THE

AMERICAN JOURNAL

OF

SCIENCE AND ARTS.

CONDUCTED BY

PROFESSOR SILLIMAN

AND

BENJAMIN SILLIMAN, Jr.

VOL. XLI.—~~OCTOBER~~, 1841.

(TO BE CONTINUED QUARTERLY.)

NEW HAVEN:

Sold by B. & W. NOYES.—*Philadelphia*, CAREY & HART, J. S. LITTELL and ORRIN ROGERS, No. 67 So. Second St.—*Baltimore, Md.*, N. HICKMAN. —*New York*, CARVILL & Co., No. 108 Broadway, THEODORE FOSTER, corner of John St. and Broadway, and G. S. SILLIMAN, No. 49 Wall St.—*Boston*, C. C. LITTLE & Co.—*London* and *New York*, WILEY & PUTNAM, No. 35 Paternoster Row, *London*, and 161 Broadway, *New York*.—*Paris*, HECTOR BOSSANGE & Co., No. 11, Quay Voltaire.—*Hamburgh*, Messrs. NESTLER & MELLE.

PRINTED BY B. L. HAMLEN.

CONTENTS OF VOLUME XLI.

NUMBER I.

NUMBER II.

ERRATA.

In Mr. Hodge's article on the tertiary of the Southern States, are the following mistakes.—For Cytherea reporta, passim, read Cytherea reposta.

" Plagiostoma palagicum, read P. pelagicum, pa. 337 l. 10 and 11.

" that the home-made, read and the home-made, pa. 341 l. 11 fr. bot.

" Venus mercinaria, read V. mercenaria, pa. 344 l. 7 fr. bot.

THE AMERICAN JOURNAL OF SCIENCE, &c.

Art. I.—*Second Letter to Prof. Faraday;* from R. Hare, M. D., Professor of Chemistry in the University of Pennsylvania.

Philadelphia, February, 1841.

TO PROF. FARADAY.

My dear Sir—In the month of July last I had the pleasure to read, in the American Journal of Science, your letter in reply to one which I had addressed to you through the same channel. I should sooner have noticed this letter, but that meanwhile I have had to republish two of my text-books, and, besides, could not command, until lately, a complete copy of all those numbers of your researches to which you have referred.

The tenor of the language with which your letter commences realizes the hope, which I cherished, that my strictures would call forth an amicable reply. Under these circumstances it would grieve me that you should consider any part of my language as charging you with inconsistency or self-contradiction, as if it could be my object to put you in the wrong, farther than might be necessary to establish my conception of the truth. Certainly it has been my wish never to go beyond the sentiment, "*Amicus Plato, sed magis amica veritas.*" I attach high importance to the facts established by your "*Researches,*" which can only be appreciated sufficiently by those who have experienced the labor, corporeal and mental, which experimental investigations require. I am moreover grateful for the disposition to do me justice, manifested in those researches; yet it may not always be possible for me to display the deference, which I nevertheless entertain. I am aware that when in a discussion, which due attention to brevity must render unceremonious, diversities of opinion are exhibited, much magnanimity is requisite in the party whose opinions are assailed; but I trust that both of us have truth in view above all other objects, and that so much of your new doctrine as tends to promote that end, will not be invalidated by a criticism which though free, is intended to be perfectly fair and friendly.

In paragraph (ii,) your language is as follows, "*my theory of induction makes no assertion as to the nature of electricity, nor at all questions any of the theories respecting that subject.*" Owing to this avowed omission to state your opinions of the nature of electricity as preliminary to the statement of your "*theory*," and because I was unable to reconcile that theory with those previously accredited, I received the impression that you claimed no aid from any imponderable principle. It appeared to me that there was no room for the agency of any such principle, if induction were an *action* of contiguous ponderable particles, *consisting* of a species of polarity. It seemed to follow, that what we call electricity, could be nothing more than a polarity, in the ponderable particles, directly caused by those mechanical or chemical frictions, movements, or reactions by which ponderable bodies are electrified. You have correctly inferred that I had not seen the fourteenth series of your researches, containing certain paragraphs. From them it appears that the polarity, on which so much stress has been laid, is analogous to that which has long been known to arise in a mass, about which the electric equilibrium has been subverted, by the inductive influence of the electricity accumulated upon another mass. This is clearly explained in paragraph iv of your letter, by the illustration, agreeably to which three bodies, A, B, C, are situated in a line, in the order in which they are named, in proximity, but not in contact. "A is electrified positively and then C is uninsulated." It is evident that you are correct in representing that under these circumstances the extremities of B will be oppositely excited, so as to have a reaction with any similarly excited body, analogous to that which takes place between magnets; since the similarly excited extremities of two such bodies, would repel each other; while those dissimilarly excited, would be reciprocally attractive. Hence no doubt the word polarity is conceived by you to convey an idea of the state of the body B. If I may be allowed to propose an epithet to convey the idea which I have of the state of a mass thus electrified, I would designate it as an electropolar state, or as a state of electropolarity.

It does not appear to me that in the suggestion of the electropolarity which we both agree to be induced upon the body B, (iv,) so long as it concerns a mass, there is any novelty. The only part of your doctrine which is new, is that which suggests an analogous state to be caused in the particles of the bodies through which the inductive power is propagated. Admitting each of the particles of a dielectric, through which the process of ordinary induction takes place, to be put into the state of the body B, it does not appear to me to justify your definition of electrical induction. I conceive that consistently with your own exemplification of that process, you should have alleged ordinary induction to be *productive* of an *affection* of particles *causing* in them a species of polarity. In the case of the bodies, A, B, C, (iv,) B is evidently passive. How then can we consider as active, particles represented to be in an analogous state? If in B there is no action, how can there be any action in particles performing a perfectly similar part? Moreover, how can the inductive power of an electrical accumulation upon A, *consist* of the polarity which it induces in B?

Having supposed (viii,) an electrified ball, A, an inch in diameter, to be situated within a thin metallic sphere, C, of a foot in diameter, you suggest that were one thousand concentric metallic spheres interposed between A, and the inner surface of C, the electropolar state of each particle in those spheres would be analogous to that of B already mentioned. Of course if there be an action of those particles, there must be an action of B; but this appears to me not only irreconcilable with any previously existing theory, but also with your own exposition of the process by which B is polarized.

Supposing concentric metallic hemispheres were interposed only upon one side of A, you aver that agreeably to your experience, more of the inductive influence would be extended towards that side of the containing shell than before, (xiv.) Admitting this, I cannot concede that the greater influence of the induction, resulting from the presence of the metallic particles, is the consequence of any *action* of theirs; whether in *contiguity* or in *prox-*

imity. Agreeably to my view, the action is confined to the electrical accumulation in the sphere A. Between the electricity accumulated in this sphere, and that existing in, or about, the intervening ponderable particles, there may be a reaction; but evidently these particles are as inactive as are the steps of a ladder in the scaling of a wall.

Suppose a powerful magnet to be so curved as to have the terminating polar surfaces parallel, and leaving between them an interval of some inches. Place between these surfaces, a number of short pieces of soft iron wire. These would of course be magnetized, and would arrange themselves in rows, the north and south poles becoming contiguous. Would this be a sufficient reason for saying that the inductive influence of the magnetic poles was an *action* of the contiguous wires? Would not the phenomena be the consequence of an affection of the contiguous pieces of wire, not of their action?

As respects the word charge, I am not aware that I have been in the habit of attaching any erroneous meaning to it, as your efforts to define it in paragraph iii would imply. I have been accustomed to restrict the use of it to the case which you distinguish as an inductive charge, illustrated by that of the Leyden jar. To designate the states of the conductors of a machine, I have almost always employed the words *excited* or *excitement.* In my text-book, these words are used to designate the state of glass or resin electrified by friction, while that of coated surfaces, whether panes or jars, inductively electrified, has been designated by the words *charge* or *charged.*

I understood the word contiguous to imply contact, or contiguity, where, as it seems that it was intended by you to convey the idea of proximity. In the last mentioned sense it is not inconsistent with the idea of an action at the distance of half an inch: but by admitting the word contiguous to be ill chosen, you have, with great candor, furnished me with an apology for having mistaken your meaning.

Any inductive action which does not exist at sensible distances, (xx,) you attribute to *ordinary* induction, considering the case of induction through a vacuum as an *extraordinary* case of induction. To me it appears that the induction must be the same in both cases, and that the *circumstances* under which it acts, are those which may be considered in the one case as *ordinary*, in the other, *extraordinary*. Thus, take the case cited in your reply, (viii, ix, x.) Does the interposition of the spheres alter the character of the inductive power in the sphere A?

Either the force exercised by the charge in A, is like that of gravitation, altogether independent of the influence of intervening bodies; or, like that of light, it is dependent on the agency of an intervening matter. Agreeably to one doctrine, the matter, by means of which luminous bodies act, operates by its transmission from the luminous surface to that illumined. Agreeably to another doctrine, the illuminating matter operates by its undulations. If the inductive power of electrified bodies be not analogous to gravitation, it must be analogous to the power by which light is produced so far as to be dependent on intervening matter. But were it to resemble gravitation, like that force it would be uninfluenced by such matter. If your experiments prove that electrical induction is liable to be modified by intervening matter, it is demonstrated that in its mode of operation it is analogous to light, not to gravitation. It is then proved, that, agreeably to your doctrine, electrical induction requires the intervention of matter, but you admit that it acts across a vacuum, and of course, acts without the presence of *ponderable* matter. Yet it requires intervening matter of some kind, and, since that matter is not ponderable, it must of necessity be imponderable. When light is communicated from a luminous body in the centre of an exhausted sphere, agreeably to the undulatory hypothesis, its efficacy is dependent on the waves excited in an intervening imponderable medium. Agreeably to your electropolar hypothesis, the inductive efficacy of an electrified body in an exhausted sphere would be due to a derangement of electric equilibrium, by which an electric state opposite to that at the centre would be produced at the surface of the containing sphere, (xxvi, xxvii.) This case you consider as one of extraordinary induction, but when air is admitted into the hollow sphere, or when concentric spheres are interposed, you hold it to be a case of ordinary induction. Let us then, in the case of the luminous body, imagine that concentric spheres of glass are interposed, of which the surfaces are roughened by grinding. In consequence of the roughness thus produced, the rays instead of proceeding in radii from the central ball would be so refracted as to cross each other. Of the two instances of illumination, thus imagined, would the one be described as *ordinary,* the other as *extraordinary radiation?* But if these epithets are not to be applied to radiation, wherefore under analogous circumstances are they applicable to induction? Wherefore is induction when acting through a plenum to be called ordinary, and yet when acting through a vacuum to be called extraordinary? In the well known case of the refracting power of Iceland spar, light undergoes an *ordinary* and *extraordinary refraction;* not an *ordinary* and *extraordinary radiation.* The candle, of which, when viewed through the spar, two images are seen, does not *radiate ordinarily* and *extraordinarily.*

If there be occasionally, as you allege, (xxi,) large intervals between the particles of radiant heat, how can the distances between them resemble those existing between particles acting at distances which are not sensible. The repulsive reaction between the particles of radiant caloric, as described by you, (xxi,) resembles that which I have supposed to exist between those of electricity; but I cannot conceive of any description less suitable for either, than that of particles which do not act at sensible distances.

