minute quantities. I laughed at his credulity, and offered to take as much as he pleased, upon which he let two drops of what he called the first dilution of glonoïne fall on my tongue. After swallowing this small quantity of fluid — I was assured the quantity did not exceed two drops — I asked what effects I must expect, but was told to wait and observe for myself. I then purposely conversed on other subjects. In about three minutes, I experienced a sensation of fulness in both sides of the neck; to this succeeded nausea; and I said, 'I shall be sick.' The next sensation of which I was conscious, was as if some of the same fluid was being poured down my throat, and then succeeded a few moments of uncertainty as to where I was, during which there was a loud rushing noise in my ears, like steam passing out of a tea-kettle, and a feeling of constriction around the lower part of my neck, as if my coat were buttoned too tightly; my forehead was wet with perspiration, and I yawned frequently. My intellects returned, however, almost immediately, and I remember saying, 'This has nothing to do with homœopathy, but it has to do with a very powerful poison.' When these sensations had passed off, which they did in a minute or so, they were succeeded by a slight headache, and dull, heavy pain in the stomach, with a decided feeling of sickness, though without any apprehension that it would amount to vomiting. This condition lasted about half an hour, at the end of which I was quite well, and walked home, a distance of half a mile, with perfect comfort.

"The physician to whom I am indebted for this overdose told me, that when his first impression that I was shamming had passed off, my condition caused him the greatest alarm, for he really thought he had killed me. I learn from him that my head fell back, my jaw dropped, I was perfectly white, breathing stertorous, and no pulse at the wrist for the space of about two minutes. He immediately rushed to a closet and procured some stimulant, which he poured down my throat."

Since the publication of the above statement by Mr. Field, other experimenters have operated with glonoïne, and the results obtained are most discordant; so much so, that its physiological action can by no means be considered as established. The general opinion, however, is, that it is an agent of great potency.

ON PEPSIN, AND ITS CHEMICAL AND PHYSIOLOGICAL PROPERTIES.

In an extended communication made by M. Boudault, to the Société de Pharmacie, Paris, the author, after discussing the general properties of pepsin, proceeds to make the following remarks upon that substance employed as a medicine: Its administration presents some rather considerable difficulties, in consequence of its liability to alteration when the vessel which contains it has been open. Besides this, its origin, its viscosity, and its disagreeable smell, were so many motives for disliking it on the part of the patient. It was necessary, then, to find a method of transforming it without injuring its medicinal action. It was to be feared, in associating it with an inert substance, that the latter would experience a kind of digestion, or would act upon the pepsin as a ferment. It was necessary, besides, that this substance should be sufficiently hygrometric to absorb the humidity of the pepsin, and not to attract, in addition, the air. Sugar was one of the substances with which it appeared most easy to associate pepsin; but at the end of some days the cane sugar is transformed, under its influence, into glucose, and afterwards into lactic acid, for here the pepsin acts as a true ferment. Starch dried at 100° (Cent.) has given to M. Boudault the most favorable results. Starch, which has the property of not injuring the digestion, forms with pepsin a pulverulent matter, the odor of which is very weak, and the taste partly disguised. The powder is preserved very well, in well-stopped bottles, and time does not modify in any way its physiological properties. Under this form, pepsin may be mixed with a number of medical substances which do not at all modify its therapeutic action: thus, with hydrochlorate of morphia, to relieve violent pains of the stomach; with strychnine, to stimulate the peristaltic movements of this organ; with nitrate of bismuth, lactate of iron, carbonate of iron, and other similar preparations. It is very efficacious in dyspepsia, and in all cases of difficult digestion which generally follow the convalescence from serious or chronic diseases; and it has been found a powerful digestive agent in cases of consumption, caused by insufficient food. Pepsin is administered in the first spoonful of soup, or even before meals, wrapped up in a wafer; and precaution must be taken not to eat immediately afterwards food which is at a higher temperature than 45°, for then the digestive properties of pepsin would be destroyed. It is employed in the acid or in the neutral state. In the acid state it takes the place of the gastric juice, when the latter is not formed in sufficient quantity in certain morbid affections; in the neutral state — that is to say, feebly acidulated — in cases where the stomach contains too great a quantity of acid. It may be shown that chemical or artificial pepsin may very well take the place of the gastric juice, and may be considered among one of our most efficient remedies.

COFFEE AND MILK AS AN ARTICLE OF DIET.

From an extended report of Dr. Caron, Physician to the prisons of the department of the Seine, France, on the influence of various articles of food with animal economy, we derive the following observations respecting the use of coffee and milk. Speaking of the popular habit of breakfasting on coffee with milk, he says: I think I am justified in attributing to this kind of aliment, the production of the nervous diseases which principally affect females of every class, particularly those inhabitating large towns. The very general coincidence of the same symptoms with the use of coffee and milk, induced me to examine what might be the cause of those phenomena, and then I was led to investigate what the action of coffee and milk is, and then the action of that mixture on the human economy. I was first obliged to make the analysis of the infusion of coffee, next to determine its physical and chemical properties; I was obliged, besides, to submit my first essays to new experiments, which conducted me to a series of most interesting researches, which I now propose to describe.

The infusion of coffee is a liquor of a dark brown, possessing a particular aromatic taste, slightly bitter — the chemical analysis containing the following principles, viz.: a coloring matter soluble in water, a volatile empyreumatic oil soluble in alcohol, which is developed by torrefaction, some tannic and gallic acid, some resin, and an extract of cafeine. This liquor, when warm and sweetened, constitutes a stimulating and pleasing beverage, known by every one; but what no one has thought of is, that, when in contract with milk, its nutritious properties are neutralized, because of its fermentation being retarded. Having put together some coffee and milk in a bottle, it was twenty-seven days before the mixture began to decompose, whilst milk and sugar were decomposed in three days; chocolate with milk was five days; pure cafeine and milk eleven days. It is evident that the astringent properties of coffee hinder the digestion of milk; but it happens also, that, during the action of coffee on the principle of milk, the cafeine is set free, and acts on the membrane of the stomach in the same manner as vegetable alkalies, producing the most evident hyposthenization, a fact which till now has been overlooked. Then Dr. Caron continues to relate the experiments he made on himself, and some other persons willing to submit to the trial, the results of which were general prostration, vital concentration, cephalagia, weakness and trembling of the legs, tottering walk, nausea supervening with fulness of the stomach, constant somnolence, great want of appetite,— he having remained since the morning till 11 o'clock at night without eating anything. But, what is particularly worth noticing, he adds, is the condition of the pulse, which, on the average, was from eighty to ninety, and which, under these circumstances, was lowered to sixty-eight. At four o'clock in the afternoon it was reduced to sixty, and two hours later to fifty-six, when he took food in order to stop the effect. While taking the meal, he was subject, from time to time, to giddiness, flushing of the face, and nausea; after the meal, the pulse rose to seventy-two, when he felt much relieved. He continues farther on, and says: A mixture of coffee and milk, as I have stated above, having the property of hindering the fermentation when in vessels, acts identically in the same manner in the stomach, and constitutes an inert liquid, on which the gastric juice has little or no action at all. Dr. Caron continues the account of his experiments, mentioning cases he has treated, and proves ultimately that many patients laboring under irritation, leuchorrhœa, and hysteria, were restored to health by simple tonic treatment after having given up the use of coffee.

ON THE ECONOMICAL APPLICATIONS OF GLYCERINE.

The following suggestions regarding economical applications of glycerine, are contained in a paper read before the American Association for the Promotion of Science, 1858, by Henry Wurtz, of New York:

It must be apparent to every one who considers the peculiar qualities of the substance, glycerine,— namely, its resemblance to oils in not being volatile at ordinary temperatures, while, unlike them, it is miscible with water, alcohol, etc.; its resistance to congelation, not being perfectly solid even at the freezing-point of mercury; its unchangeability; its agreeable taste when pure, and harmless action upon the system; its wide range of solvent powers, together with the quantity in which it may be cheaply procured, — that it must in future fulfil important purposes, not only in pharmacy, but also in the arts. Accordingly, we find that technical applications have already been proposed for it. Barreswil's method of preserving clay, which is to be used for moulding purposes, in a moist and plastic state, may be alluded to as an example.

Some uses, which are probably new, have occurred also to me.

In the first place, its conjunction of the property of compatibility with human digestion and assimilation, with that of non-evaporation and even *absorption of water* from the air, suggest applications in the preservation of articles of food and luxury which are injured by desiccation. As an example of minor importance, if *mustard*, for table use, were mixed with diluted glycerine, instead of water or vinegar, the usual vehicles, it would retain its liquidity indefinitely without drying up. So of many other condiments.

A more important application, however, of a similar kind, would be in the preparation of articles of *confectionery* composed of sugar, preserved fruits, chocolate, etc., which are frequently met with enveloped in *tin-foil* to prevent their desiccation. The same object might be accomplished more effectually, and probably more economically, by admixture, in the process of manufacture, with a certain proportion of pure glycerine.

Another article of luxury, of still more extensive consumption, the consumers of which demand that it should be preserved for them in a *moist* state, is that known as "chewing tobacco;" and a vast consumption of tin-foil arises from this requirement of the tobacco chewers. I have repeatedly prepared small quantities of chewing tobacco (the variety called "fine-cut") for persons addicted to its use, by admixture with a little glycerine, and always very much to their satisfaction. In the preparation of this drug, the manufacturer must also please the palate of the consumer by introducing some *dulcifying* ingredient. Common sugar or molasses, however, will not answer the purpose, because they render the mass liable to ferment and turn sour. An infusion of the root or extract of *liquorice* is therefore usually resorted to. This does not, however, keep the tobacco *moist*, as molasses would do; and to attain this it is necessary to press into solid compact masses and pack into tight cases; or, in the case of the finer qualities, to enclose in wrappers of tin-foil. In view of these facts, glycerine will be seen to supply every requirement of the tobacconist, as it will not only keep his product *moist* for an indefinite time, even when exposed to the air, but will also *sweeten* it. He may almost look upon glycerine as made specially for his use.

The common *water meters*, used for measuring the consumption of illuminating gas in houses, are open to two strong objections, namely: when in a warm situation the water rapidly *evaporates*, and when in a cold place it *freezes*. To avoid congelation, the usual expedient is to fill the meter, in cold weather, with *alcohol* or whiskey, thus rendering the first-mentioned difficulty, that of evaporation, still more inevitable.

Now, what liquid do we possess which is practically free from both these objections of evaporation and congelation? Evidently *diluted glycerine*. I propose, therefore, as a substitute for both water and alcohol, for filling gas meters, *glycerine* (sufficiently diluted to prevent its absorption of more water from the gas, and increasing in volume to any important extent), thus rendering the meter independent of attention within the ordinary limits of temperature.

For lubricating the bearings of fine machinery also, and particularly of *chronometers*, glycerine seems to me worthy of a trial, as it is unchangeable by the atmosphere, and remains at temperatures which few or none of the oils will resist. For chronometers, pure *oleine* and *oleic acid* have been used; but the former thickens on exposure to the air, and the latter congeals at a few degrees below the freezing-point of water.

Other uses occur to me, such as in the preparation of *copying ink*, in *water-color painting*, and in the preservation of dried plants for *herbaria* in a flexible state; mere allusions to which may at present be sufficient.

CHEMICAL CHARACTERISTICS OF PURE GLYCERINE.

According to Dr. Cap, pure glycerine, suitable for medicinal purposes, should have the following properties: It should be odorless, even when rubbed between the hands; its consistency must be that of thick syrup. It must be of honey-like taste, strongly sweet, its reäction nearly neutral; one volume of glycerine must be perfectly soluble in one volume of alcohol, acidulated with 1-100 of sulphuric acid, without forming a deposit, when standing in a cool place, even after twelve hours. Further: one volume of glycerine must dissolve in a mixture of 100-00 alcohol and 50-00 of sulphuric acid, without forming a precipitate (salts of lime), or leaving syrupy residua (adulteration with honey or simple syrup). In this way an addition of 10-00 of syrup may be detected; but, if it contains less, on adding a drop or two of sulphuric acid to the mixture, a white deposit forms immediately. Glycerine dissolved and boiled with water, should not be changed to a darker hue, which would indicate the presence of glucose. — *Med. Reporter.*

ON THE PREPARATION OF COLLODIUM. BY M. ZINKEISEN.

To detect the most advantageous process of preparing collodium, the following trials have been made by me:

1. The Codex Medicam. Hamb. prescribes:

> 20 parts of dry Nitre,
> 30 parts of English Sulphuric Acid,
> 2 parts of cotton,

which has been previously treated with soda—to be left in contact with the acids only a few minutes.

Four trials, made according to this formula, yielded, after application of a temperature from 45° to 35° R., from three minutes to one hour and a quarter, very little more than 2 ounces of wool each, of which only $\frac{3}{4}$ could be dissolved in ether, and $\frac{1}{16}$ in alcohol at most; for there remained distinct, undissolved filaments of wool. The quantity of cotton, therefore, appears too large in this process.

2. According to the prescription of Mann, there are to be taken:

> 20 ounces of Nitre,
> 31 ounces of English Sulphuric Acid (of 1.830 sp. weight),
> 1 ounce of Cotton,

which are to be left in contact for a "good while."

I had the acids working on the wool for one hour and a half, at a temperature of from 45° to 35° R., and, after drying, got 1 ounce and 1 drachm of a very fine, clear, and entirely soluble wool.

This prescription, however, is too expensive for manufacturing purposes.

3. Bertram's formula:

> 16 ounces of Concent. Sulph. Acid (1.850 sp. w. by mixing fuming and English Acids),
> 11 ounces of dry Nitre, and
> 1 ounce of Cotton.

While mixing the nitre with the acid, the temperature went up as high as 60° R., some brown bubbles of oxygen gas escaping. After cooling the mixture down to 45° R., the cotton was added, and left in contact for one hour, at nearly the same temperature. After drying, it yielded $1\frac{1}{2}$ ounces of wool, which exploded heavily, but was indissoluble. A second trial, at which the cotton was put in at 60° R., yielded no better result.

In this formula the sulphuric acid is too concentrated and its effects too violent.

4. Schacht's prescription:

> 24 ounces of Sulphuric Acid,
> 16 ounces of Nitre, and
> 1 ounce of Cotton.

Immediately after mixing the acids, the cotton is to be put in at a temperature of 45° R., and left in contact therewith for one hour, during which time the mixture cooled down to 35° R.

Result: 1 ounce and 3 drachms, easily and completely soluble, burning very slowly. This collodium answers every expectation.

5. Prescription of Bretschneider and Lüdersen:

> 6 ounces of fuming Sulph. Acid (1.850),
> 6 ounces of fuming Nitric Acid (1.410),
> ½ ounce of Cotton —

the cotton to be put in in halves, forty-five minutes in contact, at from 40 to 25° R.

Result: 5½ drachms, yellowish, quickly exploding, swelling to a gelatinous mass, with 16 parts of ether and 1 part of alcohol, and yielding, even with 32 parts of ether, a very thick collodium, the coat of which was very thin and transient.

A second trial, at which the cotton had been left in the mixture only for 10 minutes, yielded the same result.

6. Konig's formula:

> 8 ounces of fuming Sulph. Acid (1.840),
> 4 ounces of fuming Nitric Acid (1.410),
> ½ ounce of Cotton, dipped in successively.

At the first trial five minutes' influence, at 45° R.; at the second trial, one hour's influence, at 50° to 35° R. The first trial yielded an entirely insoluble wool; the second, a wool only partially soluble — both of them, however, very explosive.

The prescription of Schacht is, undoubtedly, the most advantageous, especially in a pecuniary point. In eight trials, with 1½ ounce of cotton each, I got 17½ ounces of wool, and 20 pounds of very fine collodium. I have further to state, that I made these trials with three different kinds of cotton. The chief points to be observed, in order to come to a satisfactory result, are, undoubtedly, the weight of the sulphuric acid, the temperature of the mixture, and the duration of the process. According to my experience, the sulphuric acid should not weigh below 1.820, and not above 1.840; the most advantageous temperature is 45° to 35° R., which, in general, generates of itself, when the dry and completely cooled nitre is mixed with the acid. The time of contact should not be less than half an hour, in order that all the filaments of the cotton be penetrated. A good prepared collodium wool will, however, not be decomposed if left under the influence of the acids even for a longer time.

It is very advantageous not to dry the wool by heat, but by repeated pressure between blotting-paper. — *Druggist's Circular.*

GERMAN YEAST.

Mr. Hennel, of England, has patented the manufacture of German yeast from flour. To obtain 10 pounds of this German yeast, the inventor takes 2½ pounds of flour of malted wheat, the same of flour of malted barley, and the same of rye flour. To this mixture water is added (at 30° Reaumur), and the mass stirred to a thin paste. This paste is then raised to 45° R. by hot water, and afterwards cooled down to 30° R.; next are added 2½ pounds of wheat starch dissolved in cold water; and 5 ounces of double carbonate of soda, and 2½ ounces of tartaric acid, severally dissolved in lukewarm water, together with 1½ pounds of common yeast. The mass will now require hot or cold water to bring it to 27° R., when it is left twelve hours to ferment. After this it is pressed through a hair sieve, and in eight or ten hours the yeast forms on the bottom of the cask. This yeast is taken and put into double bags, which are submitted to pressure to free it from moisture.

NEW PROCESS OF BREAD-MAKING.

The following is an account of a new process of bread-making recently put in practical operation by Dr. Dauglish, of Carlisle, Scotland:

According to the ordinary process, fermentation is produced by the action of the yeast upon the particles of starch in the flour, thus liberating minute bubbles of carbonic acid gas, which permeate the entire mass of the dough, and make it "rise." The chemical change, however, which here takes place, is such that it has been estimated by M. Dumas, that in France 17½ per cent. and in England 8½ to 12 per cent. is wasted by the decomposition which takes place in the process of fermentation. In the new process, patented by Dr. Dauglish, no yeast or baking powder is used, the rising of the dough being effected by water impregnated with carbonic acid gas. The idea of making bread with aërated water is not a new one; a patent was taken out for such a process some years ago, but it was then found that when the flour was mixed with the impregnated water the gas escaped before the bread had time to rise. The novelty of Dr. Dauglish's patent consists in preventing the escape of the gas from the water, by subjecting the materials to an outward pressure of carbonic acid gas while the flour is being mixed with carbonated water. The carbonic acid gas is generated in such apparatus as is usually employed by soda-water manufacturers; the gas is pumped into a large reservoir, from which it is forced, as it is required, into a vessel containing water — the absorbing power of water for carbonic acid being very great. The kneading machine is a strong iron retort, fitted with air-tight lids, and provided with revolving prongs in the inside for mixing the dough. In the machine now in operation, this retort is capable of containing forty stones of flour. Into this are put twenty stones of flour, with the requisite amount of salt. A stream of carbonic acid gas is forced into the retort, and a sufficient quantity of carbonated water is admitted and well mixed with the flour and salt; the gas with which the water is impregnated being prevented from escape by the pressure of the ambient carbonic acid gas. As soon as the flour and water are mixed, a pipe is opened, and the loose gas is let out. The consequence of the pressure being taken away from the surface of the paste is that the gas which was held in solution by the water operates in precisely the same manner as the gas in a bottle of soda-water when the cork is removed, the dough rises and fills the retort, occupying twice as much space as before. The bread is then ready for being worked into loaves — the only operation that will necessitate handling. The rising can be regulated by the pressure of gas: so that, did the strength of the machinery permit, the bread might be made of almost any lightness. The pressure of the gas, and the quantity of water admitted, are regulated by gauges.

PROCESS FOR DECOLORIZING THE FATTY OILS.

M. Brunner states that he has been most successful in bleaching fatty oils by proceeding as follows:

The oil is made into an emulsion with water, to which the proper consistence is given by gum or starch paste, and this emulsion is well worked up with thoroughly ignited charcoal, coarsely powdered and freed from fine dust by sifting. To one part of oil about two parts of charcoal powder are taken. The doughy mass is allowed to dry thoroughly at a temperature which should not exceed 212° F., and the oil is subsequently extracted with ether in the cold in a displacement apparatus. After this extract has deposited any charcoal powder that may have passed through during the extraction, it is put into a retort, and the ether is distilled off in the water-bath. In this way olive oil and walnut oil are completely deprived of color. It might perhaps be supposed that the charcoal has a direct decolorizing action in this case upon the oil, just as in many cases it clears many aqueous fluids. This, however, is not the case. Oils left in contact with charcoal for weeks together did not undergo the least decolorization, even when they were dissolved in ether and digested with charcoal. The presence of the water contained in the emulsion appears first to give rise to the action. It is probable that by the preparation of the emulsion, the coloring matter, which does not belong to the oil itself, is taken up by the water, and afterwards absorbed by the charcoal. The action may be similar to that set up in the operation employed by painters to bleach oils, which consists in agitating the oil sufficiently with an equal volume of water, and exposing the mixture to the sun. The water, which soon separates again from the oil, appears turbid, and often mixed with slimy flakes. The operation is repeated for weeks, the water being frequently renewed, until it is no longer rendered turbid, and the oil often appears limpid. In the above process the essential part appears to be the complete desiccation of the charcoal mixed with the emulsion. If the oil be extracted with ether before this is the case, it is obtained again with its original color.

Lastly, it is to be remarked that by this process the oils undergo a very remarkable thickening. Thus walnut oil is obtained nearly of the consistence of butter. — *Bremer Mittheilungen*, Dec. 1857.

NEW MEDIUM FOR PAINTS.

A recent improvement in the preparation of paints is thus described by the discoverer, M. Sorel, in the following communication to the French Academy:

In 1855 I had the honor of presenting to the Academy various products obtained by means of oxychloride of zinc, especially cements and mastics, as hard as marble, and quite insoluble in water, and a paint, equally insoluble, intended to replace, very economically, oil and other painting. This paint had the inconvenience of being difficult to use, and of requiring, like siliceous painting, the application of a liquid on the last layer to fix it and render it insoluble; when I wished to avoid the use of this liquid by rendering my paint more drying, I was met by an equally serious inconvenience: the paint thickened very rapidly in the vessel, and there was not time to use it. I have now succeeded, by adding certain substances to my liquid, in surmounting these difficulties, and rendering the application of this new paint easy.

The liquid which in this paint replaces oil, essence of turpentine, and the other liquids and excipients used in ordinary painting, is an aqueous solution of chloride of zinc, in which I have dissolved an alkaline tartrate. These salts possess in the highest degree the property of retarding the thickening of the new paint before being used. I add to the liquid, to give tenacity to the paint, gelatine or fecula which I have caused to pass into the state of starch by heating the liquid. It must not be heated so much as to transform the fecula into dextrine or glucose.

To form the new paint, whatever the color may be, I use the above liquid and a powder which should be in great part oxide of zinc. For colored paints I use the same powder *plus* the coloring matters. The colors usually employed in ordinary painting may be employed.

The new paint possesses the following properties: 1st. It is not necessary to grind it; the powder only requires to be diluted with the liquid, and the paint is used in the ordinary manner. 2d. It is more beautiful and more solid than oil paint; it covers better, and is not rendered black by sulphurous emanations, like paints of ceruse or other lead bases. 3d. It is absolutely without odor, and dries very rapidly. We can apply a layer every two hours in winter, and every hour in summer; which enables us to paint an apartment in one day and inhabit it the same day, without being affected by the odor of the paint. 4th. It resists moisture and water, even when boiling, and may be washed with soap like oil paint. 5th. In consequence of the chloride of zinc which it contains, the paint is eminently antiseptic, and fitted to preserve wood from perishing. 6th. It possesses to a very high degree the quality of diminishing the combustibility of wood, fabrics, and paper, and of rendering these matters uninflammable. 7th. It is perfectly innocuous both to those who prepare and those who use it.

I have also the honor of placing before the Academy a new translucid plastic matter, which is formed with the principal elements of the paint of which I have just spoken, but in very different proportions. It is a combination of potato fecula and hydrated chloride of zinc, of a sufficient density to swell the fecula without dissolving it. To modify the hardness of the matter, and to render it more or less white or more or less opaque, certain salts or powdered matters are added, such as oxide of zinc, sulphate of baryta, etc. This plastic matter is prepared cold by moistening the fecula and other matters with chloride of zinc. This new compound is easily moulded, and solidifies in the mould like plaster. The objects thus obtained are as translucid as horn, bone, or ivory; but to obtain this translucidness, very little or none of the moist pulverulent substances must be introduced, except sulphate of baryta. This salt, although insoluble, gives very little opacity to the matter. This is not the case with oxide of zinc or carbonate of lime.

To keep the objects thus obtained from moisture, they are covered with one or two layers of good varnish.

Any color may be given to this new matter, and it may be obtained more or less hard; it may even be obtained as supple as caoutchouc, but not elastic.

This new plastic compound may be used for moulding many objects of art and ornament, and in the manufacture of many things requiring either hardness, suppleness, or transparency. This substance may replace, in many cases, plaster, marble, ivory, horn, bone, gutta-percha, gelantine, etc.

ON A METHOD OF IMPARTING A RED COLOR TO BONE AND IVORY, BY DR. J. C. KELLERMANN.

The bone to be colored is laid for fifteen to twenty minutes in very dilute cold nitric acid of the strength of a good vinegar; this dilute nitric acid is obtained by mixing fully one-half a litre of soft water with about thirteen grms. of nitric acid. The bone is then immersed for fifteen to twenty minutes in a solution of protochloride of tin, made by dissolving a piece of the size of a lentil in a pint of water. The objects thus mordanted are then put into the following red-bath, which must first be heated until it begins to boil.

Red-bath.—For an experiment on the small scale, take three to five grms. of fine red carmine, pour to it ten to twelve drops of ammonia, and stir it up well until the carmine is dissolved; then add about two ounces of soft water. In this bath, when heated to boiling, the objects must be left for about fifteen minutes. The tints obtained are more vivid when the boiling of the bath is not continued whilst the objects are in it.

If it be desired to change the tint thus obtained (a very fiery carmine-red) to a more scarlet, one of the following methods may be employed. When the red-bath begins to boil, and immediately after the objects have been immersed in it, five to ten drops of tartaric acid, of the strength of a good vinegar, may be added; or the water in which the protochloride of tin is to be dissolved may be mixed with an extremely small quantity of English sulphuric acid.—*Dingler's Polytechn. Journal.*

ON THE PREPARATION OF SOME SIMPLE CEMENTS.

Dr. Davy, F. R. S., has recently published the following paper on the manufacture of various simple cements, which admit of a useful and ready application.

Gutta-percha, as is well known, is itself an admirable cement for certain purposes, when softened by hot water or by a moderate degree of heat; and it has been used in making other cements; but I am not aware of any cement described in which it forms a part; its high commercial value is an obstacle to its application in many cases where it could be employed with advantage.

Gutta-percha, though readily adapted to an almost endless variety of uses, is, however, not easily rendered fluid when alone, and hence is not quite manageable enough for certain purposes.

I have made many experiments, using different proportions of gutta-percha with pitch, resin, wax, etc., with a view to form useful cements. In the present communication I propose to notice only one cement, which I made by melting in an iron saucepan. Two parts by weight of common pitch, and adding to it one part by weight of gutta-percha, stirring and mixing them well together until they were completely incorporated with or united with each other, formed a homogeneous fluid, which may be used in this state for many purposes; but which, on account of the facility and tenacity with which it adheres to metals, stones, glass, etc., I found convenient to pour into a large basin of cold water, in a thinner or thicker stream, or as a cake. In this state, while warm, it is quite soft; but may be soon taken up out of the water and drawn out into longer, or pressed into shorter pieces, or cut, or twisted into fragments, which may again be readily reünited by pressure.

When the cement is cold, or before, it may be removed from the water and wiped dry, when it is fit for use.

From a rough experiment I made, there appeared to be a loss of about one-fourteenth of the weight of the materials in making this cement, arising from volatile matter and impurities in the pitch and gutta-percha.

Properties. — This cement is of a black color; when cold, it is hard. It is not brittle, but has some degree of elasticity, which is increased by a slight increase of heat. It appears to be not so tough as gutta-percha, but more elastic. Its tenacity is very considerable, but inferior, if I mistake not, to gutta-percha. It softens when put into water at about 100° Fahr.; and if the heat is gradually increased, it passes through intermediate states of softness, becomes viscous like bird-lime, and may be extended into threads of indefinite length; it remains in this state, even when exposed for some time, in a crucible, to the heat of boiling water, at 212° Fahr. When heated to about 100° Fahr., it becomes a thin fluid. Water appears to have no other action upon it but that of softening it when warm or hot, and slowly hardening it when cold. The cement adheres strongly, if pressed on metal or other surfaces, though water be present, provided such surfaces be warm.

My first trials with this cement put it to a very severe test. I used it as a substitute for plumbers' solder in repairing the lead gutters on the roof of my house, which were cracked in several places, and admitted water freely in different places, and also to staunch the leaks in an old common and forcing-pump attached for yielding a supply of water for the use of two houses, and raising it about thirty feet. For these purposes I found it quite effectual. All that was necessary, in the case of the gutters, was to remove with a brush all loose earthy matters from the cracked lead, slightly warm it with a hot iron, then pour the cement in a fluid state on the cracks, so as to cover them on both sides; then a hot iron was drawn along each edge of the cement so as to soften and bevel it down to the lead, as the cement has intermediate degrees of fluidity, and is thicker or thiner as it is exposed to more or less heat. In its thicker state, it may, perhaps, be better adapted to repair cracks in lead or other gutters; but a crack in such gutters may be readily filled up by taking a piece of the dry, cold cement, and applying a warm but not too* hot soldering-iron, so as to soften the cement on the crack, then melt it on each side and cover it with the cement. The cement will adhere with great ease to the lead, and is far more manageable than any of our common solders. A hole in a gutter could be readily stopped with the cement, and a piece of lead of sufficient size to overlap the hole, say, about one-half of an inch; cover the lead on both sides with a surface of the cement; press it on the hole: then cover the lead and its edges with the cement, as in puttying a pane of glass.

In the case of the common and forcing-pump, it was only necessary to have every part that leaked quite dry, and slightly warm, when a good coating of the cement, in its thick state, was applied, so as completely to cover

* When the soldering-iron is too hot, and applied to the cement, it decomposes a portion of it, or raises it in a white vapor. When it is of the proper temperature, which is about 130° Fahr., it is merely softened or partially melted.

the cracks or apertures. The cement used in this instance did not exceed in bulk the plumber's solder which would have been used. The warm soldering-iron was lastly applied to fill up any interstices, and produce throughout a uniform surface.

I entertain no apprehension that the warmth of our climate at any time will impair the efficacy of this cement when applied to repair lead, zinc, or iron gutters; for though it softens at a comparative low temperature, it still adheres most tenaciously to metals and other substances, and does not allow water to pass through it. My gutters were repaired with the cement before the very hot weather we had last summer, and not the least appearance of a leak has been since observed in the gutters.

In a similar way the cement may be readily applied to repair holes in tin cans, garden watering-pots, iron or other metal vessels which are used only for cold water. Vessels thus repaired should be left a few hours before they are used, as the cement takes some little time to set or harden.

That the presence of water does not interfere with the action of the cement, was shown in cases where I put a large hammer, also a seven-pound weight, into hot water, for a few minutes. I then removed them from the water, and, without wiping them, pressed the end of a small stick of the cold cement on the surface of each, when it softened, and strongly adhered to the metals. I then poured a stream of cold water on each of the articles, when they could be raised from the table and carried about, being firmly supported by the cement, and considerable force was necessary to separate the articles from the cement.

This cement is applicable to many useful purposes. It adheres with great tenacity to metals, wood, stones, glass, porcelain, ivory, leather, parchment, paper, hair, feathers, silk, woollen, cotton, linen fabrics, etc. It is well adapted for glazing windows, as a cement for aquariums. As far as my experience has yet extended, this cement does not appear to affect water, and will apparently be found applicable for coating metal tanks; to secure the joints of stone tanks; to make a glue for joining wood, which will not be affected by damp; to prevent the depredations of insects on wood. It may be highly deserving of inquiry, whether the cement may not be applied to preserve surfaces of metal and wood exposed to the atmosphere and to fresh water; also to protect anchors, chain-cables, etc., from the corroding agencies of sea water.

According to Krebs, an excellent cement for luting distilling apparatus may be prepared by mixing meal of beans with water, or with paste of starch, into a plastic mass. This may be used for distillation, in a small or large way, by applying it simply with wet fingers. After a few hours the cement becomes as hard as stone.

Casein Cement. — Wagner recommends using a cold saturated solution of borax or alkaline silicate for dissolving casein, instead of alkaline carbonate, as recommended by Bracconot. The solution of casein with borax is a clear viscous liquid, exceeding gum in adhesiveness, and applicable to many purposes as a substitute for glue; woollen and cotton fabrics saturated with the solution may be tanned with tannic acid or acetate of alumina, and rendered waterproof.

ON HYDRAULIC MORTARS.

The different varieties of this material may be divided into two classes, according to the chemical processes to which the hardening under water is due, viz., the *Roman Cements*, which contain caustic lime when fresh, and the *Portland Cements*, which do not contain caustic lime.

Fuchs has satisfactorily shown that the hardening of Roman cement, apart from the production of carbonate of lime, is essentially due to the production of basic silicate of lime (3 CaO, 2 SiO_3), by the combination of an acid silicate, or of free soluble silica with caustic lime.

The hardening of Portland cement under water, is, on the contrary, due to the decomposition of a silicate, consisting of three or four equivalents of base combined with one equivalent of silica, with the production of caustic lime, and such compounds of lime with silica and with alumina as may be produced in the wet way.

Consequently, the hardened Portland cement contains the same substances as hardened Roman cement; but they are produced in a different way, in the hardening by the action of water.

Mr. Winckler has made a series of analyses of different specimens of Portland cement, and comes to the conclusion that the silica may be replaced by alumina or peroxide of iron; that the presence of alumina does not interfere with the hardening of the cement, but renders it less capable of resisting the action of carbonic acid; and that the presence of peroxide of iron renders the cement less hard and less durable.

He also found that, during the hardening of Portland cement, lime is gradually abstracted by water. The composition of the cement, after this action had gone on for some days, was found to correspond closely with the formula $3\ CaO,\ SiO_3 + CaO,\ Al_2\ O_3$; and he regards this compound as the final result of the action of water upon Portland cement.

He considers the presence of magnesia in Portland cement injurious, owing to the fact that the tribasic silicate of magnesia and lime is not decomposed by water.

The presence of alkalies in the cement seems to accelerate the hardening, probably by facilitating the penetration of water into the mass.

SOLUBLE GLASS, WATER GLASS, MINERAL GUM.

Soluble silicate of soda is now largely manufactured in England, and sold, under the above names, for various important commercial purposes. Long known to chemists, it has within a recent period attracted the attention of manufacturers, who find it possesses properties highly useful, and susceptible of a great variety of applications. It is prepared most cheaply from quartz-sand and soda-ash. The former is very finely ground, and then dissolved in a heated solution of the alkali. The solution is then concentrated to 20°, 30°, or 50° of Twaddle, and sold of the strength best adapted for the purpose contemplated. A large demand has recently arisen for it as a size for calicoes, etc., in lieu of British gum, the glaze being superior, and the price (£6 to £7 a ton) being lower.

It is also in demand for soap-making; being itself a detergent, and mixed with common soap, prevents the usual loss by evaporation, and increases the weight. It has also been used for saturating timbers, scenery, dresses, and other inflammable material; being itself of a highly incombustible nature, and thus protecting other substances to a great extent from the action of fire. In contact with lime, it consolidates, and is partly converted into silicate of lime. Hence it has been employed, especially in Berlin, in the process called "Stereochrome," a revival of fresco painting on the walls of buildings; the design being perfectly protected by the application of a solution of the "water glass," as a varnish over the surface. This may be freely washed, and, if necessary, renewed from time to time.

The same substance is the essential element in Ransome's artificial stone process, and other similar processes, in which a porous standstone or limestone is saturated with this silicate, which not only consolidates, but combines with the lime, forming a compact mass of flinty hardness, and impervious to atmospheric influence.

Its solution possesses highly adhesive properties, and may be considered the basis of a good cement for glass and china. By itself it holds fragments with great tenacity in the cold, but yields on the application of heat. The solution for this purpose must not be too strong; and if mixed, when used, with a little lime, the cement is very firm.

ON ANIMAL AMMONIA, ITS FORMATION, EVOLUTION, AND OFFICE.

The following is an abstract of a paper on the above subject, presented to the British Association, 1858, by the Rev. J. B. Reade. The author, after referring to the testimony of Brande and Schlossberger, as to our ignorance of the cause of the coagulation of the blood, and pointing out the near approach to the solution of the problem by Raspail, proceeds to show that Dr. Richardson, in his recent work on the subject, has the undivided and justly rewarded merit of proving that coagulation proceeds solely in proportion to the evolution of ammonia. With reference to his own views on the subject of the paper, the author makes the following observations: — Ammonia, as well as fibrin, exists in the blood, and we have now sufficient, or rather cumulative proof, that the necessary solution of fibrin is caused by the agency of this volatile alkali. It is also equally apparent that a nice adjustment of the quantity of this alkali is indispensable; since an excess, operating beyond the production of fluidity, would tend to dissolve the blood corpuscles themselves, and a defect would be marked by the deposition of fibrin in the heart or arteries. But though ammonia is formed, and that in larger quantities than is required for its primary office and operation, viz., the solution of fibrin, yet the excess is with great care drawn away from the blood and used where nature requires it. As a gentle stimulant, its presence is required throughout the whole system, and accordingly it enters, along with fibrin, into the formation of muscular tissues. This I showed many years ago, in a paper read before the Microscopical Society of London. It is true that my experiments on the presence of ammonia, *quasi* ammonia, in breath, flesh, and animal tissues generally, were received with much caution, or, rather, I may say, with hesitation and doubt, and even ocular demonstration failed, for a time, to remove foregone conclusions; but the existence of ammonia as a normal excrete of the body, is now recognized by all parties as an important and undisputed fact, and its power and office as a solvent of the fibrin of the blood, is exactly determined. The primary source and formation of this alkaline solvent, or what leads to its normal development, is a physiological problem yet unsolved. Its elements are well known; but whence derived, or how, or in what part of the body, if in it at all, the chemical combination is effected, are questions which are supposed to point to additional illustrations of the limit of human investigation and reason. Yet that it is absolutely within the body that the formation of the alkaline compound takes place, appears to be capable of proof. For it is quite certain, as the result of repeated experiment, that the ammonia found in the breath — varying so much in different persons at the same time, and in the same person on different parts of the same day, and especially during the different conditions of rest, exercise, and fatigue — is not the mere return of the minute portion inspired with the air. And if the air, when charged with its uniform small quantity, fail so manifestly in supplying even the ammonia of the breath, it must, of course, be rejected as the source of that additional quantity which, at the same time, is found in every part of the body. The source and formation of this alkali, therefore, is not *ab extra.* It seems, perhaps, probable that animal ammonia is formed initially in the blood, of which the two leading ingredients, albumen and fibrin, are equally rich in *nitrogen;* for this element exists in albumen, according to the analysis of Gay-Lussac, in the proportion of 15·7 per cent. and of 19·9 per cent. in fibrin; while *hydrogen*, the other element of ammonia, is in the proportion of 7 per cent. in each. The elements of the alkali, therefore, are present, and are partly used for the formation of substances which are products of subtle chemical action. Now, in the vegetable kingdom, the combination of these elements for the formation of vegetable ammonias is a common and recognized phenomenon; and similarly, — to extend the views of Dr. Richardson, — in the exquisite balance of the chemical forces in the blood, it is arranged that the blood should be feebly alkaline from fixed alkali or alkaline salt; not sufficiently alkaline to hold fibrin in solution, but sufficiently so to leave the volatile alkali free for this purpose, when formed in the closed chambers of the circulation. I am, therefore, less disposed than my friend Dr. Richardson to leave this point an open question, but rather to meet his inquiry, where is ammonia first formed? with the reply, in blood itself. It is with some satisfaction I can add, that Dr. Richardson himself gives his *imprimatur* to this theory. If this view, then, be anything like an accurate statement of the chemistry of nature, it confirms and harmonizes with the fact, that the formation of ammonia is a continuous process. The portion which maintains fluidity at a given moment does not remain to exercise this office for hours or days, but its evolution direct from the blood is as necessary and continuous an act as its formation. Hence it passes along with fibrin, in fact carries the fibrin, as its solvent, to every part of the body, to supply its daily waste; and having performed this office, and satisfied every other demand, the excess is evolved, in consequence of its equal diffusion, from every excretory surface, and very largely, as I have heretofore proved, from the surface of the lungs in the expired air of the breath. The evolution of ammonia from the surface of the body may be proved by an interesting experiment which happens not to have found a place in Dr. Richardson's admirable list of 400 save one. If a glass vessel, of suitable shape, having its inner surface just moistened with hydrochloric acid, be placed on any part of the body when warm with exercise, and, therefore, in a slight state of perspiration, evolved ammonia will be taken up by the acid; and if collected in a little distilled water, the hydrochlorate may be received and crystallized by evaporation on a slip of glass for the microscope. The same experiment may also be performed on the bodies of horses and other animals. The general experiments which prove the existence of ammonia in breath are now too well known to need description; but there is a new experiment of considerable importance, as confirming the proof of these two propositions — that there is a volatile alkali evolved in the breath, and that this alkali, having the property of maintaining the fluidity of the blood, is ammonia. Dr. Richardson has proved that in the experiment of passing the vapor of blood through blood, coagulation is suspended by the agency of a volatile principle; and he has also proved by experiment that this volatile principle is ammonia. Now, the vapor of blood is a large constituent of the vapor of breath, and the effect of passing this latter vapor through blood is precisely similar to that of the former. If a portion of blood be received in a vessel, and the expired air and vapor of breath, collected in quantity and in a suitable apparatus, be passed through it, the fluidity of the blood is maintained so long as the experiment is continued; thus furnishing a proof of the escape of a volatile agent in the breath, which agent, by direct experiment upon it, is proved to be ammonia. This experiment is, in all respects, most satisfactory. Had it failed, the whole subject would again be enveloped in its ancient mystery, and we should say with Brande, that the cause of coagulation is still unexplained. True, we should know that the vapor of blood sustains fluidity, and that its volatile alkali, ammonia, sustains fluidity also; but so do the fixed alkalies, potash and soda, which are proved to be inoperative as the cause of coagulation. If, then, the vapor of breath, which is characterized by the same volatile agent as the vapor of blood, failed to prevent coagulation, we must unavoidably be led to the conclusion, that, notwithstanding the evidence of experiment in a given direction in favor of ammonia, there is a still more subtle agency at work, even during the evolution of this alkali from newly-drawn blood, which is the true and ultimate cause of coagulation. Ammonia, like potash and soda, would then be looked upon as a mere proximate agent in sustaining fluidity, and its evolution would cease to be acknowledged as the final and efficient cause of coagulation.

ON THE FORMATION OF UREA BY THE OXIDATION OF ALBUMINOUS MATTERS.

Béchamp has succeeded in showing that urea is one of the products of the oxidation of albuminous bodies. The author effects the decomposition by an alkaline solution of hypermanganate of potash. The fibrine of the blood and gluten yield also urea by the same process. From these experiments it is clear that the oxidation of albuminous matters under an alkaline influence yields products very different from those obtained at a higher temperature by means of oxidizing mixtures of peroxide of manganese or bichromate of potash and sulphuric acid. — *Ann. de Chimie et de Physique*, xlvii, 348.

ON THE PRESENCE OF COPPER IN THE TISSUES OF PLANTS AND ANIMALS.

Drs. Odling and Dupré, in a communication to the British Association, stated that they had made more than one hundred examinations, by a great variety of processes, and had recognized the presence of copper in nearly every instance. In several specimens of wheat grain and human viscera the copper had been estimated. From 100 grains of wheat-ash the authors had obtained 251 thousandths of a grain, and from a sheep's liver rather more than one-half a grain of oxide of copper. The process was to precipitate the copper electrolytically on a platinum wire, to dissolve in nitric acid, and to ignite the residue of the evaporated solution.

SOIL ANALYSIS.

A recent writer in the North British *Agriculturist* makes the following remarks on the subject of soil analysis:

"To analyze a soil, and determine from the results the degree of its fertility and its adaptation to particular crops, was one of the problems placed before the agricultural chemist, and from its solution the greatest advantages to agriculture were anticipated. *As yet these expectations have not been realized*, nor can this be considered as a matter of surprise. The progress of our knowledge, in place of simplifying, has complicated the question, and has shown that the fertility and infertility of a soil is dependent upon a *variety* of circumstances, of which its chemical composition is *only one*. Instances exist in which the barrenness of a soil can be distinctly traced to the deficiency of some one or other of the necessary elements of plant-life; but in other cases, a barren and a fertile soil may present an almost perfect similarity in composition, and contain all the elements required by plants in proportions known to be amply sufficient for their healthy growth. The difficulty of explaining these facts has been increased, just in proportion as soil analyses have become more minute; for their tendency has been to show that the instances in which infertility is due to the *absence* of any of the essential constituents of the plants are comparatively *rare*, and that quantities which we are apt to overlook as totally unimportant, may be amply sufficient for all that is required. One-tenth of a per cent. of potash, soda, or phosphoric acid, may appear a quantity so small that the chemist might be justified in neglecting it, and yet a soil containing these quantities is capable of affording an *abundant supply* of these elements to *many generations of plants*; and, notwithstanding this, there are soils containing a much larger quantity of these substances, which, if not absolutely barren, are only capable of supporting a very scanty vegetation. These facts have rendered it obvious that it is not merely the presence, but the accessibility, so to speak, of the constituents of a soil that must be determined; and when the chemist, in addition to the exact proportion of these minute quantities, is required to ascertain the various forms of combination in which they exist, it is natural that he should show little disposition to enter upon a branch of investigation of such complexity, and which, in the present state of our knowledge, is likely to give only negative results.