Aware that the materiality of heat, and the Newtonian theory, which ascribes radiation to the projection of heat or light producing particles, have been questioned, I should not have appealed to a doctrine which assumes both the materiality of heat, and the truth of the Newtonian theory, had not you led the way; but, agreeably to the doctrine and theory alluded to, I cannot accord with you in perceiving any similitude between the processes of conduction and radiation.

Consistently with the hypothesis that electricity is material, you have shewn that an enormous quantity of it must exist in metals. To me it seems equally evident that, agreeably to the idea that heat is material, there must exist in metals a proportionably great quantity of caloric. The intense heat produced when wires are deflagrated by an electrical discharge, cannot otherwise be consistently accounted for. Agreeably to the same idea, every metallic particle in any metallic mass, must be surrounded by an atmosphere of caloric; since the *changes* of dimension consequent to variations of temperature, can only be explained by corresponding variations in the quantity of caloric imbibed, and in the consequent density of the calorific atmospheres existing in the mass which undergoes these changes.*

Such being the constitution of expansible bodies, agreeably to the hypothesis in question, it seems to me that the process, by which caloric is propagated through them by *conduction*, must be extremely different from that by which it is transmitted from one part of space to another by *radiation*. In the one case the calorific particle flies, like a cannon ball, with an inconceivably greater velocity, which is not sensibly retarded by the reflecting or refracting influence of intervening transparent media: in the other case it must be slowly imparted from one calorific atmosphere to another, until the repulsion sustained on all sides is in equilibrio. It is in this way that I have always explained the fact that metals are bad radiators, while good reflectors.†

---

* I subjoin the language which I have held respecting the constitution of expansible solids, during the last twenty years.

"The expansion of matter, whether solid, liquid, or aëriform, by an increase of temperature, may be thus explained.

"In proportion as the temperature within any space is raised, there will be more caloric in the vicinity of the particles of any mass contained in the space. The more caloric may abound in the vicinity of the particles, the more of it will combine with them; and in proportion to the quantity of caloric thus combined, will they be actuated by that reciprocally repellent power, which, in proportion to its intensity, regulates their distance from each other.

"There may be some analogy between the mode in which each ponderable atom is surrounded by the caloric which it attracts, and that in which the earth is surrounded by the atmosphere; and as in the latter case, so probably in the former, the density is inversely as the square of the distance.

"At a height at which the atmospheric pressure does not exceed a grain to the square inch, suppose it to be doubled and supported at that increased pressure by a supply of air from some remote region; is it not evident that a condensation would ensue in all the inferior strata of the atmosphere, until the pressure would be doubled throughout, so as to become at the terrestrial surface, 30 lbs. instead of the present pressure of 15 lbs.? Yet the pressure at the point from which the change would be propagated, would not exceed two grains per square inch. In like manner, it may be presumed that the atmospheres of caloric are increased in quantity and density about their respective atoms, by a slight increase in the calorific tension of the external medium."

† I will here quote the rationale which has been given in my lectures for the last twenty years. "Metals appear to consist of particles so united with each other, or with caloric, as to leave no pores through which radiant caloric can be projected. Hence the only portion of any metallic mass which can yield up its rays by radiation, is the external stratum.

"On the other hand, from its porosity, and probably also from its not retaining caloric within its pores tenaciously as an ingredient in its composition, charcoal opposes but little obstruction to the passage of that subtile principle, when in the radiant form; and hence its particles may all be simultaneously engaged in radiating

In paragraph (xxv,) you allege that conduction of heat differs from electrical induction, because it passes by a very slow process; while induction is in its distant influence simultaneous with its force at the place of action. How then can the passage of heat by conduction, be "*a process precisely like that of radiation*," (xxi,) which resembles induction in the velocity with which its influence reaches objects, however remote?

Although, (xxi,) you appeal to the "modern views respecting radiation and conduction of heat," in order to illustrate your conception of the contiguity of the particles of bodies subjected to induction, yet in (xxv,) you object to the reference which I had made to these views, in order to shew that the intensity of electropolarization could not be inversely as the number of particles interposed between the "inductric" surfaces. Let us then resort to that above suggested, of the influence of the poles of a magnet upon intervening pieces of iron wire. In 1679, 14th series, you suggest this as an analogous case to that of the process of *ordinary* electrical induction, which we have under consideration. Should there be in the one case a thousand pieces of wire interposed, in the second an hundred, will it be pretended that the intensity of their reciprocal inductive reaction would be inversely as the number; so that the effect of the last mentioned number of wires would be *equivalent* to that of the first? Were intervals to be created between the wires by removing, from among the number first mentioned, alternate wires, it would seem to me that the diminution of effect would be commensurate not only with the reduction of the number of the wires, but likewise with the consequent enlargement of the intervals.

---

any excess of this principle with which a feeble affinity may have caused them to be transiently united, or in receiving the rays emitted by any heated body, to the emanations from which they may have been exposed.

"We may account, in like manner, for the great radiating power of earthen ware and wood.

"For the same reason that calorific rays cannot be projected from the interior of a metal, they cannot enter it when projected against it from without. On the contrary, they are repelled with such force as to be reflected without any perceptible diminution of velocity. Hence the pre-eminence of metallic reflectors.

"It would seem as if the calorific particles which are condensed between those of the metal, repel any other particles of their own nature which may radiate towards the metallic superficies, before actual contact ensues; otherwise, on account of mechanical imperfections, easily discernible with the aid of a microscope, mirrors would not be as efficacious as they are found to be in concentrating radiant heat. Their influence, in this respect, seems to result from the excellence of their general contour, and is not proportionably impaired by blemishes."

If as you suggest, the interposition of ponderable particles have any tendency to promote inductive influence, (xiv,) there must be some number of such particles by which this effect will be best attained. That number being interposed, I cannot imagine how the intensity of any electro-polarity, thus created in the intervening particles, can, by a diminution of their number, acquire a proportional increase; evidently in no case can the excitement in the particles exceed that of the "inductric" surfaces whence the derangement of electrical equilibrium arises.

The repulsive power of electricity being admitted to be inversely as the squares of the distances, you correctly infer that the aggregate influence of an electrified ball, B, situated at the centre of a hollow sphere, C, will be a constant quantity, whatever may be the diameter of C. This is perfectly analogous to the illuminating influence of a luminous body situated at the centre of a hollow sphere, which would of course receive the whole of the light emitted whatever might be its diameter, provided that there were nothing interposed to intercept any portion of the rays. But in order to answer the objection which I have advanced, that the diminution of the density of a "*dielectric*" cannot be compensated by any consequent increase of inductive intensity, it must be shown in the case of several similar hollow spheres, in which various numbers of electrified equidistant balls should exist, that the influence of such balls upon each other, and upon the surfaces of the spheres, would not be directly as the number of the balls and inversely as the size of the containing spaces. Were gas lights substituted for the balls, it must be evident that the intensity of the light, in any one of the spheres, would be as the number of lights which it might contain. Now one of your illustrations (viii,) above noticed makes light and electrical induction, obey the same law as respects the influence of distance upon the respective intensities.

From these considerations, and others above stated, I infer, that if electrical induction were an action of particles in proximity operating reciprocally with forces varying in intensity with the squares of the distances, their aggregate influence upon any surfaces, between which they might be situated, would be proportionable to their number; and since experience demonstrates that the inductive power is not diminished by the reduction of the number of the intervening particles, I conclude that it is independent of any energy of theirs, and proceeds altogether from that electrical accumulation with which the inductive change is admitted to originate.

In paragraph (xxxi,) you say "that at one time there was a distinction between heat and cold. At present that theory is done away with, and the phenomena of heat and cold are referred to the same class, and to different degrees of the same power."

In reply to this I beg leave to point out, that although, in ordinary acceptation, cold refers to relatively low temperature; yet we all understand that there might be that perfect negation of heat, or abstraction of caloric, which may be defined absolute cold. I presume that, having thus defined absolute cold, you would not represent it as identical with caloric. For my own part this would seem as unreasonable as to confound matter with nihility.

Assuming that there is only one electric fluid, there appears to me to be an analogy between caloric and electricity, so far that negative electricity conveys, in the one case, an idea analogous to that which cold conveys in the other. But if the doctrine of Du Fay be admitted, there are two kinds of electric matter, which are no more to be confounded than an acid and an alkali. Let us, upon these premises, subject to further examination your argument (1330,) that insulation and conduction should be identified, "*since the moment we leave in the smallest degree perfection at either extremity, we involve the element of perfection at the opposite end.*" Let us suppose two remote portions of space, one, replete with pure vitreous electricity, the other with pure resinous: let there be a series of like spaces containing the resinous and vitreous electricities in as many different varieties of admixture, so that in passing from one of the first mentioned spaces, through the series to the other, as soon as we should cease to be exposed to the vitreous fluid, in perfect purity, we should begin to be exposed minutely to the resinous, or that, in passing from the purely resinous atmosphere, we should begin to be exposed to a minute portion of the vitreous fluid; would this be a sufficient reason for confounding the two fluids, and treating the phenomena to which they give rise as the effect of one only?

But the discussion, into which your illustrations have led me, refers to things, whereas conduction and insulation, as I understand them, are opposite and incompatible properties, so that, in as much as either prevails, the other must be counteracted. Conduction conveys to my mind the idea of *permeability* to the electric fluid, insulation that of *impermeability;* and I am unable to understand how these irreconcilable properties can be produced by a difference of degree in any one property of electrics and conductors.

If, as your infer, glass have, comparatively with metals, an almost infinitely minute degree of the conducting power, is it this power which enables it to prevent conduction, or in other words to insulate? Let it be granted that you have correctly supposed conduction to comprise both induction and discharge, the one following the other in perfect conductors within an inexpressibly ' rief interval. Insulation does not prevent induction; but, so r as it goes, it prevents discharge. In practice this part of the process of conduction does not take place through glass during any time ordinarily allotted to our experiments, however correct you may have been in supposing it to have ensued before the expiration of a year or more in the case of the tubes which you had sealed after charging them. But conceding it to have been thus proved that glass has, comparatively with metals, an infinitely small degree of the conducting power; is it this minute degree of conducting power, which enables it to prevent conduction, or in other words to insulate?

Induction arises from one or more properties of electricity, insulation from a property of ponderable matter; and although there be no matter capable of preventing induction, as well as discharge, were there such a matter, would that annihilate insulation? On the contrary would it not exhibit the property in the highest perfection?