"The difficulties of this investigation have been so fully recognized by Liebig, that he has pronounced it *impossible* to arrive at a satisfactory knowledge of the composition of the soil, and its suitableness for particular crops, by analysis alone."

ON THE ANNUAL YIELD OF NITROGEN PER ACRE IN DIFFERENT CROPS.

The following is an abstract of a paper on the above subject, presented to the British Association, 1858, by Messrs. Lawes & Gilbert:

The authors stated, in commencement, that they had, at a former meeting of the Association, announced, that the amount of nitrogen yielded per acre per annum, in different crops, even when unmanured, was considerably beyond that annually coming down, in the form of ammonia and nitric acid, in the yet measured and analyzed aqueous deposits from the atmosphere. The investigations upon this subject were still in progress; and a desirable introduction to the record of the results would obviously be, to illustrate, by reference to direct experiment, that which had been before only assumed, regarding the yield of nitrogen in our different crops. To this end had been determined the annual produce of nitrogen per acre, in the case of various crops, which were respectively grown, for many years consecutively, on the same land; namely, wheat, fourteen years; barley, six years; meadow hay, three years; clover hay, three years out of four; beans, eleven years; and turnips, eight years. In the majority of the instances referred to, the yield of nitrogen had been estimated, both for the crop grown without manure of any kind, and for that with purely mineral manure, — that is, excluding any artificial supply of nitrogen. It was the object of the present communication to give a summary view of some of the facts thus brought to light. Beans and clover were shown to yield several times as much nitrogen per acre as wheat or barley. Yet the growth of the leguminous crops, carrying off so much nitrogen as they did, was still one of the best preparations for the growth of wheat; whilst fallow (an important effect of which was the accumulation within the soil of the available nitrogen of two years into one), and adding nitrogenous manures, had each much the same effect in increasing the produce of the cereal crops. Other experimental results were adduced, which illustrated the fact, that four years of wheat, alternated with fallow, had given as much nitrogen in eight years as eight crops of wheat grown consecutively. Again, four crops of wheat, grown in alternation with beans, had given nearly the same amount of nitrogen per acre as the four crops grown in alternation with fallow; consequently, also much about the same as the eight crops of wheat grown consecutively. In the case of the alternation with beans, therefore, the whole of the nitrogen obtained in the beans themselves was over and above that which was obtained, during the same series of years, in wheat alone, — whether it was grown consecutively, or in alternation with fallow. Interesting questions arose, therefore, as to the varying sources, or powers of accumulation, of nitrogen, in the case of crops so characteristically differing from one another as those above referred to. It had been found that the leguminous crops, which yielded in their produce such a comparatively large amount of nitrogen over a given area of land, were not specially benefited by the direct application of the more purely nitrogenous manures. The cereal crops, on the other hand, whose average yield of nitrogen, under the same circumstances, was comparatively so small, were very much increased by the use of direct nitrogenous manures. But it was found that, over a series of years, only about four-tenths of the nitrogen annually supplied in manure for wheat or barley (in the form of ammonia salts or nitrates), were recovered in the immediate increase of crop. Was any considerable proportion of the unrecovered amount drained away and lost? Was the supplied nitrogenous compound transformed in the soil, and nitrogen, in some form, evaporated? Did a portion remain in some fixed and unavailable state of combination in the soil? Was ammonia, or free nitrogen, given off during the growth of the plant? Or, how far was there an unfavorable distribution, and state of combination, within the soil, of the nitrogenous matters applied directly for the cereal crops, — those, such as the leguminous crops, which assimilated so much more, gathering with greater facility, and from a different area of soil, and leaving a sufficient available nitrogenous residue within the range of collection of a succeeding cereal crop? These questions, among others, which their solution more or less involved, required further elucidation before some of the most prominent of agricultural facts could be satisfactorily explained. Comparing the amount of nitrogen yielded in the different crops, when grown without nitrogenous matter, as above referred to, with the amount falling in the measured aqueous deposits, as ammonia and nitric acid, it appeared, taking the average result of the analyses of three years' rain, that all the crops yielded considerably more, and some very much more, than so came down to the soil. The same was the case when several of the crops had been grown in an ordinary rotation with one another, but without manure, through two or three successive courses. Was this observed excess in the yield over the yet measured sources at all materially due merely to exhaustion of previously accumulated nitrogenous compound within the soil? Was it probably attributable chiefly to the absorption of ammonia or nitric acid, from the air, by the plant itself, or by the soil? Was there any notable formation of ammonia or nitric acid from the free nitrogen of the atmosphere? Or did plants generally, or some in particular, assimilate this free nitrogen? As already intimated, some of the points which had been alluded to were at the present time under investigation. Others, it might be hoped, would receive elucidation in the course of time. There, of course, still remained the wider questions of the original source, and of the distribution and circulation of combined nitrogen in the soil, in animal and vegetable life on the earth's surface, and in the atmosphere above it.

BOUSSINGAULT'S RESEARCHES UPON THE RELATION OF NITROGEN AND THE NITRATES TO THE SOIL AND VEGETATION.

Several years ago Boussingault demonstrated, in the clearest way, that plants are incapable of assimilating the free nitrogen of the atmosphere. Two years ago, in a paper communicated to the French Academy of Sciences, he showed that nitrates eminently favor vegetation. He now shows, by decisive experiments —

(1). That the amount, even of ternary vegetable matter, produced by a plant, depends absolutely upon the supply of assimilable nitrogen (ammonia and nitrates). A plant, such as a sunflower, with a rather large seed, may grow in a soil of recently calcined brick, watered with pure water, so far as even to complete itself by a blossom; but it will only have trebled or quadrupled the amount of vegetable matter it had to begin with in the seed. In the experiments, the seeds weighing 0·107 grammes, in three months of vegetation formed plants, which, when dried, weighed only 0·392 grammes, — a little more than trebling their weight. The carbon they had acquired from the decomposition of carbonic acid of the air was only 0·114 grammes; the nitrogen they had assimilated from the air in three months was only 0·0025 grammes.

(2.) Phosphate of lime, alkaline salts, and earthy matters, indispensable to the constitution of plants, exert no appreciable action upon vegetation, except when accompanied by matters capable of furnishing assimilable nitrogen. Two plants of the same kind, grown under the same conditions as above, but with the perfectly sterile soil adequately supplied with phosphate of lime, alkali, in the form of bicarbonate of potash, and silex, from the ashes of grasses, resulted in only 0·498 grammes of dried vegetable matter, from seeds weighing 0·107 grammes; and had acquired only 0·0027 grammes of nitrogen beyond what was in the seeds.

(3.) But nitrate of potash, furnishing assimilable nitrogen, associated with phosphate of lime and silicate of potash, forms a complete manure, and suffices for the full development of vegetation. Parallel experiments, with nitrate in place of bicarbonate of potash, resulted in the vigorous growth of the sunflower plants, and the formation of 21·248 grams of organic matter, from seeds weighing, as before, only 0·107. This 21·111 grams of new vegetable matter, produced in three months of vegetation, contained 8·444 of carbon, derived from the carbonic acid of the air, and 0·1666 grams of nitrogen. The 1·4 grams of nitrate of potash applied to the soil, contained 0·1969 grams of nitrogen, leaving a balance of 0·0303, nearly all of which was found, unappropriated, in the soil.

Finally, Boussingault made a neat series of comparative experiments, introducing into calcined sand the same amount of phosphate of lime and carbonate of potash, but different proportions of nitrate of soda, or, in other words, of assimilable nitrogen, and watering with water free from ammonia, but containing a quarter of its volume of carbonic acid. The soil divided among four pots, each having two seeds of sunflower (*H. argophyllus* was the species used in all the experiments); the pot

No. 1	received of	nitrate of soda,	0·00	grams.
" 2	"	"	0·02	"
" 3	"	"	0·04	"
" 4	"	"	0·16	"

The results of fifty days' vegetation are given in the rate of growth, size and number of the leaves, weight of product, etc.:

No. 1	made of new	vegetable matter,	0·397	grams.
" 2	"	"	0·720	"
" 3	"	"	1·130	"
" 4	"	"	3.280	"

In No. 2, so little as three milligrams of assimilable nitrogen introduced into the soil enabled the plant to double the amount of organic matter. The proportion of the weight of the seeds to that of the plant formed was, in

No. 1,	as	1 : 4·6 gr.
" 2,	"	1 : 7·6
" 3,	"	1 : 11·3
" 4,	"	1 : 30·8

In no case did the nitrogen acquired by the plant exceed that of the nitrate added to the soil.

In the experiments where no nitrate was added to the soil, the two or three milligrams of nitrogen acquired by the plants during three months of vegetation, came, in all probability, from ammoniacal vapors and nitrates existing or formed in the atmosphere. To establish their presence, Boussingault arranged an apparatus which detected the production of some nitrates. And, in exposing to the air 500 grams of calcined sand, which had ten grams of oxalic acid mixed with it, in a glass vessel, with an open surface equal to that of one of the flower-pots used in the above experiments, the sand took 0·0013 grams of nitrogen from the air, of which a part was certainly ammonia.

The object of the researches, of which a summary is given in the second paper, was, to determine the quantity of nitrates contained, at a given moment, in one *hectare* of cultivated ground, one of meadow, one of the forest soil, and in one *metre* of river or spring water. The quantity in the soil was, of course, found to vary extremely with the extremes of wet or dry weather. Garden soil, highly manured every autumn, contained, on the 9th of August, 1856, after fourteen dry and warm days, 316·5 grams of nitre in a cubic litre of soil. On the 29th of the month, after twenty rainy days, the same quantity contained only 13 grams of nitre. The greater part had been dissolved out of the superficial soil.

Some specimens of forest soil, in a state of nature, furnished no indication of nitrates; others gave 0·7 and 3·27 grams of nitre to the cubic meter.

The soil of meadows and pastures afforded from 1 to 11 grams of nitre to the cubic metre. Nineteen specimens of good cultivated land gave, four of them, none; others, from 0·8 to 1·33; the richer ones, from 10·4 to 14·4; and one fallow, of exceptional richness, as much as 108 grams of nitre to the cubic metre. To the latter, much calcareous matter had been added.

The soil of a conservatory, from which the nitrates would not be washed away by rains, contained 89, or 161, and some rather deep soil 185 grams of nitre in the cubic metre.

The sources of the nitre are not difficult to understand when we reflect that a manured soil, especially a calcareous one, is just in the condition of an artificial nitre-bed. The ultimate result of the decomposition of ordinary manure is a residuum of alkaline and earthy salts, phosphates, and nitrates; the latter, with the ammonia, furnishing the assimilable nitrogen, all-essential to productive vegetation. In incorporating with the soil undecomposed manure, instead of the ultimate results of the decomposition, less loss is suffered from prolonged rains washing out the formed nitrates.

The soluble matters washed out of the soil are to be sought in the water. River and spring water, therefore, act as manure by the silex and alkali, the organic matter, and the nitrates which they hold. The spring waters poorest in nitre of those examined contained from 0·03 to 0·14 milligrams of nitre to the *litre;* the richer ones from 11 to 14 grams in the cubic metre.

As to river water: the Vesle, in Champagne, held 11 grams, the Seine, at Paris, 9 grams the cubic metre. These were the richest. The Seine, at Paris, carries on to the sea, in times of low water, 58,000 kilograms, in times of high water, 194,000 kilograms of nitre, every twenty-four hours. What enormous amounts of nitre must be carried into the sea by the Mississippi, the Amazon, and by every great continental river; and how active, beyond all ordinary conception, must the process of nitrification be over all the land; and how vast the supply of assimilable nitrogen for the use of the vegetation! — *Silliman's Journal.*

ON THE ABSORBENT POWER OF SOILS.

At a recent meeting of the London Chemical Society, a paper from Liebig was read, on the absorbent powers of soils, in which he maintained that the spongioles of plants, in obtaining their supply of saline matter, did not act by simple absorption, but exerted a real decomposing action upon certain ill-defined compounds which the saline matter formed with the insoluble constituents of the soil.

ON THE COMPOSITION OF VARIOUS PHOSPHATES OF LIME.

At a recent meeting of the Boston Society of Natural History, Dr. A. A. Hayes made a communication, reporting progress in experiments elucidating points in connection with the composition of the various phosphates of lime, proceeding from changes during decay. He had stated in former communications, that the so-called bone phosphate of lime, of bones forming the earthy part of the "phosphatic guanos," or the guanos of the rainy latitudes, in presence of decaying organic matter, had lost one, two, or more equivalents of the lime base—leaving a monobasic phosphate. In subsequent actions, this lime disappeared by solution, while the apparently more soluble phosphate remained.

It was deemed important, at this stage of the investigation, to study the changes which the bone phosphate had suffered in the midst of putrefying matter, resulting in the production of Peruvian guano, where the conditions are peculiar. Analyses of a number of samples, from the different localities, have proved that in every case the lime of the bone phosphate has in part become engaged with other acids; the oxalic acid being generally the most powerful acid present; while the phosphoric acid disengaged, has united to ammonia and other bases found in the mass. This change of composition in the bone phosphate, under entirely different conditions of exposure, is interesting, apart from the additional evidence it affords *of a natural proximate decomposition of the tri-basic phosphate,* without the presence of abundance of water. In this new case, it is true, the phosphoric acid finds other bases to combine with; while in those before reported it remains in larger proportion with the lime and water only. It is generally believed that Peruvian and similar guanos are alkaline in constitution, from the fact that carbonate of ammonia is readily formed from them. Perfect samples are, however, always in the *acid state* naturally, and a large part of the volatile matter consists of salts of the volatile, oily acids and ammonia; carbonic acid being rarely found, unless as part of calcareous matter, accidently present. This fact establishes a resemblance which was unexpected, between the two different modes of decomposition of organic matter; one in presence of abundance of water; the other, where mere moisture and decaying organic remains are abundant.

Another course of experimenting has been pursued on this subject of bone decomposition, which had been alluded to before, and the results have a more general interest, as they affect physiological conclusions. Chemical physiologists, with hardly an exception, consider the earthy part of bones as composed of tribasic phosphate of lime, bi-basic phosphate of magnesia, carbonate of lime, and fluoride of calcium; alkaline salts being occasionally present. This mixture of earthy bodies, rarely definite, is subject to great variations of proportions in disease, and it is so loosely connected with the cartilage, that absorption and deposition take place, without derangement of vital functions.

On looking over the evidence supporting this important conclusion, one is surprised to find that it is all derived from two courses of analytical inquiry.

1st. Carefully cleansed and fresh bones are treated by means of dilute acids, for the solution of the earthy salts, leaving the cartilage in nearly its natural form. Carbonic acid is usually determined in this operation, and the phosphoric acid and bases form the solution; the acids and bases are then apportioned by calculation.

2d. Bones which have been washed, and reduced to coarse powder, are heated with free contact of air, until the organic part of the cartilage is destroyed; the remaining ash is then analyzed for acids and bases, and the compounds calculated.

If we carefully make the analytical experiments, on exactly the same portion of fresh bone, by *both these methods*, we do not arrive at corresponding results; and it will thus appear, that neither of these courses will bear criticism. As this part of the subject will be presented to the Society in connection with the evidence, it may be remarked now, that the kind of information such results afford, is like that we obtain respecting the salts existing in vegetable productions, when we analyze their ashes, instead of confining our trials to the tissues and cell walls.

In the analyses of osseous tissue which have been made since my communication to the Society, the simple process of decomposition in water has been continued in application. As before observed, the bones readily impart their earthy part to this fluid, both before and after the cartilage becomes changed, in the act of decomposing into simpler forms of matter. Thus far, the results have accorded with those of earlier trials, showing that in the osseous tissue, *bi-basic* phosphate of lime may exist, and that the presence of mono-basic phosphate is not inconsistent with a neutral condition in the tissues. It remains to be proved that *protein*, as well as water, may act as a constitutional element equivalent to a base, in connection with lime, to form double phosphates; and we shall then have a simple and consistent explanation of the fact of the existence of mono and bi-basic phosphates, as now found in the secretions both healthy and morbid.

ON THE EVOLUTION OF AMMONIA FROM VOLCANOES.

Dr. Daubeny, in a recent communication to the London Geological Society on the above subject, referred to the existence of a chemical compound of titanium with nitrogen, known from the researches of Wöhler and Rose; and pointed out its bearing on one part of the theory of volcanoes — namely, the evolution of ammonia, and the consequent presence of ammoniacal salts amongst the products of their operations. He then commented on the hypotheses already suggested by Bischoff and Bunsen, to account for the volcanic production of ammonia — viz., 1st, the decomposition of carbonaceous or other organic substances; 2ndly, the conversion, by the hot lava overflowing the herbage, of the nitrogenized matter present in the latter into ammonia, and the combination of this with the muriatic acid in the lava, giving rise to the sublimation of sal-ammoniac. To both of these hypotheses the author pointed out serious objections. He had himself proposed to account for the presence of ammonia in volcanic outbursts by assuming that the gaseous hydrogen, although incapable of combining with nitrogen under ordinary pressures, might unite with it under that exercised upon it in the interior of the earth; and he still believes this idea to be worthy of consideration, though, perhaps, it is impracticable to secure by experiment the conditions necessary for the chemical union of these two gases. The affinity, however, which certain metals possess for nitrogen, seems to afford more solid grounds on which to build a theory respecting the production of ammonia. Titanium has been found, by MM. Wöhler and St. Claire Deville, to absorb nitrogen from the air; and the union of heated titanic acid with nitrogen (forming a nitride of the metal) takes place, indeed, with so much energy as to generate light and heat; and thus constitutes a genuine case of combustion, in which nitrogen, and not oxygen, acts as the supporter. Although titanium is evidently present to some extent in most volcanoes, the author is not disposed to think that it abounds sufficiently to account for the large quantities of sal-ammoniac that are known to occur; but, rather, he argues by analogy that probably not only titanium, but other metals, such as iron, and probably even hydrogen, may combine directly with nitrogen in the interior of the globe, under conditions of great pressure, and other circumstances likely to modify the nature of those reäctions which take place under our eyes. In a postscript the author adverted to the recently discovered fact, that boron, like titanium, has the property of combining directly with the nitrogen of the air, and that the compound which it forms with it also possesses the property of evolving ammonia under the influence of the alkaline hydrates.

ON THE CHEMISTRY OF THE PRIMEVAL EARTH.

The primitive rocks which filled so large a place in the geological systems of the last century, are now being forgotten. We have learned that the oldest visible portions of the earth's crust are made up of sediments, the ruins of still older rocks, which were as varied in their character as are their derivatives. The primeval substratum has thus constantly receded before advancing science, and we are led to the conclusion that mechanical and chemical conditions similar to those of the present epoch presided over the formation of the most ancient rocks known.

But although the *materia prima* of the sedimentary rocks has long since been buried beneath its own ruins, its nature offers an interesting subject of consideration to the chemical geologist. If we admit the igneous theory of the earth, we may obtain a conception of the nature of the once liquid globe and of its atmosphere, by supposing the now existing matters of the earth's crust and surrounding fluids to be made to reäct upon one another under the influence of an intense heat. The quartz would decompose the carbonate of lime with the production of silicate and the liberation of carbonic acid, whose volume would be farther augmented by the combustion of all the mineral carbon at the expense of the atmospheric oxygen. The reäction between quartz and the chlorids of the sea, in the presence of aqueous vapor, would result in the formation of silicates and hydrochloric acid, while the sulphur would likewise be liberated as a volatile acid.

From these reäctions there would result on the one hand a more or less homogeneous mass of silicates of alumina and alkalies, with silicates of lime, magnesia and iron, a mixture probably resembling dolerite, while the atmosphere would be made up of watery vapor, nitrogen, a probable excess of oxygen, with carbonic, sulphuric and hydrochloric acids, representing all the carbon, sulphur, and chlorine of the globe.

When the cooling of the globe had so far advanced as to allow of the precipitation of water from this dense atmosphere, it would descend as an acid rain, which attacking, at an elevated temperature, the silicates, would give rise to chlorids of calcium, magnesium and sodium, mingled with sulphates of these bases. The liberated silica would probably separate during the cooling of the heated waters in the form of quartz.

The subsequent decomposition of the exposed portion of the primeval crust would result in the conversion of its feldspar into kaolin, and a soluble alkaline silicate, which, decomposed by excess of carbonic acid, would be carried to the sea as a bicarbonate; where, decomposing the lime salt, it would give rise to chlorid of sodium and bicarbonate of lime, which would be partly precipitated in a crystalline form, and partly secreted by marine animals. The carbonates of lime and magnesia set free during the slow decomposition of the primitive rock, would also go to augment the proportion of carbonates in the ocean, and help to fix in mineral masses the carbonic acid of the atmosphere.

At length we reach the carboniferous period of the earth's history, when a luxuriant vegetation completed the work of purifying the atmosphere, by transforming, as Brongniart long since suggested, the remaining excess of carbonic acid into carbon and oxygen gas, thus preparing the air for the support of warm-blooded animals.

By this hypothesis I think we get a clear conception of the generation from a primeval homogeneous mass of the quartzose, argillaceous and calcareous materials which make up the bulk of the stratified rocks, and we obtain at the same time a notion of the origin of the saline constituents of the sea. —*T. Sterry Hunt, Com. to Silliman's Journal.*

EFFECTS OF CARBURETTED HYDROGEN ON PLANTS.

The recent proceedings of the Philadelphia Academy contain a detailed statement of the effects of carburetted hydrogen gas on a collection of exotic plants, as experienced in an extensive greenhouse in Philadelphia, through the breakage of the city "mains" and the consequent leakage of a large quantity of gas. As a general result of the accumulation of gas, a collection of plants, numbering near three thousand, was almost entirely ruined. The plants not in leaf did not suffer, nor did a row of maple trees immediately over the leak; the injury sustained being entirely through their breathing organs. The effect produced upon the plants is detailed at length, a classified catalogue being given, with remarks upon each. In this list it seems apparent that the general sympathy known to exist between the genera of the same natural order extends to the action of this deleterious substance upon them. The beautiful *Amantiaceæ* were so keenly sensitive to the poison that even large old specimens were stripped at once. The stage was covered with the leaves, and oranges and lemons in all stages of growth, from fruit just formed to that fully matured. The trees, by careful pruning and nursing, were somewhat restored. *Camellia* were in the full glory of bloom, about one hundred and twenty varieties. Not a leaf, bud, flower, or woodbud remained upon the largest and finest plants; and at the slightest touch the leaves fell off in showers.

GEOLOGY.

RECENT PROGRESS OF GEOLOGY.

The following is an abstract of an address given before the Geological Section of the British Association, for 1858, at the opening session, by its Chairman, Prof. W. Hopkins:

The existence of mammalian life, in its earlier stages, on the surface of our planet, the condition of its existence, and the period of its introduction, have always furnished questions of the highest philosophical as well as palæontological interest. You will be aware that some geologists regard each new discovery of mammalian remains, in formations preceding the older tertiaries, as a fresh indication of the probable existence of mammalia in those earlier periods in which no positive proof of their existence has yet been obtained; while others regard such discoveries only as leading us to an ultimate limit, which will hereafter define a period of the introduction of mammalia on the surface of the earth, long posterior to that of the first introduction of animal life. Be this as it may, every new discovery of the former existence of this highest class of animals must be a matter of great geological interest. An important discovery of this kind has recently been made, principally by the persevering exertions of Mr. Beckles, who has detected, in the Purbeck beds, a considerable number of the remains of small mammals. The whole of them are, I believe, in the hands of Professor Owen, for the determination of their generic and specific characters; but Dr. Falconer seems already to have recognized among them seven or eight distinct genera, some of them marsupial, and others probably placental, of the insectivorous order.

The subject of the motion of glaciers is one of interest to geologists; for, unless we understand the causes of such motion, it will be impossible for us to assign to former glaciers their proper degree of efficiency in the transport of erratic blocks, and to distinguish between the effects of glacial and of floating ice, and those of powerful currents. An important step has recently been made in this subject by the application of a discovery made by Mr. Faraday, a few years ago, that if one lump of ice be laid upon another, the contiguous surfaces being sufficiently smooth to insure perfect contact, the two pieces, in a short time, will become firmly frozen together into one continuous transparent mass, although the temperature of the atmosphere in which they are placed be many degrees above the freezing temperature. Dr. Tyndall has the merit of applying this fact to the explanation of certain glacial phenomena. There are two recognized ways in which the motion of a glaicer takes place; one by the sliding of the whole glacial mass over the valley in which it exists; and the other by the whole mass changing its form in consequence of the pressure and tension to which it is subjected.

The former mode of progression is that recognized by the sliding theory; the second is that recognized by what has been termed the viscous theory of Professor Forbes. The viscous theory appeared to be generally recognized. Still, to many persons, it seemed difficult to reconcile the property of viscosity with the fragility and apparent inflexibility and inextensibility of ice itself. On the other hand, if this property of viscosity, or something of the kind, were denied, how could we account for the fact of different fragments, into which a glacier is frequently broken, becoming again united into one continuous mass? Dr. Tyndall has, I conceive, solved the difficulty. Glacial ice, unlike a viscous mass, will bear very little extension. It breaks and cracks suddenly; but the separate pieces, when subsequently squeezed together again, become, by regelation (as it is termed), one continuous mass. After some general remarks on the cause of the laminated structure of glaciers, during which he remarked that there was no doubt Dr. Tyndall was right in supposing the laminæ of blue and white ice to be perpendicular to the directions of maximum pressure, he said that it remained to be decided whether the explanations which had been offered were correct; but the actual perpendicularity of the laminæ of ice to the directions of maximum pressure within a glacier, and the probable perpendicularity to those directions of the laminæ in rock masses of laminated structure, would seem to establish some relation between these structures in rocks and glacial ice, giving an interest to this peculiar structure in the latter case, which it might not otherwise appear to possess for one who should regard it merely as a geologist.

ON THE GENERAL AND GRADUAL DESICCATION OF THE EARTH AND ATMOSPHERE.

At the Leeds meeting of the British Association, a paper, entitled as above, was read by Mr. J. S. Wilson, in which the author drew attention to the fact, that those who had travelled in continental lands, especially in and near the tropics, had been forced to reflect on the changes of climate that appeared to have occurred. There were parched and barren lands, dry river channels and waterless lakes, and, not unfrequently, traces of ancient human habitations, where large populations had been supported, but where all was now desolate, dry, and barren. After quoting largely from the works of various travellers and writers (among the latest of whom was Dr. Livingston), and giving interesting descriptions of dried-up rivers and desolate tracts of country in Australia, Africa, Mexico, and Peru, which had formerly been inhabited by man, Mr. Wilson concluded that there was a gradual solidifying of the aqueous vapors, and, consequently, of water, on the face of this terrestrial world, which, he inferred, was approaching a state in which it will be impossible for man to continue an inhabitant.

ON THE FORMATION OF CONTINUOUS TABULAR MASSES OF STONY LAVA ON STEEP SLOPES.

The question whether lava can consolidate on a steep slope, so as to form strata of stony and compact rock, inclined at angles of from 10° to more than 30°, has of late years acquired considerable importance, because geologists of high authority have affirmed that lavas which congeal on a declivity exceeding 5° or 6° are never continuous and solid, but are entirely composed of scoriaceous and fragmentary materials. From the law thus supposed to govern the consolidation of melted matter of volcanic origin, it has been logically inferred that all great volcanic mountains owe their conical form principally to upheaval, or to a force acting from below and exerting an upward and outward pressure on beds originally horizontal or nearly horizontal. For in all such mountains there are found to exist some stony layers dipping at 10°, 15°, 25°, or even higher angles; and according to the assumed law, such an inclined position of the beds must have been acquired subsequently to their origin.

Sir Chas. Lyell, in a recent communication to the Royal Society, after giving a brief sketch of the controversy respecting "Craters of Elevation," describes the result of a recent visit to Mount Etna, in company with Signor Gemmellaro, and his discovery there of modern lavas, some of known date, which have formed continuous beds of compact stone on slopes of 15°, 36°, 38°, and, in the case of the lava of 1852, more than 40°. The thickness of these tabular layers varies from 1½ feet to 26 feet; and their plains of stratification are parallel to those of the overlying and underlying scoriæ which form part of the same currents.

An attempt is then made to estimate the proportional amount of inclination which may be due to upheaval in those parts of the central nucleus of Etna where the dip is too great to be ascribed exclusively to the original steepness of the flanks of the cone. The highest dip seen by the author was in the rock of Musarra, where some of the strata, consisting of scoriæ with a few intercalated lavas, are inclined at 47°. Some masses of agglomerate and beds of lava in the Serra del Solfizio were also seen inclined at angles exceeding 40°. Some of these instances are believed to be exceptionable, and due to local disturbances; others may have an intimate connection with the abundance of fissures, often of great width, filled with lava, for such *dikes* are much more frequent near the original centres of eruption than at points remote from them. The injection of so much liquid matter into countless rents may imply the gradual tumefaction and distention of the volcanic mass, and may have been attended by the tilting of the beds, causing them to slope away at steeper angles than before, from the axis of eruption. But instead of ascribing to this mechanical force, as many have done, nearly all, or about four-fifths of the whole dip, Sir C. Lyell considers that about one-fifth may, with more probability, be assigned as the effect of such movements.

From various data, discussed at length in the memoir, Sir C. Lyell concludes, first, that a very high antiquity must be assigned to the successive eruptions of Etna, each phase of its volcanic energy, as well as the excavations of the Val del Bove, having occupied a lapse of ages, compared to which the historical period is brief and insignificant; and secondly, that the growth of the whole mountain must nevertheless be referred, geologically, to the more modern part of the latest Tertiary epoch.

ILLUSTRATIONS OF SURFACE GEOLOGY.

In a memoir, under the above title, communicated to the 9th volume of the Smithsonian contributions to knowledge, by Pres. Hitchcock, the author presents the results of a large amount of personal examination, made mainly in New England, though to some extent also in other countries. The heights and positions of numerous terraces along the Connecticut valley and some of its tributaries, and some on the Merrimack river, are given in the text, and the plates contain illustrations both by sections and neat maps. The great subject of terraces — a phenomenon that characterizes the whole breadth of the continent (except perhaps at the south), and therefore one of the grandest in the science — is approached in the right method, and an important step is taken towards solving the great problem. It is impossible in a brief notice to give a satisfactory review of the facts brought out in this long and valuable paper. We mention a few of the deductions.

1. True unstratified drift never covers the terraces; it is covered by the terrace material, and is therefore of older date.

2. The successive " beaches" and " terraces" are made out of the drift material, with few exceptions, all by essentially similar operations.

3. The river valleys were excavated before the terrace epoch, for in the rocky gorges, as at Bellows Falls, drift scratches are found, near the present water level; these gorges, therefore, were not closed at that time by rocky barriers.

4. The highest distinct river-terraces noticed by the author in this country are as follows: at Bellows Falls, on the Connecticut, 226 feet *above the river;* on the Deerfield, 236 feet; on Genesee river (New York State) at Mount Morris, 348 feet. Above these heights, there are other levels designated *beaches*, and the most distinct of these are stated to be from 800 to 1200 feet high; while others, less distinct, are mentioned as found at 1200 to 2600 feet in the White Mountains and elsewhere.

5. The number of terraces is usually larger, and the height greater, on the small than on large streams; there are seldom over *three* or *four* on the Connecticut; while on some of its tributaries, there are sometimes as many as *ten*.

6. The terraces on the opposite sides of the same stream very often do not correspond in elevation.

7. The terraces, in general, slope with the stream. They are usually somewhat the highest about a gorge, as if the gorge had occasioned a higher level to the river floods, and therefore more elevated depositions. They show by the stratification that they are the result of water-action; though stranded ice, it is urged, may have aided in making the variety of terrace called *moraine-terrace.*

8. The beaches also must have been produced by water, and this water must have been the ocean. "Hence, I feel sure," says the author, "from the facts which I have stated, that over the northern parts of this country, this body of water must have stood at least 2000 feet above the present sea-level; and I might safely put it at 2500 feet; for up to that height I have found drift modified by water."

9. As a consequence of these conclusions, it is argued that the drift period was a period of submergence for the larger part of the continent, and perhaps nearly the whole, and since that period, the ocean has stood 2000 feet deep over New England.

10. The formation of the succession of beaches might have taken place by a gradual elevation and drainage of the continent, without pauses in the movement of elevation; yet it is possible that some such pauses may have taken place. Still, the author argues against the probability of such pauses, and finally against the possibility of pauses for river terraces in general, on the ground mainly of their unequal heights on opposite sides of the streams.

11. As regards North America, "we may as yet safely say, that there is no evidence of the existence of life in the seas that covered it during the period

of unmodified drift; and indeed we might say the same of a considerable part of the period of the modified drift and alluvium."

12. Glaciers probably existed on the mountain summits that stood above the waters, as facts appear to show.

13. The apparent elevation of the continent may have been a consequence of an actual sinking of the bed of the ocean, drawing away the waters from the land.

14. The drift scratches, and transportations of stone, gravel, etc., were produced mainly by icebergs during a period of continental submergence.

THE GEOGRAPHICAL DISTRIBUTION OF LAKES, CATARACTS, AND NAVIGABLE RIVERS. BY DANIEL VAUGHAN.

Though lakes are very numerous in the Eastern States and in British America, they are almost entirely absent from the part of our continent included between the Atlantic Ocean and the Rocky Mountains, and between the thirty-first and thirty-second parallels of north latitute. This region contains over a million and a half of square miles; and the territory of Brazil, which is about double the extent, exhibits, in like manner, an absence of any large collections of fresh water. But both these regions are distinguished for their smooth streams and navigable rivers, which are unequalled in the advantages they afford for inland communication. France contains no lakes and no waterfalls of any magnitude, but its rivers are well suited for navigation; while, in Sweden and the State of Maine, lakes are numerous, and the rivers are so much interrupted by cataracts that they cannot be navigated to any considerable extent, even by the smallest vessels.

The fact that rivers are most free from cataracts and best suited for navigation, in regions where there are no lakes, is an evidence that running waters take a considerable part in forming channels for themselves. After one or two thousand centuries, most of the cataracts of Sweden, of Maine, and of British America, will be obliterated; their lakes will be drained or silted up, and their rivers, being cleared of impediments, will be well adapted to the purposes of navigation. Such changes have been produced in other lands during geological times. On several of the rivers of France and Spain, geologists have discovered the sites of ancient lakes, which have long since disappeared by aqueous action. It would seem also that, in former times, the Alleghany mountains enclosed many sheets of fresh water, similar to those found in the Alpine valleys, or between the Scandinavian mountains. Could we fill up all the channels of erosion, and deep ravines through which rivers flow, their waters should cover a large part of the land; and we may, therefore, infer, that lakes and cascades must have diversified the scenery of all regions, at some period of their geological history.

We may, therefore, regard the presence of numerous lakes and cataracts as an indication that the regions where they occur have been only recently elevated, and that they have not been long free from the effects of subterranean violence; while smooth streams and navigable rivers are an evidence of the great age of the lands where they are found, and show that they have been long released from the dominion of the waves

ON THE GEOLOGY OF HAYTI.

At a recent meeting of the Boston Society of Natural History, Dr. D. F. Wineland read a paper on some points of the geology of Hayti, the result of personal examinations.

The northern shore of the south-western neck of Hayti is mostly an iron-bound coast. There are but few small sandy bays, which serve as landing-places for the fishing-boats, and near them are generally found huts of fishermen, or a small village.

The rock which bounds all the rest of the coast is a hard, brittle limestone, formed very generally of a conglomeration of madrepores and other corals, as Astrææ, Mæandrinæ, Milleporæ, etc., and of various kinds of shells, cemented with a mass of smaller and formless lime particles, the powdered particles of the same corals and shells. This rock is full of pores and roundish cavities, with sharp edges, perhaps the places where softer shells or fragments of coral have been washed out by the erosion or attrition of the water, or knocked out by corals, thrown up in the stormy winter season of the furious north wind. The species of corals and shells which enter into the composition of this rock, I found nearly all alive in the adjoining sea. Some of them, however, have disappeared from among the living; others are dying out, and are now very rarely found, though common in the earlier portions of the present period; for they exist in great quantities in the rock at the depth of a few feet. Such animal remains enclosed in rock, yet belonging to species now living, or to species now extinct, but which lived in the earlier ages of this period, together with species now living, we are accustomed to call modern fossils. They are the more interesting because they show how, without any remarkable revolution of our globe, certain species of animals gradually die out.

The same rock, composed of modern fossils and their detritus, I found in the interior mountain regions of the island, about thirty miles from the sea-shore, and at a height, as I should judge, of at least one thousand feet above the present level of the sea. Indeed, the whole first, that is, northernmost, ridge of mountains which runs along that northern sea-shore of Hayti, from east to west, is crowned with large layers or broken masses of this same kind of rock, which being, as stated above, a formation of the present geological period, goes far to show that this whole ridge has been raised in this present period. Thus the existence of the greater part, and the configuration of the whole of the southwestern neck of the island of Hayti, is of comparatively recent origin.

Two questions at once suggest themselves here, *whether the formation of the same rock, and whether the elevation of the land, are going on at the present time.* That the former, the formation of the limestone rock, is really in progress at the present day, seems to be evident in some places, where the whole bottom of the sea near the shore, at a depth of from one to five and six feet below low-water mark, is, as it were, a flat pavement of the same kind of rock. Crust-building corals, as Porites, Mæandrinas, Siderastræas, etc., live upon it, and in the interstices of these are thrown up from below dead shells, broken Millepores, Madrepores, and Astræans. By the powerful action of the waves on the shallow bottom, these remains are broken and ground upon each other, and their form is lost.

The lime powder which results from this pulverizing action furnishes the cement which fills the shells and unites the pieces into one solid mass, *Maçonnerie bon Dieu* (God's Masonry), as the negroes of the French colonies call it. In consideration of these facts, the first part of our question may be answered in the affirmative.

But whether the whole coast is constantly and gradually rising (as we know is the case with the coast of Sweden), or whether those different layers of that submarine pavement have been thrown up at various periods, by sudden volcanic agencies, I am at a loss to decide, from my own observations. I will only state, that the layers which lie now just above low-water mark, are (for instance, in some places in the neighborhood of Jeremie) quite undisturbed banks, running in a plain parallel to the level of the sea. This seems rather to favor the idea of a gradual rising than of a sudden upheaving, and the latter would be more likely to fracture the layers and to change their original horizontal position into an angle towards the horizon.

I conclude, from the information afforded me, that this limestone formation must extend over the whole northern part of Hayti, from its western cape (Tibouron) to the neighborhood of Port-au-Prince. Further, the rocky part of the sea-shore of Barbadoes, Maria Gallante, of Grand-terre in Guadaloupe, of Antigua, St. Bartholome, St. Martin, Arguilla, and Santa Cruz, seems to be of the same composition and age.

The great respiration of the ocean, the ebbing and flowing of the tide, hardly touches that coast. Neither the native fishermen along the coast, nor the American captains in the harbor, speak of high and low water there. The great tide wave is not only broken by the wall of islands in front of the Mexican Gulf, but, perhaps, is even neutralized by a continual current, which runs from east to west all along the northern shore of Hayti.

All the motion of the sea on that shore depends upon the wind. Its agencies are twofold; first, *the daily change of sea and land wind*, the former beginning to blow in the morning about eight o'clock, the latter in the evening, between six and nine. The latter is much more constant, and being also more powerful, it depresses, every evening, the level of the sea all along the coast, from one to two feet. But there is, secondly, another, a *yearly change of the winds*, viz.: a prevailing northerly wind in winter, particularly in December, January, and February, and a prevailing southerly wind in summer. This great change produces this effect, that, in the season of the North, as they call it there, the level of the sea is constantly, on the average, eight feet higher than in the summer.

How much this change bears, also, upon the organic life of the sea-coast, is evident. I will only state, that, during the last of May and the first of June, in one place not larger than an acre, more than a hundred Actineæ and Holothuriæ died, because left upon dry land; not to speak of the thousands of other animals, fishes, echini, etc., and of sea-plants, which died in those natural basins near the sea, where the water, cut off from the refreshing ocean, was overheated by the nearly perpendicular rays of the tropical sun.

CLASSIFICATION OF THE METAMORPHIC STRATA OF THE ATLANTIC SLOPE OF THE MIDDLE AND SOUTHERN STATES. BY PROF. H. D. ROGERS.

The following is a concise sketch of the Geological composition of the Atlantic Slope of the Middle and Southern States, derived chiefly from a study of the formations of this portion of Pennsylvania.

Discarding from our present survey the newer deposits of the region, or those long, narrow, superficial troughs of unconformably overlying red and gray shales and sandstones of mesozoic, or middle secondary age, which partially cover the older or crystalline, and semi-crystalline strata, and restricting our attention to these, we shall find that, when carefully studied, they rank themselves, so far as they admit of subdivision at all, into three natural physical groups. All the sedimentary mineral masses, without exception, are in a condition, more or less, of metamorphism or transformation from the earthy to the crystalline state by heat, and therefore, using the term in a critical sense, all of them are Metamorphic Rocks. In the more current conventional application of this word, only some of them, however, pertain to the usually recognized Metamorphic or Gneissic series; others belong unequivocally to the Paleozoic, or ancient life-representing system, while others again constitute an extensive, intermediate group, not typically gneissic or granitoid in their degree of crystalline structure or metamorphism on the one hand, nor yet fossiliferous on the other, at least so far as the closest scrutiny can discover. For a long while, indeed, from the commencement of geological researches in this district of the Atlantic slope, until the geological surveys of Pennsylvania and Virginia had unravelled the composition and structure of the region, all of these ancient, and more or less altered strata of the Atlantic slope, from its summit in the Blue Ridge and South Mountain, to its base at the margin of tide water, were regarded and designated alike as primary rocks, and were supposed to constitute but one group, and that the oldest known to geologists. Early, however, in the course of those surveys, it came to light that by far the larger portion of the rocky masses of at least the middle and northwestern tracts of the Atlantic slope, including much of the Blue Ridge and of the Green Mountains, was of a different type and age from the oldest metamorphic, or true gneissic system. The evidence in support of this conclusion was, first, an obvious and very general difference in the composition of the two sets of strata; secondly, a marked difference in their conditions of metamorphism, and thirdly and more especially, a striking contrast in their directions and manner of uplift, the plications and undulations of the less metamorphic series, dipping almost invariably southeastward, while the gneiss presents in many localities, no symmetrical foldings, but only a broad outcrop, dipping to a different quarter. These structural dissimilitudes imply essential differences in the direction and date of the crust movements, lifting and transforming the respective groups, and led the geologists of Pennsylvania and Virginia to a conviction, that over at least many tracts, a physical unconformity, both in stripe and dip, would be yet discovered. It was not, however, till a relatively late date in the prosecution of the geological survey of Pennsylvania, that the geologists of that state detected there positive evidences of this physical break, and interval in time between the two groups of strata, and established by ocular proof the correctness of the previous induction. This unconformity, reflecting so much light on the whole geology of the Atlantic slope, was first clearly discerned in tracing the common boundary of the two formations from the Schuylkill to the Brandywine, and the Susquehanna; but it was quickly afterwards recognized on the borders of the gneissic district, north of the Chester County limestone valley, and again, soon after, in the Lehigh Hills at their intersection with the Delaware.

Prior to the suspension of the geological survey from 1843 to 1851, the true Paleozic Age of the non-fossiliferous crystalline marbles and semi-crystalline talcoid slates, and vitreous sandstones of the Chester and Montgomery Valley, had been clearly demonstrated by the State geologist, through a comparison of the strata with their corresponding formations in a less altered condition further north; but it was not until the resumption of field research, upon the revival of the survey in 1851, that any distinctive fossils were detected in these greatly changed rocks, which even in their original state seem to have been almost destitute of their usual organic remains.

Assembling all the evidence which we now possess, we have in the Atlantic slope by actual demonstration but one physical break or horizon of unconformity throughout the whole immense succession of altered crystalline, sedimentary stratra, and within this region but one paleontological horizon, that, namely, of the already-discovered dawn of life among the American strata. This latter plane or limit, marking the transition from the non-fossiliferous or azoic deposits to those containing organic remains, lies within the middle of the primal series or group of the Pennsylvania Survey, that is to say, in the Primal White Sandstone, which even where very vitreous, and abounding in crystalline mineral segregations, contains its distinctive fossil, the *Scolithus linearis.* The primal slates beneath the sandstone, and in immediate alternation with it, possess not a vestige of organic life, nor has any such been discovered anywhere within the limits of the Atlantic slope, or on the northern or western borders of the great Appalachian basin of North America, either in this lower primal state, or in the other semi-metamorphic grits and schists physically conformable with it, and into which the true Paleozoia sequence of our formations physically extends downward. We have thus, then, two main horizons, subdividing the more or less metamorphic strata of the Atlantic slope into three systems or groups; the one, a physical break or interruption in the original deposition of the masses; the other, a life-limit or plane, denoting the first advent, so far as is yet discovered, of organic beings. As these two planes are not coincident, but include between them a thick group of sedimentary rocks, separated from the lower physically, from the upper ontologically, we are fully authorized, in the existing state of research, to employ a classification, which recognizes a threefold division of all these lower rocks. To the most ancient or lowest group, it is proposed to continue the name of gneiss, preferring, however, to call this division generically the Gneissic Series, employing sometimes the technical synonyme Hypozoic, proposed by Professor John Phillips, for these lowest of the metamorphic strata. To the great middle group, less crystalline than the gneissic, and yet destitute of fossils, the descriptive terms semi-metamorphic or Azoic are applicable. And to the third uppermost system, or entire succession of the American Appalachian strata from the primal, containing the earliest traces of life, to the latest true coral rocks, or last deposits of the Appalachian sea, it is here proposed to affix, as for many years past, the well-chosen title, conferred on corresponding formations in Europe, of the Paleozoic, or ancient life-entombing system or series. Thus we have the *Hypozoic* rocks, or those *underneath* any life-bearing strata; *Azoic,* or those destitute of any discovered relics of life; and *Paleozoic,* or those entombing the remains of the earth's most ancient extinct forms of once living beings.

The Atlantic slope of Pennsylvania includes all these three systems of strata. Where the azoic strata display their maximum amount of crystalline structure or metamorphism, they often simulate the true ancient hypozoic or gneissic rocks so closely, and they are indeed so identical with them in mineral aspect and structure, that the observer is baffled in his attempts to distinguish the two groups lithologically; nevertheless, it clearly appears, as the sections illustrating this country prove, that they are distinct systems, occupying separate zones, susceptible of independent definition on the geological map.

At the time of the first construction of the general geological map of the State, the true limits separating the hypozoic or gneissic from the azoic or semi-metamorphic rocks were but vaguely understood, and the State geologist did not venture to define them on the map, but shaded the one system into the other, indicating, however, what he has since proved, that the true gneissic rocks, in their southwestward course, pass out of the State at the Susquehanna, only a short distance north of its southern boundary, while the azoic, or talco-micaceous group, as a genuine, downward extension of the primal, paleozoic series, widens progressively going westward, until, from a very narrow outcrop at the River Schuylkill, it occupies at the Susquehanna the whole broad zone south of the limestone valleys of the Conestoga and Codorus streams in Lancaster and York counties. Since the revival of the field work of the survey, the dividing limit of these two sets of metamorphic strata has been traced and mapped with precision. To the southwest of the Susquehanna it has never, it is believed, been pursued through Maryland and the other southern States, though one may readily discern it in going northward or westward from Baltimore, or ascending the Atlantic slope in Virginia. In Maryland it crosses the Baltimore and Susquehanna Railroad about twelve miles north of Baltimore, and it is intersected by the Baltimore and Ohio Railroad a little east of Sykesville; it crosses the Potomac above Georgetown, and the James River in Virginia, west of Richmond. The line of boundary is, however, not a simple one, but is intricately looped, in consequence of numerous nearly parallel anticlinal foldings of the strata, sending promontories or fingers of the older rocks, within the area of the newer or semi-metamorphic, to the west of their average boundary, and causing, of course, corresponding troughs, or synclinal folds of the newer, to enter eastward of the average boundary, the general area of the older. The Atlantic slope has received hitherto so little exact geological study, that we are, as yet, without the data for determining with any precision, either the succession of its much broken and closely-plicated strata, or the geographical limits which separate even the larger sub-groups. It is sufficient, however, for our present purpose, to show the existence and the approximate range of two great metamorphic systems, separated by a physical break; and the conformable relations of the later or upper of these to well-known lower paleozoic formations of the Appalachian chain.

ON THE GEOLOGICAL STRUCTURE OF THE APPALACHIAN CHAIN IN WESTERN MASSACHUSETTS.

At a recent meeting of the Boston Society of Natural History, Mr. C. H. Hitchcock exhibited a diagram of a geological section from Greenfield to Charlemont, Mass., and gave the following explanation of it:

This section was measured in October, 1857. The design of it is to show the amount of erosion since the strata were brought into their present position.

I will enumerate the rocks in order, going from east to west, beginning at

Greenfield. At Greenfield we find the Connecticut River Sandstone dipping forty degrees east. Leaving the valley we strike the Calcareomica-slate in Shelburne, having a dip of sixty-seven degrees east. This rock consists of micaceous slates and schists interstratified with bluish-gray silicious limestone. The dip gradually increases to thirty-eight degrees east at two and a third miles from the commencement of the rock; when, upon East Mountain, we find a beautiful mica-slate which cleaves into large tables, and is generally destitute of limestone. The dip of this is forty degrees east, and it extends one and one-third miles. Then, just below the top of East Mountain, upon the west side, there is about fifty feet thickness of hornblende slate, of which the dip is twenty-eight degrees east. Passing into the valley of the Deerfield River, gneiss is found, becoming gradually nearly horizontal. Between this rock and the hornblende slate above, it is hard to draw the line. Most of the gneiss has hornblendic layers interstratified with it sparingly; but at the top of the gneiss the hornblende predominates; the rock being in some places nothing but a heavy, unctuous, shining mass of hornblende crystals. West of Shelburne Falls the gneiss begins to dip to the west. The extent of the gneiss is four and one-third miles, mostly in a deep valley.

At East Charlemont the hornblende slate is found above the gneiss, corresponding in character and thickness to the same rock in Shelburne. Then follows the beautiful mica-slate, sparingly interstratified with limestone, and lastly the calcareomica-slate, corresponding to the rock at the east end of the section. Here, at the end of the section in Charlemont centre, the strata are perpendicular, running north and south, and stand side by side with talcose slate.

The Deerfield River makes a bend just above Shelburne Falls, so that the section crosses the river twice, and continues for two or three miles further in the valley. Had a section been measured across the river at Shelburne Falls village at right angles to the stream, it would have exhibited a mountain, west of the river, possessing all the characters of East Mountain in a reverse order. Thus we find an anticlinal axis, with the same strata upon the opposite sides of the ridge at their proper distances. The inference is that the strata were once continuous, and that the material has been denuded. A measurement upon the protracted section gives 3,350 feet, or three-fifths of a mile, as the height above the present level of the former strata. This is taken from the lowest level upon the section. As we go east or west from this central point, the surface rises; consequently the thickness of the denuded strata constantly diminishes in these directions. The denuded surface is eleven miles wide.

From the bend in Deerfield River above Shelburne Falls the stream continues westward in a deep valley for twenty miles, to Hoosac mountain, before turning northwards. This valley has probably been excavated in like manner; but no exact measurement can be made of the amount of erosion, because the river crosses perpendicular strata. A line drawn from the summits on either side of the valley to each other would give large results; but they would not be equal to the truth.

Let us now look at the first described erosion in another light. The gneiss rock at the bottom is exposed over an oval area about three miles by two. If ten observers should start from the centre of the oval, and travel in as many different radiating directions, they would all see the same succession of rocks, and in the same order. The dip is quaquaversal, gradually returning to an anticlinal axis at a few miles distance north and south. Hence there has been a cap denuded, three-fifths of a mile thick. Doubtless, all along this anticlinal axis, wherever the upper rocks have been removed, the gneiss beneath will be discovered. It corresponds to the gneiss and hornblende slate of eastern Hampshire and western Worcester counties, and would seem thus to lie below the Metamorphosed Silurian rocks. If so, it should be included among the Hypozoic rocks.

Sections similar to the foregoing have been measured and described by English geologists. Not being aware of any similar work in our own country, I have described this section in hope of drawing attention to the subject, and thus insure descriptions of sections far more grand and interesting. I can vouch, on the part of the Vermont survey, for a careful attention to this subject.

ORIGIN OF SLATY CLEAVAGE.

The view of Mr. Sharpe, presented to the Geological Society of London in 1487, that slaty cleavage is owing simply to pressure, and is at right angles to the line of force, has been established by various facts brought forward by Mr. J. Tyndall. (Phil. Mag. [4] xii. 35.) He shows that a fine clay, or almost any impalpable material, when subjected to pressure and at the same time allowed to spread laterally, takes a laminated structure. White lead, wax, and even cheese, are among the substances which have afforded him evidence on this point. He attributes the effect to the small inequalities which exist in the texture of substances of all kinds. Under pressure the mass yields and spreads out, and the little nodules become converted into laminæ, separated from each other by surfaces of weak cohesion, and thus the mass becomes cleavable. The air cavities or fissures also spread out thin under the pressure, and aid in producing the cleavage: for even dried pipeclay shows such cavities in great numbers, many too small to be seen without a magnifier.

Mr. H. C. Sorby had before attributed the cleavage to the effect of such lateral pressure on all the grains or pebbles in the rock; the force making the particles, whether of mica or any stones, to place their larger diameters in the plane at right angles to the direction of the pressure, that is, in the plane of cleavage. He had appealed to facts in the slates of Wales, showing that where mica scales were present, they had this position. This cause may contribute to produce lamination, though not appearing to be necessary to the result.

In the Philosophical Magazine for December last, Rev. S. Haughton gives the results of some calculations to ascertain the amount of the pressure which was exerted in cases of the production of slaty cleavage, using as data the degree of distortion or compression of fossils. — *Silliman's Journal.*

ON LAMINATION AND CLEAVAGE OCCASIONED BY THE MUTUAL POSITION OF THE PARTICLES OF ROCKS WHILE IN IRREGULAR MOTION.

Mr. S. P. Scrope, in a recent communication to the London Geological Society, on the above subject, referred to a former paper read by him before the Society in April, 1856, in which this topic was touched upon, and proposed to carry on the inquiry as to the probable effect, upon the internal structure of rocks, of the mutual friction of their component parts when forced into motion under extreme and irregular pressures. He commenced by examining the laws that determine the internal motions of substances possessing a more or less imperfect liquidity, whether homogeneous, or consisting of solid particles suspended in, or mixed with, or lubricated by, any liquid, under unequal pressures; and showed that unequal rates of motion must result in the different parts of the substance, and that in the latter case, there will be more or less separation of the solid and coarser from the finer and liquid particles into different zones or layers; those composed of the former moving less readily than those composed of the latter; and also that the former will, by the friction attending this process, be turned round so as to bring their major axes into the line of direction of the movements; and, if susceptible of tension or disintegration, will be elongated or drawn out in the same direction. In illustration of this law, specimens of marbled paper were produced, being impressions from superficial films of colored matter floating upon water in circular or irregular forms, after they had been subjected to motion in one or more directions by lateral pressure, the appearances produced bearing a very exact resemblance to those presented by the lamination and occasionally sinuous or contorted structure of the ribboned lavas of Ponza, Ischia, the Ascension Isles, etc., as well as that of the gneiss and mica-schist. The author proceeded to state that the expansion of a subterranean mass of granite by increase of temperature, to which all geologists agree in ascribing the elevation of overlying rocks, *must* be accompanied by great internal movements, and consequent mutual friction among the component parts, and even among the individual crystals; that, if a lubricating ingredient, such as water holding silex in solution, or gelatinous silex, be intimately mixed up with the more solid crystals (as there is great reason to believe to have been the case in granite), the friction will be lessened, especially in the central or inferior parts of the mass, where the expanding movement, or intumescence, may be supposed nearly uniform in all directions. But in the lateral and higher portions directly exposed to the resistance and pressure of the overlying rocks, shouldered off on either side by the expanding granitic axis, the movement will probably have been so predominant and extreme in a direction at right angles, or nearly so, to the pressure, as to give rise to a lameller arrangement of the solid crystals, in the manner before indicated. In this manner, he supposes the foliation or lamination of gneiss and mica-schist to have been produced through the "squeeze and jam" of the lateral and superficial portions of a granitic mass expanding by increase of temperature, and the giving way of the overlying rocks, those portions being forced to move in the direction of the lamination while subject to intense pressure at right angles, or nearly so, to that direction. The author argues that it is not inconsistent with this view to suppose that a certain amount of recrystallization may have accompanied or followed this lameller arrangement, in which case also the major axes of the crystals would be likely to take a direction perpendicular to the pressure, since the mobility necessary to the crystallific action would be freer in that, than in any other direction. He likewise points out that the influence of internal friction accompanying motion under extreme and irregular pressures, must have been equally operative in the case of aqueous as of igneous rocks, under similar circumstances of imperfect liquidity, and irrespective of changes of temperature. And he suggests that to this cause may be attributable the internal structure of some veined marbles, calcareous breccias, serpentines, etc., as well as the cleavage of the slaty rocks; as, indeed, the experiments of Mr. Sorby and of Professor Tyndall have already indicated. He concludes by suggesting to all geologists engaged in the examination of rocks, the above mechanical considerations, as likely to lead to more definite views than at present prevail as to the origin of the metamorphic schists, and the internal structure of many of the older and more disturbed rocks of all characters.

ON A GRADUAL ELEVATION OF THE COAST OF SICILY.

At a recent meeting of the London Geological Society, Sir Charles Lyell read a paper from Signor Gemmellaro, "On the gradual elevation of a part of the Coast of Sicily, from the mouth of the Simeto to the Onobola," in which the author described in detail the physical evidences observed by him along a great part of the eastern coast of Sicily, which prove: 1st. That from the shores of the Simeto to the Onobola, undeniable characters of the former levels of the sea, in the recent period, are traceable from place to place. 2ndly. That great blocks of lava, with blunted angles, and rolled and corroded on the surface, a calcareo-siliceous shelly deposit, and a marine breccia, which are seen at different heights above the present sea-level, are the effects of the continued and daily action of the waves of the sea at successive levels. 3rdly. That the existence and disposition of the holes of the *Modiola lithophaga* (Lamarck), in the calcareo-siliceous shelly deposit, and the local presence of shells, both Gasteropods and lamellibranchiates, in their normal positions, support the view of a slow and gradual elevation of the coast. 4thly and lastly. That the lithodomous molluscs and the calc-siliceous deposit being found on the Cyclopean Islands (Faraglioni) up to the height of almost thirteen metres, and large rolled blocks of lava, invested with *Serpulæ*, being also found there to the height of fourteen metres, a mean height of thirteen metres and five decim. is established, as the greatest extent of the now undeniable gradual elevation of this portion of the coast of Sicily during the present period.

ON THE GEOLOGICAL CAUSES THAT HAVE INFLUENCED THE SCENERY OF CANADA AND THE NORTH-EASTERN PROVINCES OF THE UNITED STATES.

The following is a *resumé* of a paper recently read before the Royal Institution, London, by Prof. Ramsay, on the above subject:

The island of Belleisle, and the Laurentine chain of mountains between the shores of Labrador and Lake Superior, consist of gneissic rocks, older than the Huronian formation of Sir William Logan. This gneiss is probably the equivalent of the oldest gneiss of the Scandinavian chain, and of the north-west of Scotland, underlying that conglomerate, which, according to Sir Roderick Murchison, in Scotland represents the Cambrian strata of Longmynd and of Wales. The mountains of the Laurentine chain present those rounded contours that evince great glacial abrasion; and among the forests north of the Ottowa the mammillated surfaces were observed by the speaker to be often grooved and striated, the striations running from north to south. The whole country has been moulded by ice. Above the metamorphic rocks, in the plains of Canada and the United States, south of the St. Lawrence, and around Lake Ontario, and Lake Erie, the Silurian and Devonian strata lie nearly horizontally, but slightly inclined to the south. Consisting of alternations of limestone and softer strata, the rocks have been worn by denudation into a succession of terraces, the chief of these forming a great escarpment, part of which, by the river Niagara, overlooks Queenstown and Lewiston, and, capped by the Niagara limestone, extends from the neighborhood of the Hudson to Lake Huron. Divided by this escarpment, the plains of Canada bordering the lakes, and part of the United States, thus consist of two great plateaus, in the lower of which lies Lake Ontario, Lake Erie lying in a slight depression in the upper plane or table land, 329 feet above Lake Ontario. The lower plain consists mostly of Lower Silurian rocks, bounded on the north by the metamorphic hills of the Laurentine chain. The upper plain is formed chiefly of Upper Silurian and Devonian strata. East of the Hudson, the Lower Silurian rocks that form the lower plain of Canada become gradually much disturbed and metamorphosed, and at length, rising into bold hills trending north and south, form in the Green Mountains part of the chain that stretches from the southern extremity of the Appalachian mountains to Gaspé, on the Gulf of St. Lawrence. Between the plains of the lakes and this range, the steep-terraced mass of the Catskills, formed of old red sandstone, lies above the Devonian rocks facing east and north in a grand escarpment. The whole of America south of the lakes, as far as latitude 40°, is covered with glacial drift, consisting of sand, gravel, and clay, with boulders, many of which, during the submergence of the country, have been transported by ice several hundred miles from the Laurentine chain. Many of these are striated and scratched in a manner familiar to those conversant with glacial phenomena. When stripped of drift, all the underlying rocks are evidently ice-smoothed and striated, the striation generally running more or less from north to south, indicating the direction of the ice-drift during the submergence of the country at the glacial period. The banks of the St. Lawrence near Brockville, and all the Thousand Islands, have been rounded and *moutonné* by glacial abrasion during the drift period. The submergence of the country was gradual, and the depth it attained is partly indicated in the east flank of the Catskill mountains. This range, near Catskill, runs north and south, about ten or twelve miles from the right bank of the Hudson. The undulating ground between the river and the mountains is seen to be covered with striations wherever the drift has been removed. These have a north and south direction; and ascending the mountains to Mountain House, the speaker observed that their flanks are marked by frequent grooves and glacial scratches, running, not down hill, as they would do if they had been produced by glaciers, but north and south horizontally along the slopes, in a manner that might have been produced by bergs grating along the coast during submergence. These striations were observed to reach the height of 2,850 feet above the sea. In the gorge, where the hotel stands at that height, they turn sharply round, trending nearly east and west; as if, at a certain period of submergence, the floating ice had been at liberty to pass across its ordinary course in a strait between two islands. During the greatest amount of submergence of the country, the glacial sea in the valley of the Hudson must have been between 3,000 and 4,000 feet deep, and it is probable that even the highest tops of the Catskills lie below the water. In Wales, it has been shown that during the emergence of the country in the glacial epoch, the drift, in some cases, was ploughed out of the valleys by glaciers; but, though the Catskill mountains are equally high, in the valleys beyond the great eastern escarpment the drift still exists, which would not have been the case had glaciers filled these valleys during emergence in the way that took place in the passes of Llanberis and Nant-Francon, and in parts of the Highlands of Scotland. It has been stated above that the upper plain around Lake Erie, and the lower plain of Lake Ontario, are alike covered with drift. Part of this was formed, and much of it modified, during the emergence of the country. In the valley of the St. Lawrence, near Montreal, about 100 feet above the river, there are beds of clay, containing *Leda Portlandica,* and called by Dr. Dawson, of Montreal, the *Leda-clay.* Dr. Dawson is of opinion, that, when this clay was formed, the sea in which it was deposited washed the base of the old coast line that now makes the great escarpment at Queenstown and Lewiston overlooking the plains around Lake Ontario. It has long been an accepted belief that the Falls of Niagara commenced at the edge of this escarpment, and that the gorge has gradually been produced by the river wearing its way back for seven miles to the place of the present Falls. In this case, the author conceives that *the Falls commenced during the deposition of the Leda-clay, or near the close of the drift period,* when, during the emergence of the country, the escarpment had already risen partly above water.

If the 35,000 years suggested by Sir C. Lyell as the minimum for the time occupied in the erosion of the gorge of Niagara be approximately correct, though probably below the reality, we have an idea of the amount of time that has elapsed since the close of the drift-period. And, if it be ever found possible to accurately determine the ancient rate of recession, we shall have data for a first approach to an actual measurement of a portion of geological time.

GEOLOGICAL STRUCTURE OF SOUTHERN AFRICA.

Livingston's theory of the structure of the southern part of the African Continent is essentially new to the great body of naturalists, misled, as they have been since the beginning of the century, by the huge blotch of red paint by which geologists have chosen to represent a supposed central plateau of lava, in correspondence with a similar formation in the centre of the Peninsula of British India. It turns out that this is a mere analogy, a fiction and mistake. Murchison developed, in his anniversary speech of 1852, from a study of Bain's map, the same idea which Livingston obtained by his own experience on the ground. Travelling among large Cape heaths, rhododendrons and Alpine roses, the botanist felt himself moving over a high table land, although under a tropical sun. Descending suddenly from the centre of Africa five thousand feet into the land of Cassange, watered by the river Quango, guided in his estimate of depth by the rude method of plunging his thermometer into boiling water, noticing the thin red strata of mud-rocks to lie nearly horizontal, and remembering the enormous shallow lakes and labyrinth of mighty rivers forming the Zambesi and issuing through a deep gorge upon the Indian coast, he thought he saw beneath him a recently and slowly uplifted continent, of platter shape, with broken edges on the east and west. The great north and south valley of Congo River cuts this continental plateau to its base. Mounting the opposite or western wall of the valley, and crossing the western division of the plateau, he descended upon the Loango coast, over the upturned edges of the rocks. He saw at once why the long western coast of Africa is so straight. Like that of South America, it runs along a deep geological break in the earth's crust, but unaccompanied by erupted Andes and volcanic cones. It seems, however, to have escaped both Dr. Livingstone and Sir Roderick Murchison that the wide north and south cleft through the plateau, at the bottom of which the Congo meanders, must have been occasioned by just such a broad anticlinal wave in the rocks as that which geologists in America call the Cincinnati axis, separating the eastern and western coal-fields, although its nearest likeness is to the north and south valleys of the Jordan and the Nile.

CONDUCTING POWER OF ROCKS—ALTITUDE OF MOUNTAINS NOT INVARIABLE. BY CHARLES MACLAREN.

Mr. Hopkins, of Cambridge, has made some rather interesting experiments on the *conductivity* or conducting power of different substances for heat, of which an account was laid before the Royal Society of London, in June last. Without attempting to describe his processes, we give his more important results, and in decimals, the conductivity of "igneous rock" (trap or granite, we presume), saturated with moisture, being taken as unity.

Chalk, in the state of *dry powder*	·056
Clay, " "	·070
Sand, " "	·150
Sand and clay, "	·110

The conductivity of the following rocks is given in two states—*dry*, and *saturated* with water:

	Dry.	Saturated.
Chalk in block	·17	·30
Oolite rock	·30	·40
Hard compact limestones	·50	·55
Siliceous new red sandstone	·25	·60
Freestone	·33	·45
Hard compact sandstone (millstone grit)	·51	·76
Hard compact old sedimentary	·50	·61
Igneous rocks	·53	1.00

The effect of *pressure* on the conducting power of substances was also tried, and proved to be almost nothing. A pressure of 7500 lbs. on a square inch of bees-wax, spermaceti, and chalk, had no appreciable effect. Uncompressed clay, which had a conducting power of ·26, had the same raised to ·33 by a pressure of 7500 lbs.

Sandstone, with conducting power of ·5, divided into strata each one foot thick, when compared with a similar mass in one block, had its conducting power diminished $\frac{1}{20}$th. When the strata were only six inches thick, the diminution was $\frac{1}{10}$th. The effect of discontinuity of substance is, therefore, small. Saturation with moisture, on the other hand, produces generally a great effect, as will be seen on comparing the dry and saturated blocks of chalk, the dry and saturated new red sandstone, and again the dry and saturated "igneous rocks."

These facts have a certain bearing on a geological question—namely, the transmission of heat from the interior of the earth to the crust. The oolite, for instance, conducts heat much better than the chalk, the sandstone better than the oolite, the igneous rock better than the sandstone, and in all cases the rock charged with moisture better than the dry rock. But Mr. Hopkins would have added to the value of his paper, if he had ascertained by experiment the quantity of water absorbed by each rock at given temperatures, and whether the conductivity is exactly in proportion to the absorption.

In illustration of the use that may be made of the tables, we would refer to certain remarks made by Dr. Robinson, on a paper read by Professor Hennessey, at a recent meeting of the British Association. The subject was "The Direction of Gravity at the Earth's Surface." In alluding to certain supposed local and temporary changes of level, he mentioned the following curious fact: — "He found *the entire mass of rock and hill on which the Armagh Observatory is erected, to be slightly, but to an astronomer quite perceptibly, tilted or canted at one season to the east, at another to the west.* This he at first attributed to the varying power of the sun's radiation to heat and expand the rock throughout the year; but he subsequently had reason to attribute it rather to the infiltration of water to the parts where the clay-slate and limestone rocks met. The varying quantity of this (water) through the year he now believed exerted a powerful hydrostatic energy, by which the position of the rock is slightly varied." With the light furnished by Mr. Hopkins's experiments, we may pronounce the explanation satisfactory. Armagh and its observatory stand on a hill at the junction of the mountain limestone with the clay-slate, having, as it were, one leg on the former, and the other on the latter, and both rocks probably reach downwards one or two thousand feet. When rain falls, the one will absorb more water than the other; both will gain an increase of conductive power, but the one which has absorbed most water will have the greatest increase; and being thus the better conductor, will *draw a greater portion of heat from the hot nucleus below to the surface* — will become, in fact, temporarily hotter, and, as a consequence, *expand more than the other.* In a word, *both rocks will expand at the wet season; but the best conductor, or most absorbent rock, will expand most, and seem to tilt the hill to one side; at the dry season it will subside most, and the hill will seem to be tilted in the opposite direction.*

The fact is curious, and not less so are the results deducible from it. *First,* hills are higher at one season than another, a fact we might have supposed, but never could have ascertained by measurement. *Second,* they are highest, not as we would have supposed at the hottest season, but at the wettest. *Third,* it is from the *different rates* of expansion of different rocks that this has been discovered; had the limestones and clay-slate expanded equably, or had Armagh Observatory stood on a hill of homogeneous rock, it would have remained unknown. *Fourth,* though the phenomenon is in the strictest sense *terrestrial,* it is by converse with the *heavens* that it has been made known to us. A variation of probably a second, or less, in the right ascension of three or four stars, observed at different seasons, no doubt revealed the fact to the sagacious astronomer of Armagh, and even enabled him to divine its cause; which has been confirmed as the true cause, and placed in a clearer light by the experiments of Mr. Hopkins. One useful lesson may be learned from the discovery — to be careful to erect observatories on a homogeneous foundation. — *Edin. Phil. Journal.*

ON THE INTERNAL HEAT OF THE EARTH.

In a discussion on the above subject, at the last meeting of the British Association, Prof. Hennessey said, that Prof. Phillips had described certain changes in the structure of sedimentary rocks, which changes he had been led to attribute to the influence of heat. Heat might act in some instances only upon limited masses and in limited areas; it might also act in a more general manner, and from as general a source as the whole interior mass of the earth. It was necessary to refer distinctly to the possibility of the last source of metamorphic calorific agency, as it was sometimes called in question by gentlemen of very high authority. There are, however, abundant reasons for believing that, at a comparatively small depth beneath the earth's surface, heat exists sufficient to melt the most refractory rocks. A conclusion announced by Mr. Hopkins is supposed by some geologists to render these results inadmissible. But this conclusion requires for its establishment conditions in the structure of the earth which Mr. Hopkins does not prove, but which he is compelled to assume, in order to reduce his equations to numbers. Instead of assuming conditions, Mr. Hennessey was led to inquire what would be the most probable structure of the earth, if the formation of its solid crust followed the physical laws which are observed on the solidification of fused masses of rock. It follows from the whole of his inquiries, that not only do we possess no reasons for believing that the earth's crust has a thickness so great as that announced by Mr. Hopkins; but that its limiting thickness will probably be found to depend upon the limit of fusibility of the substances of which it is composed. There could, therefore, be no difficulty in perceiving how, during remote geological epochs, the earth's crust might be so thin as to allow a very important influence to be exercised by the high temperature of the interior fluid matter on the structure of the sedimentary strata deposited on the outer surface of the solidified crust.

EXPERIMENT ON THE MELTING AND COOLING OF BASALT.

At a recent meeting of the Manchester Society of Engineers, Mr. Hawkes described an experiment recently made by him on the melting and cooling of basalt. About 31 cwt. of basalt was melted in a large double reverberatory furnace, and after a slow cooling during thirteen days, it presented an upper stratum of stony vesicular matter, about one inch thick, next a layer of black glass, from two to eight inches deep on that side of the mass which was exposed to the air from the door of the furnace (elsewhere, immediately under the vesicular layer was solid stone, interspersed here and there with air-bubbles). Mr. Hawkes added some observations relating to the results of experiments which he had made to ascertain the temperature of melted cast-iron, and of melted basalt.

ARCTIC GEOLOGY.

One of the most important results brought out by recent Arctic Explorations, is the seeming establishment of the conclusion, long since announced, that the sea temperature of the arctic zone in the Silurian, Devonian, and Carboniferous ages, was not essentially different from that in the temperate zone. The study of the geographical distribution of animals is showing that species have but a narrow range of temperature in which they can flourish in full vigor, and even families and tribes have often but a limited range. 20° F. (between 68° and 88°) is the whole range of the existing coral reef corals — and species have still more restricted limits. The existence, therefore, of the same species of corals, molluscs and trilobites, or of *closely related or representative*, if not identical, species, in latitudes 40° and 75°, leads to but one conclusion. The near uniformity of the climate is at once suggested, and must be admitted until this life-thermometer reads otherwise.

Whether the relative size of related species indicates a degree of difference is yet undetermined.

The Jurassic fossils extend the same approximate uniformity onward to the Jurassic period, or prove a recurrence of it, if it had been interrupted.

Capt. M'Clintock, in the narrative of his expedition, thus speaks of the fossils of Prince Patrick's Land. "The fossils are all small, and of only a few varieties, some being Ammonites, but the greater part bivalves." "I picked up also what appeared to be a fossil bone (*Ichthyosaurus?*) only part of it appearing out of the fragment of the rock." The *supposed* Ichthyosaurian bone was afterwards lost. Jurassic fossils have also been found at Katmai Bay, or Cook's Inlet, in northwest America (60° N., 151° W.); and Dr. Grewingk mentions the species of *Ammonites Wosnessenski*, *A. biplex?* (the *A. biplex* of Mayen from Jurassic deposits in the Andes, in latitude 34°, south of Valparaiso, is stated to be not distinguishable from this), *Belemnites paxillosus*, and *Unio Liassinus.* Professor Haughton of Dublin, in an addenda to Capt. M'Clintock's narative, remarks upon these interesting discoveries as follows: — The discovery of such fossils *in situ* in 76° north latitude, is calculated to throw considerable doubt upon the theories of climate which would account for all past changes of temperature by changes in the relative position of land and water on the earth's surface. No attempt, that I am aware of, has ever been made to calculate the number of degrees of change possible, in consequence of changes of position of land and water; and from some incomplete calculations, I have myself made on the subject, I think it highly improbable that such causes could have ever produced a temperature in the sea at 76° north latitude which would allow of the existence of Ammonites, especially Ammonites so like those that lived at the same time in the tropical warm seas of the south of England and France, at the close of the Liassic and the commencement of the lower Oolitic period." — *Abridged from Silliman's Journal.*

ON THE HABITABILITY OF THE MOON.

Sir John Herschel, in the last edition of his "Outlines of Astronomy," 1858, has added the following paragraph on the "habitability of the moon:"

"On the subject of the moon's habitability, the complete absence of air, *if general over her whole surface*, would of course be decisive. Some considerations of a contrary nature, however, suggest themselves in consequence of a remark lately made by Prof. Hansen, viz., that the fact of the moon turning always the same face towards the earth is in all probability the result of an elongation of its figure in the direction of a line joining the centres of both the bodies acting conjointly *with a noncoincidence of its centre of gravity with its centre of symmetry.* To the middle of the length of a stick, loaded with a heavy weight at one end and a light one at the other, attach a string, and swing it round. The heavy weight will assume and maintain a position in the circulation of the joint mass further from the hand than the lighter. This is not improbably what takes place in the moon, and the reader may consider the moon as retained in her orbit about the earth by some coercing power analogous to that which the hand exerts on the compound mass above described through the string. Suppose, then, its globe made up of materials not homogeneous, and so disposed in its interior that some considerable preponderance of weight should exist excentrically situated: then it will be easily apprehended that the portion of its surface nearer to that heavier portion of its solid contents, under all the circumstances of a rotation so adjusted, will permanently occupy the situation most remote from the earth. Let us now consider what may be expected to be the distribution of air, water, or other fluid on the surface of such a globe, supposing its quantity not sufficient to cover and drown the whole mass. It will run towards the lowest place, that is to say, not the nearest to the centre of figure or to the central point of the mere *space* occupied by the moon, but to the centre of the *mass*, or what is called in mechanics the centre of gravity. There will be formed there an ocean, of more or less extent according to the quantity of fluid, directly over the heavier nucleus, while the lighter portion of the solid material will stand out as a continent on the opposite side. And the height above the level of such ocean to which it will project will be greater, the greater the excentricity of the centre of gravity. Suppose then that in the case of the moon this excentricity should amount to some thirty or forty miles, such would be the general elevation of the lunar land (or the portion turned earthwards) above its ocean, that the whole of that portion of the moon we see would in fact come to be regarded as a mountainous elevation above the sea-level. In what regards its assumption of a definite level, air obeys precisely the same hydrostatical laws as water. The lunar atmosphere would rest upon the lunar ocean, and form in its basin a *lake of air*, whose upper portions at an altitude such as we are now contemplating, would be of excessive tenuity, especially should the lunar provision of air be less abundant in proportion than our own. It by no means follows, then, from the absence of visible indications of water or air on this side of the moon, that the other is equally destitute of them, and equally unfitted for maintaining animal or vegetable life. Some slight approach to such a state of things actually obtains on the earth itself. Nearly all the land is collected in one of its hemispheres, and much the larger portion of the sea in the opposite. There is evidently an excess of heavy material vertically beneath the middle of the Pacific; while not very remote from the point of the globe diametrically opposite rises the great table-land of India, and the Himalaya chain, on the summits of which the air has not more than a third of the density it has on the sea-level, and from which animated existence is forever excluded."

It has been very much the fashion, in works treating of the planets, to take their actual distance from the sun, the regular formula for the diminution of radiated heat, and thence to construct theories which sound very pretty in a lecture-room — butter would be oil in Venus and a rock of ice in Saturn — but which will hardly bear philosophical investigation.

Sir J. Herschel throws some light on this subject by hinting at the possibility of different kinds of atmospheres. "All men do not dress in flannels. It is very probable that a dense atmosphere surrounding a planet, while allowing the access of solar heat to its surface, may oppose a powerful obstacle to its escape, and that thus the feeble sunshine on a remote planet may be retained and accumulated on its surface in the same way, and for the same reason, that a very slight amount of sunshine, or even the dispersed heat of a bright though clouded day, suffices to maintain the interior of a closed greenhouse at a high temperature." Then, again, "the intensity of gravity, or its efficacy in counteracting muscular power and repressing animal activity on Jupiter is nearly two and a half times that on the earth, on Mars not more than one half," and so on. "Lastly, the density of Saturn hardly exceeds one-eighth of the earth's, so that it must consist of materials not heavier on the average than dry fir-wood. Now, under the various combinations of elements so important to life as these, what immense diversity must we not admit in the conditions of that great problem, the maintenance of animal and intellectual existence and happiness?" In other words, we are in possession of but one or two facts out of a great many, and know, so far, very little about the matter.

FORMER CONNECTION OF AUSTRALIA, NEW GUINEA, AND THE ARU ISLANDS.

Mr. A. R. Wallace, in a paper on the Aru Islands, a group one hundred and fifty miles South of Western New Guinea (Ann. and Mag. Nat. Hist., xx, Jan. 1858, p. 473), shows that the zoölogy of the islands is closely related to that of New Guinea and Australia; and that shallow seas not only connect the two last, as others had before stated, but that they extend and include the Aru group. The depth of water over the whole to Australia is very nearly uniform at about thirty to forty fathoms. Mr. Wallace says:

"But there is another circumstance still more strongly proving this connection: the great Island of Aru, eighty miles in length from north to south, is traversed by three winding channels of such uniform width and depth, though passing through an irregular, undulating, rocky country, that they seem portions of true rivers, though now occupied by salt water, and open at each end to the entrance of the tides. The phenomenon is unique, and we can account for their formation in no other way than by supposing them to have been once true rivers, having their source in the mountains of New Guinea, and reduced to their present condition by the subsidence of the intervening land."

Nearly one-half of the Passerine birds of New Guinea hitherto described are contained in the author's collections made in Aru, and a number also of species in the other tribes.

The author farther observes, on the absence of the peculiar East Indian types: "In the Peninsula of Malacca, Sumatra, Java, Borneo and the Philippine Islands, the following families are abundant in species and in individuals. They are everywhere *common birds.* They are the *Buceridæ, Picidæ, Bucconidæ, Trogonidæ, Meropidæ, Eurylaimidæ*; but not one species of all these families is found in Aru, nor, with two doubtful exceptions, in New Guinea. The whole are also absent from Australia. To complete our view of the subject, it is necessary also to consider the Mammalia, which present peculiarities and deficiencies even yet more striking. Not one species found in the great islands westward inhabits Aru or New Guinea. With the exception only of pigs and bats, not a genus, not a family, not even an order of mammals is found in common. No Quadrumana, no Sciuridæ, no Carnivora, Rodentia, or Ungulata inhabit these depopulated forests. With the two exceptions above mentioned, all the Mammalia are *Marsupials*; while in the great western islands there is not a single marsupial! A kangaroo inhabits Aru (and several New Guinea), and this, with three or four species of *Cuscus*, two or three little rat-like marsupials, a wild pig and several bats, are all the mammalia I have been able to obtain or hear of."

ON THE PROBABLE ORIGIN OF THE ORGANIZED BEINGS NOW LIVING ON THE AZORES, MADEIRA, AND THE CANARIES.

The following letter, on the above subject, has been addressed by Oswald Heer to M. de Candolle:

In your Geography of Plants you have adopted the opinion of Edward

Forbes, that in the Miocene period the European continent extended to the Azores and Canaries, and supported it by fresh proofs.* In fact, the predominant European character of these islands, which occurs in their insects as well as in their flora, proves that they were anciently joined to the continent. Nevertheless, we must not forget that, as compared with Europe, these islands are very different from those of the Mediterranean. They are distinguished, in the first place, by a much greater number of peculiar species, which constitute a third or a fifth of the plants; and in the second, by some American types, which make their appearance in all these islands. There are not only certain American species which might have reached them accidentally by the agency of the winds and currents, or of man, but American genera which are represented by peculiar species. I will instance the genera *Clethra*, *Bystrobogon*, and *Cedronella*, as also the unique pine of the Canaries (*Pinus canariensis*, Sm.), which belongs to the American forms with acicular ternate leaves. The relations of the laurels is very remarkable in this respect; they form a great part of the forests of Madeira and the Canaries, dividing into four species, and playing an important part. Two species (*Oreodaphne fœtens* and *Persea indica*) are essentially American types; the third (*Phœbe Barbusana*, Webb) belongs to a genus which occurs in India and America; and the fourth (*Laurus canariensis*, Webb) corresponds with the European species. By the possession of these laurel forests the islands of the Atlantic differ greatly from the African continent, where they are entirely wanting, and approach America rather than Africa, notwithstanding the proximity of the latter.

These facts obtain great importance by the observation that the flora of the Atlantic islands has much resemblance to the Tertiary flora of Europe.

In my "Flora Tertiaria Helvetiæ," I have proved that a considerable number of plants of the Tertiary epoch corresponded with species peculiar to Madeira and the Canaries, in such a manner that there must be a relation between the two floras. On the other hand, our Tertiary flora indicates a great resemblance to the flora of the southern United States. Many perfectly characteristic genera, such as *Taxodium*, *Sequoia*, *Liquidamber*, *Sabal*, etc., were distributed over the whole of our tertiary country, and composed partly of species very closely allied to those which now grow in America; other genera belong equally to America and Europe (such as *Quercus*, *Corylus*, *Populus*, *Acer*, etc.), and occur in the European Tertiary epoch, composed of species corresponding with the American forms. We find similar cases amongst the terrestrial mollusca and insects, although this is not so positive as with regard to plants.

These remarkable circumstances are explicable, if we suppose that during the Tertiary epoch a terrestrial formation united the continents of Europe and America, and that this surface was extended by some projection to the Atlantic islands. A glance at the map of the depths of the ocean by Maury, shows that the bottom of the Atlantic forms a longitudinal valley, of which the deepest parts are between the twentieth and fortieth degrees of north latitude, nearly at an equal distance from Europe and Africa; but that on the two sides of this deep valley there is a vast *maritime plateau*, which includes the Atlantic islands, as well as the whole space between the European continent, Newfoundland, and Acadia. Beyond this space another long

* DeCandolle, Géographie Botanique raisonnée, p. 1310.

valley, but of less depth, takes its rise, in a direction from south to northeast between Madeira and the Azores; it loses itself close to the coast of Oporto. If we may attribute any importance to these very general data, we must admit that during the miocene period the maritime plateau above indicated was solid ground. This country, this ancient Atlantis, would have had the same plants as central miocene Europe, of which the remains are found in the molasse of Switzerland in such astonishing profusion, that I shall be able to give descriptions and figures of about six hundred species in my "Flora Tertiaria." On the coast of this country the marine shells presented a great conformity in America and Europe; and this remarkable phenomenon is still reproduced, that Europe has more littoral than deep-sea species of shells and fishes in common with America; which proves that at one period a band of firm ground must have united these two parts of the world. The Atlantic islands had already risen towards the south coasts of this continent at the diluvian period. That this country was at the bottom of the sea during the miocene epoch, is shown by the fossil shells of Porto Santo and St. Vincent in Madeira and those of the Azores; but that it had emerged at the diluvian period is proved by the terrestrial mollusca of Caniçal, and the fossil plants of St. Jorge in Madeira.

The islands formed at this epoch would have received their vegetation from the Atlantis in the diluvian period, and consequently at an epoch when this continent had entered upon a new phase of development. If we suppose, that then, by a subsequent depression of the soil, the connection with America was destroyed, and subsequently that which existed with Europe, we shall obtain the elements for the explanation of the existing flora of these islands. We there find the remains of the flora of the ancient Atlantis, and, in consequence, many types of the Tertiary flora are retained there whilst they have disappeared in Europe. These remains, with a certain number of other species, form the peculiar plants of these isles, corresponding in part with the American species, because they have issued from the same centre of formation. But it is with Europe that these islands have the most species in common, probably because their connection with this continent lasted longer.

At the diluvian period the flora of central Europe was displaced by great changes of climate (extension of glaciers, etc.); and as by the depression of the Atlantis the connection with America was destroyed, the new European vegetation could not extend on that side, but only towards the east. It is thus that the characters of the new vegetation would be explained, particularly that of the lower countries, whilst the Alps and the north have undergone less change. This also is the reason of the great analogies which occur between the north of Europe, Asia and America. I arrive, therefore, at this same conclusion with yourself as regards these latter countries, namely, that the alpine vegetation is certainly the most ancient in our country, and that subsequently, when the climate became warmer, after the glacial epoch, it rose from the low countries to the mountains and Alps. — *Ann. Mag. Nat. Hist.*

ON THE REMAINS OF DOMESTIC ANIMALS DISCOVERED AMONG POST-PLEIOCENE FOSSILS IN SOUTH CAROLINA.

The above is the title of a pamphlet recently published by Prof. F. S. Holmes, the well-known palæontologist of South Carolina, adducing evidence to show that, among the fossils collected in South Carolina, from beds of the Post-Pleiocene (Tertiary) Age, a number have been found apparently belonging to animals having specific characters in common with recent, or living species not considered indigenous to this country, such as the horse, hog, sheep, ox, etc. These remains, although apparently belonging to recent species, Prof. Holmes believes to be true fossil remains, inasmuch as they were obtained not only from the banks and deltas of rivers, but also in large number from excavations several feet below the surface, and at a distance from any stream, creek, pond, bog or ravine; and in some cases from excavations below the high sandy land of cotton-fields.

Prof. Leidy, of Philadelphia, who has examined the collections made by Prof. Holmes, and others, from the recent geological formations in South Carolina, and also the localities from whence the most interesting specimens were obtained, writes as follows: "The collections consist of a most remarkable intermixture of remains of fishes, reptiles and mammals, from the eocene, and post-pleiocene formations, and consist usually of teeth, often well preserved, but frequently in small fragments, more or less water-worn. Most of the fossils are stained brown or black. By far the greater portion of these are obtained from the post-pleiocene deposit of the Ashley river, about ten miles from Charleston. The country in this locality is composed of a base of whitish eocene marl, containing remains of *squalodon — sharks, and rays* — above which is a stratum of post-pleiocene marl, about one foot in thickness, overlaid by about three feet of sand and earth mould.

"The post-pleiocene marl contains great quantities of irregular, water-worn fragments of the eocene marl rock from beneath, mingled with sand, blackened pebbles, water-rolled fragments of bones, and more perfect remains of fishes, reptiles, and mammals, belonging to the post-pleiocene and eocene fossils.

"On the shores of the Ashley river, where the post-pleiocene and eocene formations are exposed, the fossils are washed from their beds, and become mingled with the remains of recent indigenous and domestic animals, and objects of human art; so that when a collection is made in this locality, it is sometimes difficult to determine whether the animal remains belong to the formations mentioned or not. Generally, however, we have been able to ascertain where the fossils belong, which we have had the opportunity of examining, from the fact that the greater number were obtained from the deposits referred to in digging into them some distance from the Ashley river.

"The collections contain remains of the horse, ox, sheep, hog, and dog, which, I feel strongly persuaded, with the exception of many of those of the first-mentioned animal, are of recent date, and have become mingled with the true fossils of the post-pleiocene and eocene formations, where they have been exposed on the banks of the Ashley river and its tributaries. In regard to the remains of the horse, from the facts stated in the account given of them in the succeeding pages, I think it will be conceded that this animal inhabited the United States during the post-pleiocene period, temporarily with the *mastodon, megalonyx,* and the great broad-fronted bison.

"Many of the mammalian remains are of recent animals, or, at least, are undistinguishable from the corresponding parts of the latter; and if they are not accidental occupants of the post-pleiocene deposit, are highly interesting, as indicating their contemporaneous existence with many species and genera now extinct.*

* Remains of the tapir, peccary, and capybara, present a similar association of life to that now confined to South America.

"It appears to be quite well authenticated that the horse, which is now so extensively distributed, both in a wild and domestic condition, throughout North and South America, did not inhabit these continents at the time of their discovery by Europeans. With this fact in view, in conjunction with the circumstance that animal remains of late periods may become accidental occupants of earlier geological formations, we should require strong evidence to be advanced before it is admitted that the horse belonged to an ancient fauna of the western world. At the present time the evidence appears to be sufficiently ample to justify the latter conclusion, and it is further sustained by the discovery, in the same part of the world, of the remains of two species of the closely allied genus *Hipparion*.