As respects the residual charge of a battery, is it not evident that any electrical charge which affects the surface of the glass, must produce a corresponding effect upon the stratum of air in contact with the coating of the glass? If we place one coating between two panes, will it not enable us to a certain extent to charge or discharge both? Substituting the air for one of them, will it not, in some measure, be liable to an affection similar to that of the vitreous surface for which it is substituted? In the well known process of the condensing electromoter, the plate of air interposed between the disks is, I believe, universally admitted to perform the part of an electric, and to be equivalent in its properties to the glass in a coated pane.

When I adverted to a gradual relinquishment of electricity by the air to the glass, I did not mean to suggest that it was attended by any more delay than the case actually demonstrates. It might be slow or gradual, compared with the velocity of an electric discharge, and yet be extremely quick, comparatively with any velocity ever produced in ponderable matter. That the return should be slow when no coating was employed, and yet quick when it was employed, as stated by you, (xxxviii,) is precisely what I should have expected; because the coating only operates to remove all obstruction to the electric equilibrium. The quantity or intensity of the excitement is dependent altogether upon the electrified surfaces of the air and the glass. You have cited (1632,) the property of a charged Leyden jar, as usually accoutered, of electrifying a carrier ball. This I think sanctions the existence of a power to electrify by "convection," the surrounding air to a greater or less depth; since it must be evident that every aërial particle must be competent to perform the part of the carrier ball.

Agreeably to the Franklinian doctrine, the electricity directly accumulated upon one side of a pane repels that upon the other side. You admit that this would take place were a vacuum to intervene; but when ponderable matter is interposed, you conceive each particle to act as does the body B when situated as described between A and C, (iv.) But agreeably to the view which I have taken, and what I understand to be your own exposition of the case, B is altogether passive, so that it cannot help, if it does not impede the repulsive influence. Moreover it must be quite evident, that were B removed, and A approximated to C, without attaining the striking distance, the effect upon C and the consequent energy of any discharge upon it from A, would be greater instead of less. If in the charge of a coated pane the intermediate ponderable vitreous particles have any tendency to enhance the charge, how happens it that, the power of the machine employed being the same, the intensity of the charge which can be given to an electric is greater in proportion to its tenuity?

In reference to the direction of any discharge, it appears to me that as, in *charging*, the fluid must always pass from the cathode to the anode, so in reversing the process it must pursue, as I have alleged, the opposite course of going from the anode back to the cathode. Evidently the circumvolutions of the circuit are as unimportant as respects a correct idea of the direction, as their length has been shown by Wheatstone, to be incompetent to produce any perceptible delay.

The dissipation of conductors being one of the most prominent among electrical phenomena, it appears to me to be an objection to your theory, if, while it fails to suggest any process by which this phenomenon is produced, it assumes premises which seem to be incompatible with the generation of any explosive power. If discharge only involves the restoration of polarized ponderable particles to their natural state, the potency of the discharge must be proportionable to the intensity of the antecedent polarity; yet it is through conductors, liable, as you allege, to polarization of comparatively low intensity, (xxxi,) that discharge takes place with the highest degree of explosive violence.

Having inquired how your allegation could be true, that discharge brings bodies to their natural state and yet causes conductors to be dissipated, you reply (xxxiv,) that different effects may result from the same cause acting with different degrees of intensity; as when by one degree of heat ice is converted into water, by another into steam. But it may be urged, that although in the case thus cited, different effects are produced, yet that the one is not inconsistent with the other, as were those ascribed to electrical discharges. It is quite consistent, that the protoxide of hydrogen which *per se* constitutes the solid called ice, should by one degree of calorific repulsion have the cohesion of its particles so counteracted as to be productive of fusion; and yet that a higher degree of the same power should impart to them the repulsive quality requisite to the aëriform state. In order to found upon the influence of variations of temperature, a good objection to my argument, it should be shown, that while a certain reduction of temperature enables aqueous particles to indulge their innate propensity to consolidation, a still further reduction will cause them, in direct opposition to that propensity, to repel each other so as to form steam.

In your concluding paragraph you allege, "that when ponderable particles intervene, during the process of dynamic induction, the currents resulting from this source do require these particles." I presume this allegation is to be explained by the conjecture made by you (1729) that since certain bodies when interposed did not interfere with dynamic induction, therefore they might be inferred to cooperate in the transmission of that species of inductive influence. But if the induction takes place without the ponderable matter, is it right to assume that this matter *aids* because it does not prevent the effect? Might it not be as reasonably inferred in the case of light, that although its transmission does not require the interposition of a pane of glass, yet that when such a pane is interposed, since the light is not intercepted, there is reason to suppose an active cooperation of the vitreous particles in aid of the radiation? It may be expedient here to advert to the fact that Prof. Henry has found a metallic plate to interfere with the dynamic induction of one flat helix upon another. I have myself been witness of this result.

Does not magnetic or electrodynamic induction take place as well in vacuo as in pleno? Has the presence of any gas been found to promote or retard that species of reaction? It appears, that agreeably to your experiments, ponderable bodies, when made to intervene, did not enhance the influence, in question; while in some of those performed by Henry it was intercepted by them. Does it not follow that ponderable particles may impede, but cannot assist in this process.

I am happy to find, that among the opinions which I expressed in my letter to you, although there are several in which you do not concur, there are some which you esteem of importance, though you do not consider yourself justified in extending to them your sanction; being constrained, in the present state of human knowledge, to hold your judgment in suspense. For the present, I shall here take leave of this subject, having already so extended my letter as to occupy too much of your valuable time. I am aware that as yet I have not sufficiently studied many of the results of your sagacity, ingenuity, and skill in experimental investigations. When I shall have time to make them the subject of the careful consideration which they merit, I may venture to subject your patience to the additional trial resulting from some further commentaries. I remain, with the highest esteem, respectfully yours,

ROBERT HARE.

Art. II.—*On the Magnetical Dip in the United States;* by John Locke, M. D., Prof. Chem. and Pharm., in the Medical College of Ohio.

TO THE EDITORS.

In reply to the article by Prof. Loomis, in Vol. xxxix of your Journal, permit me to observe by way of narration, that the correspondence between him and myself, originated in the following manner. Prof. Loomis being about to republish a chart of the magnetism of the United States, requested as a favor, that I would furnish him with some of the results of my observations. I cheerfully complied, by sending him the principal ones. When his paper appeared, I saw my results marked "in error" to a considerable amount. Upon examination, I found that the expression "in error," meant merely that the observations did not agree with an assumed hypothesis, by the amount noted. I objected to this mode of expressing differences, chiefly because it would mislead popular readers, and with them would do me discredit as an observer. My paper on that subject in your Journal, Vol. xxxviii, will be found at every point, complimentary to Prof. Loomis. The reader will please to observe that I have not made a single remark on Prof. L.'s own observations. I should have felt great delicacy, especially in undertaking to criticise or point out any errors or faults which I might suppose attended them. This is a task which I conceive belongs to the observer himself, after he shall have made all of the comparisons necessary for the labor, and shall have ascertained all of the circumstances possible, which may explain the real or apparent anomalies. A second person may misapprehend many things which the observer himself could have rectified, or explained instantly, and may thus produce injury of reputation, and of feeling, by hastily publishing to the world, a supposed discovery of errors, where none ever existed. I have merely sought that my own observations should be presented to the popular reader in such a manner that he should not misapprehend them to my discredit. Prof. Loomis will certainly grant that the notes of an unfinished labor of mine were communicated to him, with a tacit understanding to that effect. I did not in my last paper, presume that he had intended to use them in any other manner, than that which should be to my honor.

In the paper before me, Prof. Loomis makes a distinct announcement of the "hypothesis" in the following words: Are these differences to be regarded as errors of observation, or as errors of the *hypothesis of parallel, straight, and equidistant isoclinal lines?** That is *the* question, which would have given a clear commencement to the subject. Had the reader been advised that it was upon grounds purely *hypothetical*, that my observations were pronounced in error, it was all I desired. Had Prof. Loomis stopped at this point, the whole thing would have been satisfactory to me; but in the latter part of his last paper, he has made a special *effort* to discredit my observations. I have great dislike to even the *appearance* of controversy, especially in such a journal as yours, and were it not that some instruction may arise to your readers, I would suppress the following remarks. I have been drawn into the subject unintentionally, by an attempt to serve a friend.

In the first place, I question the above quoted "hypothesis" itself. It is certainly anti-Baconian, to assume an hypothesis, and then require observed facts to be stretched out, or cut off, by + or −, until they agree with the assumption. We propose first to consider the consistency of the hypothesis itself, as used by Prof. Loomis, in his last paper, and next to reply to his special pleadings, to sustain that hypothesis, in which he endeavors to discredit the facts observed by me. In the first group of observations, consisting of mine and his together, he "adopts as" a central position, "lat. 41° 22′, long. 84° 54′," and by the proper formulæ, determines "the direction of the isoclinal lines* to be N. 80.1′ W." "*Computing* from these data, the dip at several stations," he obtains the so called "errors of observation." Here we have first the general "hypothesis," and secondly an assumed "central position."

In the second group of observations, consisting of those made by myself in the region of the Mississippi, the central position adopted, is "lat. 42° 00′ N. long. 90° 10′ W." By the formulæ used above, he determines the direction of the isoclinal lines in this group, to be "N. 65° 01′ W." Again, from these data, he *computes* the dip, and determines the so called "errors of observation." Agreeably to these *calculations*, the lines of equal dip,

---

* Lines of equal dip.

in the two groups, have bearings differing 19° 53′ from each other. What will be the bearings of those lines in the intermediate region? Ought the two lines to be produced, the one from Iowa, and the other from Ohio, until they meet? The *two* would not certainly form *one* "*straight line*," which is called for by the hypothesis. Or, ought there to be an intermediate "central position adopted," and a new bearing of the isoclinal line determined? This would likely give us a polygon. Now "adopt," or assume an infinite number of "central positions," and calculate as many bearings of the isoclinal or equal dip lines, and the result will be *curves*, precisely what I contend for. But this is contrary to the "hypothesis," which calls for "straight lines." Here might be a very great modification of the "computed errors of observation." My own opinion, founded on observations of considerable extent, is that the lines do proceed in curves which, in the present state of our knowledge, can be traced only by observation. The hypothesis that the magnetical forces proceed in lines, less tortuous in their course than observations would seem to indicate, is one by no means of recent date; for it is found in the philosophical writings of the Baron Swedenborg, together with a very specific account of the sources of error, in making observations. The appearance of mathematical calculation, especially under the symbols of modern analysis, is very imposing to many a popular reader, who may not reflect that all mathematical results are dependent on the *data*, the *assumptions*, the *hypotheses;* if these be false or erroneous, the superstructure is but a wreath of mist. Grant the data, and the calculations in these times, are but a schoolboy's task.*

On page 87, Prof. Loomis observes in reference to my observations, as treated in his previous paper, "at Prairie du Chien, the discordance is more considerable. The difference [from the hypothesis] I now find is −7′.3; in my former paper −20′. The discordance is owing in part to the *curvature I ascribed* to the isoclinal lines, by which most of the observations seemed best represented, though the apparent error of this observation was increased." All this is perfectly candid. He admits a curvature in the "straight lines," and that the lines are those of his own creation, subject to be *changed* at pleasure; not the isoclinal

* It is evident from the manner in which Prof. L. uses the hypothetical straight lines, that they were not really the lines of equal dip, but that they were tangents to those lines.

lines marked by the Architect of the Universe. My observations were intended to be tried by no other standard than the last.