"Remains of the horse, discovered in Brazil, Buenos Ayres, and Chili, have been indicated by Dr. Lund, Prof. Owen, M. Weddell, and M. Gervais. These remains exhibit no well-marked characters distinguishing them from corresponding portions of the skeleton of the recent horse; and from a comparison of the figures and descriptions which have been given of most of them, together with some remarks of the latter author, it is doubtful whether they belong to more than a single species, the *Equus neogæus* of Dr. Lund.

"Prof. Buckland and Sir John Richardson have described remains of the horse, discovered in association with those of the elephant, moose, reindeer, and musk-ox, in the ice-cliffs of Eschscholtz Bay, Arctic America.

"In the United States, remains of the horse, chiefly consisting of teeth, have been noticed by Drs. Mitchell,* Harlan,† and DeKay;‡ but these gentlemen have neither given descriptions nor figures by which to identify the specimens. Some of the latter are stated to have been found in the vicinity of Neversink Hills, New Jersey; others in the excavation for the Chesapeake and Ohio Canal, near Georgetown, District of Columbia; and some in the later tertiary deposit on the Neuse River, in the vicinity of Newbern, North Carolina. Dr. DeKay, in speaking of such remains, says, 'they resemble those of the common horse, but from their size apparently belonged to a larger animal,' and he refers them to a species with the name of *Equus major*.

"Dr. R. W. Gibbes § has given information of the discovery of teeth of the horse in the pleiocene deposit of Darlington, South Carolina; in Richland District, of the same State; in Skidaway Island, Georgia, and on the banks of the Potomac river. He further observes that he obtained the tooth of a horse, from eocene marl, in the Ashley river, South Carolina; but the researches of Prof. Holmes || indubitably indicate the specimen to have been an accidental occupant of the formation.

"Specimens of isolated teeth, and a few bones of the horse, from the post-pleiocene and recent deposits of this country, have frequently been submitted to my inspection. Many of these I have unhesitatingly pronounced to be relics of the domestic horse, though I feel persuaded that many remains of an extinct species are undistinguishable from the recent one.

"Whether more than one extinct species is indicated among the numerous specimens of teeth I have had the opportunity of examining, I have been

* Catalogue of Organic Remains, 1826, 7, 8.
† Med. a Phys. Researches, 1835, 267.
‡ Zoölogy, New York, pl. 1, Mammalia, 108.
§ Proc. Amer. Assoc. 1850, 66.
|| Ibidem, 68.

unable satisfactorily to determine. The specimens present so much difference in condition of preservation, or change in structure; so much variation in size, from that of the more ordinary horse to the largest English dray horse; and such variableness in constitution, from that of the recent horse to the most complex condition belonging to any extinct species described, — that it would be about as easy to indicate a half dozen species as it would two.

"Under the circumstances, I would characterize the extinct horse of the United States as having had about the same size as the recent one, ranging from the more ordinary varieties to the English dray horse, with molar teeth, frequently comparatively simple in construction, but with a strong disposition to become complex.

"Among the number of teeth of the horse in Prof. Holmes's collection, labelled as coming from the post-pleiocene deposit of Ashley river, there are several, which, from their size, construction, and condition of preservation, I feel convinced are of recent date; and these, no doubt, became mingled with the true fossils of that formation where it is exposed on the Ashley river, in which position I personally found undoubted remains of the recent horse, and other domestic animals, and objects of human art, mingled with remains of fishes, reptiles, and mammals, washed by the river from the banks, composed of eocene and post-pleiocene deposits.

"Teeth of an extinct species of horse, however, undoubtedly belong, as true fossils, to the post-pleiocene formations in the vicinity of Charleston. These are usually hard in texture, stained brown or black from the infiltration of oxide of iron, sometimes well preserved, but more frequently in a fragmentary condition and water-worn. Generally, they are not larger than the teeth of the more ordinary varieties of the domestic horse, and sometimes are quite as simple in the plication of their enamel; but usually are more complex, and sometimes exceedingly so.

"Among the specimens collected by Prof. H., is a first superior molar tooth, neither larger nor more complex in structure than the corresponding tooth of the recent horse. This specimen, which is dense and jet-black in color, was obtained from a stratum of ferruginous sand, two inches thick, exposed on the side of a bluff, on Goose Creek, about twelve miles from Charleston.

"Having expressed a desire to see the locality from which the tooth just mentioned was obtained, Prof. Holmes afforded me the opportunity of doing so. The bluff is about thirty feet high; its base is formed of a pleiocene limestone, about fifteen feet thick, and composed of the debris of marine shells; above this is the stratum of ferruginous sand, of post-pleiocene age, containing numerous pebbles and rolled fragments of bone, all blackened like the tooth obtained from the same position. Overlying the latter stratum there is a layer of stiff blue clay, about two feet in thickness, and above this there are about twelve feet of sand and earth-mould.

"A remarkably well-preserved specimen of an upper molar tooth, jet-black in color, and an incisor, yellow and quite friable in texture, both belonging to the extinct horse, from North Carolina, have been submitted to my inspection by Professor Emmons. Among the most interesting of the fossils discovered by Prof. Holmes, in the post-pleiocene beds of the Ashley river, are two molar teeth of a species of the equine genus *Hippotherium*. These are the first remains of the latter discovered in America, and they indicate the smallest known species. Both specimens are from the upper jaw; and they are well characterized, not only by the isolation of the internal median enamel column, but also by the complex plication of the interior or central enamel columns. The larger specimen is firm in texture; has the enamel stained jet-black, and the dentine and cement gray. Teeth of the beaver, jet-black in color, have likewise been obtained from the post-pleiocene deposit of Ashley river.

"The collections contain numerous specimens of blackened molar teeth, together with a few incisors and fragments of jaws, from the Ashley post-pleiocene deposit, which neither differ in form nor size from the corresponding parts of the recent musk-rat.

"Remains of *Lepus sylvaticus* — common gray rabbit — have been found, in association with those of other rodents and of the extinct peccary, near Galena, Illinois. A few specimens of molar teeth, black in color, apparently belonging to this species, were obtained from the post-pleiocene beds of the Ashley river.

"Several small fragments of teeth of the *Megatherium*, in Prof. Holmes's collection, were obtained from the post-pleiocene bed of the Ashley river. Previously to the discovery of these specimens, remains of the *Megatherium* had been found in no other locality of North America than in the State of Georgia."

As regards specimens of human art found in connection with these fossils, Prof. Holmes remarks, that this is the case at only one locality, — Ashley Ferry, — which is adjacent to a farm-yard. At other localities, where similar fossils are found, no relics of art have ever been noticed.

The fossils from Ashley Ferry present, as a group, the same appearance as those procured inland at some distance from the river, by digging from three to five feet below the surface. Many specimens from the ferry were considered as recent by Prof. Leidy; they appear quite fresh and unchanged in color, and their texture not in the slightest degree altered. To one familiar with the fossils of the South Carolina post-pleiocene, this excites no surprise, as it is of common occurrence, more especially among the shells; for example, the olive shell — *Oliva literata* — is found as fresh and highly polished as the recent ones from the sea-beaches along the coast; and *Cardium magnum* retains often the delicate yellow and brown markings common to the species.

The color or texture of a fossil, therefore, does not always absolutely determine its relative age; as Professor Leidy has himself remarked in a foot-note to his letter alluded to above, viz.:

"Fossilization, petrification, or lapidification, is no positive indication of the relative age of organic remains.

"The Cabinet of the Academy of Natural Sciences, of Philadelphia, contains bones of the megalonyx, and of the extinct peccary, that are entirely unchanged; not a particle of gelatin has been lost, nor a particle of mineral matter added, and, indeed, some of the bones of the former even have portions of articular cartilage and tendinous attachments, well preserved."

From the foregoing it would appear that of the ancient fauna of America, which included representatives of many of our present domestic animals, some species have undoubtedly become extinct; but I confess I am not yet prepared to admit, from any evidence yet adduced, or from my own examinations, that all of the living species are distinct from those found fossil in the post-pleiocene. The teeth and bones of the rabbit, raccoon, opossum, deer, elk, hog, dog, sheep, ox, and horse, are often found in these beds; and though associated with those known to be extinct, such as mastodon, megatherium, hipparion, etc., need not necessarily be referred to extinct races also; since their remains cannot be distinguished from the bones and teeth of the living species.

Of the mollusca from the same beds, about ninety-five per cent. are, to my mind, identically the same with species now living on the coast of South Carolina. Two species of these shells, though extinct, or not in existence here, are now living in numbers on the coast of Florida and the northern shores of the Gulf of Mexico; and two have no living representatives that we can discover.

The question, therefore, naturally suggests itself — Are the living horses, dogs, hogs, raccoons, opossums, deer, elk, tapirs, beavers, etc., and the one hundred and fifty living shells of the coast, the descendants of the animals whose remains we find fossil in the above-named beds?

It has been just remarked that about ninety-five per cent., or nearly all of the one hundred and fifty shells of molluscous animals from these beds, are specifically identical with the recent or living species of the coast, — two are found only at the south of this, and two are extinct. Of the vertebrates from the same bed, the tapir, peccary, raccoon, opossum, deer, musk-rat, rabbit, beaver, and elk, have still their living representatives, generically, if not specifically; and even of the identity of species there seems to be no doubt, as no anatomical differences can be discerned. Two of these species, like the mollusca just alluded to, no longer live in South Carolina; the tapir and peccary are only found in South America and Mexico; the musk-rat, elk, and beaver, though extinct on the Atlantic coast, are still living in the interior of the country. And though it has been acknowledged that the mastodon, megatherium, elephant, glyptodon, and two species of Equine genera, etc., are entirely extinct, yet the discoveries made of the remains even of some of these, would indicate that they still existed at a period so recent, that, in the language of Prof. Leidy, "it is probable the red man witnessed their declining existence."

The peccary, or Mexican hog, an animal common in Mexico, is not indigenous to the Atlantic United States; but his bones have been found associated with human remains in caves used as cemeteries by the Aborigines. "A tomb in the city of Mexico," according to Clavigero (?), "was found to contain the bones of an entire mammoth, the sepulchre appearing to have been formed expressly for their reception." And "Mr. Latrobe relates that, during the prosecution of some excavations near the city of Tezcuco, one of the ancient roads or causeways was discovered, and on one side, only three feet below the surface, in what may have been the ditch of the road, there lay the entire skeleton of a mastodon. It bore every appearance of having been coeval with the period when the road was used."

Again I extract from Prof. Leidy's letter:

"The early existence of the genera to which our domestic animals belong, has been adduced as presumptive evidence of the advent of man at a more remote period than is usually assigned. It must be remembered, however, even at the present time, that of some of these genera only a few species are domesticated; thus of the existing six species of *Equus* (horse) only two have ever been freely brought under the dominion of man.

"The horse did not exist in America at the time of its discovery by Europeans; but its remains, consisting chiefly of molar teeth, have now been so frequently found in association with those of extinct animals, that it is generally admitted once to have been an aboriginal inhabitant. When I first saw examples of these remains I was not disposed to view them as relics of an extinct species; for although some presented characteristic differences from those of previously known species, others were undistinguishable from the corresponding parts of the domestic horse, and among them were intermediate varieties of form and size. The subsequent discovery of the remains of two species of the closely allied extinct genus Hipparion, in addition to the discovery of remains of two extinct Equine genera of an earlier geological period, leaves no room to doubt the former existence of the horse on the American continent, contemporaneously with the mastodon and megalonyx, and man probably was his companion."

The result of the whole seems to be, that of the animals found fossil in the post-pleiocene beds, all the mollusca of the present day are undoubtedly a perpetuation of the same species; that of the higher order of vertebrata, the tapir, peccary, raccoon, opossum, deer, elk, and musk-rat, are equally entitled to be considered the descendants of this ancient race. And if the claims of the mollusca to this distinction rest upon a secure basis, because they are peculiar to this country, and not obnoxious to suspicion of foreign immigration, it must be recollected that this is equally true of the above-named animals.

Those which have hitherto been regarded as of recent and European origin, are the horse, sheep, hog, and ox; and it must be reserved perhaps for future consideration to determine how far the negative proof of the non-existence of these animals in the country at the time of its discovery may be regarded in each individual case sufficiently strong to settle the question of his extinction and reïntroduction, when so many of his associates and contemporaries have succeeded in maintaining an unbroken line of descent down to the present day.

Prof. Agassiz's opinion in relation to these relics is expressed as follows: There is hardly anything of interest in the bones themselves, since they all belong to well-known types; yet their simultaneous occurrence in the same beds, showing that they lived together at a time when the white man had not yet planted himself upon this continent, render their association undisputed.

How does it happen that horses, sheep, bulls, and hogs, not distinguishable from our domestic species, existed upon this continent, together with the deer, the musk-rat, the beaver, the hare, the opossum, the tapir, which in our days are peculiar to this continent, and not found in the countries where our domesticated animals originated? The whole matter might seem to admit of any easy solution by supposing that the native American horse, sheep, bull, and hog, were different species from those of the Old World, even though the parts preserved show no specific differences; but this would be a mere theoretical solution of a difficulty which seems to me to have far deeper meaning, and to bear directly upon the question of the first origin of organized beings.

The circumstances under which these remains are found, admit of no doubt but the animals from which they are derived existed in North America long before this continent was settled by the white race of men, together with animals which to this day are common in the same localities, such as the deer, the musk-rat, the opossum and others only now found in South America, such as the tapir. This shows, beyond the possibility of a controversy, that animals which cannot be distinguished from one another, may originate independently in different fauna; and I take it that the facts you have brought together, are a satisfactory proof that horses, sheep, bulls, and hogs, not distinguishable at present from the domesticated species, were called into existence upon the continent of North America prior to the coming of the white race to these parts, and that they had already disappeared here when the new comers set foot upon this continent. But the presence of tapir teeth among the rest, show also that a genus peculiar to South America and the Sunda Islands existed also in North America in those days, and that its representative of that period is not distinguishable from the South American species.

It would be desirable, in this stage of the inquiry, to compare your tapir teeth with those of the species from Central America, which is considered distinct from the Brazilian species. This circumstance leads naturally to the question of the specific identity of all these animals with those now living in the same locality, and with the domesticated species. And here I confess the difficulty to be almost insuperable, or at least hardly approachable in the present state of our science, when the views of naturalists are so divided as to what are species among the genera bos, ovis, capra. For myself, I entertain doubt respecting the unity of origin of the domesticated horses. But whatever be the final result of this inquiry, this much is already established by the fossils you have collected, that horses, hogs, bulls, and sheep, were among the native animals of North America, as early as the common American deer, the opossum, the beaver, the musk-rat, etc.

Fossil Remains of the Horse in New York. — At a recent meeting of the Boston Society of Natural History, Dr. A. A. Gould announced, on the authority of Dr. Skilton, of Troy, N. Y., the discovery of a number of teeth of the fossil horse, in Brunswick, Renssalaer County, N. Y., during the trenching of a spot of marshy ground. The whole number of teeth obtained was seventeen.

INTERESTING EXPLORATION OF A BONE CAVERN.

At the last meeting of the British Association, Leeds, 1858, Mr. W. Pengelly read a paper in the geological section, describing a recently discovered bone cavern at Brixham, near Torquay, which yielded, on exploration, upwards of 2,000 bones of the rhinoceros, bos, horse, reindeer, cave bear, and hyena, with which were mingled well-marked specimens of the remains of savage men, in the shape of flint-knives and arrow-heads. There is so great a disposition felt, however, in England, as well as in this country, to dislike, upon theological grounds, the obvious consequences of such discoveries, to carry back the life of man upon the earth beyond the remotest era of any known history or any calculated chronology, that the paper, when read, excited not only its due interest, but earnest discussion. As it is acknowledged to be now no longer possible to separate the remains of man from those of extinct animals, and some new line of defence must be adopted by holders of the old ideas, the effort was made to bring the era of the animals down into the commonly accepted era of mankind.

Prof. Owen said he was glad that means had been taken for the careful exploration of this cave, but it would be premature to raise any hypothesis until the whole of the facts were before him. He had not seen any of the bones, and, indeed, was entirely indebted for what he knew on the subject to the paper which Mr. Pengelly had read, and he should refrain, therefore, from expressing any opinion; but he wished to caution them against coming to conclusions as to the antiquity of these remains which were not really warranted. He proceeded to show, from the remains of tigers, elephants, and other animals found in this country, in Siberia, and other parts of the world, where the climate was much colder than was supposed to be compatible with their existence, that there was undoubted evidence that these animals could adapt themselves to cold and temperate climates as well as to torrid ones, and remarked, that the conditions of animal life were not those of climate, but of food and quiet. Wherever there was the prey undisturbed by man, there also would be the destroyer. They had evidence from the writings of Julius Cæsar of the existence in England, 2,000 years ago, of three distinct species of animals, including two gigantic species of ox, and one of the reindeer, and he was himself satisfied that they had once had a native British lion, all of which, however, were now extinct in this country; and he saw nothing in the remains which had been discovered at Brixham to lead him to suppose that the animals lived before the historic period, or which was inconsistent with the concurrent existence of a rude race of barbarians. At the same time he was open to conviction, and should be very glad to see a good fossil human being, which should prove that man had been much longer upon the earth than historical evidence had led them to suppose.

Professor Owen, with reference to this part of the subject, said that some time ago he was sent for to the North, to examine a fossilized tree, which had been found in digging the Jarrow dock, which bore undoubted evidence of having been cut by human hands. It was supposed to be a most important discovery, as showing the antiquity of the human race, and at first everything appeared satisfactory. On prosecuting his inquiries, however, he learned that one of the navvies, not then on the works, was said to have discovered a similar tree in another part of the dock, which he cut to lay down a sleeper. The man was sent for, and on his arrival he declared that the tree pointed out was the one he had cut. It was endeavored to be explained that that was impossible, as the place had not been excavated before; but, looking with supreme contempt upon the assembly of geologists and engineers, the man persisted in the identification of his own work, and exclaimed, "the top of the tree must be somewhere," upon which he (Professor Owen) offered half-a-crown to the first navvy who would produce it. Away ran half a dozen of them, and in a few minutes they returned with the top. This explained the mystery. The man had cut off the top with his spade, the stump afterwards got covered up with silt, and on being again uncovered it was supposed to be a great discovery. Never had he so narrow an escape from introducing a "new discovery" into science, and never had he a more fortunate escape.

Mr. Teale brought to the notice of the same section fine specimens of elephant and hippopotamus bones from the clays of Aire Valley. Glacial clays he considers them, resting on the upturned edges of the coal measures. The lowest is blue clay deposited by glacial waters in an era of *submergence;* on it a yellow clay also deposited by ice, but in an era of *emergence.* On this again a "warp," with angular and also rolled stones, the debris of the blue and yellow clays and gravel from distant hills, *subsequent to the glacial period* in England, and containing the animal remains.

EVIDENCES OF THE ANTIQUITY OF THE HUMAN RACE.

In a memoir published in the proceedings of the Royal Society, England, for 1857, Mr. Horner gives a detailed account of a series of researches recently undertaken near Cairo, in Egypt, with a view of throwing light upon the geological history of the alluvial deposits of the Nile. The researches were made by sinking shafts in rows from the foot of the Lybian rocks across to the banks of the Nile, around the lonely obelisk of Heliopolis, and alongside of the equally solitary statue of Rameses II.; and they have revealed the following facts:

That the alluvium consists of desert sand and river mud, alternately, all the way down, the bottom layer of mud being exactly like the top; that no extinct organic forms are present in it, but only microscopic infusorial shells, and recent land-shells and bones of domestic animals; that no rock was touched by any of the shafts, the deepest of which was sixty feet; and that fragments of burnt brick and pottery were interspersed throughout the whole deposit. Mr. Horner states that he has in his possession a fragment of pottery, an inch square and a quarter of an inch in thickness, the two surfaces being of a brick-red color, which had been obtained from the lowest part of a boring, near the statue of Rameses II., and thirty-nine feet below the surface of the ground. Now the statue of Rameses II. has been determined by Lepsius to date between 1394 and 1328 B. C. Yet its foundation rests on a bed of sand but twelve feet beneath the surface, while the borer brought up a fragment of pottery from a mud layer twenty-seven feet further down. The French engineers of the last century decided the rate of vertical increase of the delta to be five inches in a century. The researches at Heliopolis give 3·18 inches to a century. But the statue of Rameses fixes it within a small fraction of 3½ inches to a century. Allowing this last estimate to be correct, the fragment found at the depth of thirty-nine feet is a record of the existence of man 13,375 years before A. D. 1858 — 11,517 years before the Christian era — and 7,625 years from the beginning assigned by Lepsius to the reign of Menes, the founder of Memphis — of man, moreover, in a state of civilization, so far, at least, as to be able to fashion clay into vessels, and to know how to harden it by the action of strong heat.

FOSSILS OF NEBRASKA.

Messrs. Meek and Hayden have recently published, in the proceedings of the Philadelphia Academy of Natural Sciences, a complete catalogue of all the remains of invertebrata hitherto described and identified from the interesting Cretaceous and Tertiary formations, generally known as the Bad Lands of Nebraska.

In glancing over this catalogue (we quote from the authors), the palæontologist will not fail to be struck with the great preponderance of *Lamellibranchiata*, *Gasteropoda*, and *Cephalopoda* over all the other invertebrate forms of life. Among all the collections we have yet seen from this region, the *Bryozoa* are represented by but one rare species of *Reticulipora*, and the *Brachiopoda* by only one species of *Caprinella* and one of *Lingula*, both so rare that but a single specimen of each has been found; while of the whole great class of *Echinodermata*, which existed in such vast numbers, and presented such an infinite variety of beautiful forms, during these epochs in some parts of the world, we have yet only seen, from this region, a single fragment, too imperfect to give any clue to its generic relations. The paucity of some, and entire absence of others, of the more common genera of *Mollusca*, such as *Ostrea*, *Gryphæa*, *Exogyra*, etc., in these collections is worthy of notice. Future investigations, it is true, may add more species to our present meagre list of these rare forms, yet it is probable we have here something like an expression of the numerical proportions in which many of the lower types of life existed in these ancient seas.

Of the one hundred and ninety-one species enumerated in this catalogue, forty-four belong to the Tertiary system, and one hundred and forty-seven to the Cretaceous. None of the former are known to occur in the States, or on the other side of the Atlantic, while, of the Cretaceous species, nine appear to be common to the Nebraska formations and those of the States, and four are identical with forms occurring in the Old World. Of these nine species having so great a geographical range, six, or nearly one-third of all that class of *Mollusca* contained in the list, belong to the *Cephalopoda*, while nearly all the remaining one hundred and seventy-six species, which appear to be restricted to the north-west, belong to the *Lamellibranchiata* and *Gasteropoda*. This, however, is not so surprising when we bear in mind the fact that the habits and organization of these ancient *Mollusca* must have been such, from what we know of their existing analogues in our present seas, that the former depended on accident, or feeble locomotive organs, for their gradual distribution over the world from their various centres of creation, while the *Cephalopoda*, owing to their superior locomotive powers, were capable of wandering freely far out over the most profound parts of the ocean.

It would, perhaps, be premature to attempt, at the present time, the task of tracing out in much detail the parallelism of the various members of the Cretaceous system in Nebraska, with those of New Jersey and other well-known districts in the States, or with those of the south-western territories; yet the occurrence of several of the more common and characteristic fossils of the upper two Nebraska formations, such as *Ammonites placenta*, *Scaphites Conradi*, *Baculites ovatus*, *Nautilus Dekayi*, etc., in the first and second Green Sand beds and intervening ferruginous stratum of New Jersey, as well as in the "Rotten Limestone" of Alabama, clearly indicates the synchronism of these deposits, notwithstanding their widely separated geographical positions.

That these beds, or formations of the same age, are widely distributed over a vast area of country, extending from near the great bend of the Missouri, in lat. 44° 15′′, long. 99° 20′, westward to, and perhaps beyond, the eastern slope of the Rocky Mountains, and far south into Texas and New Mexico, is highly probable, from the occurrence of their characteristic fossils at many widely separated localities in this region.

ON THE EXISTENCE OF POTSDAM SANDSTONE ON THE EASTERN SLOPE OF THE ROCKY MOUNTAINS.

Messrs. Meek and Hayden, in a paper published in the Proceedings of the Academy of Natural Sciences, Philadelphia, March, 1858, announce the existence of the Potsdam Sandstone on the eastern slope of the Rocky Mountains, — a discovery made during Lieut. Warren's expedition to the Black Hills, in the summer of 1857. These hills seem to have furnished the key to its existence in the Far West, where it is found to contain quite numerous well-preserved fossils, as Lingula (L. antiqua), Trilobites, etc., similar or identical with those characterizing the same formation in Minnesota and New York.

New Fossils from the Potsdam Sandstone. — At a recent meeting of the Boston Society of Natural History, Mr. Daniels, State Geologist of Wisconsin, presented some minute Trilobites, and other fossils, from the base of the Potsdam Sandstone of Wisconsin. The localities were various: the valley of the Black River, in the north-western part of the State, the mouth of Black River, and a spot sixty miles up the same river. He stated that they were interesting, being the oldest fossil forms yet found in this country, the sandstone resting directly upon the upturned edges of the Azoic rocks. Upon a small island in Black River he had found perfect impressions of Crustaceans, consisting of double rows of parallel tracks, precisely like those in Montreal.

PERMIAN ROCKS IN KANSAS.

According to the researches of Prof. Swallow, of Missouri, the Permian rocks of Kansas are 820 feet thick — 263 of the whole being separated as the *upper* Permian, and the rest the *lower* Permian. The rocks are limestones, with some shales or clay layers, some of the limestones also containing hornstone. Above the Permian, in Kansas, there are 420 feet of sandstone, with some calcareous and argillaceous layers, and occasional beds of gypsum. These rocks are referred, with a query, to the Triassic, but may be Jurassic or Cretaceous. A *trilobate exogenous* leaf is mentioned as the only observed fossil plant, and this would favor the last conclusion. Above these beds, there are in the section two feet of Cretaceous rocks, and 169 of "Quarternary."

The Permian is a direct continuation, apparently, of the Coal Measures, without unconformability. The Carboniferous rocks have a thickness in Kansas of 1070 feet, and are supposed to be higher in the series than the Upper Coal Series of Missouri. Between the Permian and the overlying strata there appears to be an entire unconformability.

Of the seventy-five species of fossils found in the Permian rocks, and published by Prof. Swallow, only sixteen occur also in the Carboniferous; yet, while the majority of the species are peculiar to the Permian, the majority of the individuals are Carboniferous.

THE TERTIARY CLIMATE.

Professor Unger, of Vienna, has found genuine reef-forming Corallidæ in the Tertiary strata of the Pannonian basin (south-east from Vienna) in latitude 47°, while at present the northern limit of such corals in the Red Sea and Persian Gulf is at 29°, thus furnishing a new proof of the higher temperature which prevailed in Europe at the Tertiary period. — *Edin. New Phil. Jour.*

ON THE FOSSIL REMAINS OF THE GENUS ELEPHAS.

The following is an abstract of a paper recently read before the Geological Society, London, by Dr. Falconer, on the remains of elephants and other

Proboscidea from the drift and upper tertiary deposits. The author reviewed, in a brief but comprehensive manner, the labors of Cuvier, De Blainville, Blumenbach, Kaup, Nesti, Owen, and others, pointing out the very wide range which had in the first instance been assigned by Cuvier to the Elephas primigenius, or great Siberian mammoth. The author believed that many teeth belonging to other species, and even to other subgeneric forms, had been confounded with those of Elephas primigenius, and owing to such erroneous and improper determination, nearly one half of the whole world had been yielded as the *habitat* of this peculiar elephant; whereas he hoped to show that these teeth belonged to distinct species, and, in some cases, even to distinct sub-genera of the Proboscidea. Most of the subsequent writers had followed Cuvier in his erroneous determinations on this subject. The author referred to a valuable synopsis which he had prepared, and which was suspended in the room, showing that the Proboscideans, or mammals with prehensile trunks, were divisible into three great genera, namely, the dinotherium, the mastodon, and the elephant.

The genus Dinotherium contained two species. The mastodon was divisible into three sub-genera, namely, the trilophodon, containing seven species; the tetralophodon, containing six species; and the pentelophodon, of one species. The genus Elephas he has divided also into three sub-genera, namely, the stegodon, containing four species; the loxodon, four species; and the enelephas, or elephants proper, containing six species.

All the preceding genera and sub-genera are now extinct, except two species of enelephas, namely, the Asiatic or Indian elephant, and the African elephant.

The Dinotherium is an extinct genus of the Proboscidea, established by Professor Kaup from remains which were abundantly met with at Eppelsheim, in Hesse Darmstadt, in beds which the continental geologists consider equivalent of the mioscene of Lyell. The restoration, by Professor Kaup, of the Dinotherium giganteum, represents an enormous amphibious animal, with a head nearly four feet in length, extreme length of body about eighteen feet, head furnished with a long oblique proboscis, and with two remarkable incurved tusks like those of the walrus, and the general form resembling that of a gigantic tapir.

The mastodon is distinguished from the elephant by the structure of the molar teeth. The ridges of dentine, surrounded by enamel, which pass transversely across the tooth, are further apart, are more wavy in their outline, and in young specimens are broken into little isolated conical elevations. When these wear down, the hard ridges, or ridge-plates as Dr. Falconer terms them, resemble more the lozenge shapes on the teeth of the African elephant than the elongated narrow laminæ of Elephas indicus.

The sub-genera of mastodon had been named from the number of transverse crests or ridges on the teeth (lophodon, tooth-crested, from λόφος, crest, Οδους, tooth); hence trilophodon, teeth three-crested; tetralophodon, teeth four-crested; pentelophodon, teeth five-crested, etc.

Pictet and other palæontologists have long complained of the way in which the remains of mastodon have been confounded with Elephas. The genus mastodon appears to have had a wide range, although no remains have yet been met with in Britain except in the marine crag of Norfolk, from which Mr. Morris quotes M. angustidens. The remains of the mastodon are abundant near Eppelsheim; they have also been found in the environs of Zurich, in Auvergne, in Bavaria, and Saxony. Species of mastodon have also been described by Mr. Clift, from the banks of the Irawadi in Birmah, and by D'Orbigney and Humboldt, from South America.

The sub-generic distinctions of Elephas were also founded on peculiarities in the teeth, the stegodon having crown ridges increasing in each tooth as the numbers two, five, seven; the loxodon having three, six, and seven; while in Enelephas the increase was much more rapid, as four, eight, twelve, etc., up to twenty-four. The Enelephas primigenius, or fossil elephant of Siberia, was designated as the true mammoth, with rhomboidal shaped crown ridges, placed close together, the enamel surrounding the dentine, thin, and without waviness, crimping, or festooning. The most perfect remains of this elephant are to be seen in the museums of Mannheim and Darmstadt. They are to be found abundantly in the loep of the Rhine and in the drift of northern Germany. In England they are found in the boulder clay of Norfolk, in the marine crag of Norwich, in the valley of the Thames, and other glacial deposits. The author believes the true mammoth did not extend south of the Alps, as he has never found it in the museums of Italy.

The teeth of the loxodon have a thick transverse digitation, without any rhomboidal form in the ridge-plates. The enamel surrounding the dentine is thick, and sections of the teeth present an intermediate form between that of the elephant and the mastodon.

The Loxodon priscus is found abundantly in the Val d'Arno, in Italy, also in Britain and Auvergne.

Dr. Falconer glanced at the various localities for remains of Proboscidea in England, and took some objections to the subdivisions which had been introduced into British geology by the terms pliocene, newer pliocene, and post-pliocene. He observed that whatever evidence of distinction and separation was afforded by the mollusca, the mammalian evidences drawn from these beds failed entirely to support their separation and subdivision. Mammalian remains, which were found isolated and at considerable distances apart in England, had been met with on the Continent in the same deposit, and all associated on the same spot — a fact which went to prove that the corresponding deposits in England belonged to one and the same age.

The author insisted, also, on the separation of the genus Elephas into the three sub-genera above indicated, and these again into the several species represented in his synopsis. The paper was profusely illustrated with a magnificent series of colored diagrams, representing teeth of all the known forms of proboscidea from all parts of the world, including those with which the author had become acquainted during his residence in India.

Professor Owen paid an eloquent and well-deserved compliment to the remarkable research and ability which Dr. Falconer had exhibited in this communication. He hailed the paper as a great contribution to the history of the fossil mammalia — a branch of natural history which Dr. Falconer had made peculiarly his own. Notwithstanding the clear and able manner in which Dr. Falconer had marshaled his evidence on the subject, he was not prepared to admit all the generic and specific distinctions which Dr. Falconer relied on in subdividing his genus Elephas. The Professor pointed out many changes which age, sex, habits, etc., would effect in the grinding surfaces of the molar teeth, and he thought the evidence in support of separation on such grounds was to be received with great caution. The paper of Dr. Falconer would clear the way of many difficulties for future observers, and would doubtless lead to a more close observation and accurate determination of the larger mammalian remains.

DEVONIAN TREES.

Fossil wood is announced, by J. W. Dawson (Proceedings Amer. Assoc., tenth meeting, p. 174), as occurring in the Devonian standstones of Gaspé. The wood was black and silicified, being a trunk four feet long, and seven to nine and a half inches in diameter. A cross section exhibited a distinct cellular tissue, with circular, not crowded, cells; and there were well-defined rings of growth, averaging about a line in breadth. In a vertical section, the cells were seen to be elongated, and to terminate in conical points; they showed traces of transverse and diagonal fibres, sometimes decussating, but no medullary rays or disk structures were visible. The tree was referred to the Coniferæ, but stated to differ materially from any previously observed form. The author adds that it may be related to fossil trees of the Devonian "Cypridina schists" of Saalfield in Meiningen, described by Professor Unger as of very singular organization, as if the prototypes of the "Gymnosperms."

ON THE PLANTS OF THE COAL SERIES OF PENNSYLVANIA.

A memoir, published in the Journal of the Bost. Soc. Nat. His. vol. VI., by Messrs. Rogers and Lesquereux, contains descriptions of one hundred and six new species of coal-plants from the coal-fields of Pennsylvania. Prof. Rogers, in his introductory observations, states that M. Lesquereux has found that, out of over two hundred species examined by him, one hundred are "identical with species already recognized in the European coal-fields, and some fifty more of them show differences so slight that a fuller comparison with better specimens may result in their identification likewise;" moreover, "those new species, which seem to be restricted to this continent, are every one of them in close relationship with European forms." The new species are of Calamites 2, Asterophyllites 5, Annularia 1, Sphenophyllum 2, Nœggerathia 3, Cyclopteris 5, Neuropleris 13, Odontopteris 2, Sphenopteris 8, Hymenophyllites 3, Pachyphyllum (new genus) 5, Asplenites 1, Alethopteris 5, Callipteris 1, Pecopteris 7, Crematopteris 1, Scolopendrites 1, Caulopteris 2, Stigmaria 5, Sigillarea 9, Lepidodendron 10, Lepidophyllum 6, Brachyphyllum 1, Cardiocarpon 3, Trigonocarpum 1, Rhabdocarpus 1, Carpolithes 3; and Pinnularia 5 (named but undescribed).

FOSSILS FROM TEXAS.

At a late meeting of the Boston Society of Natural History, Prof. Jeffries Wyman gave an account of some fossil bones, presented to the Society by Dr. Chas. Martin, U. S. N., which were purchased by him while attached to the Coast Survey during the winter of 1855–6, at the mouth of the Brazos river. They were discovered in the bed of the river, during its low stage, about fifty miles from the coast. The collection is very valuable and interesting, not only as representing three distinct races of gigantic quadrupeds, but as indicating a new locality in the geographical distribution of the animals to which they belonged. It is not a little remarkable that three such genera as Mastodon, Elephant, and Megatherium, should be represented in a collection of no more than eight specimens taken at random. Six of the eight appear to have undergone similar changes of density and mineralization; these are the symphysis of the lower jaw, and ultimate molar, and the femur of an elephant, the tibia of megatherium, and the two molars of a mastodon. The others are lighter colored, and much less dense. Coming as they do from the bed of a river, it is impossible to determine how far they were originally associated in the same geological formation. It is not impossible that those first mentioned were from the same locality.

The dimensions of the tibia of the megatherium from the Brazos indicate that it must have belonged to a much larger animal than the one whose remains are preserved in the museum of Madrid: — thus, the circumference of the bone around the lower extremity, without the fibula, is two feet seven inches; that of the Madrid specimen, *with the fibula*, is two feet six inches and a quarter. The breadth of the Madrid specimen, *with* the fibula, is twelve and a half inches, and that of the specimen from the Brazos river, *without* the fibula, is thirteen inches. In North America the megatherium has hitherto been found only in two localities, viz., Skiddoway Island on the coast of Georgia, and on the banks of the Ashley river in South Carolina.

ON THE MUD VOLCANOES OF THE COLORADO DESERT.

The following is an abstract of a paper communicated to the California Academy of Natural Science, by Dr. John Veatch, descriptive of a visit to the Mud Volcanoes of the Colorado Desert, in the month of July, 1857. Among the numerous objects in California inviting the investigation of the scientific and the attention of the curious observer, none are more conspicuous than the "Salses" or "Mud Volcanoes" of the Colorado Desert. Hidden amidst the burning sands of a frightful waste, few persons have had the temerity to encounter the labor and risk of visiting them. Even the Indians, inhabiting the border of this Western Sahara, do not willingly venture so far into its midst, unless it be during the annual rains. At any other period, to miss one of the few springs of brackish water, or to find the place occupied by drifting sands — a not unusual occurrence — would entail the certainty of the horrors of thirst, if not loss of life. From personal experience I cannot blame the repugnance of the natives to visit a district, which, in addition to its physical repulsiveness, they suppose to be the abode of dark and malignant spirits.

The striking peculiarities of this wild region are, however, too striking to remain long unsubjected to thorough exploration. The entire desert is supposed to have been the bed of a great brackish or fresh-water lake, and is said to lay many feet below the level of the ocean. The part I lately visited showed deep lacustrine deposits, inclosing, in myriads, the conchological records of the former sea.

It was in the month of July that I had occasion, in the progress of a mineralogical excursion, to visit one of the above-named Salses. It is situated about one hundred and fifty miles from San Diego, and sixty miles, in a north-easterly direction, from the Indian village of San Felipe — the nearest inhabited habitable place.

Starting from the above-named village, with an Indian guide, and horses carrying provisions and water, the route for two days lay for the most part over a desert track of sands and clays, thickly covered in some places with forest of prickly cacti. On the morning of the third day, the white clouds of steam arising from the Salse became apparent at a distance of about ten miles, while the dull roar of the eruption could occasionally be heard. Approaching on horseback as near as was prudent, Dr. V. dismounted, and proceeded on foot, over a soft and muddy ground, to the Salses. The scene here, he says, is difficult of description, and the effect can only be known by one who has heard the wild rush of steam, the rude sounds of the mud explosions, and the dull murmur of the boiling cauldrons of slime. The space occupied by the Salses is a parallelogram, 500 yards long and 350 broad — a table of hardened bluish clay, a little elevated above the surrounding plain. The adjacent ground is low and muddy, and during the rains is entirely covered with water. There is a gentle slope towards the north and east, the mud and water of the Salses running off slowly in that direction, where a lake of salt water exists in the rainy season, but presenting now a vast sheet of crystalline chlorid of sodium. Into this lake the arm of the Colorado, known as New River, discharges itself. The lake, having no outlet, would probably soon regain its ancient area if the channel of New River afforded a regular and more generous supply of water.

The steam-jets of the Salse issue from conical mounds of mud varying from three to fifteen feet in height, the sides presenting various angles, some being sharp and slender cones, others dome-shaped mounds that seem to have spread and flattened out with their own weight, upon the discontinuance of the action that formed them. Out of some of the cones the steam rushes in a continuous stream, with a roaring or whizzing sound, as the orifices vary in diameter or the jets differ in velocity. In others the action is intermittant, and each recurring rush of steam is accompanied by a discharge of a shower of hot mud, masses of which are thrown sometimes to the height of a hundred feet. These discharges take place every few minutes from some of the mounds, while others seem to have been quiet for weeks or months. During our short stay we had specimens of the rapidity with which a sharp, conical mound could be built up and again tumbled down. In one place a stream of hot water was thrown up from fifteen to thirty feet, falling in a copious shower on every side, forming a circle within which one might stand without danger from the scalding drops, unless the wind chanced to drive them from their regular course. It issued from a superficial mound, out of an opening about six inches in diameter; but the column of steam and water, immediately upon issuing, expanded to a much greater size. The orifice was lined with an incrustation of carbonate of lime, and around it, and particularly on the southeast side, stood a miniature grove of slender stalagmitic arborescent concretions of the same substance. They were from half an inch to one and a half inches in diameter, and from four to eight inches in height. Many of them were branched, and the tips colored red, contrasting beautifully with the marble-whiteness of the trunk, and resembling much a coral grove. Some were hollow, and delicate jets of steam issued from their summits, and this seemed to explain the mode of their formation. Some were not hollow throughout, being closed at the summit; but when detached from their base, a small orifice in the centre suffered hot steam to pass, and some degree of caution was required to remove them without scalded fingers. To approach the spot was a feat of some difficulty, surrounded as it was by a magic circle of hot rain. I retreated, scalded, from the only attempt I dared to make; but my son, more adventurous, or more attracted by the beauty of the specimens, succeeded in bringing away several. The falling water ran off into a pool a foot deep, but what became of it was not apparent, as it had no seeming outlet. I brought away a bottle of it for examination. It was transparent, but had an intensely bitter and saline taste. A little beyond, on either hand, are two huge cauldron-like basins, sunk five or six feet below the general level, and near a hundred feet in diameter. Within these cauldrons a bluish argillaceous paste is continually boiling with a dull murmur, emitting copious sulphurous vapors, and huge bubbles, bursting, throw masses of mud to the height of several feet. These kettles sometimes boil over, and the matter runs off in a slimy stream toward the salt lake. This seems to have been the case recently, as we encountered the track of one of these streams, not yet dry, a mile from the Salse.

The volcanic action was far more violent at some former period than at present, as is proved by fragments of pumice scattered over the plain. Our visit lasted only an hour and a quarter, owing to a deficiency of water. The tempting objects in the vicinity, which would require many days for examination, could only be greeted with a farewell glance, and our horses' heads were turned towards the nearest point at which a supply of water could be obtained.

For the first three or four miles after leaving the Salse, the plain presented a smooth surface of sand and bluish clay, baked and fissured, and strewn sparingly with volcanic cinders and obsidian fragments. Round holes marked the escape of gas when the ground was softened by water. Soon the plain became cut up with ravines three or four feet in depth, which Josè said were the arms of "New River," which branched out before entering the salt lake. The remains of a most luxuriant vegetation, now dead and dry, proved the place to be a desert only for want of water.

Thus ended a hurried trip to a most interesting spot in the midst of a no less interesting district. The shells obtained were submitted to Dr. Trask, and were found to consist of two species of Amnicola (A. protea and A. longinqua, Gould), and the Physa (P. humerosa, Gould), before named. A large bivalve was observed, but so thin and fragile that the specimens broke to small pieces for the want of safe means of transporting them.

The water from the volcano has the specific gravity of 1·075, and holds in solution free *boracic acid*, with *borates* and a large quantity of chlorid of sodium, and other salts. These matters would indicate the true volcanic origin of the Salse, and but little doubt rests on my mind of its being so. The evidence of former volcanic action in the neighborhood, and the testimony of the boracic acid, establish its true character. The acid and its compounds exist only in small quantities, but sufficient to be unequivocally determined. Similar Salses exist some thirty or forty miles farther south. One made its appearance during the earthquake of November 29, 1852, a few miles below the line of the State. Two others exist in the same district, as I was informed by a person who professed to have visited them. One is represented as a single jet of steam and water from an opening, a yard in diameter, situated in a plain of hardened clay. The other consists of several pools of warm water, through which hot gas is continually escaping. Another is again spoken of in the adjacent mountain, partaking of the true volcanic character, emitting fire and smoke. I hope some one will soon have occasion to examine these and other interesting localities at a season when it will be practicable to pass a few days on the desert without danger of perishing from thirst.

The real character of this desert has not been generally understood. In its present condition it is truly a desert. But only a portion, however, of its immense area is condemned to irretrievable barrenness, viz., the part covered with drifting sands. The greater part, from the constituents of its soil, must be fertile in the extreme, and only wanting moisture to produce a wilderness of vegetation. This is proven in the case of New River, while it continues to run. This arm of the Colorado might be made permanent, but a far more convenient supply could be furnished by Artesian wells, or, better still, by windmills raising water from common wells, as is now so successfully practised throughout the fertile valley of San José.

EARTHQUAKE PHENOMENA IN SOUTHERN ITALY.