I come next to Prof. Loomis's remarks against the truth of my observations.

On page 88, he doubts whether I have attended to reading both ends of the dipping needle, so as to avoid the error "arising from the eccentricity of the axis of the needle in relation to the vertical circle, on which the readings are made." He says, "this error in my instrument, commonly amounts to one or two minutes, and sometimes to even five or more. It is corrected by reading at both extremities of the needle." From my saying that I determined the dip by 8 distinct readings, he thinks I "implied that I did not attend to the above precaution." This is all *naivete* enough. I regret that Prof. Loomis should have formed so low an estimate of my skill, as to suppose I would use a graduated circle, where it was possible to obtain the mean of two opposite readings without doing so,—a thing so obvious that I deemed it unnecessary to say any thing about it. My needle No. 1, when properly adjusted, shows no eccentricity, but reads alike at both extremities. No. 2, has almost uniformly a difference of 5 minutes at the opposite ends. Both ends of both needles were always observed. If such be called a separate reading, then 16 should be substituted for 8 in the quotation above, and my readings should all be counted double the number which I have assigned to them.

The next paragraph is upon "the uncertainty of the readings themselves." I should not have known how to reply to that expression, but he subsequently becomes more specific. "The error arising from friction on the axis," he says, "is exhibited in Prof. Locke's observations in a *striking light.*" I find by the subsequent remarks that he refers to an observation at Davenport, as published in Vol. xxxix, of this Journal, in which there was an erratum, which had been the subject of correspondence between the Junior editor and myself. This erratum was the result of transcription. The amount of the several items, was correctly written, but one of the items which in the field-book reads 72° 55′ had been transcribed 72° 05′. The Junior editor discovered the discrepancy between the items and their amount, and very naturally corrected the latter instead of the former, at the same time kindly addressing a note to me on the subject, which, as I was absent, I did not receive. It went to press in the erroneous form, and has been used by Prof. Loomis to show the imperfections of my observations in a "striking light." It appears, on page 89, first as −42′, second corrected to −46′.5, and again, "By far the greater error here, is −46′.5, which I obtained from the first observation." Here the whole affair is of no account, because the *datum* on which the calculation was founded, had no real existence. The same observation has been corrected by myself, and was reprinted in the January number for 1841, page 150. This typographical error should have been detected, as such, from its inconsistency with the other items of the same group.

Prof. Loomis assumes in the next paragraph, that the differences of the readings with the face of the compass *east*, and with it *west*, if they exceed a certain constant quantity by him calculated, and called "twice the zero error of the instrument," are "errors of observation," and upon this assumption, makes two tables of errors consisting of 88 items each, at the head of which stands the "*typographical error.*" Here Prof. Loomis has fallen into the prevailing sin of mathematicians, the hasty assumption of *data*, which being granted, they can kill an army "by computation." The proper zero error of the instrument arises from a want of exact adjustment of the spirit level to the zero of the graduated circle, and would be a *constant quantity*. There is also another source of error, arising from want of exact adjustment of the agate planes on which the pivots of the needle are supported; both of which errors are more or less merged by the reversal of the instrument, as they would be plus with the face of the instrument in one position, and minus with it in the opposite position. Now this last error is a *variable* one, being dependent upon the total magnetic force, at any place, which force is *variable.* Probably Prof. Loomis intended by "zero error" the conjoint effect of the relation of both the zero of graduation and of the agate planes to the spirit level. A nice calculator, especially when he is pointing out the errors of other people's labors, should have made a distinction between them, for one is a constant quantity, and the other a variable one. But Prof. L. has treated the whole as a constant. He has also assumed mechanical perfection in the parts of the instrument in contact, the pivots and the agate planes, a condition which never exists in fact. These vacillations in the various reversals which Prof. Loomis has tabulated, were of course known to me, and were always compared during the observations. They were apparent on the face of the published results and must be apparent by a *coup d'œil* to every professional observer. Why then such a labored table of them? But they all amount to nothing, *literally they amount to nothing*; for in my mode of manipulating, adopted since Sept. 1839, they disappear from the "mean results;" like the zero error they are mostly corrected by the reversals. I could point out what I conceive to be the cause of these vacillations, and the mode in which I have succeeded in merging them; but I have neither time nor an inclination at present, to go into details on these points. I had intended to publish the result of my experience, but the duty to which I am here called, is very far from that of being an instructor of others. I certainly ought not to be forward in volunteering my services in that capacity.

The evidence which convinces me of the correctness of the mean results of my observations, is of the most popular kind; a sort of evidence which every body can understand, equally applicable to moral and to physical snbjects, entirely independent of *hypothesis* and *assumptions* of any kind, and as ancient as the pentateuch. It is simply the agreement of two independent physical witnesses to the same result, under repeated and varied trials. The dip is twice taken, at each place, by a mean of 16 readings of two separate needles. Out of 16 pairs of mean results, at different places, published in the last number of this Journal, there were but two, where the separate needles disagreed more than 1′ of a degree. What is common sense to infer from this? that there is an error of "30 minutes" in any one of them, both the needles taking a fancy, or conspiring, to agree to the same falsehood. Prof. Loomis will not dispute the facts above stated, and we submit the case, whether they do not authorize the conclusion which I have drawn, "that the results are true to within 2 or 3 minutes."

Prof. Loomis has taken no notice of this part of the evidence of the accuracy of the mean results of my observations. What must I infer from this?*

Lastly, Prof. Loomis cites a discrepancy between the dip at Cincinnati, as observed by me in 1837, and the same observed more recently, as proof that my observations are erroneous. I had myself published the fact of that discrepancy, and had it un-

* Should the two needles each have pivots of any other form than that of the cylinder and be alike, and alike placed in reference to the axis of the needle, they might agree in an erroneous indication. But such a concurrence can scarcely happen.

der consideration. Prof. L. will observe that I have attached no uncommon accuracy to my mean results, until the observation at Davenport in Sept. 1839, when I changed my mode of using the instrument. The observation of 1837 was the second one ever made by me, the instruments had imperfections since remedied, and the probability is, that the compass had accidentally been turned out of the meridian, a thing specially guarded against in all subsequent researches.

Prof. Loomis mentions the "disheartening anomalies" which he meets with in his own researches, and makes some remarks concerning his own observations. He will perceive by the rules which I have prescribed to myself, that I can say nothing upon either of those topics.

RECAPITULATION.

1. Prof. Loomis has marked the results of my observations in error, by an assumed hypothesis, *admitted to be such, by himself.*
2. He has thought it "implied" that I did not read both ends of the dipping needle, and that the results were therefore not entitled to so much confidence. *This is not true.*
3. He has found a discrepancy in an item of an observation, at Davenport, as published in Vol. XXXIX, of this Journal. *This turns out to be a typographical error.* Prof. L. calculates that the item *ought to have been* 54.1 minutes. The item *actually was* 55 minutes.
4. He has tabulated the small vacillations in the parts of an observation amounting at most to 8′.75 as "errors of observation." *These errors correct each other, and nearly disappear in the mean results.*

Art. III.—*Observations on the Melanians of Lamarck;* by S. S. Haldeman.

Lamarck included the genera Melania, Melanopsis, and Pirena, in this family, without having had it in his power to examine the animals. Férussac unites Pirena to Melanopsis, because he has examined P. atra, Lam. preserved in spirits, and finds that it does not differ from Melanops buccinoidea and costata, which he described from living individuals. Rang takes the same view, and places Melanopsis between Cerithium and Planaxis. Cuvier admits Melanopsis and Pirena, placing them after Melania and Rissoa; and before Acteon (Tornatella) and Pyramidella. Deshayes (Lam. vol. VIII, p. 488,) appears inclined to unite the three genera, for besides placing part of Pirena in Melanopsis, he unites the remainder to Melania, into which he believes Melanopsis can be merged by the gradual disappearance of its conchological characters. Thus there is a great difference of opinion upon these genera, and much of the confusion can be traced to the fact that the characters of the animal of Melania, instead of being taken from an undoubted species, (as M. subulata or Virginica,) have been drawn from Pirena aurita, Mül.; which, although it approaches Melania in its conchological characters, is generically distinct; so that MM. Deshayes' and Rang's generic characters of the former, apply to the latter alone. Pirena aurita, (figured as Melania aurita, by Rang, on pl. XII of Guerin's Mag., 1832,) is represented with quite a long foot, which is much extended beyond the mouth, and tapering posteriorly; the tentacles are not annulated, the head is scarcely proboscidiform; and the edge of the mantle is fringed—("ayant ses bords découpés."—Desh.) Melania, on the contrary, has the following characters, drawn from M. Virginica.

Animal with a truncated proboscidiform head, bearing two annulated tentacles, upon an enlargement of the outside basal portion of which the eyes are situated, but never beyond the middle of the tentacle; the mouth is provided with a double row of file-like teeth on each side; the foot is oval, not extending beyond the muzzle, slightly thickened, and of medium size; edge of the mantle continuous and simple. The exposed parts are colored with blackish lines upon a yellowish ground, which run transversely across the rostrum and tentacles.* Oviparous.

Thus it will be perceived that this genus has scarcely any essential point in common with those mollusca from the rivers of West Africa, of which P. aurita may be considered the representative. Melania is more like Paludina, but the distinctions are sufficient to place them in different, though adjoining families. An animal with a fringed mantle can scarcely enter the family of Melanians proper, so that P. aurita and its congeners, must be placed in another, of which this is, perhaps, the most tangible character; namely, the CERITHINAE.

I have never seen a living Melanopsis, but as Deshayes places Anculosa prærosa in this genus, and as Férussac's description of the animal applies pretty well to Anculosa, I will take it for

* MELANIA, pl. I, fig. 1—2, of my Monograph of the N. American fluviatile univalves.

granted that the two are nearly allied, and discuss the question of the identity of Melanopsis with Melania. If they are connected, it must be by means of Anculosa, which stands between both. The head, neck, and foot of the Melaniæ, are protruded to a considerable extent; they inhabit rivers, in running water, and are continually moving from place to place, but they are not found in the ripples or more rapid parts of the stream. The Anculosæ, on the other hand, live attached to stones and rocks in the most rapid waters; they are of sedentary habits, seldom moving, except to leave the water occasionally, by climbing up a wet rock. The animal extends but little beyond the shell, as it would be liable to receive injuries from the rapidity of the current; the foot is very small, discoidal in shape, and adapted to enable the animal to hold with great tenacity. They are thus separated from Melania by habit and structure, and a short shell is necessary, to prevent them from being forced from their position by the current; which would of course, have a greater hold upon a long shell. On this account I think it probable that Pirena atra bears the same analogy to the typical Melanopsides, that Melania does to Anculosa; and as these differences could not be well distinguished in dead and contracted specimens, the fact that Férussac referred the former to Melanopsis proper, is of little account. I accordingly adopt this species as the type of the genus Pirena, as Lamarck and Cuvier have done; and retain it among the Melanians, because the mantle is not fringed, but merely sinuated according to the outline of the aperture, as described by Férussac.

"Pirena aurita" is not congeneric with Pirena (atra) nor Melania, but must be placed in the family Cerithinæ; next perhaps to Potamis. The type is distinguished from the typical Melaniæ by the tubercles, and the sinuated labrum; and it cannot be separated from its American representatives, Melania undulata, Say, and similar shells, which, although the characters are less highly developed, have been characterized as distinct by Rafinesque and Swainson. The former author asserts that the animals are distinct from Melania, but the shells of several intermediate species appear to indicate a connection, and if the soft parts present a similar change, there will be some difficulty in pointing out the extent of the genus; and indeed, to separate the Melanians from the Cerithinæ. If 'Pirena aurita' be placed in the former, the distinctions between the two families, must be looked for in the animal of Potamis.

Art. IV.—*Short notices of American Fossil Fishes;* by W. C. Redfield, Mem. Yale Nat. Hist. Soc., &c.

[Received March 15, 1841, and read before the Yale Nat. Hist. Soc. April 28.]

The limited attention which is given by naturalists to the fossil fishes of the United States, is probably owing to the somewhat rare occurrence of these fossils in our rock formations. Hitherto, they have been found chiefly in the red sandstone formations of Massachusetts, Connecticut and New Jersey.

The earliest notices of these fossils appear to have been given by the late Dr. Mitchill, Prof. Hitchcock, and Dr. Dekay. At a later period, some imperfect specimens and drawings of American species received the notice of Prof. Agassiz: to whose invaluable labors this department of science is so greatly indebted. Two species from the Connecticut sandstone have been noticed by my son, Mr. John H. Redfield, in the Annals of the New York Lyceum of Natural History. The existence of fossil fishes in the rocks which overlie the bituminous coal deposit near Richmond, in Virginia, had also been casually noticed, in the American Journal of Science. More recently, it has been made known that these fossils are also found in the red sandstone of New Jersey.*

With the partial exceptions above stated, there appears to have been no attempts to characterize or describe these interesting fossils. I venture, therefore, to notice and designate, provisionally, the several species which, within a few years past, have fallen under my observation.

All the species hitherto found in the above named formations are distinguished, like other ancient fishes, by angular or rhomboidal scales covered with bony enamel: and hence they belong to the order *Ganoides*, in the arrangement of Agassiz; a living type of which is found in the *Esox osseus* or bony pike of our southern and western waters. They are also included in the family *Lepidoides;* and are referable to at least two distinct genera.

* Remains of one or two species of Holoptychus have been found recently in the *old* red sandstone beneath the coal measures, at Blossburg in Pennsylvania. Mr. Conrad has specified the *H. nobilissimus.* See Am. Jour. of Science, Vol. xxxviii, p. 89.

Genus I. Palaeoniscus, of Agassiz.

In this genus the dorsal fin is found opposite the center of the interval between the ventral and anal fins; and many species have been described by Agassiz, in his great work on fossil fishes. An imperfect specimen from this country he has designated as the *P. fultus;* referring to the stout character of the fins and their insertions. But this character is also found to pertain, in a greater or less degree, to all the known American species of this genus, and would perhaps warrant their separation from the *Palaeonisci*. The raylets or armatures attached to the anterior margins of the several fins are also strong, few in number and of unequal length and inclination. The scales, and apparently, the vertebræ, are prolonged into the upper lobe of the tail; but to a more limited extent, than in the European species of the genus.

1. *Palaeoniscus fultus: Agassiz.*—The specimen figured by Prof. Agassiz, is destitute of the dorsal and head, as well as the upper portion of the body. The length was probably about four and a half inches; but this is often exceeded in other specimens. The fins and their bony insertions appear stouter than in the *P. latus*, but less stout than in some other species.*

Found at Westfield,† Middlefield,† and Durham, in Connecticut; Sunderland, in Massachusetts; and Boonton, in New Jersey.

2. *Palaeoniscus latus: J. H. Redfield.*—Broad Palaeoniscus. The common length of this species is from four to five inches; and its width from one and a half, to two and a quarter inches. It is figured in the Annals of the New York Lyceum of Natural History, Vol. IV.

Found at Westfield, Middlefield, and Durham, Ct., and Boonton, N. J.

3. *Palaeoniscus macropterus: W. C. R.*—Long finned Palaeoniscus. This species is distinguished by the longitudinal extension of the dorsal and anal fins; which thus seem to present a remote resemblance to the wings or forked tail of the common swallow. Its length is commonly from five to seven inches; and its width from one and a half, to two inches.

---

* Another specimen shown in the drawings of Prof. Agassiz, was copied from one of the early and imperfect figures published by Prof. Hitchcock.

† Westfield and Middlefield are outlying districts or parishes of tho township of Middletown, in Connecticut.

Found at Westfield, Middlefield, and Durham, Ct.; Sunderland, Mass.; and Boonton, N. J.

4. *Palaeoniscus Agassisii: W. C. R.*—Large Palaeoniscus. To this, which is the largest of the American species yet discovered, I propose to affix the name of this distinguished naturalist. Its length, in the specimens hitherto obtained, varies from seven and a half, to ten inches, and its width from three to four inches. The fins, with their armatures and insertions, are also of more remarkable thickness than in the species already noticed. The large scales or plates which belong to the anterior portion of the dorsal line, are commonly found doubled together at their lateral edges, by the incumbent pressure, which gives them the appearance of short spines, or flattened rays; and hence these are sometimes mistaken for an anterior comb-like dorsal.

The same appearance is found, not unfrequently, in some of the smaller species above noticed, and in two of the figures which were published by Prof. Hitchcock, these narrowed and erected scales are made to appear as a fringe-like dorsal fin. Prof. Agassiz has been led to place one of the figures thus drawn, in his genus *Eurynotus*, under the name of *E. tenuiceps*. The specimen thus figured, was probably a *P. latus*, or perhaps belonged to another undescribed species.

5. *Palaeoniscus ovatus: W. C. R.*—Wide, or round-shaped Palaeoniscus. This species is shorter than *P. Agassisii*, and exceeds all the known American species in the comparative width or roundness of its form; and is also remarkable for the large size of its scales. It is of rare occurrence, and owing probably to its great thickness, is seldom obtained in a perfect form. This fossil also exhibits the spine-like erections of the dorsal scales which have been noticed above.

Found at Westfield and Middlefield, Ct.; Sunderland, Mass.; and Boonton, N. J.

Genus II. Catopterus, of J. H. Redfield.*

This genus is characterized by the backward position of the dorsal fin, which is nearest the caudal extremity and opposite to the anal fin. It is also distinguished by the articulated and delicate form of the rays which compose the several fins. The anterior margins of the latter are fringed with numerous small ray-

* Described in the Annals of the New York Lyceum of Nat. Hist. Vol. iv.

lets; which present a finely serrated appearance. The scales and vertebra appear to extend into the upper lobe of the tail, but in a more limited degree than appears in most of the older fossil fishes of Europe. The following species may be assigned to this genus.

1. *Catopterus gracilis: J. H. R.*—Larger Catopterus. The length of this species varies from near seven to nine and a half inches; its width is from one and a half to near two inches. The dorsal fin is small, nearly triangular, and is placed opposite the center of the broad and flowing anal fin. The pectoral fins are of an elongated form, and are strengthened on the anterior margin by one or two large and partially flattened rays, to the front of which, the fringe of fine raylets is attached. Owing to this peculiarity of structure, the smallest section of the pectoral fin will often serve to identify this species.

Found at Westfield, Middlefield, Durham, and Southbury, Ct.;* Sunderland, Mass.; and Boonton, N. J.

2. *Catopterus macrurus: W. C. R.*—Large finned or Virginia Catopterus. This beautiful species is distinguished by its broad and flowing fins; of which the anal is so extended as to be nearly joined to the caudal fin. The latter is finely extended. The length of this species is from four to five inches; its width from one and one eighth, to one and three eighth inches. The fringes of raylets on the anterior margins of the fins are remarkably fine and beautiful. The posterior margins of the scales seem to be curled slightly outward, giving the surface a somewhat roughened appearance.

Found in Chesterfield county, Virginia; twelve miles from Richmond. Parts of near twenty specimens were found on a single piece of the rock which was presented to the New York Lyceum, the extreme length of which did not exceed twelve inches.

3. *Catopterus anguilliformis: W. C. R.*—Eel shaped Catopterus. This remarkable species, as hitherto found, is from seven to near ten inches in length; width from half to three fourths of an inch. It has a finely forked and extended caudal fin of delicate structure; a well extended dorsal; and all the fins are fring-

* The *Palaeonisci* are also found at Southbury, in the red sandstone basin of the Housatonic valley, as appears from some fragments obtained by Mr. J. H. Redfield; but no distinguishable specimens have come under my observation.

ed with the fine raylets which pertain to this genus. The impressions of the fins are, usually, but faintly visible; owing, probably, to their delicate structure. The scales are equally indistinct; and the impression of the head is seldom visible.

Found at Westfield and Middlefield, Ct.; Boonton, N. J.; and, as I have been informed, at Sunderland, Ms.

4. *Catopterus parvulus: W. C. R.*—Little Catopterus. This small and delicate fossil is but obscurely developed in the few specimens which have been obtained. The extremely fine spread caudal and other fins; with their slender frontal raylets; serve to mark it as a member of the genus: although these raylets are fewer in number and of greater and more unequal length than in the other species. In the few specimens obtained, the caudal extremity is commonly found in a bent or half twisted position.

Found at Middlefield, Ct.; Sunderland, Ms.; and Boonton, New Jersey.

In view of the paucity of organic remains in the red sandstone rocks of New England and New Jersey, geologists will appreciate the value of these fossils, as indicating the comparative age or identity of the formations in which they are found. The rocks containing these fossils, also exhibit peculiarities of stratification, dislocation and lithological appearance, as well as a similarity in other small but undetermined fossils, which tend to establish the cotemporaneous character of these formations.

New York, February 3, 1841.

Art. V.—*Contributions to English Lexicography;* by Prof. J. W. Gibbs.