One of the most terrible earthquakes experienced in Southern Italy during the present century occurred in the kingdom of Naples on the night of the 16th of December, 1857, a season of the year which, by a comparison of all the known dates of earthquakes, has been ascertained to be more subject to disturbances than any other. The sky was clear, the air still; indeed, unusual stillness had prevailed the whole of that day. A sharp undulatory shock of twenty seconds' duration, immediately preceded, and, accompanied by a hollow, rumbling noise, had scarcely awakened the inhabitants, who had generally retired to rest, when, after a hardly perceptible pause of about three minutes, a second and most violent successive shock of twenty-five seconds' duration crushed thousands of them under the ruins of their falling houses. The seat of this earthquake was in the central group of mountains in the provinces of Basilicata and Principato Citra, part of the main chain of the Apennines, which are the watershed between the streams flowing into the Tyrrhenian, the Ionian, and the Adriatic Sea, and form the upper basins of the Calore or Tanagro, the Sele, the Ofanto, the Bradano, the Basento, the Sinno, and the Agri rivers. The centre of action, as far as it can be judged from the intensity of its terrific effects, was almost in the heart of the province of Basilicata, in a group of compact limestone mountains of the cretaceous period, the southern branch of the said central group, which, running from north to south between the heads of the valleys of the Sinno and the Agri on the east, and the valley of Diano on the west, swells further south into the lofty peaks of Monte Cocuzzo, Monte del Papa, and Monte Pollino, on the frontiers of Calabria. On the declivities or lower peaks of this group, which are covered with beds of tertiary marine marl, sands and conglomerate, and within a district extending over an area of about 216 square miles, stand, or rather stood, the towns and villages of Montemurro, Saponara, Viggiano, Tramutola, Marsico Vetere, Marsico Nuovo, Spinosa, and Sarconi, with an aggregate population of 35,570. Out of this number more than 12,000, or more than one-third, in less than half a minute were crushed to death; 2,000 severely wounded! The ground was cracked and convulsed in the strangest manner; chasms and deep fissures were opened in several places, fertile hills became bare rocks, valleys were raised up, small pools formed, mountains cleft by deep ravines. The towns of Montemurro and Saponara, especially, were nearly entirely swept away; the former lost 5,600 out of 7,000, and the latter 3,000 out of 4,000 inhabitants. Saponara, which rose in the Middle Ages out of the ancient Grumentum, where Hannibal sustained a slight defeat by the Consul Claudius Nero, was almost entirely levelled with the ground; there remain only a few shattered houses standing. Of Montemurro, originally a Saracenic settlement of the tenth century, literally nothing was left but a heap of rubbish. On the morning of the 17th of December, 5,600 of its inhabitants were dead or dying under the ruins, 685 disabled by wounds; the few remaining unhurt found themselves torn from their dearest ones, houseless, amidst a mass of ruins, without means of subsistence or help, and exposed to all the inclemency of a severe winter on a high peak of the Apennines! A few days later the stench of the dead human beings under the ruins made life unbearable to the few surviving ones. Both at Montemurro and Saponara, most of the houses standing on beds of conglomerate had been overturned, or shuffled in the strangest manner, and the ruins deposited in the ravines beneath; the contents of the lower stories were, in several instances, thrown up into the stories above, or scattered in different directions, as if propelled by a central force. The scenes of misery and horror that took place in those doomed towns exceed what imagination can fancy. Viggiano came next, a town whose inhabitants from time immemorial have been in the habit of wandering with their harps over different parts of the world, and returning home with their savings in summer. It lost 1,700 out of 6,634 inhabitants, and had most of the houses and churches overthrown. At this place an extensive fire added to the horrors of the night. From the centre of a triangle formed by these three towns, on which the fury of the convulsion was more violently wreaked, the distances, in a direct line, are — to the Gulf of Policastro, 24 miles; to Pæstum, on the Gulf of Salerno, 58 miles; to the mouth of the Agri, on the Gulf of Tarentum, 47 miles; to the extinct volcano of Mount Vulture, 55 miles; to Mount Vesuvius, 94 miles; to Bari, on the Adriatic, 80 miles; and to Mount Etna, 195 miles. Beyond this district, the terrific effects of the earthquake extended, though somewhat diminished in intensity, over an area of more than 3,000 square miles, destroying or injuring, more or less, about 200 towns and villages, with an aggregate population of more than 200,000 inhabitants, of whom no less than 10,000 were killed. The whole number of persons destroyed by this earthquake in a few seconds has been carefully estimated as 22,000; and it also appears, from reliable data, that in the course of 75 years, from 1783 to 1857, the kingdom of Naples has lost by earthquake agencies at least 110,000 inhabitants, or more than 1,500 per year, out of an average population of 6,000,000.

CURIOUS PHENOMENON ACCOMPANYING EARTHQUAKE MANIFESTATIONS.

Dr. C. Forbes of Chili, S. A., in a letter to the London Geological Society, states, that for some time previous to the occurrence of a severe earthquake-shock, on or about the 30th August, 1857, the Bay of Payta swarmed with crabs, of a kind not generally observed, and ten days after the earthquake they were thrown up on the beach, in a raised wall-like line, three to four feet wide, and to the height of about three feet, along the whole extent of the bay, and above high-water mark. At the same time as the upheaval of the crabs took place, the water of the bay became changed, from a clear blue, to a dirty blackish-green color, much resembling that off the Island of Chiloe, Concepcion, and the southern parts of Chili. Ten days afterwards, Dr. C. Forbes found that living specimens of the crabs were still numerous in the bay; but all appeared to be sickly, and numbers came ashore to die. There were no appearances of any alteration of the relative position of sea and land in the vicinity, nor had any ebullition of gases been observed; although probably, to both these causes combined, the phenomenon described was due.

SUBMARINE EARTHQUAKE.

A submarine earthquake was observed on 25th of November, 1857. William Cook, master of the British schooner *Estremadura* of Glasgow, thought he perceived a squall abaft the beam, but this turned out to be a warm mist, or steam. Where the mist was, the sea boiled or surged up from the bottom. It was in those seas that the island of Salerina appeared in 1811.

ON CERTAIN CONNECTING POINTS BETWEEN LUNAR AND TERRESTRIAL VOLCANOES.

The following paper was recently read before the Royal Astronomical Society, by Prof. C. Piazzi Smith:

In a recent publication of our Society the upper parts of Teneriffe were described as a most moon-like region. The expression is not a little descriptive; and why? Because, at those elevations the air is thin and transparent; no cloud floats therein during a large part of the year; vegetation is reduced to a minimum; sharp jagged rocks rear their naked forms around, brilliantly, and even blindingly illumined by the intense rays of an undimmed sun on one side, while they throw on the other shades as remarkable for their darkness; and, finally, all those rocks, plains, and slopes, are thoroughly and purely volcanic. Every astronomer will at once understand and allow the connection, but would yet do unwisely were he to overlook the opinions of several eminent geologists, who affirm that the telescopic features made out on the lunar surface are not volcanic at all. However positively this view is maintained in conversation, I have not yet been fortunate enough to meet in print with anything that could, with due respect to geologists of standing, be considered a full exposition of their reasons. Rather, then, than attempt to discuss opinions evidently weak and confessedly imperfect, and without entering into the extensive subject of volcanoes generally — though for their natural development and proper action, *pur et simple*, the moon might perhaps be shown to be a fitter region than the earth — I will at present merely bring forward some few facts, equally acceptable, I trust, as facts to either party, and calculated to supply, in some measure, a short link in that large gap which intervenes between the methods of observation hitherto employed on lunar or terrestrial volcanoes, real or imaginary. An immense difference of this sort must always prevail; for whereas actual eruptions, and chemical analysis of the materials erupted, are the most powerful of proofs for earthly craters, we can never hope to employ them in the moon. There are naught but extinct volcanoes, so distant from us, too, that rare indeed it is to find a man who can completely realize what he sees of figure and surface with the telescope, so as to form in his mind as rational an idea of them as he does of an earthly mountain that he has actually walked over. To approximate these respective methods of research, and in that way eliminate their peculiar sources of error, we may evidently, with much advantage, leave the active volcanoes of the earth (say Vesuvius) where the fire and fiery smoke, and devastations of the last or present eruption, force themselves too prominently on the eyes and nerves of all beholders, and turn rather to some extinct specimen, where we may contemplate the traces of successive eruptions during myriads of years, and the last of them as impassively as the first; always provided that such instance be not overlaid by any geological changes consequent on our dense and watery atmosphere, and that its features can be viewed from such heights and distances as in the naked eye to subtend something like the same angles that those of the moon do in the telescope. Had we to search over the whole breadth of the earth for such an example, we could hardly find a better one than the colossal Peak of Teneriffe, of which, through the liberality of the Admiralty, I am enabled now to lay some particulars before the Society. First I would request attention to the fine model, on the scale of $\frac{1}{30000}$th, kindly prepared for the occasion by my friend, Mr. James Nasmyth, who, while employing upon it all that artistic skill for which he is so well known to all present, has yet conformed most faithfully and conscientiously to all the measurement, particulars of length, breadth, and height, that I could furnish him with. The space thus represented is about sixteen miles square, and embraces part of the northern coastline of the Island of Teneriffe, together with the Peak, the great crater, and all the most elevated parts of the interior. The coloring is according to nature; the green near the sea-level representing the vegetation, abundant at and below the summer-cloud level, or four thousand feet; above that line the hues of the lava rock predominate; the oldest, light and bright yellow, are the most extensive; the latest, black, are chiefly confined to the upper part of the Peak, and to some special crater mouths in the other parts; while the intermediate are red or brown. By throwing a strong ray of side light on the model,* the variations of form may be brought out prominently; and amongst them the huge size of the "elevation crater" is most remarkable, being somewhat more than eight miles in diameter. It is on the floor of this crater that has been formed the central cone, known as the Peak of Teneriffe. From the brink of the southern wall of the great crater, eight thousand nine hundred feet above the sea, and two thousand feet above the crater-floor, and again, from a station on the flanks of the central cone, at an elevation of ten thousand seven hundred feet, excellent birds-eye views were obtained, during the two months we spent there, over features of the volcanic landscape; which then, seen in the thin transparent air above the clouds, and under a vehement solar illumination, assumed very much indeed of a lunar look. In comparing these features with lunar volcanoes, the first remark may be, that our great crater, eight miles in diameter, is still nothing to compare with many in the moon, some fifty or sixty miles in diameter, Are those great lunar circles, then, therefore, not craters? To this end we may answer: — 1st. That the frequency with which small craters break through the walls of large ones in the moon, and never large through small, indicates that the earlier volcanoes there were, on the whole, always the larger; and this practical result is quite agreeable to the theory of volcanic action, which ascribes its leading features to the remains of heat due to the mode of planetary formation. 2d. That, so far as Teneriffe is concerned, the large volcano was the earlier, as, too, it should be, according to the theory just mentioned, which is equally applicable to the earth as to the moon. The distance, however, which we can go back in the volcanic history of the earth, is as nothing compared to its actual age, or compared to what we can in the moon, for this very simple and patent fact, the presence of an ocean on the earth, combined with secular variations of land and sea surface. These variations, which are still going on, have been in force for such immense periods of time, that geolo-

* This was done at the lecture by means of a Drummond lime-ball light, which was afterwards employed in magnifying and exhibiting, by optical pictures, a series of thirty-six photographs of volcanic features, at from 7,000 to 12,200 feet above the sea-level. Mr. Nasmyth had also very kindly allowed six of his large and unequalled drawings of the moon's surface to be suspended in the room, for the purpose of contrast and comparison.

gists have found no part of the earth whatever, save new volcanoes recently thrown up, which has not been more than once beneath the ocean, so long and at such a depth as to have sedimentary strata thrown down upon it to a depth of many thousands of feet. There is no part of the world, not even the giant chain of the Andes, which seems to have escaped this submersion and precipitation. What, then, can have been the fate of the earlier and more powerful volcanoes of our globe, than in their turn to have sunk under the sea, and had all their irregularities first rasped and removed by the destructive action of ages of surf and waves gradually creeping over them, and then having them covered under such depth of strata of hard rock, that, when once more lifted up into the atmosphere, no quarrying by man can ever expose their full proportions. When we go back from Chajorra or Rambleta, at present unextinct, and some three quarters of a mile in diameter, to the great crater of Teneriffe, eight miles in diameter, and extinct during the human period, or equally from Vesuvius, at present active, and a quarter of a mile in diameter, to Somma, two miles in diameter, and perfectly quiet ever since Italy has been dry land, — we find the older craters to have been the larger; and if they are not very large as compared with those of the moon, it is because their age is, after all, quite in the modern times of geology, and as shown by the shells found in the lower slopes of either volcano to belong to the post-pliocene period. The grand volcanic circles, then, of the older "primary" and "secondary" days can never be seen by man; but would he form some idea of their mighty proportions, when the crust of the earth was thin, its whole interior fluid with heat, and its more volatile materials going off in oceans of vapor, agitating the whole globe, and reäcting against the weak exterior with terrific violence, let him look to the surface — our surface — of the moon, never yet depressed under an ocean, and there, as in a glass held up to us for our instruction, may be seen what must have been the throes endured by the earth, and what the size of volcanic mouths in its early history of fiery ordeal. Many further differences may be found, on close observation, between lunar and terrestrial volcanoes, as dependent on the infinitesimal atmosphere around the former, and the smaller force of gravity acting on them. To assist in investigating the nature of such modifications, we have, happily, in Teneriffe, specimens of volcanoes which, at the time of their activity, were some of them submarine and some subaërial; and when we look to the smooth slopes of the former, some 12°, increasing to 28° in the latter, with extreme roughness of surface, we can hardly but allow that this is a strong approach to the still steeper and more jagged forms in the moon. In short, with its rare atmosphere (22 mercurial inches), so dry that the rocks do not disintegrate, vegetation does not spread, and a slow change of color is all that marks the lapse of successive centuries, the little volcanic world of the great crater of Teneriffe, raised high above the clouds, is a most important region to be studied and mapped with reference to lunar investigations. To map this region correctly would be a work of years; and all that I have done is to point out the character of the phenomena visible from two points of the circle, viz., the stations established on Guajara and Alta Vista. The terrestrial part of the problem is thus still only begun, and the greater part remains still to do; while the astronomical portion, or the telescopic part, will have more and more work prepared for it, as often as any characteristics of form are made out by theory or earthly analogy as necessary to volcanic action. Amongst them may be already placed the dynamic waves and wrinkles of lava streams; and though no telescope has yet seen them, nor perhaps can expect to do so, unless mounted on such a peak as Teneriffe, high above the clouds and the tremors of the atmosphere, yet the security which the discovery of such a fact would give to investigations into the moon's physical history, might render the attempt well worthy of attention.

GEOLOGY OF CRYSTALS.

An interesting paper on this subject was recently read before the Geological Society of London, by H. C. Sorby, F. R. S., in which his experience in the microscopical examination of crystals was given. In some of these, dry cells are found; in others there are water cavities. Crystals having cavities with water, he concludes, were formed from aqueous solutions; crystals containing dry cavities were formed from matter in a state of igneous fusion; crystals containing both water and dry cavities were formed under great pressure, by the combined influence of highly heated water and melted rock. The amount of water in some of these cavities may be employed to deduce the temperature at which the crystals were formed; those containing few cavities were formed more slowly than those containing many.

Applying these general principles to the study of natural crystalline rocks, minerals, etc., it appears that the fluid cavities in rock-salt, in some calcareous spar, in limestone, and in some gypsum, indicate that these minerals were formed by deposition from solution in water, at a moderate temperature, and the same conclusions apply to other minerals in veins in various rocks. The constituent minerals of mica-schist and the associated rocks contain many fluid cavities, which indicate that they were metamorphosed by the action of heated water, and not by mere dry heat and partial fusion, as the plutonist geologists have taught.

The structure of minerals in erupted lava proves that they were deposited from a mass in a state of igneous fusion, like the crystals in the slags of furnaces; but in some blocks projected from volcanoes there are water as well as dry cavities, which indicate that they were formed under pressure at a dull red heat, when both water and liquid rock were present. Quartz in veins has a structure proving that it has been rapidly deposited from a solution in water, and sometimes at a high heat (about 329° Fah.), and when the temperature was greater, mica, tinstone, and even felspar were deposited. Solid granite, far from contact with stratified rocks, sometimes contains fluid cavities; this is especially the case with coarse-grained quartzoze granites, in which the water constitutes two per cent. of the volume of the quartz, and the cavities are so numerous and minute as to number thousands in a cubic inch; felspar and quartz in this granite contain dry cavities, thus showing that these minerals were formed with water under fusion of high temperatures. The conclusion arrived at from this is, that granite is not a simple igneous rock, formed as geologists have generally taught, when the earth was a mass of fire, and when no water could be found resting upon its surface.

ON SOME PECULIARITIES IN THE ARRANGEMENT OF MINERALS IN IGNEOUS ROCKS.

In a paper on the above subject, read before the British Association, 1858, by Mr. H. C. Sorby, he stated, "that very often, in igneous rocks, infusible minerals had been formed upon such as were far more fusible; which was a very unintelligible peculiarity, if they supposed that the temperature at which they crystallized was the same as their own fusing-point, when heated alone. This fact, however, as well as several important peculiarities in the microscopical structure of the minerals, may be readily explained by supposing that the fused rock is simply a liquid that melts at a high temperature, capable of dissolving various minerals, in the same manner as salts are dissolved in the very fusible substance, water. On cooling to a certain extent, the crystals are deposited from solution, and thus crystallize out at a temperature which must be somewhat lower than the fusing-point of the mineral when heated alone, and may be much lower than that. This supposition completely explains why a fusible mineral may act as a nucleus for one that is much less fusible; and it was shown that this peculiarity may be imitated artificially, for when saline aqueous solutions are cooled so as to solidify, crystals of very infusible salts are actually deposited on previously formed crystals of ice."

GIGANTIC CRYSTALS OF BERYL.

At a late meeting of the Boston Society of Natural History, Mr. Francis Alger spoke of the great *Beryl formation* in the town of Grafton, New Hampshire, describing its crystals of gigantic dimensions which had been discovered there. One of these crystals, which he had caused to be removed and conveyed to Boston, weighed nearly two and one-fourth tons, and was five feet in length. Another, the largest single crystal in the world, as far as is known, is nine feet in length, being a six-sided prism, the several faces of which measure respectively in width through the greatest diameter of the crystal, two feet eight inches, two feet, one foot eleven inches, one foot ten inches, one foot six inches, and one foot nine inches — thus giving it a circumference of twelve feet. This crystal yet remains at the locality, but the quartz and feldspar surrounding it have been carefully removed by chisels, so that its position in its native bed can be readily observed. Three weeks labor of two men was expended in this process, as ordinary blasting by gunpowder would have destroyed the crystal. Its weight is probably not less than five tons.

THE CASPIAN SEA.

A new map of the Caspian Sea, has recently been published in St. Petersburg, by Lieutenant Jwatschintson, a naval officer in the Russian service, who was employed by the government to take soundings and examine the shores of this important lake. According to his measurement, the Caspian Sea covers an area of three hundred and fifty-two thousand square versts. Its greatest length is six hundred and fifty geographical miles, and the extreme breadth three hundred miles.

ARTESIAN WELL AT NIONDORF, GERMANY.

This well has reached a depth of 2247 Parisian feet, having passed through 54·11 metres of lias, 206·02 keuper, 142·17 muschelkalk, 311·45 variegated sandstone, 16·21 older slates and grauwacke. The temperature at bottom is 27·63° C., equivalent to an increase of 1° C., in depth for every 31-04 metres. *Walferdin, in Compt. Rend.* xxxvi. 250.

TIN ORE FROM AUSTRALIA.

At a recent meeting of the Boston Society of Natural History, Dr. A. A. Hayes exhibited a specimen of octohedral tin ore from the gold washings of Owen's river, on the way from Melbourne to Sydney, Australia. The ore is accompanied by titaniferous and chromiferous iron ores, garnets, and yellow quartz. In this connection, he stated that he had examined the black sands of many of the gold washings of California, in which, besides garnets and topazes, cinnabar is generally found, without detecting tin ore. Some of the titaniferous iron crystals yield the slight traces of oxide of tin, often found in the ore, but no crystals of pure oxide of tin have been found. Although, in general, a resemblance exists between the sands of Australia and those of California, the heavy ores found are not the same in both.

ON THE REMAINS OF THE GIGANTIC ELK, CERVUS EURYCEROS.

M. de Morlot, in the Journal of the Geological Society, London, announces the discovery of the remains of the gigantic elk, in association with works of human industry, during the draining of a small lake in the Canton of Berne, Switzerland. In the bed of the lake, in connection with fragments of pottery, stone-chisels, arrow-heads, cut-wood, etc., were found many fragments of the bones both of domesticated and of wild animals, viz., horned cattle, horses, swine, dogs of various size, goats, sheep, cats, elks, stags, aurochs, bears, wild boars, foxes, beavers, tortoises, several birds, and other animals still undetermined. An atlas and jaw, however, sent by M. Trogon to Prof. Pictet, of Geneva, were ascertained by this eminent palæontologist to belong to *Cervus euryceros*. The length of the atlas, is 0·265 metre, and its breadth 0·088 metre; both differing only by $\frac{1}{1000}$ from the measurements stated by Cuvier.

BOTANY.

STATISTICS OF VEGETATION.

Prof. Henfrey, F. R. S., in a recent work on Botany, furnishes the following curious and interesting statistics of vegetation:

"Theophrastus (390 B. C.) enumerated 500 kinds of plants, and Pliny (A. D. 79), in his 'Historia Naturalis,' increased the number to double. The researches of the Greek, Roman, and Arab naturalists made known no more than 1400 species, and even in the beginning of the seventeenth century the discrimination of the different kinds had only raised the number of distinguished forms to 6000.

Humboldt, at the commencement of the present century spoke of 44,000 plants, Phanerogamous and Cryptogamous.

"De Candolle ('Essai Elêmentaire de Gêographie Botanique,' 1820) next calculated that the writings of botanists and the various European collections of dried specimens, might be assumed to contain, together, upwards of 56,000 species of plants. In 1820, however, the number of species in the herbarium of the Jardin des Plantes was estimated at the same number, and the collection of M. Benjamin Delessert of Paris was supposed to contain at the time of his death, in 1847, as many as 86,000 species, a number which, about ten years previously, had been conjectured by Lindley to represent the whole of the species existing on the globe ('Introduction to Botany,' second edition, 1835). The Royal Herbarium at Schönberg, near Berlin, is estimated by Dr. Klotsh to contain 74,000 distinct species.

"Humboldt ('Aspects of Nature') has entered into some interesting calculations to prove how far all these figures fall short of the number of species of plants which may be supposed to exist. The number of species of flowering plants named in Loudon's 'Hortus Britannicus' (1832), as at that time, or within a moderate period before, cultivated in Britian, was 26,660; the catalogue of species actually under cultivation in the Berlin Garden, carefully prepared by Kunth, gave rather more than 14,060 species, 375 of which were Ferns, leaving 13,685 flowering plants. Among these the following important Orders were represented : the Compositæ by 1,600 species, the Leguminosæ by 1150, the Labiatæ by 428, the Umbelliferæ by 370, the Orchideæ by 460, the Palms by 60, and the Grasses and Cyperaceæ by 600 species.

"When these numbers are compared with those of the species of their Orders described in recent works, we find that this Garden contains only 1–7th of the Compositæ (about 10,000, De Candolle and Walpers), 1–8th of the Leguminosæ (8068), and 1–9th of the Grasses (Grasses 3544, Cyperaceæ 2000, Kunth), and of the smaller Orders of Labiatæ (2190) and Umbelliferæ (1620), about 1–5th or 1–4th.

"Supposing all the flowering plants cultivated at one time in all the botanic gardens of Europe to amount to 20,000, and assuming from the foregoing comparisons that the cultivated species amount to about the eighth of those described and preserved in collections, the latter would amount to 160,000 species. Large as this number is, it will scarcely be thought excessive, when we recollect how small a proportion of many large Orders are to be found in our gardens, scarcely 1–100th part, for example, of the Guttiferæ, Malpighiaceæ, Melastomaceæ, Myrtaceæ, and Rubiaceæ.

"If we apply this mode of calculation to the number of species given by Loudon (26,660), the estimate of 160,000 rises to 213,000 species."

These deductions, based upon Kunth's inferences, refer to the species that have been described, and are now existing in herbaria. It remains to estimate the whole number of species upon the globe, judging by the proportion under the cultivation of art and the examination of science. The following statement exhibits the principle on which the calculations are based:

"Walpers' 'Repertorium,' supplementary to De Candolle's 'Prodromus,' brings the number of Leguminosæ up to 8068 species in 1846. The proportion of the number of the Leguminosæ to that of the entire Phanerogamous flora is 1–10th within the tropics, 1–18th in the temperate, and 1–35 in the north frigid zone; so that we may assume the mean proportion of this family to be 1–21th. The 8068 described Leguminosæ would therefore lead us to suppose that there existed only 169,400 species of flowering plants upon the surface of the globe, whereas the Compositæ, as stated above, indicate, by Kunth's mode of deduction, more than 160,000 already known species.

"Of the Compositæ, Linneus was acquainted with only 785 species, while 10,000 are now known. The greater part of these appear to belong to the Old World, De Candolle describing only 3590 American, with 5093 for Europe, Asia, and Africa. But this seeming abundance of the Compositæ is to a certain extent deceptive and only apparent. The proportions of this Order are 1–15th between the tropics, 1–7th in the temperate, and 1–13th in the frigid zones, giving a mean of 1–12th, which shows that even more species of Compositæ than of Leguminosæ have escaped investigation hitherto, since a multiplication by 12 would give us the improbably low number of 120,000 Phanerogamia.

"The Grasses and Cyperaceæ give still lower results, as comparatively fewer still of these have been collected and described. The mean proportion of the Grasses seems to be about 1–12th. Taking the number of known species of plants according to the above calculations at 160,000 or 213,000, the Grasses ought to amount to 13,333 in the first case, and 17,750 in the second, while only either 1–4th or 1–5th of these numbers is known. When we reflect what enormous extent of plain still remains unexplored in almost all parts of South America, and in Northern and Central Asia, this deficiency does not appear extraordinary; and, indeed, it becomes by no means difficult to believe that we are so deficient of knowledge of species of Grasses, that the total number of flowering plants might be taken at double the number known, which would lead to the conclusion that only 1–8th or 1–10th of the Grasses had as yet been discriminated."

ON THE GROWTH OF PLANTS.

At the Dublin meeting of the British Association, Prof. Buckman, from the committee appointed to experiment "On the effect of external conditions on the growth of plants," reported, that he believed he had successfully proved that many species of plants regarded as species by botanists, were only varieties or hybrid forms. Thus, he had produced *Avena sativa* from *Avena fatua*, *Symphytum officinale* from *Symphytum asperrimum*, and many others. He had not succeeded in producing wheat from any species of Ægilops.

NEW ENGLAND MYCOLOGY.

For the last four years Mr. Charles J. Sprague, a member of the Boston Society of Natural History, has been engaged in collecting and describing the various species of fungi belonging to New England. The number of species thus far enumerated and catalogued in the publications of the Boston Society of Natural History is 678, of which a large number are entirely new to science.

ON THE PROTECTION OF PLANTS FROM THE FROST.

M. Boussingault has devoted a long article in the *Annales de Chimie et de Physique* to the preservation of plants from frost, by filling the air with smoke. This is not recommended on nights when the thermometer at a distance above the soil indicates a temperature below 32°, for it would then have no effect, nor on windy nights, for then there is no frost; but it may possibly be found of service in protecting fruit-trees and delicate plants from the late frosts of spring, by which their blossoms are so often destroyed.

ON THE VITALITY OF SEEDS.

It has long been a disputed question among botanists, whether the uniformity existing in the vegetation of different islands and continents having no other communication with each other but a wide expanse of ocean, is owing to a special creation in each instance, or to an interchange of seeds transported from one shore to another by the waters of the sea. M. Ch. Martens, professor at Montpellier, in a letter to M. Flourens, recently communicated to the Academy of Sciences, gives an account of certain experiments he has instituted for the purpose of ascertaining — First, whether many kinds of seeds are specifically lighter than sea-water, so as to swim on the surface; and, secondly, whether, after having undergone the action of sea-water for a certain length of time, they are still in a condition to germinate. With regard to the first question, M. Martens has found that out of a certain number of different kinds of fresh seeds, chiefly of a large size, taken at random, two-thirds will swim on the waters of the Mediterranean, the density of which is 1·0258. To ascertain the second question, M. Martens caused a large box of sheet-iron to be made, divided into one hundred compartments. Ninety-eight of these compartments received a certain number of seeds of different kinds, and the apparatus thus prepared was fastened to a buoy. A large number of minute holes pierced in the side of the box allowed the water free ingress and egress, without any danger of the seeds being washed away. After a lapse of six weeks, the box was taken out of the sea and opened, when, out of the ninety-eight kinds of seeds, forty-one were found completely rotten. The remaining fifty-seven kinds were immediately sown in pots filled with earth taken from a heath. Of these, thirty-five kinds only germinated, including seventeen of those which are specifically heavier than sea-water, and could not therefore be transported to any distance ; so that, out of ninety-eight species, eighteen only might germinate after a six weeks' voyage, under the most favorable circumstances. Repeating the experiment with the thirty-five kinds which had resisted the action of sea-water for this space of time, M. Martens left them for three months exposed to its action, and then found eleven in a rotten state; of the other twenty-three, only nine germinated, two of which were specifically heavier than sea-water; so that, after a three months' sojourn in the sea, a period most likely to be the usual one, seven kinds only out of ninety-eight might have some chance of germinating. The Ricinus communis and Cucurbita pepo are among the number. Now, if all the dangers be taken into consideration to which a seed must be exposed during a long voyage, as well as the difficulties it must meet with to find a congenial soil on landing, with other circumstances calculated to promote its germination and subsequent preservation from destruction, M. Martens concludes, with M. Alph. De Candolle, that the transportation of seeds by sea must have had a very small share in the propagation of plants to other shores, and that the hypothesis of simultaneous creations in different parts of the world acquires much probability.

ON THE BOTANY OF THE SORGHUM VULGARE

Mr. C. J. Sprague, in a recent communication to the Boston Society of Natural History, stated that a suite of specimens of the *Sorgho sucré*, *Imphee Dourrha*, and *Broom Corn*, had been placed in his hands for examination by Mr. Olcutt, of Westchester County, New York, with a request that the following points of interest might be examined: — Whether these plants are, or are not, of the same species? whether they will hybridize? and whether they are likely to lose their peculiarities by careless planting and management? Some varieties possess more of the saccharine secretion than others. Is this excess a specific peculiarity, or the result of varied cultivation of the same species in different localities? Will these peculiarities continue fixed, or will the varieties lose their distinctiveness when grown in company with one another?

The specimens consist of portions of the panicles of eighteen varieties of Zulu Kaffir *Imphee*, grown in South Carolina, from seeds ripened in France, and received from Mr. Wray. These specimens were gathered in a field, where they grew promiscuously, by Mr. Olcutt himself, in company with Mr. Wray, who identified the varieties, and furnished the Kaffir names. There are four specimens of *Dourrha*, the seeds of which were received from France in the same package with the *Imphee*, and planted in the same field. Also, four specimens of *Dourrha*, *Broom Corn*, and their hybrids with *Sorgho sucré*, grown by Mr. Olcutt in Westchester. I have added to these four specimens of *Imphee*, grown in the District of Columbia, that the suite of specimens may be yet more full. My remarks upon these specimens will be confined to the fruit alone, as I have not seen the growing plants, and cannot, therefore, speak of the differences which may exist in their foliage and port.

Steudel, in his synopsis of the grasses, enumerates the following allied species of *Andropogon* growing in Asia and Africa: — *A. Sorghum*, Auct.; *A. rubens*, Willd.; *A. subglabrescens*, Steud.; *A. Saccharatus*, L. (sub. Holcus); *A. verticilliflorus*, Steud.; *A. niger*, Kunth; *A. cernuus*, Roxb.; *A. bicolor*, Roxb.; and he implies that most of these may be varieties of the *Andropogon*

Sorghum. Besides these, is *A. Drummondii*, Nees, from New Orleans. These so-called species are, in all probability, founded on permanent varieties of the grass which has been grown for its grain and foliage for centuries in the East Indies and Africa. It was placed first in the genus *Holcus* by Linnæus, but has been separated from it and ranked in that of *Andropogon.* It is still kept there by some of the best botanists of the day; but by others it is placed in that of *Sorghum,* a genus separated from *Andropogon* mainly from its paniculate inflorescence and coriaceous glumes. The species named for Drummond, by Nees, is probably a form of the same plant which had established itself at New Orleans. An authentic specimen in Dr. Gray's herbarium does not appreciably differ from some of the varieties grown in South Carolina.

The thirty-one specimens laid before you are thought to represent four species, and many varieties. The seeds came from France, but the plants furnishing them originally came from widely separated localities. The differences which they exhibit are in the color, shape, and hairiness of the glumes; the color, shape, and prominence of the corn beyond the glumes; and the open or compact growth of the panicle. If these differences were constantly exhibited together, — if the difference of shape were always attended by a difference in color, and that color always accompanied by the same hairiness and exsertion of corn, — there would be strong ground to establish specific differences. But such is not the case. The specimens, placed side by side, exhibit a complete gradation between the extremes of the series. Those which vary most in shape are similar in color. Those which differ in color are identical in shape. The hairiness and the degree of exsertion are coëxistent with the extremes of shape and color. There are four which are especially interesting. Mr. Olcutt grew Broom Corn and Dourrha in rows on each side of Sorgho sucré. The result was a plant partaking equally of the characteristics of the parents on each side. The eighteen varieties of Imphee, thought to be so distinct that different native names have been given them, exhibit every intermediate form imaginable. Some glumes are nearly white; some are specked with brown and black; some are all brown; others all black. Some have ovate pointed glumes of every hue; others have obtuse glumes, with a broad, scarious point, or rounded glumes with no point, through the same series of color. The corns are either enclosed or exserted through the whole series, irrespective of color or form. Some of the varieties of Imphee present a peculiar appearance, from the persistence and prominence of the sterile spikelets; some, differing in no other respect, have these scarce visible; and some have them not at all. Color and hairiness are among the least reliable of botanical characters, and should have but little weight in plants so closely allied; and the other differences are exhibited almost as prominently in different panicles of the same acknowledged variety.

The question of the hybridity of species of plants has lately received close and careful attention. M. Charles Naudin has recently made a series of interesting experiments on the cultivated pumpkins and squashes. He has arrived at the conclusion, that nearly all those grown in our gardens may be referred to one single species. He has particularly examined the changes which artificial impregnations will produce. We often hear that cucurbitaceous plants should not be grown together, or they will injure each other. This gives rise to the question, whether the fruit of the same season can acquire another's peculiarities without first being grown from the seed, the result of such impregnation. Such has proved to be the case. The influence of the pollen on the fruit of the same year is such as to communicate to it the characteristics of the plant furnishing the pollen. But M. Naudin finds that true species, undoubtedly distinct, can scarce be made to hybridize, and that extensive and ready hybridation takes place only among varieties of one species. Dr. Gray has shown me recently an ear of corn exhibiting a hybridation more or less common. It was sweet corn, in which kernels of hard, smooth, yellow corn were irregularly distributed, contrasting with the white, wrinkled kernels of the sweet. Here the mere impregnation of the germ of white corn by the pollen of the yellow had been sufficient to convert those grains which it touched into perfect yellow corn.

The sports and varieties of corn have a strong bearing upon the question of the specific identity of these varieties of Sorghum. Though some botanists have made species out of the varieties of Indian corn, it is generally believed that these are all the results of cultivation on one species. One peculiarity of one form claims attention here. The plant has been found growing, apparently wild, with the grain entirely covered by the glumes, which project far beyond it. But it is said that, after a little cultivation, these glumes disappear, or become so abbreviated as to allow the grain to be entirely uncovered, as in our garden growths. This same difference is to be seen in the varieties of Sorghum under consideration. The Dourrha most exhibits this abbreviation of glume and prominence of grain, and this variety is that which is known to have been longest under cultivation.

The question, then, arises, whether plants would so freely hybridize and exchange peculiarities, were they of different species. Does not this hybridity point to identity? We do not see other grasses, which grow broadcast in our fields, hybridizing naturally, and so perfectly as to become diversified in an inextricable series of graduated forms. The Poas, Panicums, and Festucas, which abound in our fields and meadows, do not interchange their specific peculiarities, but grow side by side and maintain their identity. But the Sorgho is no sooner placed side by side with Broom Corn and Dourrha, than the three hybridize, and produce an offspring combining the peculiarities of all.

The *Sorghum vulgare* has been cultivated for untold centuries as a forage plant, and as food for animals and man. The question of its production of syrup and sugar is by no means a recent one. Experiments were made upon it more than half a century ago in Europe, and one of its names arose from the saccharine secretion of its culm. Its native country is unknown; but it is supposed to originate in the same places where it has been so long cultivated. Its grains have been found in Egyptian sarcophagi; and these are said to have produced plants identical with the modern Dourrha or Juari. After this long cultivation in all kinds of soil and climate, and under such varied treatment, it would be strange indeed if it did not exhibit a wide departure from its normal type. If the Indian corn has become so astonishingly changed in a shorter period of time, we may well understand that the Sorghum should wander into all the varieties upon which botanists have sought to found distinct species.

I am induced to believe, therefore, that *Broom Corn*, *Sorgho sucré*, *Imphee*, and *Dourrha*, are varieties of one primitive species, the *Andropogon Sorghum* of authors, or, allowing the genus Sorgum to stand, SORGHUM VULGARE.

The establishment of this fact will answer many of the questions which have been asked regarding its economic value. If they be one species, they will of course hybridize and exchange whatever properties they possess. The saccharine secretions of one variety will be diminished by hybridation with another not possessed of an equal amount. And the saccharine qualities peculiar to one may be lost by planting in a soil or climate differing from that which has brought them forth in unusual quantity. If their cultivation as a forage plant, and a syrup or sugar-producing plant, shall prove profitable, the use of the grain in the form of flour, as well as food for cattle and poultry, may considerably diminish the cost of cultivation.

ON THE DURATION OF THE LIFE OF PLANTS.

The following paper was recently read before the Botanical Society of Edinburgh, by Prof. Fleming:

The phrases ordinarily employed to express the duration of life in plants are annuals, biennials, and perennials. Viewing the subject, however, in reference to function rather than seasons, divisions much more consistent with the phenomena must be resorted to. Thus, in the case of annuals, it may happen, in an unfavorable season, that the plant may outlive the winter, flourish during a portion of the following season, and thus become a biennial. But in many cases those plants termed biennials merely extend themselves during the first season, and in the following flower, ripen their seeds, and perish. But in both cases the plant dies after having once executed the function of reproduction. Those plants have their vitality completely exhausted by the seed-producing process, and, in consequence of this functional character, they constitute a very distinct group, to which the somewhat ambiguous term Monocarpous has been applied by De Candolle and Lindley. It suggests the idea of the plant producing only one carpel or seed-vessel. As defined by Lindley, however, it may be conveniently employed. He says: "Monocarpous, bearing fruit but once, and dying after fructification, as wheat. Some live but one year, and are called annuals; the term of the existence of others is prolonged to two years,— these are biennials; others live for many years before they flower, but die immediately afterwards, as the *Agave americana.*"

In proof that it is the production of the seed which consumes the vitality of the plant, it will be found that by destroying the flower-buds the life of the plant will be prolonged until new flower-buds be produced, or those already existing but in an imperfect state become developed. Thus, I have kept the common oat, *Avena sativa*, for four seasons by cutting off the flowering stem. The annual bean may be easily converted into a biennial. The tree mallow, *Lavatera arboræ*, usually considered a biennial, in one case outlived the greater part of the second winter with me, but perished by the severity of the frost in the spring of 1855, having a stem displaying spurious annual rings of growth, about eighteen in number, marking intermittent action, irrespective of the dead or winter season, and well calculated to give a salutary warning to the vegetable palæontologist. The circumstance of monocarpous plants having their life prolonged by being prevented from flowering, and the production of new parts for flowering purposes, give no countenance to the assertion of Knight, in his paper "On the Reproduction of Buds:" "Nature appears to have denied to annual and biennial plants (at least to those which have been the subject of my experiments) the power which it has given to perennial plants to reproduce their buds." This character of the individual plant being capable of reproduction only *once*, was well known to Ray, who states that such plants may live even five years. The second physiological group, to which I shall now direct your attention, has this property in common with the preceding, that the stem, after flowering and ripening the seed, perishes, together with the root by which it is nourished. In this respect it may be termed an annual; and as examples, may be quoted the tulip, onion, monkshood, and very many of the plants termed herbaceous. These differ, however, from the ordinary annuals, or once-flowering plants, by the production, at the base of the stem of the present year, of a bulb or tuber, destined in the following spring to form its own roots, independent of the parent bulb or tuber, now exhausted and dead. This mode of secondary reproduction or extension is well illustrated by the common orchids, as the common *Orchis mascula*, where the bulb which is to give rise to the stem and flowers of next season may be observed of a paler color and firmer texture than the one in the course of being exhausted and ready to die. In the case of the two bulbs of the *Neottia spiralis*, or ladies' traces, Keith, in his "System of Physiological Botany," i. 38, states: "If a pair of these knobs is taken and separated, and then immersed in water, the one will be found to sink, and the other to swim. This is a phenomenon which seems also to have puzzled the simplists of antiquity not a little, and to have given rise to a great deal of idle and superstitious conjecture. It was thought that the knob that swims must necessarily have possessed some peculiar and potent properties, and accordingly some potent properties were liberally ascribed to it. If prepared in a particular manner, and worn about any one's person, it was believed to have the singular property of exciting, by means of proper management, a violent attachment to the wearer in the breast of any one he pleased. And this belief," he adds, "is still a vulgar error among the ignorant and superstitious." The group to which we have now referred, has been, in a great measure, overlooked by more recent botanists, although its characteristics were known to Ray, and confounded by them with the group we now proceed to consider. This third group was denominated by Linnæus Suffrutices, and thus defined, "truncis sublignosis quotannis fere supra radicem pereuntibus." (Phil. Bot. 74.) Lindley has a division of plants which he terms Polycarpous, "having the power of bearing fruit many times without perishing," and a subdivision of this group he terms Rhizocarpous, "or those whose roots endure many years, but whose stems perish annually, as herbaceous plants," (Introd. to Bot., 475.) The modifications of this group exhibit considerable variety of character. The following may readily be distinguished: 1. Where the flowering-stem and leaves perish, while the collar and root remain, for the benefit of the buds to be evolved from the former in the following spring, such as strawberry and horseradish. 2. Where the flowering-stem perishes together with the collar, but where rhizomes are produced with buds of an equally monocarpous character as the parent, as mint. 3. Where the stem, collar, and root, perish after reproduction, having given rise to a stem with its roots capable of outliving the winter, and producing flowers and fruit during the following season. The common rasp is a good example of this group. 4. Where the whole plant dies after maturing the seed, and forming from the stem a tuber, as in the potato. Here we have an aggregation of flower-buds destined to produce individuals with the annual or monocarpous character. These groups of rhizocarpous plants do not seem to have occupied, to any extent, the consideration of botanists, although, in a physiological point of view, of great interest. The field, indeed, may be regarded as in a considerable degree unoccupied by our botanical writers. The last great group, in reference to the term of life, denominated Perennial, or, in the phrase of Lindley, Caulocarpous, are those "whose stem endures many years, constantly bearing flowers and fruits, as trees and shrubs." In this group the efforts of life are of two kinds — the production of buds of extension and those of fruit. The fruit, flower, or seed buds, resemble in some degree, in their function, an annual or monocarpous plant. Death follows the reproductive process. It is otherwise with the extension buds. Both, however, are greatly under the influence of external circumstances. An abundant supply of nourishment makes a tree generate extension buds almost exclusively; whereas a scanty supply of food promotes the reproductive efforts, and fruit buds predominate, a process the reverse of that which prevails in the animal kingdom, where it has long been alleged "sine Cerere et Libero friget Venus "(Horace). By many vegetable physiologists it has been supposed that the life of a tree is confined to its buds; that the stem is a sort of dead soil, or, rather, support; and farther, that the bud, when it evolves in spring, acts like a seed, sending downwards certain vessels to act as roots, and another set upwards, for extension of the individual and the formation of new buds for development in the following season. In this view of the matter, the tree, with the exception of the buds, is an aggregation of dead cells. The authors who have adopted this notion have been chiefly influenced by considering the power which buds possess of developing themselves in certain circumstances, even when detached from the stem, as in the act of budding, and even by the more ordinary process of extension by slips. To this view of vegetable life there have ever appeared to me to be grave objections, which, to save the time, I shall state very briefly. 1. I shall not here dwell on the fact, that, by particular processes, the leaves, stem, and roots can be made to produce buds, or the parts supposed only subservient to vitality can exercise living functions from vital centres, nor on the action of poisons. 2. When a tree is grafted —say a cultivated apple on a crab stock — the buds of the graft may extend into a lofty tree, and yet its downward roots, although becoming continuous, never embracing the stock and reaching the soil. The stock remains the same in its bark, wood, and pith, and, after many years, if it produces buds and suckers, these invariably retain the characters of their crab original. The practice of dwarfing fruit trees would prove a failure if the buds contained the whole life of a tree. A slow-growing stock is selected, on which is inserted a fast-growing graft, or one inclined to generate extension rather than fruit buds. If the buds of the graft annually sent down their roots to the ground, the influence of the stock should cease by the second year, an event which does not occur. 3. The difference between summer and winter felled wood is equally hostile to the notion that the life of a tree is limited in winter to its buds. The cells of the newer layers of wood are storehouses of nourishment: the sap, when beginning its ascent, is nearly pure water; as it ascends it becomes more and more loaded with the contents of the cells through which it has travelled, and the buds are thus supplied with nourishment by the living agency of the former year, which made the buds and provided for its development. Hence the comparative lightness of timber felled after the bud has evolved its leaves. The stem of a tree is the common support of all the organs, the receptacle of the peculiar juices, and the storehouse of nourishment. The buds evolve simultaneously or successively according to a law of a symmetry and coöperation, as among the composite zoöphytes, giving to the individuals of a species their characteristic expression.

NEW VARIETY OF WHEAT.