No. II. Account of some American Indian Words found in English.*

Cacao, (probably an American Indian word;) the chocolate-tree, a species of the *Theobroma*, a native of the West Indies.

Cacique, (from the native Mexican and Guatemalan languages through the French; comp. Span. *cacique*, Port. *cacico*, Fr. *cacique;*) the title of the native chiefs in Mexico, Guatemala, and perhaps other parts of America, at the time of the conquest by the Spaniards.

---

* For No. I. see Vol. xxxiii, p. 324.

Calumet, (thought by Dr. Duponceau to be derived from Fr. *chalumeau*, a reed; but more probably an American Indian word, see the quotation from a French writer below;) an Indian pipe for smoking tobacco, the accepting of which, with proper ceremonies, was a sign or symbol of peace and friendship, and the rejection of it, a symbol of war.

"Ils [les savages] ont comme les autres l'usage de la pipe, qu'ils appellent *calumet*."—*Father G. Marest in Lettr. Edif.* iv. 21.

Cariboo, (probably an American Indian word;) an animal of the stag kind.

Chocolate, (Mexican, *chocolatl*, probably connected with *cacao*, which see;) a paste or cake composed of the kernel of cacao, with other ingredients.

Hackmatack, (thought by Dr. Dwight to be an American Indian word,) the popular name of the red larch, *Pinus microcarpa*.

Hommoc, (thought by Dr. Webster to be an American Indian word;) a hillock or small eminence of a conical form.

Hommony, (Powhatan, *homony*, broken maize;) maize hulled and broken, but coarse, prepared for food by being mixed with water and boiled. Hence a vulgar comparison in many parts of this country "as coarse as hommony."

Inca, or *Ynca*, (Peruvian, *king* or *lord*, comp. Choctaw, *ingka*, father;) an appellation given to a dynasty of Peruvian kings, and to the princes of the blood, before the conquest of that country by the Spaniards, and to the descendants of these kings since.

Maize, (probably an American Indian word;) Indian corn, or the native corn of America, *Zea Mays*.

Moccason, (a word derived from the Massachusetts Indians, but found in most of the languages of the Algonkin-Lenape family; as, Knistinaux, *moscasin;* Algonkin and Chippeway, *makisin* and *maukisin;* Scoffi, *masteshun;* Souriquois, *mekezen;* Abenaki, *mkessen;* Massachusetts, *mohkissonah;* Narraganset, *mocussinass;* Mohican, *mkissin;* Delaware, *maksen;* Nanticoke, *meckissius;* Pamptico, *moggison;* Powhatan, *mockasin;* Miami, *m'kasiu;* Illinois, *mahkissina;* Menomeni, *maukahshen;* some of which have a plural termination;) an Indian shoe or covering for the foot.

Mohawk or *Mohock*, (the native name of one of the Iroquois tribes of Indians;) an Indian of the Mohawk tribe; by extension, a ruffian in the streets of London.

Moose, (a word derived from the Algonkin-Lenape family of languages; as, Narrag. *moosquin*, a fawn; *moose*, the skin of the red deer; Miami, *musuoh* or *mohsokeh*, a deer; Illinois, *mousoah*, a deer;) an animal of the genus *Cervus*.

Mush, (according to Dr. Webster from Germ. *mus*, pap; perhaps a corruption of the Indian word *maize;*) the meal of maize boiled in water.

Netop, (Narraganset, *netop*, a friend;) a friend or crony, said by Dr. Pickering to be used in some of the interior towns in Massachusetts.

"What cheares, nétop, is the general salutation of all the English towards them, [the Indians.]"—*Roger Williams*, 1643.

Papoose, (Massachusetts, *pappouse;* Narrag. *papoos;* Mohican, *papoose;*) an Indian child.

Potato, (Amer. Ind. *batatas;*) a plant and esculent root, of the genus *Convolvulus*, a native of America.

Powwow, (Massachusetts, *powwow;* Narrag. *powwaw;*) an Indian priest, exercising also the offices of physician and conjurer.

Quahaug, (thought by Dr. Webster to be an Amer. Ind. word;) a species of clam.

Sachem, (a word derived from the Massachusetts Indians, but found in many of the languages of the Algonkin-Lenape class; as Knistinaux, *okemow;* Chippeway, *ogima* or *okimau;* Ottawa, *okemah;* Algonkin, *okimaw;* Abenaki, *sangman;* Etchemin, *sockum;* Massachusetts, *sachem* or *sagamore;* Narraganset, *sachim;* Delaware, *sakima;* Shawno, *okema;* Menomeni, *okomow;*) an Indian chief or prince.

Sagamore, (merely another form of the word *sachem*, which see;) according to Dr. Dwight, an inferior sachem, but probably synonymous with sachem.

Sagoin, (probably an American Indian word;) an animal of the genus *Simia*.

Samp, (Massachusetts, *nasampe;* Narraganset, *nasaump;* see the quotation from Roger Williams below;) maize boiled with milk.

"*Nasaump*, a kind of meale pottage, unpartch'd. From this the English call their *samp*, which is the Indian corne, beaten and boild, and eaten hot or cold, with milke or butter, which are mercies beyond the *Natives* plaine water, and which is a dish exceedingly wholesome for the English bodies."—*Roger Williams*, 1643.

Sapajo, (probably an American Indian word;) an animal of the genus *Simia*, found in America.

Squash, (according to Richardson, a pumpkin which is easily *squashed*, but more probably an American Indian word, comp. Narraganset, *askutasquash*, a vine apple; see the quotation below;) a plant of the genus *Cucurbita*.

"*Askutasquash*, a vine apple. Which the English from them [the Indians] call squashes about the bignesse of apples of severall colours, a sweet, light, wholesome, refreshing."—*Roger Williams*, 1643.

Squaw, (a word derived from the Massachusetts Indians, but found in most languages of the Algonkin-Lenape family; as, Knistinaux, *esqui;* Ottowa, *uque;* Algonkin, *ickweh;* Sheshatapoosh, *sehquow;* Abenaki, *naukskoue*, girl; Massachusetts, *squau* or *eshqua*; Narraganset, *squaws;* Mohican, *peesquasoo*, girl; Long Island, *squah;* Delaware, *okhqueh* or *khqueu;* Nanticoke, *acquahique;* Illinois, *ickoe;* Shawno, *equiwa;* Sauki, *kwyokih;*) an Indian woman.

Succotash, (an American Indian word, Webster;) food of green maize and beans boiled together.

Tobacco, (Haytian, *tabaca*, a pipe for smoking;) the name of various species of the *Nicotiana*.

Tomahawk, (an Algonkin-Lenape word; as, Micmac, *tomehagan;* Abenaki, *temahigan;* Massachusetts, *togkunk;* Mohican, *tumnahecan;* Delaware, *tamahicun;*) an Indian hatchet.

Tomato, (thought by J. Thomson to be an American Indian word;) the love-apple, *Solanum lycopersicum*, originally from S. America.

Wampum, (a contraction of Massachusetts, *wampumpeage*, Indian money, thought to be connected with Massachusetts, *wompi*, white, or Iroquois, *wampum*, a marine shell;) shells or strings of shells used by the Indians for money.

Wigwam, (an Algonkin-Lenape word; as, Knistinaux, *waskyegun;* Chippeway, *wikiwam;* Ottawa, *wigwauk;* Algonkin, *wikiwam;* Micmac, *wigwom;* Abenaki, *wigwam;* Massachusetts, *wikwam;* Mohican, *weekuwuhm;* Delaware, *wiquoam;* Miami, *wikameh*; Illinois, *ouitiame*; Shawno, *wiggewoam;* Menomeni, *weekeewaum;*) an Indian hut or cabin.

Yankee, (according to Heckewelder, Indian, *Yengees*, a corruption of the name *English*, which the Indians applied to the people of New-England;) an inhabitant of New-England.

Art. VI.—*Origin of the Names of Beasts, Birds, and Insects;* by Prof. J. W. Gibbs.

The later investigations in comparative philology, it is thought, enable us to give, with more precision and plausibility than has been usual, the origin of the names of certain animals, and at the same time to throw light on the origin of common nouns generally.

The names of beasts, birds, and insects, are formed

I. By derivation;

1. From the verbal root, by change of vowel or internal inflection merely; as,

Ape, (Anglo-Sax. *apa*, Germ. *affe*, Old Germ. *affo;*) from Old Germ. √ *av* or *af*, to imitate; as if *the imitator.*

Buck, (Germ. *bock*, Old Germ. *pocch;*) from the root of Old Germ. *puhhan*, to thrust, and Eng. *to poke*; as if *the thruster.*

Bull, (Germ. *bull*;) from the root of Germ. *bellen*, to bark, Anglo-Sax. *bellan*, to roar, and Eng. *to bell*, (to cry as a hart,) or *to bawl;* as if *the roarer.*

Chough, (Anglo-Sax. *ceo*, Fr. *choucas* and *chouette;*) from the root of Eng. *to caw* or *to haw;* as if *the cawer* or *hawer.*

Cow, (Sansc. *go*, Germ. *kuh*, Old Germ. *chua;*) from the root of Germ. *kauen*, Old Germ. *chiuwan*, and Eng. *to chew* or *to chaw;* as if *the chewer* or *chawer*, that is, *the ruminator.*

Crab, (Gr. *κάραβος*, Lat. *carabus*, Anglo-Sax. *crabba;*) from the root of Anglo-Sax. *creopan*, and Eng. *to creep;* as if *the creeper.*

Crow, (Germ. *krähe*, Anglo-Sax. *crawe;*) from the root of Germ. *krähen*, Anglo-Sax. *crowan*, to crow, croak, and Eng. *to crow*; as if *the croaker.*

Duck, from the root of Eng. *to duck;* as if *the plunger.*

Flea, (Lat. *pulex*, Germ. *floh*, Anglo-Sax. *flea;*) from the root of Germ. *fliehen*, Anglo-Sax. *flean*, and Eng. *to flee;* as if *the fleer.*

Fly, (Germ. *fliege*, Anglo-Sax. *fleoga*;) from the root of Germ. *fliegen*, Anglo-Sax. *fleogan*, and Eng. *to fly;* as if *the flyer.*

Fox, (Germ. *fuchs*, Old Germ. *vuhs;*) from Old Germ. √ *vu*, yellow; as if *the yellow-colored.*

Frog, (Anglo-Sax. *froga* or *frocga*, Germ. *frosch*, Old Germ. *vrosc*;) from the root of Eng. *to frisk;* as if *the leaper.*

Hare, Anglo-Sax. *hara*, Germ. *hase*, Sansc. *sasa;*) from Sansc. √ *sas*, to spring; as if *the springer.*

Hen, (Germ. *henne* and Old Germ. *henna*, fem. of Germ. *hahn* and Old Germ. *hano*, a cock;) from Old Germ. √ *han*, (=Lat. √ *can*,) to sing; as if *the singer.*