At one of the meetings of the French Academy, during the past year, M. Guerin-Mèneville produced a number of wheat-halms of more than seven feet in height, each of them bearing several splendid ears. This fine species of wheat derives its origin from five grains that were found in an Egyptian tomb, and thus had for thousands of years been preserved from all external influence. Sown out in 1849, they grew up luxuriantly, and yielded twelve-hundred-fold produce, — in consequence of which M. Drouillard made various comparative experiments in Southern and Central France, as well as in Brittany. In 1850, these experiments were made on a large scale, and assumed a more important character. Since then they have been regularly continued, and the results have been officially confirmed. One half of a field was sown with the Egyptian, the other half with our common wheat; the former gave sixty-fold, the second a fifteen-fold produce, while commonly a seven or eight-fold produce is considered a fair one. Sown out by single grains, the Egyptian wheat yielded a five-hundred-and-fifty-six-fold harvest. The experiments are now made in always increasing extension, and not less than 1,000 kilogrammes of "mummy-wheat" have been sown this year in the *arrondissement* of Morlaix.

NORTHWARD AND SOUTHWARD RANGE OF HERBACEOUS PLANTS IN THE UNITED STATES.

Of the 1745 phænogamous herbaceous plants of the Flora of the Northern United States, diminished to about 1690 by the exclusion of the alpine and subalpine species, here left out of view —

843 species, or 50 per cent., range southward to the borders of the Gulf of Mexico.

538, or not quite 32 per cent., extend northward into the Saskatchawan basin, or to Labrador.

107 of these reach or cross the Arctic circle.

24 species, or less than 1½ per cent., range from the Gulf of Mexico to the Arctic circle.

180, or 10½ per cent., range from the Gulf of Mexico to the Saskatchawan, or Labrador.

248 species, or over 14½ per cent., range from the Gulf of Mexico to the Great Lakes, or the St. Lawrence. — *Prof. Asa Gray.*

THE WILD INDIGO PLANT OF THE SOUTHERN STATES.

Mr. Niesler states that the common wild indigo plant of the Southern United States (*Indigo fera Caroliniana*) is commonly used by the country people in Georgia, in place of *I. tinctoria*, and it yields an indigo which, to all appearance, is equal to the commercial article. "Just when it is beginning to bloom, the old wives collect from the woods as much of the plant as they can procure; they steep it in water some twenty-four hours, until it assumes a greenish tinge, when the liquid is drawn off and *churned* until it assumes its proper blue color; it is then *curdled* by the addition of a small quantity of lye from wood ashes, and allowed to settle; the sediment is then collected, put into coarse bags, and drained dry, and it is then used in the same way as the ordinary commercial article. Those who have used it most insist that the color is better and more permanent than that of the exotic indigo, and that the same quantity of the plant will yield much more than *I. tinctoria*. They are in the habit of cutting it three or four times in the season from the same root. Sometimes they gather the seed and sow it in convenient places, where it will flourish and yield a supply for years. It seems likely that this wild indigo might be made a profitable crop in the South; with this great advantage, that it would be produced on lands now waste and useless for any other purpose. — *Prof. Asa Gray, Silliman's Journal, quoted.*

ON THE ORIGIN AND DISTRIBUTION OF SPECIES IN PLANTS.

Dr. Hooker, of England, in his recently published work on the "Botany of the Antarctic Voyage," in discussing the relations and distribution of species in plants, lays down the following propositions as axioms:

"1. That all the individuals of a species have preceeded from one parent (or pair), and that they retain their distinctive (specific) characters. 2. That species vary more than is generally admitted to be the case. 3. That they are also much more widely distributed than is usually supposed. 4. That their distribution has been effected by natural causes; but that these are not necessarily the same as those to which they are now exposed."

"Hybridization has been supposed by many to be an important element in confusing and making species. Nature, however, seems effectually to have guarded against its extensive operation and its effects in a natural state, and as a general rule the genera most easily hybridized in gardens are not those in which the species present the greatest difficulties. With regard to the facility with which hybrids are produced, the prevalent ideas on the subject are extremely erroneous. Gärtner, the most recent and careful experimenter, who appears to have pursued his enquiries in a truly philosophical spirit, says that 10,000 experiments upon 700 species produced only 250 true hybrids. It would have been most interesting had he added how many of these produced seeds, and how many of the latter were fertile, and for how many generations they were propagated. The most satisfactory proof we can adduce of hybridization being powerless as an agent in producing species (however much it may combine them), are the facts that no hybrid has ever afforded a character foreign to that of its parents, and that hybrids are generally constitutionally weak, and almost invariably barren. Unisexual trees must offer many facilities for the natural production of hybrids, which, nevertheless, have never been proved to occur, nor are such trees more variable than hermaphrodite ones."

ON THE TRANSMUTATION OF WHEAT INTO CHESS.

It is a popular and widely-extended belief, that the purest and best wheat may be planted — that it may germinate, grow, and form a plant — but that the occurrence of certain casualties — as sudden freezing and thawing while the ground is wet — will, by some mysterious process, transmute the plant into a widely different species, viz., chess. A similar belief prevails, less extensively, in regard to the change of barley into oats.

The advocates of the transmutation of wheat into chess have been repeatedly called on to demonstrate the alleged change, and as an inducement for them to do this, *premiums* have actually been offered.

A late revival of the transmutation controversy induced Benj. Hodge, Esq., of Buffalo, New York., to offer a premium of one hundred dollars to any one who should prove that wheat had turned to chess, — the premium to be awarded under the supervision of a committee appointed by the New York State Agricultural Society. The premium has been claimed by Samuel Davidson, of Greece, Monroe County, New York. The society appointed a committee of investigation, Prof. Dewey, of Rochester, chairman, the result of whose examination is thus detailed in the New York *Country Gentleman:*

"The experiment to prove transmutation was the following: A quantity of earth was passed through a fine sieve, to separate all chess seeds. It was placed in a pan, and several heads of wheat planted in it. When the wheat came up, it was subjected to all the hard treatment that usually produces winter-killing, viz., flooding with water, and alternately freezing and thawing for several times. Late in the spring, the whole contents of the pan were removed and set out in open ground. When the plants of wheat threw out their heads, there appeared chess heads also. This mass of wheat and chess plants was brought in and placed before the committee. Stalks of chess were shown, the roots of which were found to proceed directly from the planted heads of wheat, which yet remained entire, and in some instances they were found to issue from the half-decayed grains of wheat themselves. This was looked upon as conclusive.

"The roots were taken by the committee and first soaked in water, and afterwards gently washed, by moving them backwards and forwards slowly through it. They were then carefully examined by microscopes. The roots of the chess were now perceived to issue, not from near the end of the grain of wheat, as is usual in sprouting, but from the *side*, and, in fact, from almost any part. Further examination showed that they merely passed *through* crevices in the decayed wheat grains, and they were separated from the grains without tearing, being merely in contact, without adhesion or connection. Some of the more minute chess fibres were observed by an achromatic microscope to extend over the inner surface of the bran, where they had gone in search of the nourishment (which is known to abound just within the bran), in the same way that grape roots have been observed to spread over the surface of a rich decaying bone. But they easily separated, and had no connection with the grain. It was satisfactorily proved that the chess plant could not have come from these grains, by the fact that the same single stalk of chess was thus connected with five or six different grains, which could no more have originated it than five or six cows could have one calf. The examination, therefore, did not prove anything in favor of transmutation; and as there were many possible ways in which the chess might have become scattered on the soil, the whole experiment was admitted by all parties to be inconclusive."

ZOOLOGY.

ON THE CONSTRUCTION OF THEORIES IN PHYSIOLOGY.

THE facility with which theories are extemporized by many who have little or no knowledge of the nervous structure, is only surpassed by the facility and confidence with which men attribute phenomena to electricity. It may be well, therefore, to state that our knowledge of the nervous system is at present in its infancy; we have not even established a secure basis; we have not established the primary *data.* To quote the emphatic language of one who has given his life to the subject, "Our knowledge even of the coarser framework of the nervous system is still too much in its infancy to permit us to venture, with any success, on the construction of theories respecting the functions of its various elements." — STILLING, *Ueber den Bau der Nerven-primitivfaser.*

ON THE SO-CALLED CHOLERA CORPUSCLES, OR FUNGI.

Dr. Lander Lindsay, of England, in a recent publication, makes the following remarks on the fungus origin of cholera:

"The isolated or disintegrated individual cells of the tissues probably include many, if not most, of the 'annular bodies,' 'cholera corpuscles,' or 'fungi,' which so startled the histological and medical world during the cholera epidemic of 1848–9. At least the ultimate elements of these tissues or substances, as observed by myself, correspond in their character to those published as delineative of the bodies in question by their original discoverers. I believe that potatoes, oatmeal, bread, and the vegetables of common broth, will furnish most of the forms of the once famed 'annular bodies;' that they are not, therefore, fungoid in their nature or origin; and that they have no essential or causative relation to cholera. I have found them equally in other diseases — as in the stools of diarrhœa and dysentery. * * It will be evident, then, that I can see no satisfactory groundwork for the fungus-theory of cholera, which, I am not a little surprised to find, still possesses powerful advocates."

CURIOUS INSECT DEPREDATIONS.

At a recent sitting of the Academy of Sciences at Paris, Marshal Valliant drew attention to the fact that a number of balls in the cartridges brought from the Crimea had been pierced partly or entirely through by an insect, belonging apparently to the tribe *Hymenoptera.* As the piercings are evidently not made for the purpose of shelter, and as no fragments from them could be found, the inference is that the insect eats the lead — and yet French entomologists assert that such a thing is not likely. The insect being entirely unknown to French *savans*, the Marshal announced that he had, in the name of the Academy, writtten to the Russian Embassy, to ask if Russian ball-cartridges in the Crimea had ever been noticed to have been pierced by insects; and, if so, if Russian entomologists can give any details respecting the insect and its way of living. The Marshal adds, that it seems not to have any similitude with the *Cetonia aurata*, which pierces through lead, but casts aside the lead it cuts away. M. Dumeril, in the course of some observations on the subject, said that examples existed of balls having been pierced through by insects at Toulon, and that the insect which had attacked those from the Crimea was undoubtedly a *Urocera.*

NOTES OF EXPERIMENTS ON DIGESTION.

Dr. Harley, in a communication to the British Association, Leeds meetings, stated that, contrary to an opinion lately published by Bernard, the distinguished French physiologist, he had found that the human saliva contains both sulphocyanide of potassium and iron. The latter substance, however, can only be detected after the organic matters contained in the secretion are destroyed by burning. Dr. Harley had ascertained that a person of nine stone secreted between one and two pounds of saliva in twenty-four hours. The gastric juice, the author said, does not destroy the power possessed by the saliva of transforming starch into sugar; consequently, the digestion of amylaceous food is continued in the stomach. The gastric juice has the property of changing cane into grape sugar. The author made some remarks upon the cause of the gastric juice not digesting the living stomach; and said that his experiments showed that it is not the epithelium lining the organ which prevents its being digested, but the layer of thick mucus which covers its walls. When the latter substance is absent, the gastric juice attacks the walls of the living stomach, and digests them, causing perforation and death. As regards the bile, it seems that this secretion takes an active part in rendering the fatty matters of our food capable of being absorbed into the system. The most curious of all the digestive fluids, however, is the pancreatic secretion, for it unites in itself the properties of all the others. It not only transforms starch and other such substances into sugar, but it emulsions fat, and even digests protein compounds. As a remedy in indigestion, pancreatine should be greatly superior to pepsine, which can only digest one kind of food, namely, protein. The author said he had been laboring to obtain pancreatine in a perfectly pure state, and had been to a certain extent successful. With pancreatine we should be able to digest any kind of food we pleased; and, therefore, the obtaining of it in a state of purity would prove an invaluable boon to suffering humanity.

THE SALIVARY GLANDS.

The saliva appears to possess *three* most important properties; firstly, it destroys vitality in all animal and vegetable matter; secondly, it loosens the tissues, thereby preparing them to receive the saliva itself, and ultimately to admit the gastric juice; and thirdly, it mechanically softens and dilutes hard or dry food. When a cow fills her paunch with grass, she places there a large amount of living vegetable material; lying in that organ, or transferring it to the second stomach, no way affects its vitality; but when thrown back into the mouth, and it comes in contact with the saliva, then it instantly dies, and becomes materially altered in appearance. Examine the contents of the first three stomachs of a cow, or a sheep; in the two first the food is evidently living grass, but in the third it has the appearance of a thoroughly well-boiled vegetable — more nearly allied in color and appearance to spinach, and, as yet, it has only come in contact with the saliva, which must be held responsible for its changed condition. Arrest a caterpillar in the act of eating a leaf of a cabbage; kill it instantly, open its crop, and examine the leaf you saw it consume but a minute before; it will have lost its bright green color, and be reduced, in every respect, to the appearance of the grass in the third stomach of the cow. As it cannot have come in contact with any other material than the salivary secretion, it is surely justifiable to attribute its altered appearance to the action of that fluid. When man eats raw, ripe fruits, he eats living vegetables, and if he put them into his stomach in that state, there they will remain, for no stomach has the power to destroy the vitality of anything, as, if it had, assuredly it would destroy and digest itself, a contingency that always happens in death. Nothing is more common, at post-mortem examinations, than to find that a portion of the stomach has actually thus acted upon itself.

To show the universality of this particular chemical property of destroying life, let us see what takes place among the lower animals. Bulk for bulk, weight for weight, can anything exceed the pain of a mosquito bite, to say nothing of the long continued after consequences?

What gives rise to this extreme suffering? Surely it cannot be the insertion of its tubular sheath and tiny jaws, because if the flesh were stabbed at the same time with a dozen large stocking needles, the pain would not be nearly so great, and the wound would sooner heal. When a spider bites a fly, why does the insect die instantly, and its body swell up prodigiously? If a rattlesnake, or other, so-called, *poisonous serpent* bite a man, why is the wound almost universally fatal? If a dog, not rabid, bite a man, or if a cow, horse, hog, raccoon, fox (and many other animals), do the same thing, or if *one man bite another*, why, in any or all these circumstances, should the bitten person be liable to hydrophobia? To these questions, which might be greatly extended, there is but one answer, namely, that the person bitten has been in every instance inoculated with the saliva of the other animal, and that one of its chief properties is *to destroy life*. To them and to us it is a natural secretion, and so harmless is it, under some circumstances, that a man may drink any quantity of the poison (saliva) *of a rattlesnake*, and it will have none other effect than to help him to digest his food! But if inoculated into the circulation of the blood, it becomes a virulent, a fatal poison. Who can doubt that, if a mosquito were as large as a good sized dog, its saliva would be as immediately and certainly fatal as the bite of a rattlesnake? The pain that we share with domestic and other animals, from the bite of parasitic insects, is solely due to this cause — inoculation by their saliva. The division of the salivary glands among the reptiles would appear to throw some light on the function of each, or certainly some of them; thus: the *poisonous* reptiles possess only *parotid glands*, the secretion of which descends by the channels of the fangs of the upper jaw; the use they make of them would seem to establish the function and properties of these particular glands. The boa constrictor (*Python Tigris*) has no parotid glands, neither can he destroy his prey by a bite, but he entwines his body around his victim, and kills him as a bear would, by an embrace. But what is now to be done? he has no grinding teeth to enable him to insalivate the food and loosen the tissues, by partially decomposing the body of the goat he has killed, and so prepare it for the action of the stomach; in other words, how can he perform the important function of insalivating it?

He does it in this way: *he licks it all over*, and wherever the tongue, covered with saliva, touches it, the flesh becomes almost rotten under its influence. Now, as it is well known that persons have been bitten by a rabid dog and escaped hydrophobia, while other persons have been bitten by sound and healthy dogs and yet this fearful disease has supervened, how is this to be explained unless we admit the differing chemical property of the salivary glands respectively?

If the teachings of the rattlesnake and the boa constrictor have any practical value, it would appear that the parotid glands *alone possess the power of destroying life*, and that the secretion of the other glands can only be employed upon already dead matter, to effect its speedy decomposition. If this theory be true, it is very easy to explain the bites and their consequences of the two dogs; in the case of the *rabid dog*, whose bite proved innocent, the saliva of inoculation may have come only *from the submaxillary* and *sublingual glands*, and consequently it was harmless; whereas, in the case of the *sound dog*, the saliva came from the *parotid glands*, and was therefore fatal. This view is sustained by the following considerations: The ducts of the parotid glands are situated, as we have seen, in immediate proximity to the molar teeth, and the secretion is only evolved by their action; the probability is that the incisor teeth, used in biting, and the interior of the mouth, are usually lubricated by the secretion of one or both of the other pairs of glands, while the parotid glands are reserved for mastication alone.—*Goadby's Animal and Vegetable Physiology.*

ON THE FEEDING AND GROWTH OF THE AMERICAN ROBIN.

At a recent meeting of the Boston Society of Natural History, a communication was read from Prof. Treadwell, of Cambridge, giving a detailed account of the feeding and growth of the American robin (*Turdus migratorius*, Linn.), during a period of thirty-two days, commencing from the 5th of June.

When caught, the two birds experimented on were quite young, their tail-feathers being less than an inch long, and the weight of each about twenty-five pennyweights, less than half the weight of the full-grown bird; both were plump and vigorous, and had evidently been very recently turned out of the nest. He began feeding them with earth-worms, giving three to each bird that night; the second day he gave them ten worms each, which they ate ravenously; thinking this beyond what their parents could naturally supply them with, he limited them to this allowance. On the third day, he gave them eight worms each in the forenoon; but in the afternoon he found one becoming feeble, and it soon lost its strength, refused food, and died. On opening it, he found the crop, gizzard, and intestines entirely empty, and concluded, therefore, that it had died from want of sufficient food — the effect of hunger being perhaps increased by cold, as the thermometer was about 60°. The other bird, still vigorous, he put in a warmer place, and increased its food, giving it the third day fifteen worms, on the fourth day twenty-four, on the fifth twenty-five, on the sixth thirty, and on the seventh thirty-one worms. They seemed insufficient, and the bird appeared to be losing plumpness and weight; he began then to weigh both the bird and its food, and the results were given in a tabular form. On the fifteenth day he tried a small quantity of raw meat, and, finding it readily eaten, increased it gradually to the exclusion of worms; with it the bird ate a large quantity of earth and gravel, and drank freely after eating. By experiment it appears that though the food was increased to forty worms, weighing twenty dwt., on the eleventh day, the weight rather fell off; and it was not until the fourteenth day, when he ate sixty-eight worms, or thirty-four dwt., that he began to increase — on this day the weight of the bird was twenty-four dwt.; he therefore eat forty-one per cent. more than his own weight in twelve hours, weighing after it twenty-nine dwt., or fifteen per cent. less than the food he had eaten in that time; the length of these worms, if laid end to end, would be about fourteen feet, or ten times the length of the intestines. To meet the objection that the earth-worm contains but a small amount of solid nutritious matter, on the twenty-seventh day he was fed exclusively on clear beef, in quantity twenty-three dwt.; at night the bird weighed fifty-two dwt., but little more than twice the amount of flesh consumed during the day, not taking into account the water and earth swallowed. This presents a wonderful contrast with the amount of food required by the cold-blooded vertebrates, fishes and reptiles, many of which can live for months without food; and also with that required by mammalia — a man, at this rate, should eat about seventy pounds of flesh a day, and drink five or six gallons of water. The question immediately presents itself, how can this immense amount of food, required by the young birds, be supplied by the parents? Suppose a pair of old robins with the usual number of four young ones — these would require, according to the consumption of this bird, two hundred and fifty worms, or their equivalent in insects or other food daily — suppose the parents to work ten hours, or six hundred minutes, to procure this supply; this would be a worm in every two and four-tenths minutes; or each parent must procure a worm or its equivalent in less than five minutes during ten hours, in addition to the food required for its own support. He was unable to reconcile this calculation with actual observation of robins, which he had never seen return to their nests oftener than once in ten minutes. After the thirty-second day the bird had attained its full size, and was entrusted to the care of another person during his own absence of eighteen days; at the end of that period the bird was strong and healthy, with no increase of weight, though its feathers had grown longer and smoother. Its food had been weighed daily, and averaged fifteen dwt. of meat, two or three earth-worms, and a small quantity of bread each day; the whole being equal to eighteen dwt. of beef, or thirty-six dwt. of earth-worms; and it has continued to eat this amount to the present time. The bird having continued, in its confinement, with certainly much less exercise than in the wild state, to eat one-third of its weight of clear flesh daily, he concludes that the food it consumed when young was not much more than must always be provided by the parents of wild birds. The food was never passed undigested; the excretions were made up of gravel and dirt, and a small quantity of white semi-solid urine.

He thought that every admirer of trees may derive from these facts a lesson, showing the immense power of birds to destroy the insects by which our trees, especially our apples, elms, and lindens, are every few years stripped of their foliage, and often many of them killed. The food of the robin, while with us, consists principally of earth-worms, various insects, their larvæ and eggs, and a few cherries; of worms and cherries they can procure but few, and those during but a short period, and they are obliged therefore to subsist principally upon the great destroyers of leaves, canker-worms, and some other kinds of caterpillars and bugs. If each robin, old and young, requires for its support an amonnt of these equal to the weight consumed by his bird, it is easy to see what a prodigious havoc a few hundreds of them must make upon the insects of an orchard or a park. Is it not, then, to our advantage, he asks, to purchase the service of the robins at the price of a few cherries? There has lately been some improvement in preserving our birds, and with a little more protection, he thinks that such an increase of them might be obtained as would save us from all the labor required for the appliances of tar, oil, zinc plates, and all other methods by which we seek, with very imperfect success, to destroy our mischievous insects.

Dr. C. T. Jackson observed, that it was the opinion of Mr. Townend Glover, now engaged by the U. S. Patent Office in studying the insects injurious to cotton and other American crops, that among the most inveterate foes to noxious insects are insectivorous insects themselves.

ON THE CHANGE OF COLOR IN BIRDS AND ANIMALS.

At a late meeting of the Boston Society of Natural History, Dr. D. F. Weinland called attention to a question now discussed in the European journals of Ornithology, viz., The cause of the change of color in the feathers of birds, and in the hairs of mammalia, and the manner in which this change is effected.

It is a well-known fact that many birds, particularly the males, have a very differently colored plumage in different seasons; for instance, that the male of many singing birds has a far more beautiful plumage in the reproductive season than during the rest of the year: furthermore, that many northern birds and mammalia become pure white in winter, while they are yellow, red, brown, gray, or of a still darker color in summer.

Till within the last few years, this change of color was supposed to be effected simply by the production of a new feather or hair; but there are on record several instances which are entirely at variance with this supposition; and Dr. Weinland was of the opinion, that, although this change is generally produced by molting, many instances are proved, by past and recent observations, in which it has taken place without loss of the feather.

Human Pathology has shown many cases, in which the hair of men, from sudden terror or from grief, has turned gray or white in so short a time (sometimes in one night) that there was no possibility of a change of the hair itself. A case is known in Ornithology, in which a starling in one day became white all over, after being rescued from the claws of a cat.

These facts, however, seemed to be exceptions only, till quite recently some distinguished ornithologists — Schlegel in Leyden, and Martin in Berlin — at the same time affirmed *that many birds get their wedding plumage without molting.*

Experiments were made by many ornithologists; some affirmed the new statement, others denied it. But the most striking observation which had come to the knowledge of Dr. Weinland, was made by a friend of his, Mr. Junghaus, of Berlin, on a blue-throated warbler (*Sylvia suecica*), which he had in a cage. From June, 1854, till the middle of February, 1855, the throat of this bird, from the bill down to the breast, was pure white, over the breast ran three bands, blue, black, and yellow, the black one being the narrowest. In the middle of February, the blue band became darker, and spots of the same color appeared all over the white throat, with the exception of a small spot in the centre. On the 21st of February, all the throat was blue except that spot, which remained pure white till the 23d, when it became reddish. On and after the 24th, this reddish color also changed to blue; but on the 1st of March there appeared again, in the midst of this blue, a lighter spot of beautiful silvery appearance; and it is worth remarking, that this new color began at the basis of the feather, and proceeded outwards. Meanwhile, the black band on the breast had become larger, and shaded insensibly into the blue, while the yellow band remained unchanged through all these mutations. Thus the bird had got its wedding plumage, *without losing one feather*, and this it kept through all the reproductive season. At the same time, Dr. Gloger, of Berlin, showed that a very similar observation had been previously made in this country by Audubon, on a male gull, which changed the color of its head, in a fortnight, from gray to the purest black, and, as he supposed, without changing a feather.

There can be no longer any doubt about the fact; but the question is, how can a feather change its color, when its blood-vessels and nerves are dried and dead, as is the case with every feather soon after it has reached its full growth. Dr. Weinland had only heard of one explanation, viz., that the wearing away of the fine laminæ of the veins of the feather, the so-called pinnulæ, might produce the change of color. This seemed to him not only an unphysiological view of the subject, that a bird should get its wedding plumage by such a kind of decay of the feather, but, in the cases which he had observed, the changed feathers showed no traces of such a wearing process. The following explanation of the fact seemed to him the most natural:

Conservators of museums very frequently notice that certain birds in the collections bleach, particularly when exposed to light. A red-breasted Merganser (*Mergus merganser*) which Dr. Weinland saw, when just shot, with a red breast, and which, after having been deposited in the museum for some time, presented a pale whitish breast, showed this very remarkably. He afterward obtained a bird of the same kind, and, when fresh, examined its breast-feathers with a high power of the microscope, and found all the pinnulæ filled in spots with *lacunes* of a reddish fluid, which, from the dark appearance of their margins, seemed to be of an oily character. Some weeks afterwards the same feathers, having been exposed to the light, had become nearly white, and he found in the pinnulæ, instead of the reddish *lacunes*, only air-bubbles, which it is known produce a white color, as in the case of the lily, which is rendered white by the air in its cells. This observation led him to the conclusion, that in this case the evaporation of the reddish fluid, and the filling of the spaces with air, produce the change of color. If this fluid is an oily matter, as there is reason to suppose, it will be readily admitted, physiologically, that it may be furnished by the organism, by imbibition through the tissues, in consequence of a certain disposition of the nerves leading to the skin and to the sac of the feather in the skin, (even if the vessels and the nerve in the feather itself should be dried), for fat goes through all tissues without resistance, and also through horn. Thus the fat coloring matter may flow out into the feathers during the time of reproduction, which is the richest season in every living organism; and then again, from want of food, cold temperature, weakness, decrepitude, or from strong emotions of the central nervous system, from sudden terror or grief, — the same coloring fat may be called back to furnish the suffering organism.

This process, effected by different physiological conditions of the organism, seems to be a reasonable explanation of the fact, that many northern mammalia and birds become white in winter, while they are dark-colored in summer; that the hair of men or mammalia, or the feathers of birds, may become suddenly gray or white from sudden terror, hard labor, or debility, while they are dark-colored in mature life or in the more vigorous seasons of the organism. And if we add the hypothesis, that in the oily fluid there may take place still other chemical processes effected by different conditions of the nervous system, such as oxidation, or deoxidation, we may explain in this way still other changes of color; for instance, from yellow, through red to black; which, from observations made during the last winter, seems to be really in certain turtles (*Emys picta, and marginata*).

THE LAW OF TYPES IN THE ANIMAL CREATION.

Dr. George Ogilvie, in a recent (English) work, "The Master-Builder's Plan," thus sketches the law of typical formation in Zoölogy.

"In each division of animals, we can point out a very definite type, according to which the several species are constructed — a type, the essentials of which are never violated, even when it seems in a manner incompatible with the habits of particular animals — the necessary conformity being obtained in such cases, not by a departure from the type, but by a comparatively slight modification of some parts of the organization, and that in a way quite consistent with its general character. Obviously the organic creation is constructed upon a great systematic plan: it is not to be compared to an overgrown village, in which the houses — commodious and well constructed as they may be, each in itself — are scattered about without any order, every man having built as was good in his own eyes; it answers rather to our notion of a well-planned town, with the houses in regular streets, in each of which a certain uniformity prevails, while the streets themselves are arranged in that particular order which to the founder of the city seemed the most appropriate."

And in this view it will not be claiming too high a position for the conclusions arrived at, to contend, with the author, that,

"Late as may be its discovery, the law of typical conformation will not yield in importance, as a fundamental principle in Zoölogy, to that of the circulation of the blood in Physiology, or that of the revolution of the planets round the sun in astronomical science, for it gives the character of an inductive science to one which was previously only descriptive; and it admits of being applied to the elucidation of phenomena before — beyond all others — incapable of explanation: those of the production of monstrous forms."

ON THE FORMATION OF THE CELLS OF BEES.

The following is an abstract of a paper on the above subject, presented to the British Association, 1858, by Mr. W. B. Tegetmeir:

Having recently been engaged in making a series of experiments with a view to determine the typical form of the cells of bees, and having arrived at some interesting results, I am desirous of bringing them before the Association. My first experiment consisted in placing a flat parallel-sided block of wax in a hive containing a recent swarm. In this, cells were excavated by the bees, at irregular distances. In every case where the excavation was isolated, it was *hemispherical*, and the wax excavated was added at the margin so as to constitute a *cylindrical* cell. As other excavations were made in contact with those previously formed, the cells became flat-sided, but, from the irregularity of their arrangement, not necessarily hexagonal. When the block was colored with vermilion, the employment of the excavated wax in the formation of the sides of the cells was rendered more evident. The experiment has been repeated, with various modifications as to the size and form of the block of wax, but always with the same results, — namely, that the excavations were in all cases hemispherical, — that the wax excavated was always used to raise the walls of the cells, — and that the cells themselves, before others were formed in contact with them, were always cylindrical. Mr. Charles Darwin, to whom I communicated these facts, has repeated the experiments with similar results. When these experiments are taken into consideration, in connection with the facts, that, in the commencement of a comb, the rudiments of the first formed cells are always hemispherical, and that in a small extending comb the outer sides of the bases of the external cells are always circular, they appear to lead to the conclusion, that the typical form of a single cell is cylindrical, with a hemispherical base; but that, when the cells are raised up in contact with one another, they necessarily become polygonal, and if regularly built, hexagonal. On this supposition alone can those numerous cases be accounted for in which one half of a cell is cylindrical, the other polygonal. In all such cases it will be found that, in the cell adjacent to the cylindrical side, there is not room (owing to some irregularity of the comb) for a bee to work, —consequently, the cylindrical development is not interfered with. The formation of the small cylindrical cells surrounding the queen-cell appear to admit of no other explanation. The mode in which the circular bases, situated at the thin edge of a comb in the process of enlargement, become converted into polygonal cells as new bases are formed on their outer sides, has been beautifully shown by Mr. Darwin. In repeating, with many ingenious modifications, my original experiments, he colored, with vermilion and wax, the circular edges of the bases of the external cells in a small comb. On replacing this in the hive, he found that the walls of the cells were not raised directly upon these circular bases, but that, as other cells were built external to them, the colored wax was remasticated and worked up into the polygonal sides of the cells, — consequently, the color, instead of remaining as a narrow line, became diffused over a considerable portion of the sides of the cells. These observations have been much facilitated by the employment of a hive having each side formed of four parallel plates of glass, with thin strata of air between. As thus formed, the escape of heat is so effectually prevented that the bees work without the necessity of covering the hive with any opaque material, and thus they are always open to observation without being disturbed by the sudden admission of light into a hive previously dark. Crude and imperfect as these experiments may be, they appear to me to have an important bearing on the theory of the formation of cells, and my desire that they may be repeated and extended by other observers must plead my excuse for bringing them before the notice of the Association.

Dr. Whewell communicated some observations from Mr. Ellis, "On the Cause of the Instinctive Tendency of Bees to form Hexagonal Cells." He supposed that the bee was led to the exercise of this instinct by the use of their organs of sight. It was well known that, in addition to their facetted eyes, they had three single eyes; and he supposed that these eyes were placed in such a position as to enable them to work within such a range as to give the walls of their cells an angle of 120 degrees.

Mr. J. Lubbock gave an account of the experiments by Mr. Darwin, in which he had found that bees made circular cells in the circumference of their combs, but that these were always worked again into an hexagonal form when another row was placed beyond them. That the material of the circular cell was removed for this purpose, he had ascertained by painting the outside of the external row of cells with carmine, indigo, and other substances, which were invariably worked up into the next row of cells. In answer to Mr. Ellis's theory of the eyes, he could state from observation that bees, in ninety-nine cases out of a hundred, worked in the dark. Wasps made hexagonal cells from the beginning. He believed the tendency of bees to make hexagonal cells was acquired, and that originally bees made circular cells, but from a deficiency of material had at last acquired the habit of making hexagonal cells.

Mr. Bayldon stated that he kept a large number of bees, and that he had seen them make hexagonal cells at first. The outer cells alone were circular. Dr. Lankester said it was an interesting physiological question as to whether the eye or some other organ was the first recipient of the impression which induced the movements that resulted in the bees' work. An impression must be made on some organ of the animal, as all the actions of the lower animals were excito-motory, and probably the antennæ were the organs acted on. Dr. Edwards suggested that the materials with which the bee worked were sufficiently receptive of light to act upon their organs of vision, and thus the eye might be still the excitor of the instinctive actions.

PSYCHOLOGICAL VIEWS OF THE MOTIONS OF ANIMALS.

The following paper, on the above subject, has been published in the proceedings of the Boston Society of Natural History, by Dr. David Weinland: There is hardly any part of the science of natural history which has been so little studied as the psychology of animals. The ability to descend to the level of the mental constitution (ψῡχὴ) of animals, to understand their feelings, thoughts, and desires, seems to have diminished in proportion to the progress of civilization; or at least, in proportion as cultivated minds of civilized nations have secluded themselves from free nature in cities and students' closets. Still, we think the psychology of animals is by no means the least interesting subject of human thought. It is acknowledged that man is the crowning work of creation, and this has been proved and illustrated often enough by comparison of the structure of his *body* with that of other vertebrates; by showing that there exists an ideal series of development from the horizontally moving fish to the erect man. Now, may not this truth be as clearly, or more clearly traced, in following out the degrees of development of the psychical element, from the low, feeding, and propagating fish, to man as made in the image of God — that is, thinking in the same categories with him. Undoubtedly such a series of psychical development exists, but its steps have never been marked out, though many materials have been collected in regard to the subject. In the effort to attain a method of studying this part of the science of nature, the following considerations have occurred to me.

We know the condition of a man's soul, or of its representative in an animal, only by external manifestations. Thus, in order to have a standard of comparison for the different degrees of psychical development of animals, we may start from an analysis of what is called the characteristic of animals, in opposition to plants, namely, voluntary motion.

In considering closely the motions of a dog, we recognize in them two entirely different kinds. One, and that by far the most common, serves only and immediately the animal itself as the means by which to obtain whatever it desires and enjoys (food, for instance), and to shun whatever it dislikes. This kind of motions we may call subjective; that is, selfish motions; because they serve only the subject itself. But again, we see another kind of motions. Thus, the dog plays with other dogs, with other animals, and with man. It makes many movements with the head, eyes, ears, and tail, which serve no other purpose than to show to other animals, or to man, the present condition of its inner nature; to show them what it feels, what it thinks, and what it seeks. These motions are not subjective; they are made in relation to the inner natures of others, and therefore may be properly called sympathetic motions. Which of these two kinds of motions is the higher? Undoubtedly the latter. All animals have the first; the second are not common to all. Does an hermaphrodite worm, for instance, know that another being lives and feels? If not, it has no sympathetic motions.

Having considered how to view the motions of an animal, let us return to our problem, namely, to find a standard for the comparison of the different degrees in which, in the series of animals, the mental constitution is developed; and to show that the greater or less degree of development of the sympathetic motions in an animal, and of its organs to perform them, exhibits, at the same time, the degree of its psychical development. That such is the case is because no degree of this development, beyond eating and drinking, can possibly exist, except in society with, and in regard to, fellow-beings. All those animals of higher mental organization, are social animals, or, at least, are connected by certain psychical relations, with other animals. Thus, among insects, the hymenoptera rank psychically very high. The greater part of them live in communities; that is to say, each individual lives and cares not only for itself, but also for its fellow-citizens. It knows that it belongs to a certain community, has certain duties there, etc ; and whenever we admire the sagacity of a bee or an ant, it is its working and thinking in relation to other beings that we admire Moreover, only animals which are social by their nature can be domesticated, that is, made friendly to man. Man himself becomes human only when in society with fellow-men. Children lost in forests when young, growing up there, resemble beasts. The higher the civilization of men, the closer and more complicated are the relations between them Now, if this be so; if the social life is the only field where, in men or in animals, a higher growth of the spirit is possible; and if with man the social life is far more developed than with any other member of the animal kingdom, — we may draw our final conclusion, namely, that we can determine the psychical rank of any animal, from a knowledge of the degree of its ability to manifest itself to its fellow-beings, or, what is the same thing, of its organs for sympathetic motions.

An example may illustrate the truth more fully. Let us look at these organs in a fish, a lizard, and in man. The fish rests horizontally in the water; the head, neck, and trunk form one bulky mass; the dorsal column itself is the locomotatory organ; the four limbs, fins, are used for balancing the body; the ears are rudimentary; the eyes stiff, cold, without eyelids, and thus without expression, and from their position and slight mobility, of a very narrow horizon; there is no voice with which to call a companion. What means has this animal, by which to show to another being what it feels? Now, as we see in fishes hardly any organs for sympathetic motions, or senses for sympathetic perceptions, we think we are justified in saying, that there must be also in them very little sympathetic feeling or thinking. Let us rise some steps further in the series of vertebrates, to the lizard, — that quick, lively, sagacious animal. While in fishes, the greater part of the body, and all four limbs, are used in locomotion, we find here four developed legs, the body nearly exempt from the function of locomotion, and thus capable of further differentiation; and the head, neck, trunk, and tail are distinct. With the distinct neck, and consequent ability to turn the head, are immediately connected, not only a larger horizon, but also many motions which manifest whatever moves or excites the animal. Together with the larger horizon, the eyes are very well developed, and the play of the eyelids (which are wanting in fishes and even in snakes) gives expression; so much, indeed, that I have been able to tell from a glance at the eyes alone of some lizards which I once kept alive for a long time, and which were tame, whether they felt well or not. The ears, also, the organs of the real social sense, are well developed in lizards; and though the animals themselves have no voice, still they seem to like music. The tongue, which rarely exists in fishes, and when present, is a mere organ for swallowing food, has here not only become an organ of touch, but a means of expressing sympathy; for I have seen them licking each other in play. In turtles, which are higher than lizards, we find already a voice; and even the fore feet are used as organs for sympathetic motions. Prof. J. Wyman, in observing two of our common pond turtles at the breeding season, saw the male gently stroke the head of the female for some minutes.

Rising a step higher, we find in birds the voice developed to a high degree, but yet confined to a narrow range of modulated sounds. In mammalia, the organs for sympathetic motions are more developed than in birds, except, perhaps, those connected with the voice, although even this point remains to be settled. In mammalia, we find the first hints of what shall come in man. The first idea of an arm, we find in the bear, — it embraces; and this idea of an arm is connected with the ability to stand erect upon the flat of the foot. In mammalia, too, we first find the idea of a hand, hinted at already in the bear, but carried out more fully in the monkey. The features of the face we find remarkable in the dog, and still more so in the monkey. We could find a like series in the organs of reproduction, which, from this merely natural view, must be considered organs of sympathy. It is interesting to consider hermaphroditism from this stand-point: it will be evident that it cannot occur in any animal of high psychical endowments. We will, in addition, merely call attention to the fact, that fishes have no organs of copulation, or very rudimentary ones; that in many species the male does not know the mother of the eggs which it fecundates; while, on the other hand, some reptiles, many birds, and most mammalia live in pairs, or, at least, their males and females go together throughout life, helping and taking care of each other. All the family life, the only fountain of moral and intellectual beauty, rests in the distinction and voluntary union of the sexes, and this distinction and union only make possible the highest unity of two beings which exists.

We will dwell no longer on these steps, but consider man himself. If our principle of coincidence of the degree of psychical development, with the degree of the development of the organs of sympathetic motions, be true, we must find these latter in their highest condition in man. And so it is. Man, standing upright on his feet, has all his body free for sympathetic motions; and the organs by which they are performed are here in perfection. What we saw in the fish as a balancing instrument, in the lizard as a mere locomotatory organ, is in man an arm which embraces the child, the friend. With the hand, of which we saw no sign in the fish, which is a foot and a locomotatory organ in the lizard, and the same in all mammalia, even monkeys, man grasps the hand of his fellow-man, and shows him what he feels, and with it he emphasizes his language. Here are the features of the face, expressing, by the most diversified play of motions, the varying conditions of the spirit, — telling love and hate, joy and pain. Here are the eyes, the mirror of the soul. All these organs we find in a lower condition in the higher mammalia, especially in monkeys. But there is one kind of sympathetic motions, which man alone enjoys, — those employed in language, — the power to express fully his ideas, his emotions to other men, by modulated sounds, produced by the complicated motion of the larynx, the tongue, the lips, etc. Many animals, it is true, have a voice, but none of them can express a series of thoughts or feelings. The cry of an animal is always the last concluding word of a sentence. It may be the result of a series of thoughts, but this series itself is never expressed. Men have also this kind of sounds — the sounds of laughing, crying, and many others: thus the war-cry of the Indian is no language; it is an animal sound, like the cry of a wolf, when it calls others to help. But all men have, beyond these animal sounds, the free, flexible language. They not only show to each other some of the points of their thinking, and feeling, and willing; they show, or can show, all the process which goes on within; that is, their inner natures can, by means of language, communicate with each other freely. We recognize in language the highest kind of sympathetic motions.

Conclusions. Firstly, when trying to study the psychical endowments of animals, we have to start from the study of their motions, as the only manifestation of their mental constitutions (ψῦχή) which we can perceive. Secondly, there are to be distinguished in animals two kinds of voluntary motion, — the subjective and the sympathetic. The latter furnish the principal data for the study of the psychical rank; for every higher endowment flows from the sympathy of one feeling and thinking being with another. Sympathy is only a flowing forth of love, and love is the fountain of all moral and intellectual beauty in man.

ON THE MODE OF FORMATION OF SHELLS OF ANIMALS, BONE, ETC.

Dr. George Rainey, of London, the Lecturer on Microscopic Anatomy at St. Thomas's Hospital, has been recently instituting a series of experiments, with a view of producing, by artificial means, structures analogous to, or identical with, the shells of molluscs and crustaceans, and the bones of other animals. The results he claims to have arrived at are given as follows:

"Firstly, a process by which carbonate of lime can be made to assume a globular form, and the explanation of the nature of the process, 'molecular coalescence,' by which that form is produced. Secondly, the explanation of the probable cause of crystallization, and the manner in which the rectilinear form of crystals is effected. Thirdly, the discovery of a process of 'molecular disintegration' of the globules of carbonate of lime, by inverting the mechanical conditions upon which their previous globular form had depended. Fourthly, the recognition, in animal tissues, of forms of earthy matter analogous to those produced artificially. And, fifthly, the deduction from the above fact, and considerations of the dependence of the rounded forms of organized bodies on physical and not on vital agencies."

The experiments, which are minutely described in a recently published treatise by Dr. R., consist principally in the introduction into a glass phial — first, of a viscid substance, as gum arabic, saturated with carbonate of potash; secondly, of a couple of pieces of glass, to catch and fix the globules; and, lastly, of a solution of gum arabic and common water. After a certain time, the pieces of glass are removed, and are found to have become covered with clusters of globules or spherical molecules, which, on careful examination by a powerful microscope, are found to be identical in all stages of their development with those of which microscopic dissection shows that the calcareous structures met with in living creatures are ultimately composed. He compares carefully the results of these experiments with those which demonstrate the principles and process of crystalline formation, and shows that when the carbonate of lime is formed in pure water, its first form is crystalline; but when formed in the same manner in water containing a viscid substance in solution, its form is globular. The reasons for these fundamental or elementary differences in form are discussed and exemplified with much care and great acumen; but a reference to the diagrams and microscopic sketches, with which the book is illustrated, is necessary in order to follow the author through them. His final deduction, however, as regards the probable form of matter when it first came into being, is thus summed up:

"It has been shown — I think I may say demonstrated — that matter, immediately it comes into existence in some new state of combination, assumes one or other of two forms, according to the predominant force acting upon its ultimate molecules. If that force be attraction, the first forms are curvilinear; if impulsion, they are rectilinear. But I am aware that these first forms, being made up of alternate particles, are not themselves atoms, or ultimate molecules. Now, in order that the first portions of matter may have a definite form, they must either come into existence in separate places or at separate times, that is, they must not be within the sphere of each other's attraction or impulsion, for they would then be formed into globules or crystals before they had time to acquire their specific form. Now, as no experimental or natural process can be conceived by which a molecule is formed alone, this condition seems to be impossible. . . The idea of a definite form of the nascent particles of matter is unsupported by any kind of proof, and, therefore, is entirely untenable; and the only inference is, that when matter first comes into existence in some fresh state of combination, as, for instance, carbonate of lime combined with a viscid substance, it has no definite form until gravity has given it one. . . . Consequently, it may be inferred that all molecules are amorphous, and that, if there ever was a period when matter existed unacted upon by attraction or impulsion, it must have been in a chaotic or amorphous state — a something 'without form, and void.'"

THE SPINAL CORD, A SENSATIONAL AND VOLITIONAL CENTRE.