Horse, (Old Germ. *hros*, Germ. *ross;*) from Sansc. √ *rêsh*, to neigh; as if *the neigher.*

Hound, (Sansc. *swan*, Gr. *κύων*, Lat. *canis*, Goth. *hund;*) from the root of Goth. *hinthan*, and Eng. *to hend*, (in *apprehend*,) to seize; as if *the seizer.*

Lop, a flea, (Anglo-Sax. *loppe;*) from the root of Anglo-Sax. *hleapan*, and Eng. *to leap;* as if *the leaper.*

Midge, (Anglo-Sax. *myge*, *mygge*, or *micge*, Germ. *mücke*, Old Germ. *muccha;*) from the root of Old Germ. *muhhon*, to swarm over; as if *the overswarmer.*

Mouse, (Gr. *μῦς*, Lat. *mus*, Germ. *maus;*) from Sansc. √ *mush*, to pilfer; as if *the pilferer.*

Nag, (Old Dutch *negge;*) from the root of Anglo-Sax. *hnægan* and Eng. *to neigh;* as if *the neigher.*

Ox, (Sansc. *ukshan*, Goth. *auhsa*, Old Germ. *ohso*, Germ. *ochs;*) from Sansc. √ *vah*, (=Lat. √ *veh*, Eng. *wag*,) *to draw* or *carry;* as if *the drawer.*

Ram, (Germ. *ramm*, Anglo-Sax. *ram;*) from the root of Germ. *rammen*, to push, and Eng. *to ram;* as if *the pusher.*

Snake, (Germ. *schnake*, Anglo-Sax. *snaca*,) from the root of Anglo-Sax. *snican*, to creep, and Eng. *to sneak;* as if *the creeper.* Comp. Germ. *schlange*, a snake, from *schlingen*, to wind; as if *the winder.*

Snipe, (Dutch, *snip*, Germ. *schnepfe*, Old Germ. *snepha;*) from Old Germ. √ *snap*, (=Dutch and Eng. *snip;*) as if *the nipper.*

Swan, (Germ. *schwan*, Old Germ. *suan;*) from Old Germ. √ *sun*, (=Sansc. √ *swan* and Lat. √ *son*,) to sing; as if *the singer.* There are some swans that *sing.*

Toad, (Anglo-Sax. *tade*, Dan. *tudse;*) from the root of Dan. *tude*, to grumble; as if *the grumbler.*

Wether, (Germ. *widder*, Anglo-Sax. *wether;*) from the root of Germ. *wideren* and Anglo-Sax. *withrian*, to oppose; as if *the butter.*

Whelp, (Anglo-Sax. *hwelp;*) from the root of Anglo-Sax. *gilpan*, to cry out, and Eng. *to yelp;* as if *the yelper.*

Wolf, (Goth. *wulfs*, Germ. *wolf*, Anglo-Sax. *wulf;*) from the root of Goth. *wilwan*, to tear; as if *the tearer.*

2. Other names are formed by derivation from the verbal or nominal root, strengthened or increased by suffixes which have no apparent significancy.

(1.) *M* strengthened by a labial *b* or *p;* as,

Lamb, (Goth. *lamb*, Old Germ. *lamp*, Germ. *lamm;*) from the root of Old Germ. *limmen*, to bleat; as if *the bleater.*

(2.) *L* or *r* strengthened by a palatal *ge* or *ke*, which in English passes into *ow;* as,

Sparrow, (Germ. *sperling*, Old Germ. *sparo*, Upper Germ. *sperk*, Anglo-Sax. *spearwa;*) from the root of Germ. *sparen*, Anglo-Sax. *sparian*, and Eng. *to spare;* as if *the layer up.*

Swallow, (Low Germ. *swaalke*, Anglo-Sax. *swalewe*,) from the root of Anglo-Sax. *swelgan* or *swilgan*, to swallow, and Eng. *to swill;* as if *the swallower.*

(3.) *M* strengthened by *se;* as,

Breeze, (Germ. *brehme* or *bremse*, Old Germ. *premo*, Anglo-Sax. *brimsa* or *briosa;*) from the root of Old Germ. *primman* and Germ. *brummen*, to buzz; as if *the buzzer.*

(4.) The root strengthened by *el;* as,

Snail, (Germ. *schnegel*, Old Germ. *snekil*, Anglo-Sax. *snegl;*) from the root of Old Germ. *snahhan*, Anglo-Sax. *snican*, to creep, and Eng. *to sneak;* as if *the creeper.*

Weasel, (Anglo-Sax. *wesle*, Old Germ. *wisala*, Germ. *wiesel;*) from Old Germ. *wisa* and Germ. *wiese*, a meadow; as if *the meadow animal.*

(5.) The root strengthened by *en;* as,

Raven, (Anglo-Sax. *hræfen*, *hrefen*, and *ræfen*, Germ. *rabe;*) from the root of Lat. *rapio*, Anglo-Sax. *hreafian* or *reafian*, Germ. *rauben*, and Eng. *to rob;* as if *the pilferer.*

(6.) The root strengthened by *et;* as,

Cricket, (Dutch *kriek* or *krekel*, Fr. *criquet;*) from the root of Eng. *to crick* or *to creak;* as if *the creaker.*

Emmet, or by contraction *ant*, (Germ. *ameise*, Anglo-Sax. *æmet* or *emet;*) from Old Germ. √ *am*, to labor; as if *the laborer.*

Linnet, (Anglo-Sax. *linetwige*, Fr. *linot;*) from the root of Anglo-Sax. *linet*, Eng. *lint*, Fr. *lin*, and Lat. *linum;* as if *the flax-bird.*

3. Other names are formed by derivation from the verbal or nominal root by means of significant suffixes.

(1.) By means of the suffix *ard*, (=Germ. *hart* or Eng. *hard*,) forming ampliatives; as, *Bayard*, from Eng. *bay;* as if *the great bay one.*

Buzzard, (Germ. *busshard*, Fr. *busard;*) from Fr. *buse*, losing its ampliative signification.

(2.) By means of the suffix *el*, forming diminutives; as,

Cockerel, from Eng. *cock;* as if *the young cock.*

Hoggerel, from Eng. *hog;* as if *the young hog*, but applied only to a young sheep.

Spaniel, (Fr. *épagneul*, comp. *Espagnol*, Spanish;) from Fr. *Espagne* and Eng. *Spain*, losing its diminutive signification; as if *the Spanish dog.*

(3.) By means of the suffix *en*, forming diminutives; as,

Chicken, (Anglo-Sax. *cicen*, Dutch *kieken* or *kuiken*, Germ. *küchlein;*) from Anglo-Sax. *cocc* and Eng. *cock;* as if *the young cock.*

Kitten, (Fr. *chaton*, Germ. *kätzchen;*) from Fr. *chat*, Germ. *katze*, and Eng. *cat;* as if *the young cat.*

(4.) By means of the suffix *er*, denoting the personal subject; as,

Beaver, (Lat. *fiber*, Fr. *bièvre*, Germ. *biber;*) from the root of Germ. *baüen*, to build; as if *the builder.*

Chafer, (Anglo-Sax. *ceafor;*) from the root of Anglo-Sax. *ceowan* and Eng. *to chew* or *chaw;* as if *the chewer* or *chawer.*

Plover, (Fr. *pluvier;*) from the root of Lat. *pluvia*, rain; as if *the rainbird.*

Skimmer, from Eng. *to skim.*

Spinner, a spider, (Germ. *spinne;*) from the root of Germ. *spinnen* and Eng. *to spin.*

(5.) By means of the suffix *ster*, denoting the subject, whether male or female; as,

Hamster, a species of rat, (Germ. *hamster;*) from Old Germ. √ *ham*, to hide; as if *the hider.*

Lobster, (Anglo-Sax. *loppestre;*) from the root of Anglo-Sax. *hleapan* and Eng. *to leap;* as if *the leaper.*

(6.) By means of the suffix *ling*, denoting the subject; as,

Sanderling, from Eng. *sand;* as if *the sand bird.*

Starling, from Eng. *stare*, losing its peculiar significancy.

(7.) By means of the suffix *th*, forming abstract nouns; as,

Sloth, from Eng. *slow;* as if by metonymy, *the slow one.*

(8.) By means of the suffix *an* or *ant*, (Lat. *anus*, Provençal *á*, fem. *ana*, Ital. and Span. *ano*, Portug. *ano* and *ão*, Fr. *an*, *ain*, *ìen*, Walach. *ën*, *an*,) forming gentile nouns; as, *Pheasant*, (Gr. φασιανός, Lat. *phasianus*, Fr. *faisan*, fem. *faisande*, Germ. *fasan*;) from the river *Phasis*; as if *the Phasian*.

(9.) By means of the suffix *ent*, (=Lat. *ens*, gen. *entis*,) forming participial adjectives; as,

Serpent, (Sansc. *sarpa*, Gr. ἑρπετόν, Lat. *serpens*;) from Sansc. √ *srip*, (=Gr. √ ἑρπ, Lat. √*serp*;) as if *the creeper*.

(10.) By means of the suffix *on*, (Gr. ων, gen. οντος, Lat. *o*, gen. *onis*;) forming participial adjectives; as,

Dragon, (Gr. δράκων, Lat. *draco*, Germ. *drache*;) from the root of Gr. δέρκω, to see; as if *the sharpsighted*.

Lion, (Gr. λέων, Lat. *leo*, Germ. *löwe*, Old Germ. *lewo* or *hliuwa*, Anglo-Sax. *leo*;) from the root of Old Germ. *liuwon*, Anglo-Sax. *hlewan* or *hlowan*, and Eng. *to low*; as if *the lower*.

(11.) By means of the suffix *on*, denoting the subject; as,

Capon, (Gr. κάπων, Lat. *capo*, Fr. *chapon*, Ital. *cappone*;) from the root of Gr. κόπτω, and Fr. *couper*, to cut or mangle; as if *the mangled*.

Falcon, (Lat. *falco*, Germ. *falke*, Old Germ. *valho*, Fr. *faucon*, Ital. *falcone*;) from the root of Old Germ. *valo*, Germ. *fahl*, Fr. *fauve*, and Eng. *fallow*; as if *the fallow-colored*.

Griffon, (Gr. γρύψ, Lat. *gryps*, Fr. *griffon*, Ital. *griffone*, Germ. *greif*;) from the root of Germ. *greifen* and Eng. *to gripe*; as if *the seizer*.

Pigeon, (Fr. *pigeon*, Ital. *piccione*;) from the root of Ital. *piccare* and Eng. *to peck*; as if *the pecker*. Comp. Fr. *beccasse*, from *bec*, a beak.

Stallion, (Fr. *étalon*, Ital. *stallone*;) from the root of *stall*; as if *the stall horse*.

(12.) By means of the suffix *ock*, forming diminutives; as,

Bullock, (Anglo-Sax. *bulluca*, Germ. *bullochs*;) from Eng. *bull*; as if *the young bull*.