The following paper, by Mr. S. H. Lewis, was read at the Leeds Meeting of the British Association, by Prof. Owen:

The spinal cord, the author stated, was formerly believed to be nothing but a great nerve-trunk; and even now its functions have been limited to the transmission and reflection of impressions. It can conduct impressions to the sensorium and reflect them on motor nerves, producing muscular contraction; but this is all that physiologists are willing to allow. Doubts having long rested on his mind upon this point, he had made a series of experiments which had led him to a clear conviction; and that conviction and this experimental evidence, he proposed to present. Before detailing the evidence, however, for the sensorial functions of the cord, it will be necessary to fix on some broad and palpable signs, such as unequivocally indicate the presence of volition. We have such signs in spontaneity of actions and choice of actions. It will scarcely be disputed that an animal manifests volition, and its act is voluntary, when the act occurs spontaneously; I mean, prompted by some inward impulse, and not excited by an outward stimulus. Spontaneity and choice are two palpable characteristics of sensation and volition, and it is these we must seek in our experiments. Those who, for the first time, perform or witness experiments on decapitated animals, find it very difficult to believe that the animals have no sensation; but their doubts are generally settled by a reference to the admitted hypothesis of the brain being the exclusive seat of consciousness. On the strength of this hypothesis the striking facts recorded by Legallois, Prochaska, Volkmann, and others, have been explained as simple cases of the reflex actions of the cord. Against this hypothesis of the brain being the exclusive seat of consciousness, I have for some years gathered increasing strength of conviction, preferring the hypothesis of the sensorium being coëxtensive with the whole of the nervous centres; and I have been able, by experiment, to constitute three separate and entirely independent seats of consciousness in the same animal. From the mass of evidence furnished by experiments, all bearing on the same point, the sensational function of the cord acquires, in my mind, the force almost of a demonstrated truth. From that mass, a few cardinal cases may be selected. If they do not carry conviction, there can be little hope in any accumulation of such cases. Place a child of two or three years old on his back, and tickle his right cheek with a feather, he will probably first move his head aside, and then, on the tickling being continued, he will raise his right hand, push away the feather, and rub the tickled spot. So long as his right hand remains free, he will never use the left hand when the right cheek is tickled, or *vice versa*. But if you hold his right hand, he will rub with the left. The voluntary character of these actions is indisputable, in spite of their uniformity; they are prompted by sensation, and determined by volition. Let us now contrast the action of the sleeping child, under similar circumstances, and we shall find them to be precisely similar. Children sleep more soundly than adults, and seem to be more sensitive in sleep. I tickled the right nostril of a three-year old boy. He at once raised his right hand to push me away, and then rubbed the place. When I tickled his left nostril he raised the left hand. I then softly drew both arms down, and laid them close to the body, embedding the left arm in the clothes and placing on it a pillow, by gentle pressure on which I could keep the arm down without awakening him. Having done this, I tickled his left nostril. He at once began to move the imprisoned arm, but could not reach his face with it, because I held it firmly, though gently, down. He now drew his head aside, and I continued tickling, whereupon he raised the right hand, and with it rubbed the left nostril, an action he never performed when the left hand was free. The simple and ingenious experiment of Pflüger establishes one important point, namely, that the so-called reflex actions in sleep are not accompanied by sensation and volition. The sleeping child behaves precisely as the waking child behaves, except that his actions are less energetic; and we are forced to assume the presence of dim cerebral consciousness to escape the conclusion that the spinal cord is also a seat of consciousness. The actions of the sleeping and the waking child are so similar that both must be credited with sensation and volition; and if not both, then neither must be so credited. In like manner I shall show that the actions of animals, before and after decapitation, exhibit no more difference, as respects sensibility, than the actions of the waking and the sleeping child; so that here again, unless both actions are credited with sensation and volition, neither of them can put in a claim. Experiment leads decisively to this alternative, namely, either animals are unconscious machines, or decapitated animals manifest sensibility and will. [Having detailed a series of experiments with a water newt, to show that the animal's actions were precisely the same before and after decapitation, and arguing that they displayed spontaneity of action] — the paper proceeded: After allowing a quarter of an hour to elapse, in order to a more complete reinstatement of vigor, I touched the flank as before, with acetic acid. The movements at first were very disorderly. It ran about in great uneasiness, just as it had done before its head was off. In vain I waited for it to rub itself against the side of the box; it curled itself up, and seemed about to die. Some time afterwards I again touched it with the acid; it again became disorderly, and I then pushed it towards the side of the box; but it did not move until I pushed it slowly forwards, so that its flank might come in contact with the wood. This succeeded; this seemed to supply the very remedy it wanted; for it continued crawling slowly, and with intervals of rest, its body curved outwards, so as to continue in contact with the wood, and its hind leg pressed close to the tail, and thus, as before, it rubbed away the acid. There are two points noticeable here: first, the readiness with which a sensation of contact suggested a means of relief; secondly, that this was the only newt which, in my experiments, ever hit upon this plan, and this one did so as well without its head as with it. The repetition of the act precludes the idea of its being an accident. It is unnecessary to trespass on your time by citing the observations of numerous physiologists testifying to the spontaneity of decapitated animals. You will all remember such cases. I divided the cord of a newt between the fifth and sixth cervical vertebræ. The convulsions which followed were almost as severe as those which follow decapitation; but in this case it was the fore legs which were tetanic, and the hind legs pressed close to the body. After a few minutes, it tried to rise, but failed. Bubbles of carbonic acid were constantly expired. After fifteen minutes, it turned completely round, and crawled five steps forward, dragging the hinder segment after it like a log, the hinder legs not moving at all. This was repeated several times. In fifteen minutes more, sensibility was detected in the hinder segment. Here was a case which would have been pronounced very simple. Division of the cord had seemingly destroyed all power of voluntary movement in the limbs below the section. The hind legs seemed paralyzed. When the anterior segment was irritated, the animal crawled away, dragging the motionless posterior segment after it. When this posterior segment was irritated, the animal did not crawl, but simply withdrew the limb or tail. If I touched the tail or hinder leg with acetic acid, the whole of the posterior segment (in which volition was said to be destroyed) began to move, and the legs set up the crawling action, attempting to push the whole body forward, which could not be effected, because the anterior segment was perfectly motionless. The hind legs, which never moved when the anterior segment was irritated, moved now in obedience to the spinal volition; and the anterior segment, which before seemed so energetic in its voluntary movements, was now perfectly unmoved. Each centre rules its own segment. If the motionlessness of the hind legs, when the animal crawled, is a proof that voluntary power was destroyed in those legs, the motionlessness of the fore legs when the hind legs moved is equally a proof that voluntary power is destroyed in the fore legs. The real truth seems to be that each segment has its own volitional centre, and that the one is never affected by the other. I have, at this moment, a newt with the cord divided near the centre of the back. The operation was performed four days ago, and the animal has so far recovered from it that no spectator could distinguish between the voluntary power of its two segments. When the flame of a wax match is brought near the cerebral segment, the fore legs set to work, and the animal crawls away, dragging the hinder segment along. When the flame is brought near the spinal segment, the hind legs set to work, and the body moves sideways, the anterior segment remaining perfectly quiescent. All other stimuli produce similar results. I venture to submit that the explanation here proposed of two independent volitional centres is far more consistent with the phenomena than the explanation offered by the reflex theory; unless the actions of the posterior segment of the newt are evidences of sensation and volition, I know of no kind of evidence for the existence of such properties in the cerebral segment. . . . I will not occupy the attention of this meeting with the recital of other experiments. Those already cited suffice to indicate the nature of the evidence on which I found my positions. And, indeed, I might rest on one simple fact as proof that the spinal cord is a sensational centre, namely, the fact that whenever sensibility is destroyed all actions cease to be coördinated. Every one present knows how greatly our muscular sensibility aids us in the performance of actions; but it has apparently been forgotten that if sensibility be destroyed in a limb, by section of the posterior roots which supply that limb, the power of movement will be retained so long as the anterior roots are intact; but the power of coördinated movement will be altogether destroyed. With diminishing sensibility we see diminishing power of coördination, the movements become less and less orderly; and with the destruction of sensibility, the movements cease to have their coördinated harmony. Now, in the cases I have cited, it is clear that this power of coördinating movements — sometimes very complex movements — was nearly, if not quite, perfect in the decapitated animal; therefore, if coördination implies sensibility, the conclusion seems inevitable that the spinal cord is a centre of sensibility. The whole case may be summed up thus: 1st. Positive evidence proves that, in decapitated animals, the actions are truly sensorial. 2d. Positive evidence, on the other hand shows that, in human beings, with injured spines, the actions are not sensorial, but reflex. 3d. But as the whole science of physiology presupposes that between vertebrate animals there is such a general concordance, that whatever is demonstrable of the organs in one animal will be true of similar organs in another; and inasmuch as it is barely conceivable that the spinal cord of a frog, a pigeon, and a rabbit, should have a sensorial function, while that of a man has none, we must conclude that the seeming contradiction afforded by human pathology admits of reconcilement. No fact really invalidates any other fact. If the animal is such an organized machine that an external impression will produce the same actions as would have been produced by sensation and volition, we have absolutely no ground for believing in the sensibility of animals at all; and we may as well accept the bold hypothesis of Descartes, that they are mere automata. If the frog is so organized, that, when he cannot defend himself in one way, the internal mechanism will set going several other ways; if he can perform, unconsciously, all those actions which he performs consciously, it is surely superfluous to assign any consciousness at all. His organism may be called a self-adjusting mechanism, in which consciousness finds no more room than in the mechanism of a watch.

Sir B. Brodie said the paper was most valuable, but he thought the experiments might be claimed for reflex action almost as much as in favor of the theory of Mr. Lewes.

Mr. Nunneley said that the paper asserted too broadly that it had been held that sensation and volition were suspended in sleep. The person who talked in sleep and the sleep-walker both disproved the assumption of entire suspension. He had himself removed the spinal marrow of cats and rabbits, and they had lived and moved for eight hours afterwards. If they departed from the position that volition was resident in the higher masses of the spinal marrow, they must go further, and maintain that it existed in different parts of the body; and that would lead them back to the opinion of the earlier anatomists, that the nervous system was not essential to vitality.

Prof. Owen, in reply, directed attention to the comparative largeness of the human brain and the smallness of the spinal column of man, as compared with those of the animals experimented upon, and said it might be that there were some sensations felt by the lower animals which are not experienced by human beings; and if the inquiry were pursued, with this idea kept in view, they might be able to reconcile what now appeared to be conflicting.

ON THE CLASSIFICATION AND GROWTH OF CORALS.

At a recent meeting of the Boston Society of Natural History, Professor Agassiz gave an account of a recent visit to the reefs of Florida and his explorations of coralline growths. He estimated the rate of coral growth to be only a few inches in a century, a tenth or twelfth part less than was formerly supposed; and, supposing the reef rises from a depth of twelve fathoms, he would calculate its age, upon arrival at the surface of the water, to be about twenty-five thousand years, and the total age of the four distinct concentric reefs of the southern extremity of the peninsula to be one hundred thousand years. Professor Agassiz in his remarks presented evidence that Millepora is not a Polyp but a Hydroid.

Prof. Wm. B. Rogers said that the physical conditions could not have differed much in that region a hundred thousand years ago from what they now are, and consequently that such a calculation could reasonably be made upon the data accumulated by Prof. Agassiz.

Dr. Weinland called attention to a fact recently observed by him in Hayti, which seems to involve a more rapid growth of some kinds of corals than is generally assumed for this class of animals. In a small coral basin, between the town of Corail and the island of Caymites, which is never disturbed by vessels, the water being there much too shallow, he saw several branches of the large *Madrepora alcicornis* projecting above the surface of the water from three to five inches. These branches were dead, for corals always die soon after exposure to the air, while the rest of the stock, as far as it was under water, was in full life. This observation was made in the month of June. The question naturally suggested itself, when did those pieces, which were now above water, grow?

A fact, to which he alluded on another occasion, at a meeting of the Society, viz., that during the whole winter season (December, January, and February), the level of the water all along the northern shore of Hayti is from four to six feet higher than during the summer season, being raised by a constant northerly wind during those months, suggested to him the idea that those coral branches, as far as they were above water during summer, might have grown during a single winter of three months only. This would show a very rapid growth of this kind of corals. The fact that the Madrepores, when growing so near the surface, and, as stated above, partially uncovered during the summer, very often, in the course of a few years, give rise to small Mangrove islands, between the outside coral reef and the shore, was well known to a native Mulatto sailor, whom Dr. Weinland employed there. As he had observed at a former meeting, there is hardly any change of high and low tide along that shore of Hayti; so that this can have no bearing on the present question.

Prof. Agassiz also stated that he had, as the result of his investigations, been led to institute the following classification of corals:

1st. *Vegetable Corals.* These are Algæ, or at least vegetable productions, which in time accumulate in their cellular tissue so much lime as to resemble coral, and which form entire islands, as the Tortugas and Marquesas groups, the sands on the shores of which are composed of disintegrated particles of these vegetable growths. 2d. *Corals of Bryozoa.* The affinities of these are well known. They grow in clusters, and are genuine corals belonging to animals of the lower class of molluscs, and not polyps, though once thus considered. 3d and 4th. The remaining corals belong to two types, *genuine corals formed by Polyps* and *those belonging to Hydroid Acalephs, the Tabulata.* The Tabulata are known to be Hydroids by direct observation of the animal in Millepora recently made by Prof. A. in Florida. Of Rugosa no living types are known, and consequently its affinities must be determined by the structure of the solid parts. In this respect the Tabulata present striking differences from the genuine corals formed by the Polyps. These have vertical radiating partitions, extending from top to bottom, with transverse partitions extending only between two adjoining vertical partitions. In Rugosa this horizontal floor extends across the whole cavity of the animal, as in Tabulata; and the radiating partitions are limited in their vertical extent to the space between two horizontal floors; so that their affinities go with the Tabulata, in some of which there are traces of radiating partitions. Besides, in Rugosa, the quadripartite arrangement prevails as in Acalephs. The secretions of the Tabulata are *foot secretions*, whilst those of other corals are from the outer walls.

REVACCINATION.

A paper on this subject was recently presented to the Academy of Medicine of Paris, in the name of Dr. Vlemincks, one of its corresponding members. The author gives an account of the experiments instituted at Gand by Dr. Denobele, with a view to ascertain the advantages arising from a repetition of vaccination at various periods of life. The results arrived at are, that between the ages of twenty and forty, revaccination only takes effect upon four out of one hundred patients, while the proportion of those on whom it takes effect between the ages of forty and sixty is twenty-three per cent.; and between the ages of sixty and seventy, fifty-four per cent. The consequences deduced from these facts are: 1. That until the age of twenty-five, revaccination is useless; 2. That from that age to thirty-five it produces useful effects upon a very small number of persons, and that consequently it need not be very strenuously recommended at that period of life; 3. That from the age of thirty-five and upwards it becomes really prophylactic, and therefore necessary; 4. That when vaccination has not taken effect at a certain period, this is no reason for concluding that it will not take effect at some future period. Hence Dr. Vlemincks concludes that the revaccination of the pupils of schools and seminaries, as also of soldiers in the army, is useless.

THE PARASITE OF A PARASITE.

An *acarus*, infesting the parasite of a bee, has been lately discovered, and a photographic portrait of the insect, magnified one million times, surface measurement, has been taken by Mr. A. Bertsch. It is covered with a carapace, or hollow shield, and its feet are armed with sharp claws, by which it keeps a firm hold upon the microscopic creature from which it derives nourishment, and which in its turn preys upon the honey-gathering bee. As we can discover no limits to the minuteness of organized beings, so we can fix no term to this extraordinary series of parasitic animals preying one upon the other. How much further can we hope to fathom the mysteries of organic creation?

ON THE MIGRATION OF FISHES.

At a meeting of the Boston Society of Natural History, Dr. H. R. Storer presented specimens of a smelt, from Squam Lake, N. H., remarking on their peculiar interest, as affording an instance of a species originally migrating to fresh water from salt water, and now permanently resident in the former. He had learned of its existence several years since, but had until now been unable to obtain it. When full grown, the lake smelt seldom exceeds six inches in length, and is extremely attenuated; but a careful examination leaves little doubt of its identity with our marine *Osmerus viridescens*. It is found throughout the year, in both Squam Lake and Winnipisseogee, though more rarely in the latter. The modifications in shape referred to would probably be found to exist also in the smelt of Jamaica Pond, near Boston, the conditions of life being much the same in both, the latter having been imprisoned artificially, while the former had become a permanent resident in fresh water from natural causes alone.

At a subsequent meeting, Professor Agassiz, in reply to a question whether the fishes of the European coast could be transplanted to the shores of America, — said that it was extremely doubtful. From a general point of view he should not suppose that any family of fishes, which have no representatives here, could flourish on this coast; but that perhaps fish belonging to the same family with the haddock and hake, might be naturalized.

VARIATION OF COLOR IN THE VENOUS BLOOD OF THE GLANDS.

Since the discovery of the circulation of the blood, it has been admitted that the blood of the arteries is red, and that of the veins black, with this exception, that it is the reverse for the arteries and pulmonary veins. This fact has afforded Bichat the foundation for his grand division of the circulation (since adopted by all anatomists), a vascular system with *red* blood, which carries the blood from the lungs to all parts of the body; a vascular system of *black* blood, which carries the blood from all parts of the body to the lungs. But it results from the researches of Prof. Claude Bernard that this statement cannot be accepted absolutely. This skilful observer has proved, through a great number of dissections of living subjects, and in a manner which leaves no room for doubt, that the blood contained in the renal veins is sometimes black and sometimes red, and that when it has the latter color, it is black in the inferior vena cava which receives the blood from the renal vein.

This fact being established, he looked for an explanation, and found that it was due to the state of repose or activity of the kidneys, the secretory organ of the urine. He has, in fact, demonstrated, by delicate experiments, in his course of Physiology at the College of France, that when the urine runs from the kidneys, where it has just been formed, in the ureter which takes it to the bladder, the blood contained in the renal cavities is red; and that it becomes black when the flow ceases.

The same experiment performed on the submaxillary gland of the dog produced the same result. Flowing of saliva by the proper duct from this gland, and presence of red blood in the afferent vein are two phenomena which go together, as also the absence of saliva and black color of the blood in the same vein. Analogous experiments made on the parotid gland and on the glands in the abdominal parts of the digestive tube, have given similar results. But the author adds, with the habitual severity which he brings to his conclusions, that the study will be complete only when the examination shall have been extended to every gland throughout the structure.

It results from the facts, that if, as regards physiological conditions, the term *red blood* may be applied to the *arterial blood*, that of black blood cannot be used in so general a way for the venous blood. It results also, from other researches of Prof. Bernard, that physical and chemical modifications correspond to these different states of coloration, and ought to be taken into consideration in the analyses of the blood, the composition of which varies even with the state of activity or repose of an organ. The last principle applies not merely to the glands, but to all the organs of the body; so that it will be necessary to study now the venous blood in the state of repose, and in the state of functional activity. It is worthy of remark, that if the blood goes out red from the glands in activity, it goes out, on the contrary, very black, and with different physical qualities from a muscle which contracts itself.

In experimenting on the submaxillary gland, Prof. Bernard has been able, by means of electricity, to excite at will the activity of an organ, so as to produce the secretion of saliva and the coloring of the blood of the vein red. This fact suggested to him the following remark: "All those modifications which the blood undergoes in consequence of the functional activity of the organs, are always determined by the nervous system; and consequently, at this point of contact between the organic tissues and the blood, we must search for a knowledge of the special agency of the nervous system in the physico-chemical phenomena of life."

INTERESTING OBSERVATIONS ON THE BODY OF AN EXECUTED CRIMINAL.

During the past year a man by the name of McGee was executed for murder in the city of Boston, and a medical examination subsequently made of his body by Dr. Henry Clarke and other physicians of that city. From the account of this examination, published by Dr. Clarke in the *Boston Medical Journal*, we derive the following interesting particulars:

The examination may be said to have commenced before death had completely taken place, for Dr. Clarke's account begins while the frame of the malefactor was still suspended. "At the end of seven minutes (he says) all the sounds of the heart were distinctly audible, and the number of beats one hundred in the minute. At nine minutes the number was ninety-eight. At the end of twelve minutes the number was sixty, and the pulsations fainter. At fourteen minutes the sounds had disappeared. The body was lowered at 10.25, at which time a careful examination of the chest revealed no perceptible sound or impulse of the heart. A small space under the left ear seemed to have escaped active compression, so that some circulation might have been continued through the carotid and jugular of that side." Half an hour later, or a few minutes past eleven, Dr. Ellis commenced the autopsy. At 11.30 a slight but regular pulsatory movement was observed in the right subclavian vein. Upon applying the ear to the chest, this was ascertained to proceed from the heart itself, which gave a distinct and regular single beat, with a slight impulse, eighty times a minute. The chest was then opened, and the heart exposed, without in any way arresting the pulsatory movements. The right auricle was in full and regular motion, contracting and dilating with beautiful distinctiveness and energy. At twelve o'clock, the spinal cord having been previously divided, the number of contractions was forty per minute, having continued, with only a short intermission, up to this time. The peculiar movements of the anterior wall of the right auricle gradually, but occasionally, recurred, either spontaneously, or excited by a passing current of air, until 1.45. They could at any moment be excited by the point of the scalpel. At 1.45 the movements still continued without stimulus. Five were noticed in a minute, with corresponding intervals. At 2.45 all automatic movements ceased, but the part still responded to the stimulus of the knife. At 3.10 deep irritation of the same kind was followed by slight movements. The irritability was most marked at the lower part, where the vena cavæ enter the auricle. At 3.18 all movements ceased. On opening the heart, it was found to be perfectly normal. After some other and more strictly professional details, the account proceeds to narrate the discussion that followed. Dr. Gay thought the absence of cerebral congestion due to the adjustment of the rope, which allowed circulation in the left carotid. He thought death might have been owing to the sudden shock. Dr. Clarke thought the death was by asphyxia. Dr. Ainsworth remarked that "all the appearances usually observed in cases of hanging were here wanting." Dr. Clarke expressed the "opinion that, as there was no lesion of any important organ, resuscitation might probably have been accomplished by artificial respiration, etc., if efforts to that end had been made immediately upon the lowering of the body from the scaffold, that is, within half an hour after he fell. Strong shocks of electricity or galvanism would, in cases of accidental apparent death, destroy the little remaining vitality; and if these agents are used at all, they should be administered with great care." Dr. Coale alluded to the unfortunate incident in the life of the celebrated Vesalius, in consequence of which he was banished from his country and died in exile. Not allowing a sufficient time to elapse after the death of his patient before proceeding to the examination, the muscular irritability remaining in the body caused a pulsatory movement in the heart, which led to his arrest and punishment for murder and impiety.

ON FAUNA OF THE ARCTIC REGIONS.

In a recent discussion before the Boston Society of Natural History, Dr. T. M. Brewster stated that it had been ascertained that there is a greater diversity of species among the birds of the eastern and western North Atlantic coasts than was formerly supposed. Several species, bearing close resemblance, upon the two continents, have been established to be different, — for example, the Velvet Duck, the Peregrine Falcon, and the Fish Hawk. It was interesting to observe, that, for no apparent cause in their organization different from that common to both shores, many birds are found only on one or the other shore; for instance, the Manx Shearwater, the lesser Saddleback Gull, the European Scoter (differing only in size from the American) which are found only in Europe. Between the birds of the Atlantic and Pacific coasts there is more diversity, and also there are observable differences of distribution. Thus Brunnich's Guillemot, found by Dr. Kane in latitude 70° north, and rarely found so far south as Massachusetts Bay, in midwinter breeds in the harbor of San Francisco, in latitude nearly corresponding with that of Richmond, Virginia. The Uria Grylle, whose extreme southern breeding point on the Atlantic is the Bay of Fundy, breeds also near San Francisco. It may be, however, that the eastern and the western birds will yet be found to be of different species. Dr. Brewer believed that they would be.

Dr. Bryant stated that the majority of the arctic birds are identical with those of Europe; and that the arctic ornithology of the western coast of North America differed more from the eastern coast than the latter did from that of Europe.

Dr. A. A. Gould remarked that the arctic circle has ever been considered one uniform zoölogical region; he had recently examined shells collected by Mr. Stimpson in Beering's Straits and upon the northwest coast of North America, and had found them to be identical with those found between this place and Labrador. One shell in particular, Nucula thraciœformis, he alluded to; one valve of this shell, brought from Japan, exactly mated an opposite valve taken at Provincetown, Mass. At Hakodadi, Japan, the arctic fauna exists, and the shells of this coast are found; whilst at Simoda the shells are those of the China seas. Birds can traverse the ocean in the northern regions where the continents approach each other, but it is a question if mollsuca can travel such distances.

ON PARTHENOGENESIS.

This word, as its derivation implies, signifies the production of young by the female sex alone, as in the Aphides, or common plant lice, in which generation follows generation, for a dozen or more, without renewed intercourse with the other sex.

There are two modes of generation well known in plants and animals, — one by true eggs, the other by buds without eggs. The plant kingdom is known to be full of both processes. The bud from a branch, developing regularly its leaves, and capable as it often is of propagation, where separated, as well as when united to the original stem, is one variety of propagation by buds. The bud originating as a bulb at the axils of the leaves and branches, which drops off, and on finding soil, produces a new plant, is another variety of growth by buds. As each plant from such a bulb or bulbel will produce its crop of bulbels, propagation may be continued on apparently indefinitely, without necessary intervention of true flowers.

The Animal Kingdom, in its inferior departments of the Radiates and Molluscs, exemplifies the same method of propagation. The young polyp may grow from the side of the old, and be persistent, like ordinary buds of plants, but becoming an independent individual if cut off; or, in other cases, it may drop off on reaching towards maturity, and thus acquire independence, and so become the parent of a new zoöphyte. Thus far the two kingdoms have long been known to be alike in reproduction. The bulbels of the plant are not like true seed in structure, neither are the bulbels of the Campanularidæ. There is not the germinal vesicle with its germinative spot. The development is simply germination. The analogies between plants and animals, it may be stated, go still further; for as plants produce leaf buds, and then flower buds, and then flower buds in which sexual organs and seed are developed, so some Medusæ (Tabularidæ) bud out polyps to make the branching stems, and afterwards bud out Medusæ to develop sexual organs and ova; these Medusæ (as occasionally happens with the flowers of plants) separating and becoming free from the stalk that produces them. Moreover, it is now understood that the so-called alternation of generation is nothing more than the successive states exemplified in plants, of the embryo, incipient leaf bud, opened leaf bud, flower bud, and flower, all of which are often widely diverse in forms.

It was in view of such facts as these that the late Dr. Burnett undertook to determine the nature of the process of continual nonsexual propagation in the Aphides; and his conclusion was, that the egg-like bodies, developed in clusters within the producing Aphis, were of the nature of buds, and not true ova, agreeing in this with Dr. Carpenter; and that the whole was analogous to the budding process. "The germs," he says, "have none of the structural characteristics of eggs, such as a vitellus, a germinative vesicle and dot; on the other hand, they are at first, simple collections, in oval masses, of nucleated cells." He also refers to the same kind of origin, the so-called hibernating eggs of *Daphnia* among Crustacea, *Lacinularia* among the Rotatoria, and *Hydatina* and *Notommata* among the Infusoria, in which, he observes, no germinative vesicle or dot has been seen, and no connection with the ovary discovered.

Parthenogenesis in the Aphides, according to this view, is reproduction by buds, or gemmiparous reproduction, as distinct from sexual reproduction. It is like leaf-budding, the flower-budding (or sexual developments) taking place at longer intervals.

The later investigations of some zoölogists have been tending to the conclusion that even true eggs, or bodies having the structure of eggs in every essential point, may be produced in some cases by females, and develop into perfect individuals without the intervention of the male, and without any proper hermaphroditism in the parent. The most important work that has appeared on this subject is one on "Parthenogenesis in Moths and Bees," by C. T. E. von Siebold, of Munich, which has been translated in England by W. S. Dallas. The author describes the raising of brood after brood of young from some moths, without the recurrence of a single male; and in a *Psyche*, the pupacase is filled with eggs before it is left; and in a *Selonobia*, the animal, immediately after leaving the case, stuffs it full of eggs. The main point in his work, and one of more questionable character, relates to bees. He adopts the theory of a Silesian clergyman named Dzierzon, and after farther elaborating it, sustains the view that "the queen-bee, which, like all other female insects, receives the seminal fluid of the male in a peculiar receptacle, there to be retained until it comes in contact with the egg during its passage through the oviduct, possesses the power of permitting or preventing this contact, so that the eggs may be deposited in the cells, either fecundated or unfecundated, at the pleasure of the mother; and farther, that from the fecundated eggs, female larves are produced, which become developed either into queens or workers, whilst the unfecundated eggs furnish the larves of the drones or males." The following are some of the points of evidence adduced in support of this remarkable theory, as given in a notice of the work in the Annals and Magazine of Natural History:

"It is now generally admitted, even by bee-keepers, that the queen only copulates once, and that the supply of seminal fluid received at this time, and stored up in the seminal receptacle, serves for the fecundation of the immense number of eggs which she deposits during the period of her fertility, extending over several years. Sometimes, however, the stock of spermatozoids appears to be exhausted before the life of the queen comes to a close, and when this is the case she lays nothing but drone eggs, introducing confusion into the wonderfully harmonious arrangements of the hive. This was found to be the case also with a queen which had been exposed to severe cold with a view of destroying the vitality of the spermatozoids; of three queens thus treated, only one survived, and this afterwards laid nothing but drone eggs. Another queen, whose abdomen had been injured so as *probably* to displace the seminal receptacle, also produded drone-eggs exclusively. Added to this, certain workers, which, as is well known, are merely abortive females, destitute of copulative organs and of the seminal receptacle, and therefore incapable of fecundation, are found to possess imperfectly developed ovaries, which produce a very small number of eggs, and these, when deposited in the cells, are said always to produce drones. For most of these facts, von Siebold appears to be indebted to the apiarians Dzierzon and von Berlepsch; but perhaps the most remarkable observations are those made by himself, in the microscopic examination of a considerable number of newly deposited eggs. In the majority of the eggs deposited in worker-cells examined by him, he found spermatozoids; sometimes as many as four. In some instances, these singular filaments still retained the power of motion. On examining twenty-seven drone-eggs, laid by the same queen which had furnished a portion of the female eggs, von Siebold did not discover a single spermatozoid.

"Such is the outline of the results at which the distinguished author has arrived; and although many will perhaps be disinclined to give an unhesitating adhesion to his views, there can be no doubt that his work is one of the most important that has appeared for a long time; one well worthy of being carefully studied by all physiologists, and one that must, in the end, greatly advance the cause of science, if only by calling the attention of observers to this singular and much-neglected subject."

It would appear, from observed facts, that, among some of the lower animals, it is of no more account for one of them to bud out a complete animal of its kind, than for a crab to reproduce its mutilated claw. Moreover, it seems to be also true that the budding process may take place in the ovary, and that it may evolve an egg or something very like an egg, thus commencing with the first step in the reproductive process; or it may evolve a bulb-like mass from other parts of the body, like that in ordinary gemmation; and each may develop into an individual animal, or what will produce such individuals. Whether formed in one place or another, a germinating cellule, or a spot or collection of cellules, begins the development, and the whole process, from its initiatory step to the end, is a regular growth from the single budding individual.

Moreover, the observations in the plant kingdom appear to show definitely (confirmatory of Mr. Lubbock's observations in the Daphnia), that in the case of ovary reproduction, the ovule which develops without impregnation is identical in its initial growth with that prepared for impregnation according to the ordinary seed-producing process. Yet, not to lose sight of the diverse relations of the two modes of reproduction, we should remember that, normally, in every species which buds or produces budding eggs, there are also the opposite sexes for true egg-development; that even the lowest sea-weed has its conjugation of oppositely related cells for spore-reproduction; that reproduction of this one-sex kind is confined to the lower grades of species among animals, and some of these, like the Aphis, find the process so easy that they can turn off their germ-buds by the myriads, and still there is here a periodical recourse to the true sexual process; that in some animals, like the Daphnia, while the ovaries produce eggs of both kinds, the normal eggs pass to another cavity, and early show their distinctive character; that, in fine, a distinction of sex (a kind of sexual polarity) is the grand universal law for reproduction in life, and is never altogether set aside even for the inferior species, while absolutely essential in the higher. Moreover, this supplemental and inferior means of propagation, or budding, is but an expansion of the ordinary law of growth; the same law that reproduces the nails and hair in man, the tail of a mutilated snake, or the legs of a maimed crustacean, evolves the polyp from the bud of a polyp, the Aphis from the Aphis germ-bud, or the plant from an unimpregnated ovule. The Aphis germ-bud or the unimpregnated ovule may be considered as only a minuter or more concentrated form of the bulb or bulbel.

The process by which the female produces the ovule which is afterwards to be impregnated is essentially a budding process, and not until after impregnation does it become in any case a true developing egg; and it would seem that, in a few cases at least, it may develop either gemmatively, or in true egg style, that is, it may continue a germ-bud, or become an egg, according as it is or is not impregnated.

But, while this extension of the budding method of propagation subserves an end of vast importance among the inferior animals and in the plant kingdom, it cannot be properly an equivalent to the normal sexual process. There is some great difference between what the female can bud out of herself, and what the sexes combined produce. It is probable, from facts which have been observed, both in plants and animals — though not yet demonstrated — that bud propagation will in all cases, if followed exclusively, end in the decline of the race, and its ultimate extinction; and that the sexes are required to keep up the sexual system and thus to sustain the type at its normal level, and secure its perpetuity. This, if established as a real effect, is yet but a partial, or inadequate expression of the difference between the two results. The subject opens a wide field for exploration. — *Silliman's Journal* (abridged), Nov., 1857.

SILK CULTURE IN INDIA.

Mr. F. Bashford, who has been engaged in the silk culture in India for many years, states, in a paper recently presented to the London Society of Arts, that it requires in Bengal 10,000 of the best cocoons to produce one pound of good silk; in France 2,500 cocoons produce the same quantity. With a view to improve this produce, Mr. Bashford imported a large quantity of the best French, Italian, and China eggs, to engraft upon the different species of the Bengal race. Various details of the experiments were then given; but Mr. Bashford sums up by saying that, as he had spent three years in trying ineffectually to engraft a superior nature, and invigorate the common stock, he felt discouraged, and would gladly have the opinion of naturalists as to the probability of his object ever being attainable, and the proper steps to be taken for realizing it.

BUTTERFLY VIVARIUM.

The success of vivaria for fish and crustacea, has suggested to Mr. Noel Humphreys, of England, the idea of a vivarium for insects; and for the construction of this he has recently published directions in a work entitled the *"Butterfly Vivarium," or "Insect Home."* Entomologists have at all times found it necessary to make use of some kind of vivarium, for the purpose of observing the changes which insects undergo before arriving at their perfect state. The tin box, with a perforated lid for ventilation, the card-board trays for silkworms, or the wooden box sunk in the ground, with a wire lid, and filled with the various kinds of caterpillars, are, in fact, all vivaria. But the speciality of Mr. Humphreys' plan is to make the vivarium an ornamental object for a drawing-room. With this view, he proposes that it shall consist of a glass case, with proper ventilation at the top; that part of the bottom shall be filled with earth, in which plants, such as are fed upon by insects, shall be planted; that another part shall be devoted to a tank, for the benefit of such as delight in water; and that in the earth shall be inserted bottles for holding sprigs of such plants as are too large to grow in the vivarium. We cannot gather from the directions whether or not Mr. Humphreys has himself tried the experiment, or seen it tried by others. An ornamental glass case, filled with plants, on the leaves of which should appear beautiful caterpillars of various colors, and metallic beetles glittering in the sun, while around the flowers should flit moths and butterflies, and dragonflies, with their gorgeously-painted or delicately-reticulated wings, would certainly form a very attractive object. Whether such a vivarium could be maintained in good working order or not, is, however, in our opinion, a somewhat doubtful matter.

ASTRONOMY AND METEOROLOGY.

NEW PLANETS FOR 1858.

The forty-sixth asteroidal planet was discovered by Mr. Pogson, of Oxford, Eng., Aug. 16, 1857, and has received the name of Hestia.

The forty-seventh, Aglaia, was discovered on the 15th of September, 1857, by Dr. Luther, of Bilk.

The forty-eighth, Doris, and the forty-ninth, Pales, were both discovered by M. Goldschmidt, of Paris, on the same evening, namely, the 19th of September, 1857.

The fiftieth, Virginia, was discovered on the 4th of October, 1857, by Mr. Ferguson of the observatory at Washington, D. C.*

The asteroids discovered during the year 1858 are as follows:

Nemausa, the fifty-first, discovered by M. Laurent, of Marseilles, Jan. 22.

Europa, the fifty-second, discovered by M. Goldschmidt, February 6.

Calypso, the fifty-third, discovered by Dr. Luther, of Bilk, April 4.

Fifty-fourth and Fifty-fifth Asteroids. — Two asteroids were discovered on the night of the 10th of September: the one by M. Goldschmidt, at Paris, and the other by Mr. George Searle, of the Dudley Observatory, at Albany.

DONATI'S COMET.

The following account of the great comet of 1858 is derived in part from an article contributed to Runkle's Mathematical Journal, by Prof. G. P. Bond, of the Cambridge Observatory.

On the 2d June, 1858, a faint nebulosity, slowly advancing toward the north, was descried by Donati at Florence, near the star λ *Leonis*. This was the earliest observation of the great comet of 1858, its distance from the sun being then about two hundred millions of miles, while from the earth it was yet more remote. Being, at first, inclined to question whether it might not be identical with another comet just before seen in the same quarter of the heavens (the third comet of 1858), he communicated the intelligence of the discovery, with a suitable reserve, as "perhaps new;" and in a second despatch he said, "It is possible that this comet is the same as that discovered in America on the 2d of May." This conjecture, fortunately for Donati, did not prove true; although the apprehension of the Italian astronomer, from the rival zeal of his transatlantic brethren, was not without reasonable foundation, for no sooner had the moon withdrawn from the evening sky, so as to allow the comet to be seen, than it was detected almost simultaneously at three different points in America, each observer being at the time unaware

* The table of asteroids discovered in 1857, as given in the Annual of Scientific Discovery for 1858, was in some respects incorrect. Hence the repetition.

of its previous discovery in Italy. It was seen by Mr. H. P. Tuttle, on the evening of the 28th of June, and an accurate determination of its place was made on the same night at the observatory of Harvard College. On the 29th, it was detected by H. M. Parkhurst, Esq., of Perth Amboy, N. J.; and on the 1st of July, by Miss Mitchell, of Nantucket.

Three geocentric positions, obtained on the 7th, 11th, and 13th of June, furnished Donati with the means of computing approximate elements of the comet's motion, from which its interesting character was quickly recognized. Considerable difficulty was experienced in fixing the precise time of perihelion passage, a most necessary condition in predicting its path as seen from the earth. While in other respects the results deduced by various computers were sufficiently accordant, they showed wide discrepancies in designating the place of the comet in the orbit. By the middle of August, however, its future course, and great increase of brightness in September and the early part of October, had been ascertained with entire certainty.

Up to this time it had remained a faint object, not even discernible by the unassisted eye. It was distinguished from ordinary telescopic comets only by the extreme slowness of its motion, in singular contrast with its subsequent career, and by the vivid light of the nucleus; the latter peculiarity was of itself prophetic of a splendid destiny.

Traces of a tail were noticed on the 20th of August, and on the 29th it was seen with the naked eye as a hazy star. For a few weeks it occupied a position in the heavens where it rose before the sun and set after it, becoming thus a conspicuous object both in the morning and evening sky. This circumstance gave rise to the erroneous notion that two different comets had appeared. The statement, which was widely circulated, that this was the return of the comet of 1264 and of 1556, supposed by some to be identical, is equally incorrect. If it has ever before been seen by man, it must have been far back in history, since the most recent computations assign a time of revolution of about twenty-four hundred years.

On the 6th of September was first noticed the curvature of the tail, which subsequently, at the time of its greatest expansion, became one of its most impressive features. It is remarkable that this peculiarity should have been strongly enough exhibited to be distinguished at the above date, when the earth was close to the plane of the comet's orbit. The observation cannot, in fact, be reconciled with the commonly received opinion that the curvature of the tail lies in the plane of motion about the sun.

On the 20th, the first of a series of extraordinary phenomena manifested itself in the region contiguous to the nucleus. A crescent-shaped outline, obscure and very narrow, was interspersed, like a screen, between the nucleus and the sun; within this, instead of a softly blended nebulous light, indicative of an undisturbed condition of equilibrium, the fiery mass was in a state of apparent commotion, as though upheaved by the action of violent internal forces. On the 23d, two dark outlines were traced more than half way round the nucleus, and on the next evening still another. Each of these was evidently the outer boundary of a luminous envelope, the brightest being that nearest the nucleus.

On the 25th four envelops were seen, and others were subsequently formed, almost under the eye of the observer, their motion of projection from the nucleus being evident from night to night. The rapidity of their formation, and the enormous extent to which they are ultimately expanded, are phenomena extremely difficult to explain. The scene of chaotic confusion presented within the inmost envelop can only be accounted for as the result of sudden and violent disruptions from the central body, projecting immense volumes of its luminous substance towards the sun, which by some unknown law, is in turn repelled by that body, and driven off to the distant regions of space, forming the vast train of light so characteristic of these mysterious bodies.

Prof. Mitchell, of the Cincinnati Observatory, thus describes the appearance of the comet in the great refractor of that institution:

"On the evening of the 25th of September the central portion, or nucleus, was examined with powers varying from one hundred to five hundred, without presenting any evidence of a well-defined planetary disk. It was a brilliant glow of light, darting and flashing forward in the direction of the motion toward the sun, and leaving the region behind in comparative obscurity. But the most wonderful physical feature presented was a portion of a *nearly circular nebulous ring*, with its vertex directed toward the sun, the bright nucleus being in the centre, while the imperfect ring swept more than half round the luminous centre. This nebulous ring resembled those which sometimes escape from a steam-pipe, but did not exhibit the appearance which ought to be presented by a hollow hemispherical envelop of nebulous matter.

"There was an evident concentration of light in the central portions of the ring, while, in the case of a hollow envelop, the brightest portion should be at the outer edge. By micrometrical measurement, the distance from the central point to the circumference of the ring was found to be about nine thousand miles. This would give a diameter of eighteen thousand miles, in case the ring was entire. Similar measurements, made on the evening of the 26th of September, indicated a decided increase in the radius of the ring, which was now not less than twelve thousand miles in length. On the same evening I noticed the fact that the luminous envelop did not blend itself into the head portion of the tail, but appeared somewhat to penetrate into this nebulous mass, especially on the upper part, presenting the appearance of about 200° of a spiral. The tail on the 25th was decidedly brighter and better defined on the upper than on the lower portion, while on the evening of the 26th there was a much nearer approach to equality in brightness, especially near the head of the comet. Through the telescope, and near the head, the tail presented the appearance of a hollow nebulous envelop, under the form of a paraboloid of revolution, the edges being brightest and well defined, while there was a manifest fading away of light towards the central region. Through the vast depth of nebulous matter composing this wonderful appendage, the faintest telescopic stars shone with undiminished brightness."

Donati's comet attained its least distance from the sun — 55,000,000 of miles — on the morning of the 30th of September. Its least distance from the earth — 52,000,000 of miles — occurred on the 12th of October. On the 10th of October, its tail stretched over 60° of the heavens, and was 51,000,000 of miles in length, and 10,000,000 in breadth, at its extremity. The longer diameter of its orbit was estimated as 181 times that of the earth, or 35,000,000,000 miles, a space, however, considerably less than one-thousandth of the distance of the nearest fixed star. For the nucleus, Mr. Bond gives the following measurements: July 19th, 5″ = 5000 miles; August 19th, equal to a star of the 7th magnitude; August 29th, head of the comet visible to the unassisted eye as a star of the sixth magnitude; August 30th, diameter of the nucleus $6'' = 4660$ miles; September 8th and 9th, diameter $3'' = 1980$ miles. On the 23d of September, the head of the comet, to the naked eye, appeared brighter than a star of the first magnitude, and during the remaining period of its visibility it went through a series of periodic changes, acquiring more light just before an eruption, and suddenly diminishing afterwards.

This comet, says Mr. Bond, although surpassed by many others in size, has not often been equalled in the intensity of the light of the nucleus. The diameter of the surrounding nebulosity, on the other hand, was unusually small, never much exceeding 100,000 miles; while that of the great comet of 1811 was ten times larger, — its envelop attaining an elevation of more than 300,000 miles above the central body.

The various observations on Donati's comet leave no room to doubt that it is periodical, and has a time of revolution of about 2000 years, the maximum period calculated being 2415 years, and the minimum 1854. Its hourly velocity at perihelion was calculated at 127,000, and its aphelion velocity at 480 miles.

In illustration of these times and distances, the London *Times* uses the following comparisons:

Let any one take a half-sheet of note paper, and, marking a circle with a sixpence, in one corner of it, describe therein our solar system, drawing the orbits of the earth and inferior planets as small as he can by the aid of a magnifying glass. If the circumference of the sixpence stands for the orbit of Neptune, then an oval filling the page will fairly represent the orbit of our comet; and if the paper be laid on the pavement under the west door of St. Paul's, the length of that edifice will inadequately represent the distance of the nearest fixed star. That the comet should take more than 2,000 years to travel round the page of note paper we have supposed, is explained by its great diminution of speed as it recedes from the sun. At its perihelion, it travelled 127,000 miles an hour, or more than twice as fast as the earth, whose motion is about one thousand miles a minute. At its aphelion, however, or its greatest distance from the sun, the comet is a very slow body, sailing along, as if doubtful whether to return, at the rate of 480 miles an hour. This is only eight times the speed of a railway express. At this pace, even if the comet could wholly shake off the attraction of the sun — which it certainly could not — and were it to travel onward in a straight line, the lapse of a million years would find it still travelling half way between our sun and the nearest fixed star. Comets, then, can hardly be imagined visitors from our system to any other, or from any other to our own.

Donati's comet passed within nine millions of miles of Venus, and if it had any density at all comparable to our own, would have effected the orbit of that planet in an appreciable degree. Were that found to be the case, it would be the first fact of the kind in the history of these singular bodies.

Mr. Bond, in connection with his article in the Mathematical Journal, before referred to, also presents the following sketch of the more distinctive phenomena presented in the motions and physical aspect of comets generally:

The first characteristic of these singular bodies is that of their being mainly, perhaps in most instances, entirely composed of an ill-defined gaseous or nebulous substance, endowed with properties so extraordinary that it can scarcely be classed with matter in the ordinary acceptation of the term. Of its extreme attenuation and lightness there can be no question.

The planets, and among them our earth, must again and again have traversed unharmed the tails of comets. In October last, the debris of the magnificent train of the comet which has just disappeared from our western skies, swept over the region occupied by the earth a few weeks earlier. Instances of more immediate proximity are of too common occurrence to allow us to suppose that we are always to escape an actual collision; but it is inconceivable that any disastrous consequences could ensue to our earth or its inhabitants, any more than from contact with sunlight, or with the ether of the planetary spaces.

A second characteristic is that of internal condensation. All comets present this in a greater or less degree. Most of them have a minute stellar point, called the nucleus, which occupies the position of maximum density. There are others in which this latter feature is wholly wanting. But the number, in which it cannot be detected with a powerful telescope, is much smaller than has commonly been supposed. This centre of condensation, or brightest point, is, with rare exceptions, placed on the side which is nearest to the sun. It is always, however, very close to the centre of gravity, as is proved by the fact of its motion about the sun, in accordance with the law of gravitation.

The nucleus itself is a minute point compared with the immense volume of light-giving substance, of which it is the controlling centre. Whether it is solid or not, is still undecided. As far as the eye alone is to be trusted, there are comets as truly solid as the planets or stars themselves. In size and weight, however, the true nuclei, apart from their surrounding nebulosity, are probably quite small, measured by the standard of the larger planets. Still, it is possible that there may have been instances in which the mass of these bodies has been comparable with that of the earth, and yet, they may have completed their circuit around the sun, leaving no appreciable trace of their disturbing influence — the only sure test by which their mass could be detected. The evidence, from the fact that the smaller stars shine freely, even through the most condensed portions of the comets, adduced by astronomical writers in proof of their transparency, and, by inference, of their extreme tenuity and lightness, has a certain value when applied to the class of feeble telescopic comets, but is scarcely applicable to one like that of the present year, which overpowered all but the brightest stars in the neighborhood of the nucleus by its superior brilliancy.

The feature next in importance to the nucleus is the train, or tail, as it is usually called (although often preceding the nucleus in its motion), projected at an immense distance from it, and usually, although by no means invariably, in a direction opposite to that of the sun. The agency of the nucleus in the formation of the train, but still more in the subsequent control which it retains over it, is one of the most curious phenomena presented in nature. Often several of these appendages are seen radiating at once from the same nucleus. The greatest variety in curvature of outline, length, brilliancy and other peculiarities, is presented by different comets, or by the same one in different parts of its course. The portions near the axis are usually darker than the edges, giving at times the appearance of a division with a stream of light on either side.

The larger bodies of this class exhibit a wonderful complication of phenomena in the region contiguous to the nucleus. Of these the most prominent are the interposition between the nucleus and the sun of one or more well-defined and rounded screens, or caps of dense nebulosity, called envelops, partially but not entirely surrounding the nucleus, and the emission of streams or jets of luminosity, bright sectors, etc., in a direction inclined or opposite to that of the tail. With great variety of detail in other respects, these have all a well-marked tendency to appear in the first instance on the side of the nucleus next the sun. The great comet of the present year undoubtedly takes a foremost rank in respect of the multiplied and most curious changes which it has exhibited, and especially in the complete illustration which it afforded of the origin, construction, and final dissipation of a succession of envelops. In these phenomena, the process of the formation of the tail, from the substance in immediate contact with the nucleus, is intimately concerned. The astronomer, night by night, sees the work of evolution going on with an amazing rapidity, and seemingly in defiance of the best established properties of matter, the laws of gravitation and of inertia. The results are evident to all, but the secret cause is a profound mystery admirably calculated to stimulate speculation and intelligent investigation.

As regards the motion of comets in space, it is a well-established fact, so far as our present means of observation extend, that their nuclei alone move in obedience to the attractive force of the sun and planets. This property, which has been recognized with consistency and uniformity, is not the least singular peculiarity of their constitution. Immense volumes of matter, apparently of the identical substance of the nucleus, go to compose the enveloping nebulosity and the tail; but from the moment of leaving the central body, their motion is perfectly inexplicable, without assuming them to be under the influence of laws of force which greatly modify that of gravitation.

The shape of the cometary orbits, described about the sun, is nearly that of a parabola, or of an elongated ellipse, with periods of revolution varying from a few years to many centuries. The point in the orbit which is nearest the sun is called the perihelion; the distance of this point from the sun, the perihelion distance, and the time of the comet's passing it, the perihelion passage.

LUMINOUS THEORY OF COMETS' TAILS.

The following is a *resumè* of a paper on "Comets, and the Curvature of their Tails," recently read before the Boston Society of Natural History, by Dr. C. F. Winslow.

A comet, *strictly defined*, is that portion of it called by astronomers *its nucleus.*

Comets consist of gaseous and very elastic matter, whose physical constitution and luminous functions appear to be similar to, or identical with, the gaseous envelops that surround the sun.

The tail of a comet is not a material element, nor a physical constant, like the nucleus, but only results from a transient evolution of luminous waves, and is a sort of magnetic sequence, like a terrestrial aurora, depending on the progressive gravitation and repulsion (*i. e.*, condensation and reäction) that take place between the particles of the nucleus, as it approaches and passes its perihelion.

The luminous effect of solar action on comets, is similar to that exerted by the solid mass of the sun itself on its photosphere.

The luminous phenomena resulting from this action, increase and decrease in inverse ratios with the distance of the nucleus from the sun.

The *primary* effect of this action is, to intensify the force of gravitation or centralization among the particles of the comet; the *secondary* effect being the evolution of light as a result of repulsion consequent upon the conflict of this latter force with increasing condensation; both forces being excited to more positive energy in the comet, by its approach to its perihelion.

The luminous waves evolved by a comet are arrested by the luminous waves projected from the sun, which latter seize upon, mingle with, decompose, and sweep back the former into space, producing the tail.

By this union, and reäction or decomposition of solar and cometary light, a resplendency is communicated to the solar ray, which increases in visibility and persistent power of extension into space, in direct ratios with the commotion and repulsion excited within the comet, and inversely as the square of its distance from the sun.

The *curvature* apparent in a comet's tail is the resultant of compound motion; the first element being the comet's velocity through the perihelic arc of its orbit; the second being the projection of luminous waves from the comet, which are swept into space by solar action exerted at right angles to the comet's path.

These luminous waves are as independent of each other, and of the light fountain from which they spring, as if they were particles of matter; and being impelled into space by the undulations of solar light, the parts of the resplendent train more distant from the sun become diffuse, since the velocity of the comet is leaving waves of light behind, which are constantly dissolving and vanishing in the great ethereal void. (This was explained by a diagram.)

Comets' tails, not being composed of particles of matter, but only of luminous waves, rendered visible by decomposing causes, and possessed of no angular velocity, have *no such function as an orbit;* this becoming more certain, since the velocity of cometary light constantly varies, depending on the fluctuating impulse of the solar wave, and modified every instant by the repulsive energy within the nucleus, which generates the cometary waves.

DENSITY AND CONSTITUTION OF COMETS. BY D. VAUGHAN.

Although modern science has revealed the amount of matter contained in the sun and the large planets, it has hitherto failed to furnish similar information in regard to those celestial bodies which are too light to affect the astronomical balance. Babinet has recently endeavored to ascertain the density of comets, from their effects on the brilliancy of the stars over which they pass; and, as they have been generally incapable of causing any sensible diminution of stellar lustre, he concludes that cometary bodies, exceeding the largest planets in size, can contain only a few tons of matter. This conclusion has been based on the fact, that our atmosphere is capable of rendering the faint stars invisible when it is illuminated by the light of the full moon; while a far more extensive volume of cometary gases, though exposed to the direct rays of the sun, fail to produce a similar effect. But we may question the propriety of supposing that the power of gases to obscure stellar light is in direct proportion to their density; and we have no grounds for assuming that the nucleus is wholly destitute of all dense solid or liquid matter. A globe of granite ten miles in diameter would weigh about

6,000,000,000,000 tons; yet in the nucleus of most observed comets, such a mass would not be readily distinguished, even by the most powerful telescopes.

If we endeavor to estimate the attractive power of comets from its efficiency in holding their parts together in certain regions of the solar system, it would seem that they must contain far more matter than Babinet assigns to them. The tidal force of the sun, or the disturbing action which he exerts on the surface of one of his attendants, is inversely proportional to the density of the latter multiplied by the cube of its distance. On our own globe this solar disturbance amounts to $\frac{1}{20,000,000}$ of terrestrial gravity. If the earth's attraction were 20,000,000 times as feeble as it is at present, bodies could have no weight at those places in conjunction and in opposition with the sun, and the planetary form could no longer be preserved. Had a comet 8,000 miles in diameter, moved in a circular orbit, at a distance of 95,000,000 from the sun, it could not resist his dismembering effects, unless it were at least equal to $\frac{1}{20,000,000}$ of the terrestrial mass, and contained over 300,000,000,000,000 tons of matter. Had such a comet revolved in the orbit of Neptune, the attraction of 10,000,000,000 tons of matter would be sufficient to maintain its integrity.

These results, however, are obtained on the supposition that gravity is the only power concerned in keeping the cometary matter together; but it would seem that some other agent is employed to secure the same end. The head of the comet of 1811 was over a million of miles in diameter, and the attraction of a nucleus equal in mass to the planet Mercury, would be scarcely adequate to preserve the stability of so extensive a body during its perihelion passage. Yet, according to them easurements of Herschel, the diameter of the nucleus was little over 400 miles, and it would seem that the surrounding nebulous matter was too rare to compensate for the defective attractive power of so small a globe. Even a few comets of considerable size present scarcely any indications of the presence of dense solid or liquid matter in their central regions; and it may be interesting to inquire whether the constitution of comets, and the peculiar phenomena which they exhibit, may not, as Dr. Winslow supposes, be ascribed to the agency of electricity or magnetism. By tracing the legitimate consequences of electrical action, as manifested on our globe, we may easily account for the preservation of these celestial wanderers in the vicinity of the sun, without altering or modifying, in any degree, the doctrines now held by astronomers respecting universal gravity.

Several observations show that the uppermost regions of our atmosphere are most highly charged with electricity; and this is sufficient to create an attraction between the earth's surface and the higher strata of air, so as to render atmospheric pressure a little greater than could arise from terrestrial gravity alone. Were the earth deprived of this gravitative power, the air would be confined around its surface by electrical action, and would be prevented by this feeble tie from retiring into space, though it might swell to a height of many thousand miles. There can be little doubt, that the electrical conditions of the envelop or atmosphere of a comet would hold it to the central nucleus, independent of any gravitative power which the latter may exert. On our planet, the solar heat is the main source of atmospheric electricity, as it gives rise to evaporation, and by putting the winds into motion, causes the friction of the air against the land and water. By these operations electric forces are continually called into existence. If heat is attended with similar results on comets, the influence of electricity must be most decided on these bodies when they are nearest to the solar orb; the attractive power between the nucleus and envelop will be then greatest, and the compression on the latter must confine it to the most limited space. In this manner we may account for the fact that the heads of comets gradually become small as they approach the sun, and regain their size when retiring from him. The consequences of electrical action is also indicated by the commotions to which these bodies appear to be subject, and by the dismemberment which Biela's comet experienced during the present century. In another place I have shown that this wonderful catastrophe was such as might be expected to result from a great electrical disturbance, and the consequent occurrence of an extensive storm in the atmosphere of the cometary mass. (See Annual of Scientific Discovery for 1858, page 405.)

VARIABLE STARS.

The fine double star γ Virginis, is one of the most remarkable specimens of a binary system in the sidereal regions, and has been watched with great assiduity for several years past by Admiral Smyth, of England, and several other astronomers. At the last meeting of the British Association (Leeds), Admiral Smyth stated that its observed and computed places have generally been found to agree within the limits assigned to probable errors of observation, and that it now presents a system which affords, by actual changes both in angular velocity and distance — the former varying inversely as the square of the latter, with the elliptical orbital elements deducible therefrom — incontrovertible evidence of the physical connection of its constituent members. These results are converting probability into demonstration respecting its being subject to the same dynamical forces which govern our own system. Every advance tends to prove the universality of the Newtonian influence of attraction, obeying the Keplerian law of areas. In a word, by warranting the conclusion of the inconceivable extent of the controlling agency of gravitation, it forms a wonderfully sublime truth in astronomical science.

Some interesting observations have also been made recently upon Antares, the nearest of the double stars of the first magnitude. From these, the color of the smaller companion was ascertained to be of a blue-green, the star Antares itself being of a brilliant deep red; and there are traces of change in the angle and distance since 1849. With regard to another double star, α Centauri, the distance has little altered for a long time, but the angular motion is increasing.

Attention has also been recently directed to the star B, A, C, 3345, whose variability seems very great. In 1856 several attempts were made at Greenwich to observe it, amongst other moon-culminating stars set down in the Nautical Almanac for 1859, but in every instance without success. In its stead, 19 Leonis which precedes it by a few seconds of time, was observed four times; and a star which precedes 19 Leonis by ten seconds was observed twice, while nothing whatever was visible at the exact position of the star sought for. The observers reported the circumstance, and were directed to keep a vigorous watch for the missing star. In 1857–8 no difficulty in observing it has been reported, its appearance being that of the 7th magnitude. The star is identical with Lalande 19,197, who marks it as of the 9th magnitude. In Mayer's Catalogue, as revised by Mr. Baily (Mem. R.A.S. vol. iv.), it is No. 420. It is also identical with Piazzi IX. 176, and is marked of the

8th magnitude. In Taylor's Madras Catalogue it is marked of the 7th magnitude. Finally, in Argelander's *Uranometria Nova* it is called variable.

Prof. Payson, in a recent communication to the Royal Astronomical Society, describes a star in Libra (Right Ascension, 15h. 45m. and South Declination, 15° 49′) which suddenly appeared on the 3d of May, 1857, as of the 9·5 magnitude, on the 19th of the same month, as of the 11th magnitude, and on the 1st of June was no longer discernible. Its whole duration of visibility was therefore only about a month.

Mr. Baxendal, of England, has also called attention to the variability of the star 30 *Herculis.* In October and November, 1855, this star was nearly invisible to the naked eye, even in the finest night; and the mean of several comparisons with other stars, made at different times, gave its magnitude 5·9. It is now, however, a conspicuous star, and not less than 4·9 magnitude — the mean of several nights' estimations since June 18 being 4·85. Professor Argelander rated this star 5·6 magnitude, or less than *v.* 52 and 42 Herculis; but it is now brighter than any of these stars. On the other hand, it appears from the Radcliffe Observations of 1851 that it was of the 4·8—5·0 magnitude at the end of June and beginning of July of that year. It is very probable, therefore, that its light is subject to periodical changes; but the observations hitherto made are not sufficient to afford even a rough approximation to the length of the period. The red color which is so common amongst the variable stars (especially those of long period) is also very decided in this star.

OBSERVATIONS ON THE PLANET MARS.

Prof. Secchi, of Rome, under the date of July 19, forwards to Dr. Peters, of Altona, for publication in the Astronomische Nachrichten, a minute description of the surface of the planet Mars, together with two pictures of the planet taken with an interval of about one-third of a revolution on its axes. The spots seen and drawn by Captain Jacob, at Madras, in 1854, are seen in these representations also, and are therefore to be considered permanent, although there seems to be some confusion among those about the pole. On the other hand, a small round spot, portrayed by Mädler in 1830, has certainly disappeared. Any one, however, who will take the trouble to compare Secchi's drawings of the curious group of solar spots seen on successive days in March (14, 15 and 16) 1858, with a larger and better drawing of the same group accidentally made on one of the same days, March 15, by Schwabe, and both of them published by Dr. Peters, pages 236 and 342 of the *Astronomische Nachrichten,* will see how much depends on the quality of the telescope, the condition of the atmosphere, and the truth of eye and skill of hand of the observer, in determining these delicate tests of cosmical stability or instability in bodies so far beyond our reach. For Secchi's drawings would lead any one to put unhesitating faith in the popular theory that the spots of the sun are consequences of vortical or whirlwind movements in the equatorial belts of its atmosphere, so spirally has he drawn them, and so evidently have the little ones on each successive day advanced spirally a certain distance round the larger ones. Whereas, Schawbe's better drawing shows no such movement whatever, not a trace of it; but, on the contrary, a curiously cracked or shivered condition of each spot in the group, especially the larger ones, through the cracks in the even black surface of which the white light shines with much sharper edges than around the limit of the spot itself;

while the penumbras are cracked and gaped outward like old and wind-tossed palm leaves.

There is no certainty, therefore, that any but the principal spots on Mars are stationary. To reconcile the different drawings, it is quite necessary to suppose that the numerous white patches about the poles succeed each other rapidly, and therefore are more likely to be masses of storm clouds than increasing and decreasing areas of snow. The least agitation of the atmosphere makes the beautiful colors of the planet's disk grow pale and confused. The general surface is a monotonous continent crossed by an equatorial zone of red and temperate zones of blue, except in one place, where a large red island is surrounded by a blue channel. Toward the edges of the disk the red spots become yellowish, as if there were a martial atmosphere.

NEW DETERMINATION OF THE SOLAR PARALLAX.

The following communication by Lieut. J. M. Gillis, has been officially made to the Secretary of the Navy, under the date of Feb. 18, 1858:

I have the honor to communicate to you the results of the observations specially made by the United States Naval Astronomical Expedition to determine the solar parallax — the sun's distance from the earth.

It will be remembered by the Department that Dr. Gerling — an eminent geometer of Germany — suggested the practicability of determining this fundamental astronomical datum from observations of Venus near the inferior conjunction, instead of awaiting the rare phenomenon of transits of the planet across the sun's disk; that an expedition to the southern hemisphere was proposed to the Department by myself, for the purpose of making these observations, which, in connection with similar observations to be made at the Naval Observatory, would test the method; and that the earnest commendation of the measure by physicists, both in Europe and this country, induced Congress to authorize the expedition by special grants in the appropriation bills approved in 1848 and 1849.

We were absent from the United States nearly three and a half years, and the observations, constituting the more immediate object of the expedition, extended through parts of each of the years from November, 1849, to September, 1852, inclusive. So many classes of observations were embraced in the plan of operations adopted by the Department, that our small party was almost constantly occupied in observatory duty proper, and it was not possible to prepare any of the data for the final computations until after the return of the expedition to the United States. Then our first efforts were to put in proper form for the computer all the observations of the planets Venus and Mars, and the stars with which they had been compared. Whilst our men of science had been unanimous in advocating the organization of an expedition, because of the additional mass of important information certain to be collected by it, there were some who entertained an opinion that the method of determining the parallax proposed by Dr. Gerling would not afford a result as reliable as had been derived from the transits of Venus in 1761 and 1769. For obvious reasons, therefore, it was proper that the discussion of the results from our observations should be intrusted to an astronomer wholly uncommitted as to the comparative merits of the two methods.

Under the sanction of the Department, Dr. B. A. Gould, junior, of Cambridge, Mass., was selected for the purpose; and the result obtained by him for the Sun's Equatorial Horizontal Parallax is $8''{\cdot}4950$, or $0''{\cdot}0762$ less than the value commonly adopted; and he concludes that we may assume with advantage 8″·5000, corresponding to a distance from that luminary of 86,160,000 statute miles.

CHARTS OF THE ECLIPTIC.

Some time ago, on the proposition of Lalande, the Academy of Berlin undertook the first chart of the ecliptic. Their earliest reward was the discovery of the fifth small planet by Mr. Hencke, of Driessen. Toward 1847, Mr. Valz, of Marseilles, developed a plan, the execution of which would lead to the detection of all the planets in the zodiac. Mr. Chacornac, then studying astronomy with Mr. Valz, immediately applied himself to these new charts of the ecliptic. Two months afterwards, Sept. 1852, he discovered a new planet. England and Ireland engaged in the same direction; Hind and Cooper were soon to distance the French astronomers. However, Chacornac, who meanwhile had been attached to the observatory of Paris, continued his charts. They have just been published at the expense of that observatory. They will be more complete than the former, and quadruple the dimensions of those of Berlin, the scale being fifty millimetres for each celestial degree.

These charts contain all the stars of the twelfth magnitude and many of the thirteenth magnitude; they extend above and below the ecliptic to 5° of declination. Their form is square. The number of stars inscribed on them already exceeds 125,000.

OBSERVATIONS ON SOLAR SPOTS.

In a recent number of the "Transactions of the Royal Astronomical Society," Mr. R. C. Carrington gives a notice of his solar spot observations:

"During the past three years the surface of the sun has exhibited a comparative state of quiescence, the outbreak of spots being few and often far between, the spots themselves being for the most part small."

But the epoch of least action is now passed. From the observations made during 1855 and '56, which fix the date of minimum with some degree of certainty, it appears that the date of the minimum of energy, as exhibited by the spots, may be assigned with tolerable certainty to the beginning of the month of February, 1856; the ratios increasing slowly and perceptibly from that time. The year 1856 was characterized by the rather frequent occurrence of low south spots — the latitudes 27° and 35° south having been more than once visited. As a general remark, when an outbreak occurs on a parallel not previously affected for several months, it is mostly found that two or three other outbreaks succeed at moderate intervals of time — not, however, at the corresponding longitudes, when the rotation of the earth is allowed for; this being a subject for further investigation.

OCCULTATION OF A STAR BY A COMET.

By a communication from Dr. G. B. Donati, of Florence, published in the *Astronomische Nachrichten*, of June 30, it appears that this very rare phenomenon was observed by him on the 21st of April. Comets have several times been seen to pass over stars, but the light of the stars has usually been but little, if at all, diminished, even by the nucleus — on this occasion, however, it was otherwise. The star, however, was of the *twelfth* magnitude, or so small that it cannot ever be seen, except by the aid of a first-class telescope. The one employed by Dr. Donati was the great refractor of *Amici*, and had a clear aperture of eleven inches, with a focal length of seventeen feet. Dr. D. says:

"On the 21st of April, about 10h. 30m., I perceived that the centre of Brorsen's comet was about to pass exactly over a star of the twelfth magnitude, and therefore carefully observed the passage. Gradually, as the comet approached the star, the latter became fainter and badly defined, that is, its disk was no longer round, but hazy, as if the star was shining through mist. The diminution and diffusion of the stellar light increased as the comet approached still nearer, so that when the centre of the latter covered the star, it *entirely disappeared for about thirty seconds.* At the emersion, the star presented the same appearance as at the immersion, and it shone, when the nebulosity had entirely passed from over it, again perfectly round and well defined."

THEORY OF THE ASTEROIDS.

In a paper published in the *Comptes Rendus*, by M. Le Verrier, on the asteroids, that eminent astronomer shows by calculation that the sum of the mass of fragmentary planets, called asteroids, cannot exceed one-fourth of the earth's mass; and also shows it probable that their mean mass or system is at its perihelion, and consequently nearest the earth, at the time when the earth itself is on the side of the summer solstice. This, it is urged, is confirmatory of the theory that aërolites are the minute outriders of the asteroids.

CHANGES IN THE INTENSITY OF THE HEAT OF THE SUN.

In a memoir recently published in the contributions of the Smithsonian Institute, by L. W. Meech, "On the relative intensity of the Heat and Light of the Sun upon the different Latitudes of the Earth," the author deduces, among other results, that taking into view the fact that the obliquity of the ecliptic, 2000 years ago, in the time of Hipparchus, 128 B. C., was 23° 43′, and is 23° 27½′, and that therefore the sun then rose higher in summer than now, the summer heat of that period was two-tenths of a degree Fahrenheit hotter than that of the present, while the winter was the same amount colder.

ON THE ORIGIN OF HAIL.

In a discussion on the above subject at the last meeting of the British Association, Admiral Fitzroy said, that he had no doubt whatever that colder currents of air coming from the polar regions, and breaking by or mingling with the currents coming up from the equatorial regions, loaded with vapor, played a very important part in the phenomena. As it had been a question, whether hail was ever experienced in the inter-tropical parts of the earth, he could decidedly answer in the affirmative, as he had experienced several heavy hail-storms even within a few degrees on either side of the equator.

INFLUENCE OF THE MOON ON THE WEATHER.

At the Leeds meeting of the British Association, 1858, Mr. Harrison presented a paper "On the Influence the Moon exerts on Temperature," and he claimed that the following points must be regarded as established meteorological facts: 1. That the temperature before the first quarter is lower than that of the second day after it. 2. That this fall and rise prevails most in the winter months, and in the month of May. 3. That a reciprocity of action takes place between corresponding days of the moon's age. Thus, whilst it was found, both at Dublin and Greenwich, that for twenty-one consecutive years the mean temperature rose at the first quarter in more instances than it fell, it fell at the last quarter in more instances than it rose; and in the only two years in which a fall occurred instead of a rise at the first quarter, there was a rise instead of a fall at the last quarter. Between new and full moon this reciprocity of action was still more apparent. Here, for the same series of years, there was a fall in thirteen years after new moon, and a rise in thirteen years after full moon; and in five out of the eight instances in which a rise occurred instead of a fall at new moon, a fall instead of a rise took place at full moon. Also a like principle appeared to hold good in individual months. For example, in twenty-one consecutive Januarys a fall occurred in seventeen at new moon, while a rise took place in sixteen at full moon. The action thus apparent at different periods of the lunation was shown clearly in curves of temperature of each day of the moon's age. A curve of ten years' mean temperature at Greenwich, for 1837—1846, was exhibited in juxtaposition with one sent to the Dublin meeting, which was also formed of ten years' mean temperature, at the latter station, for 1847—1856. At first and last quarters the curves corresponded in a most remarkable manner at both stations. At new and full moon they alternated; the fall in the Dublin curve being at the new moon, and the rise at full moon; in the Greenwich curve the rise at new moon, and the fall at full moon. Leaving the consideration of daily mean temperatures, on extracting the maxima and minima mean temperatures for the month, it was found that more maxima occurred after first quarters than before; the proportion of maxima to minima, on the *second* day after that phase, being more than 2·1 both at Dublin and Greenwich, for the respective periods of twenty-two and forty-three years. The twenty-four highest and lowest maxima and minima in the month at Greenwich were then taken for the same forty-three years, forty-eight per cent. found to occur at first quarter, and *minima only* before the day of the change. Similar results were obtained from the highest and lowest mean temperatures at Dublin, and at Toronto from 1843 to 1848. Another point elicited during the progress of the inquiry, was the recurrence of high and low temperatures on the same days of the lunation. Taking first the maxima and minima mean temperatures for the month during twenty years at Greenwich — from 1837 to 1856 — the whole number found recurring on corresponding days (many of them three and four times in each period of twelve lunations), amounted to 236, averaging about twelve for each year, or half the maxima and minima for the month. To illustrate this recurrence of high and low temperatures, several years were selected, which presented the strongest evidence of system. Thus, in the two consecutive years commencing November 1847 and ending October 1848, maxima and minima occurred: — In 1847, twice on the third day before new moon; twice on the second day before new moon; three times on the day after new moon; twice on the third day after new moon; three times on the second day before full moon; twice on the third day after full moon. In 1848, three times on the day of new moon; twice on the day after new moon; three times on the second day before full moon; twice on the day before full moon; twice on the fourth octant, or fourth day, after full moon. In the same years there were also, amongst many others, the following remarkable instances of reciprocity between opposite phases of the moon: In December the minimum for the month occurred on the third day before new moon; in January the maximum on the third day before full moon; in February the minimum on the third day before new moon. And again, the maximum in September fell on the day after full moon. The minimum in October on the day after new moon. "In addition to this, the maxima and minima for the month were found to occur at intervals of rather more than seven, fourteen, or twenty-one days, and that for several successive months, viz., April, May, June, August, and September, and so in other years." In 1838, exactly ten years earlier, maxima or minima occurred three times on the third day after new moon; three times on the day after full moon; three times on the day of first quarter; and three times on the day of last quarter: that is to say, in twelve instances out of twenty-four *on four days of the lunation.* At the Cape of Good Hope, reciprocity of action and the recurrence of high and low temperatures were even more frequent and systematic. Thus, in 1855, eight out of the twelve maxima for the month occurred at first quarter, and nine of the twelve minima at new or full moon. In 1842, nineteen maxima and minima out of twenty-four, occurred on eight days. In 1843, fifteen on seven days; in 1844, seventeen on six days; in 1845, eleven on four days. The recurrence of maxima and minima at Toronto and Madras was equally marked. Mr. Harrison considered that the dispersion of clouds under full moon may now be taken as a fact, on the testimony of Humboldt, Sir J. Herschel, Mr. Johnson (the Radcliffe observer at Oxford), and others. Mr. Johnson having also noticed that this cloud-dispelling power commences about the fourth or fifth day of the moon's age, and lasts till she approaches the sun, the same distance on the other side; that is to say, the influence takes place at that time as well as at full moon, though not necessarily continuously. Mr. Nasmyth also, who was considered a valuable witness, from his long-continued observation of the moon for the purpose of mapping its surface, was quoted as having satisfied himself that clouds disappear when the moon is about four days old; and also, that when this is the case for any length of time at new moon, the sky is clouded to a corresponding extent at full moon, another instance of the principle of reciprocity. Several well-known observers were also mentioned, as having noticed the remarkable clearness of the morning of the 13th of September, or the fifth day after new moon. And lastly, even M. Arago's explanation of the popular notion among gardeners round Paris, that the moon which, commencing in April, becomes full in May, destroys their tender plants, it was thought might be quoted as evidence of lunar influence on the atmosphere, though given by him as a simple statement of the effects of terrestrial radiation on early vegetation. Mr. Harrison, in conclusion, expressed his belief that the remarkable regularity of the recurrence of a fall before first quarter, is due to the clearing of the atmosphere at that period, and the rise after first quarter to a more cloudy state of the sky. That the same effect is not so evident on the curves at the period of full moon, he considered might be due to the greater reciprocity of action which takes place at the syzygies, or new and full moon.

The President, Mr. Hopkins, observed, that the facts Mr. Harrison had adduced, must be considered strongly confirmatory of the view he so ably advocated. That the moon exercised an influence upon the weather, and particularly on the formation or dispersion of clouds, was, as all knew, a very generally prevailing opinion. The sailors even had a common saying, that the full moon cut up or devoured the clouds; and Sir John Herschel had somewhere admitted, that the nights about full moon, particularly at certain seasons of the year, were remarkably cloudless. This indirect influence, then, being admitted, it became more important to trace it, as Mr. H. was doing, to an influence upon the temperature.

Some years ago, Dr. Foster, an eminent meteorologist, of Bruges, announced to the English Astronomical Society that in weather journals kept by his grandfather, his father, and himself, from 1767 downwards, whenever the new moon fell on a *Saturday*, the following twenty days were wet and windy. The Society published this, the general idea being, that the statement had never been made known before. Since then, it has been found that the Saturday moon has this character even in popular rhymes, and that it is widely believed in among seamen, English, French, Spanish, and even Chinese.

A writer in the London *Athenæum*, after adverting to this circumstance, and stating that in the one instance, in which he had made observations, the theory appeared to be confirmed, makes the following suggestive remarks:

"Now here is a curious circumstance: the whole world has the notion widely scattered that a Saturday moon brings wet weather, and science has hardly the means of being positive in the negative. And this is only one such case; curious effects of the moon are in the popular belief by scores, and their is no refutation, except *à priori* — that is, no refutation at all.

"Every twenty-nine and a half days is divided into two periods, one of which has many times as much moonlight as the other. That the moonlight must have a great deal of heat when it leaves the moon, is highly probable; that it has none when it reaches the surface of the earth is certain. What then becomes of all the heat which it seems almost certain the moonlight brings with it? Sir John Herschel thinks that it is absorbed in the upper regions of our atmosphere; and that some probability is given to this supposition by the tendency to disappearance of clouds under the full moon: a fact observed by himself without knowledge of its having been noticed by any one else, and which Humboldt, he afterwards found, speaks of as well known to the pilots and seamen of Spanish America. If this theory be correct, there is a cause of weather cycles which must produce *some* effect; an enormous quantity of heat poured into the atmosphere during one half of the lunar month, and a very small quantity during the other half. In truth, it has been ascertained that the quantities of rain which fall in the four quarters of the moon are not quite the same in the long run.

But the popular mind gets hold of the question in a different way. It seizes upon the geometrical phenomena of the moon, nothingness, halfness, fulness, and makes the moments of these appearances the times at or very near which change of weather is to take place. According to the recognized old notions, it is enough if a change of weather takes place within three days one way or the other of a change, which gives twenty days every month in which a change is set down to the moon. No wonder this theory is often confirmed. The whole question of moonlight, — not position of the moon, — both as to its effects on the weather and its asserted effects on vegetable and animal life, is in the earliest infancy, so far as systematic observation is concerned. All that is said about it is mere infallibility."

ATMOSPHERIC MOVEMENTS. BY DANIEL VAUGHAN.

The remarkable uniformity in the course of the winds, throughout the greater part of the torrid zone, was ascribed by Dr. Halley to the effect of solar heat combined with the earth's diurnal motion. The air investing the equatorial regions, being rarefied by the excessive temperature which the sun imparts, is compelled to yield its place to the more cool and dense bodies of air which press from opposite sides along the surface of the land and water. Aërial currents are thus caused to flow from northern and southern localities to the equator, while others return along the upper atmospheric domain. In approaching the equator, they must pass over a part of the earth's surface having a more rapid motion than they could have acquired in the regions from which they came; and as they fail to partake of the increased velocity, an apparent westward deflection is the consequence; so that they become the north-east and south-east trade winds which prevail over a large portion of the tropical regions. But though correct in its main features, this theory fails to show why the trade winds do not extend beyond the thirtieth parallels of latitude, and that they are confined within a narrower limit on the Pacific Ocean, where the disturbances and obstructions which might arise from the presence of land, are almost entirely removed. The efficiency of the motive power which acts on the air, is proportional to the increase of mean temperature for a given diminution of latitude; and it is much greater in the temperate than in the torrid zone. It would, therefore, seem that the trade winds of the tropical oceans should prevail through a more extensive range, or that each of the temperate zones should have an independent circulation of regular winds, blowing from the north-east in our climates, and from the south-east in the southern hemisphere.

The want of a general and uniform atmospheric circulation in extra-tropical regions, must be ascribed to the motion of the earth around its axis. The centrifugal force arising from the rotation, not only keeps our planet expanded at the equator, and maintains a suitable covering of air and water between the tropics, but also lays some restrictions on the removal of matter to different localities. The principle on which this result depends, will be understood from a few considerations. Were the earth's diurnal motion suddenly reduced four per cent., there would be a diminution of eight per cent. in the centrifugal force of its parts; their equilibrium would require a difference of only twenty-four miles between equatorial and polar diameters, and much of the waters would remove from their tropical abodes to the vicinity of the poles. The air would also experience a similar movement, and it would rush northward with greater impetuosity, if the water were absent from our planet, or could be prevented from filling up the circumpolar basins. Now the strict uniformity which the earth exhibits in its rotation, cannot be preserved by every portion of its atmosphere. If a large body of air, situated between the 44th and 46th parallels of north latitude, were moving due westward at the rate of twenty-eight miles an hour, its actual velocity of rotation about the terrestrial axis must be about four per cent. less than that of the land and water over which it passed; and so great would be the reduction in its centrifugal force, that it must press towards the polar regions in the same manner that it would descend down an inclined plane with a fall of nine inches in every mile. Were the movement of the mass of air in an eastward direction, it would have an increased velocity of rotation about the earth's axis; and from its excess of centrifugal force, it would be steadily impelled to a more southern locality.

It is well known, that as any portion of our atmosphere is advancing towards the equator, it must be continually deflected to the west; but it now appears that this westward deflection must diminish centrifugal force, and create a tendency to return to a higher latitude. From this cause atmospheric movements in a northern or southern direction, meet with a resistance, which is proportional to the square of the sine of latitude multiplied by the distance to which the air has been removed from the parallel of repose, or from the part of the earth's surface having its velocity. Along the parallel of twenty degrees, the resistance would be only $\frac{1}{48000}$ part of the force of terrestrial gravity, for a change of two hundred miles in the polar distance; but it would be double that amount ten degrees further north, and be four times as great in latitude 45°; while it would be increased in an eight-fold proportion in the vicinity of the poles. To remove a cubic mile of air one degree from the north pole, would require about the same mechanical expense as the transportation of thirty-six cubic miles from the tenth to the ninth degree of north latitude.

Along the parallel of 60°, the influence of the earth's rotation would be sufficient to prevent an atmospheric circulation from extending beyond two degrees of latitude, by the heating power of the sun, if the effects of friction and other sources of irregularity were removed. A difference of two degrees in the latitude of two places on the same meridian, can rarely cause a greater difference than 3° F. in their mean temperature; and the air, by this change of temperature, must receive an additional expansion, amounting to $\frac{1}{160}$ part of its original volume. In a circulation confined to a range of two degrees, the air at the southern station must ascend with a force equal to $\frac{1}{160}$ of its own weight; but as this buoyancy must cease at an elevation of about three miles, and as it is expended in moving two columns of air each one hundred and thirty-eight miles in extent, the actual motive power operating on the entire aërial mass, must be less than the $\frac{1}{14700}$ part of the force of gravity. As the resistance arising from centrifugal force to such a circulation, in latitude 60° is $\frac{1}{11500}$ part of the force of gravity, it is evident that the movement cannot be maintained, except in places where the presence of the land gives the air considerable friction, and brings it to the velocity of the latitude at which it arrives. In the vicinity of the equator the resistance from the earth's rotation is very feeble, and hence the regular action of solar heat, which is so inefficient in high latitudes, is capable of giving the atmosphere an uninterrupted course of two thousand miles in the torrid zone.

If our day were double its present length, or if our atmosphere were four times as extensive as it now is, regular trade winds would pursue an uninterrupted course from the greater part of the temperate zones to the equator. Had the earth's axis been perpendicular to the plane of the ecliptic, our atmosphere, if secured from the effects of local disturbances, would exhibit a series of independent circulations, each being confined to a smaller range in proportion as it was near the pole. This seems to be the case with the planet Jupiter, whose belts are very wide about his equator, but become extremely narrow in high latitudes. From the position of Jupiter in our system, and his rapid rotation around his axis, we may reasonably infer that his atmosphere must be very extensive; for otherwise it could not be so sensitive to the motive power of solar heat.

On our own globe the disturbing causes are so numerous that the winds cannot manifest any degree of regularity, except in the extensive and vigorous circulation which prevails between the tropics. The unequal susceptibility of land and water to the solar heat, causes the alternating breezes which are felt on many sea coasts, and the high temperature which the sun imparts to deserts, serves also to bring our aërial ocean into a state of activity. But excessive showers of rain are attended with a still more effective cause of atmospheric commotions, according to a principle first pointed out by Professor Espy. The heat arising from the extensive condensation of vapor on these occasions, must rarefy the air so much as to give it an upward movement, and cause aërial currents to flow into the place where the greatest fall of rain occurs. Although this seems to be the chief source of the motive power which produces storms, yet there is reason to believe that the constant discharge of electricity along the moist air contributes, through the medium of electrical repulsion, to increase the rapidity of the aërial movement. The constant repulsion of the air from the discharge of its electricity, Dr. Hare regards as the chief cause of hurricanes.

In an article by Mr. Tracy, of Utica, published fifteen years ago, in Silliman's Journal, it has been shown that atmospheric movements of this kind, whether due to the action of heat or electricity, would acquire a rotation from the right hand to the left in this hemisphere; in consequence of the eastward deflection of the air rushing from the south, while that from the north is deflected to the west. But it appears, from the foregoing investigations, that the eastern and western aërial currents tend to maintain the same rotation, the former being deflected south on account of their excessive centrifugal force, while the latter from an opposite cause incline to the north. It may also be shown that, in the southern hemisphere, the rotation must be in an opposite direction. Dove's celebrated law of the rotation of storms is thus shown to be the necessary result of physical causes.

The orbital movement of storms appears to be an inevitable consequence of their rotary motion. As the air on the east side of the aërial vortex is advancing towards the pole, it must cool by the change of latitude; while on the west side the air is moving south, and has its capacity for holding vapor continually augmented by an increasing temperature. Accordingly, the focus of the rain and the storm would be constantly shifted to the north-east if the atmosphere had been previously in a state of repose; but between the tropics, the action of the trade winds must supersede the westward motion, and the whirling mass of air will be therefore transported in a north-east course in these regions. This accounts for the fact that all great storms commencing within the tropics, steadily advance towards the north and west, until their entrance into the temperate zone, when they soon change to the north-east, and continue in this course until they reach the very high latitudes.

While the regularity of the wind is much disturbed or wholly obliterated by these commotions, it is differently affected by the unevenness of the land. The friction arising from this cause tends to give the air the velocity of rotation of the places which it visits, and this prevents a retrograde movement from an excess or a deficiency of centrifugal force. The great atmospheric circulation depending on the regular action of the solar heat, must accordingly occupy a wider zone than our calculations would assign to it, and it is in consequence of the want of great friction from bodies of land, that the trade winds do not extend beyond the parallels of 25° in the Pacific Ocean, while they occupy a wider range in the Atlantic.

It might be naturally expected that the unequal temperature and density of the water in the tropical and polar seas should cause a great oceanic circulation; the cold currents pursuing their way towards the equator, at the lowest depths of the watery domain, while warm currents flow back along its surface. But from the limited expansibility of water by heat, the ocean is much less sensitive than air to solar influence; and were its bed perfectly smooth and its depth uniform, the action of temperature in producing northern or southern oceanic movements, would be much inferior to that of the earth's rotation in preventing them. The restraints of centrifugal force are, however, neutralized by the asperities of the ocean's bed; for the water rolling over them partakes of their diurnal velocity; and accordingly oceanic rivers take their journey towards the poles, in places where the roughness of the sea-bottom, or the presence of coasts or submarine ridges, prevent an eastward deflection. To such circumstances the Gulf Stream is indebted for its existence. A vast body of water impelled due north at the rate of three miles an hour in latitude 30° and unimpeded by friction, should be deflected 45° from the meridian in the course of twenty-four miles; and the small deflection of the Gulf Stream shows the influence of submerged mountains, in modifying its motion and enabling it to continue its advance to colder climates.

NOTE.—Many facts justify an extension of the theory of Espy. The force and extent of the trade winds must be augmented by the heat arising from the condensation of vapor in the "equatorial cloud ring," and the high temperature attending the great rains of Southern Asia, may be regarded as a partial cause at least of the southwest monsoon. Discharges of electricity during such rains, must also contribute to produce the same result. The inadequacy of the cause to which monsoons are usually ascribed, has been noticed by the most eminent writers who have treated on the subject.

OBITUARY

OF PERSONS EMINENT IN SCIENCE. 1858.

Barnston, James, Professor of Botany, McGill College, Toronto.

Biosoletto, Signor, an Austrian chemist and botanist.

Blyth, Dr. George L., an English chemist, well known for his labors in sanatory reform.

Bonpland, Aime, the well-known associate of Humboldt.

Brown, Robert, an eminent English botanist.

Brown, Samuel, an eminent Scotch chemist, and scientific essayist.

Cleveland, Parker, the distinguished American mineralogist.

Comstock, Dr. J. L , widely known as the author of many elementary scientific treatises.

Combe, Dr. George, of Scotland.

Cragin, Dr. F. W., a naturalist of Surinam, Parimaribo.

Deane, Dr. James, a well-known geologist, and discoverer of the foot-prints in the sandstone of the Connecticut Valley.

Duncan, J., Professor of Mathematics, St. Andrew's College, Scotland.

Eisenbeck, Kas Von, an eminent German naturalist.

Frazer, Lt. Col. Alexander, R. E.

Gregory, Dr. William, Professor of Chemistry, University of Scotland, and well known as a scientific author.

Griffiths, Mrs., of Torquay, England, a distinguished algologist.

Hale, Foster, inventor of raised letters for the blind.

Hare, Robert, the eminent American chemist.

Laradel, Count de, proprietor of the boracic acid works, of Tuscany, Italy.

Linaria, Father Sante, a distinguished Italian physicist.

Louden, Mrs. Jane, well known from her botanical writings.

Mareska, M., Professor of Chemistry, University of Ghent.

Muller, Johannes, the eminent German physiologist and naturalist.

North, Dr. Erasmus D., an American microscopist.

Orbigny, Alcide de, the eminent French geologist and paleontologist.

Pattinson, Hugh Lee, inventor of the silver-lead process.

Peacock, George, Dean of Ely, eminent as a mathematician.

Pease, Edward, of England, a friend and associate of George Stephenson in originating the railway system of Great Britain.

Pfeiffer, Madam Ida.

Platner, Prof., author of treatise on the blowpipe.

Ponton, M. de, of Stockholm, an associate of Berzetius, and a naturalist.

Reid, Sir William, well known for his investigations on the laws of storms.

Royle, Dr. J. Forbes, best known from his publications on the botanical productions of India.

Schlagintweit, Adolphe, a German naturalist and explorer, supposed to be murdered in Cashmere, Central Asia.

Shattuck, Dr. Lemuel, of Boston, Mass., a well-known statistician.

Snow, Dr. John, of London, distinguished for his researches on anæsthesia.

Temmick, Herr, a well-known ornithologist of Holland.

Travers, Dr. Benjamin, F. R. S., President Royal College of Surgeons.

Turner, Dawson, an English botanist

Tilesius, W. G., the naturalist of Krusenstein's (Russian) expedition.

Young, Ira, Professor of Mathematics, Dartmouth College, N. H.

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