4. Other names are formed by derivation from verbal or nominal roots, by means of prefixes; as,

Antelope, from Gr. ἀντί, equal to or resembling, and ἔλαφος, a stag; as if *stag-like*.

5. Other names are formed by onomatopeia, or derived from the natural sound of the bird or insect.

(1.) Where there is a repetition of the natural sound; as,

Cuckoo, (Sansc. *kôkila*, Gr. κόκκυξ, Lat. *cuculus*, Fr. *coucou*, Span. and Port. *cuco*, Germ. *kuckuk*, Dutch *koekkoek*, Old Slav.

gzegzolta, Lithuan. *geguze;*) formed from the sound, with remarkable uniformity, in languages very remote.

Hoopoo, (Gr. ἔποψ, Lat. *upupa*, Fr. *huppe*, Port. *poupa*, Dutch *huppup;*) from the root of Eng. *to whoop;* as if *the whooper*.

Owl, (Lat. *ulula*, Germ. *eule;*) from the root of Lat. *ululo*, Germ. *eulen*, and Eng. *to howl;* as if *the howler*.

(2.) Where there is considerable fancy in the representation of the sound; as,

Katydid, the popular name of an American insect, the *Pterophylla concava* of Say.

Whippoorwill, the name of a bird.

(3.) Where a suffix is added; as,

Turtle, (Lat. *turtur*, Fr. *tourterelle*, Ital. *tortora*, *tortorella*, and *tortola*, Anglo-Sax. *turtle;*) from Lat. *turturilla*, diminutive of *turtur*, and that formed onomatopoetically.

II. The names of beasts, birds, and insects, are also formed by composition.

1. By imperfect composition, the two words denoting distinct attributes of one and the same object, and their syntactical connection being that of the conjunction *and;* as,

Camelopard, (Gr. καμηλοπάρδαλις, Lat. *camelopardalis*, Germ. *kameel-pardel;*) from Gr. κάμηλος, a camel, and πάρδαλις, a panther; as if *combining the attributes of the camel and panther*.

Leopard, (Gr. λεόπαρδος or λεοντόπαρδος, Lat. *leopardus*, Germ. *leopard;*) from Gr. λέων, a lion, and πάρδος, a panther; as if *combining the qualities of the lion and panther*.

2. By perfect composition, the two words forming one complex idea.

(1.) The two parts of the composition being in apposition or concord with each other; as,

Bittour or *bittern*, (Low Lat. *botaurus*, Fr. *butor*, Span. *bitor*, Dutch *butoor;*) from Lat. *bos*, an ox, and *taurus*, a bull; as if *the bull ox*.

Cormorant, (Fr. *cormoran*, Span. *cuervo marino;*) from Lat. *corvus*, a raven, and *marinus*, belonging to the sea; as if *the sea raven*. Comp. Welsh *morvran*, i. e. sea raven, denoting the same bird.

Ostrich, (Gr. στρουθός, Lat. *struthio*, Span. *avestruz*, Port. *abestruz*, Fr. *autruche*, It. *struzzo*, Germ. *strauss;*) from the root of Lat. *avis*, a bird, and of Germ. *strotzen* or Eng. *to strut;* as if *the strutting bird*.

Porcupine, (Ital. *porco-spinoso;*) from Lat. *porcus*, a hog, and *spinosus*, thorny; as if *the spinous hog.*

So *blackbird; bluebird; grossbeak*, (as if *grossbeaked;*) *hummingbird; redbird; redbreast*, (as if *redbreasted;*) *redstart*, (as if *redtailed;*) *redtail*, (as if *redtailed;*) *wryneck*, (as if *wrynecked.*)

(2.) One part of the compound standing in rection or government in reference to the other; the latter part of the composition expressing the principal or leading idea, and the former part some modification thereof.

(a) Where the first term expresses the dative case or the case of participation; as,

Squirrel, (Gr. *σκίουρος*, Lat. *sciurus*, dimin. *sciuriolus*, Fr. *écureuil;*) from Gr. *σκιά*, a shade, and *οὐρά*, a tail; as if *having its tail for a shade.*

(b) Where the first term expresses the accusative case or the relation of the object; as,

Moldwarp, (Germ. *maulwurf*, Old Germ. *mulwerf;*) from Old Germ. *mul* or Eng. *mold*, and Old Germ. *werfan*, to turn; as if *the mold-turner.*

Ossifrage or *ospray*, (Lat. *ossifraga*, Fr. *ossifrage* or *orfraie;*) from Lat. *os*, gen. *ossis*, a bone, and *frango*, to break; as if *the bone-breaker.*

Oystercatcher, the name of a bird.

Woodpecker, a bird that pecks holes in trees.

(c) Where the first term expresses the modal case or the relation of the mode or manner; as,

Crossbill, as if *having a bill like a cross.*

Spoonbill, as if *having a bill like a spoon.*

(d) Where the first term expresses the locative case, or the relation of the place where or time in which; as,

Grasshopper, which needs no explanation. Comp. Germ. *heuschrecke*, from *heu*, grass, and *schrecken*, to spring, the name of the same insect.

May-bug, as if *the bug appearing in May.*

May-fly, as if *the fly appearing in May.*

Nightingale, (Germ. *nachtigall*, Anglo-Sax. *nihtegale;*) from Germ. *nachti* or Anglo-Sax. *nihte*, in the night, and Germ. *gällen* or Anglo-Sax. *galan*, to sing; as if *the night singer.*

Woodcock, as if *the cock living in the woods.*

(e) Where the first term expresses the instrumental case, or the relation of the place by or through which; as,

Snowbird, as if *the bird that appears with the snow.*

(f) Where the first term expresses the genitive or adnominal case; as,

Ladybird, Ladybug, Ladycow, Ladyfly; as if *the bird, bug, cow, fly, of the Virgin Mary.*

3. By inverted composition, the order of the terms being contrary to that just exhibited.

(1.) In words peculiar to the English language; as,

Cutwater.

Lapwing.

Turnstone.

Wagtail. Comp. Gr. κίλλουρος, σεισούρα, σεισοπυγίς, Lat. *motacilla*, Low Sax. *wippsterz.*

(2.) In words derived from the French; as,

Chanticleer, from Fr. *chanter*, to crow, and *clair*, clear; as if *the loud crower.*

(3.) In words derived from the Greek; as,

Hippopotamus, (Gr. ἱπποπόταμος;) from Gr. ἵππος, a horse, and ποταμός, a river; as if *the river horse.*

III. The names of animals are often derived from remote languages.

1. From East Indian dialects; as, *zebu.*

2. From Shemitish dialects; as, *ass, camel, gazelle, giraff, goat, jackal, kid, scorpion.*

3. From Mongolian dialects; as, *argali, yak.*

4. From Malay and Oceanic dialects; as, *babyrousa, kangaroo, orang-outang, wombat.*

5. From African dialects; as, *chimpanzee, gnu, koba, korin, zebra, zerda.*

6. From American Indian dialects; as, *aia, capibar, carcajo, cariboo, moose, paca, sagoin, sapajo, tapir, tamarinunau, wapiti.*

Art. VII.—*Abstract of the Proceedings of the Tenth Meeting of the British Association for the advancement of Science.*

Concluded from Vol. xl, p. 345.

Col. Sykes communicated the contents of a letter from India, from Capt Aston, on the subject of a recent singular *shower of grain.* He stated that 60 or 70 years ago, a fall of fish had occurred during a storm in the Madras Presidency. This fact is recorded by Major Harriott, in his "Struggles through Life," as having taken place while the troops were on the line of march, and some of the fish falling upon the hats of the European troops, they were collected and made into a curry for the general. This fact was probably for fifty years regarded as a traveller's tale, but within the last ten years, so many other instances have been witnessed, and publicly attested, that the story is no longer doubted. The shower of grain above mentioned, took place March 24, 1840, at Rajket in Kattywar, during one of those thunder storms to which that month is subject, and it was found that the grain had not only fallen upon the town, but also upon a considerable extent of country round the town. Capt. A. collected a quantity of the seed, and transmitted it to Col. Sykes. The natives flocked to Capt. A. to ask for his opinion of this phenomenon : for not only did the raining of grain upon them from heaven, excite terror, but the omen was aggravated by the fact that the seed was not one of the cultivated grains of the country, but was entirely unknown to them. The genus and species was not immediately recognized by some botanists to whom it was shown, but it was thought to be either a Spartium, or a Vicia. A similar force to that which elevates fish into the air, no doubt operated on this occasion, and this new fact corroborates the phenomena, the effects of which had been previously witnessed.

The Secretary read two papers from Mr. Rowall, on *Rain*, and on the cause of the *Aurora Borealis*, and *Magnetism.* His hypothesis is, that each particle of vapor in rising through the air carries with it its portion of electricity, according to its expanded surface; that if condensed within the electrical attraction of the earth, the extra quantity of electricity is withdrawn, and the vapor falls and becomes *dew;* but if it rises beyond the electrical attraction of the earth, and is then condensed, the electricity, being insulated, forms an atmosphere around each particle of vapor: which surcharge of electricity not only suspends the vapor by its lightness, but also repels the neighboring particles of vapor, and prevents the formation of rain; and on the removal (by any cause,) of the electricity including the vaporous particles, the repulsion is removed, and the particles of vapor then attract each other, and form rain. Another cause of the formation of rain he believes to be the pressure of gravitation: thus if a cloud begins to form, the accumulation of vapor is on every side, but especially from above, and clouds are often seen piled to a great height; now each particle of vapor on forming the cloud, must have its extra charge of electricity over the particles of the cloud instantly dispersed through the whole mass; would become of the same density as the mass, and would take its level according to its density in the atmosphere, if not prevented by the space being occupied, and would therefore press on the vapor below it; and although the repulsion of the particles of vapor be sufficient to prevent the formation of rain at the edges and thinnest parts of the cloud, the pressure at the greatest depths of the cloud may be sufficient to overcome the repulsion and form rain. Concussions, he conceives, such as those of thunder, would aid the process, and cause heavy rain. He entered into proof of these positions in the papers. They account in his opinion, for the fact observed by Prof. Phillips, that more rain is received in gauges near the ground than in those higher up. He conceives a test of this theory may be had by raising conductors to clouds by the aid of balloons, discharging their electricity: and thus he thinks rain might be produced exactly when needed. His views respecting the cause of the Aurora and of magnetism are consequences of his theory of vapor and rain. The particles of vapor most expanded on rising from the earth, would carry with them a greater quantity of electricity, and would be buoyed up by the electricity to a greater height in the air than that which rises in a less expanded state. Thus, in the tropics, through the action of a vertical sun, vapor would rise to a great height with a great accumulation of electricity: this vapor, carried by the superior trade winds toward the poles on each side, there would be a constant circulation of electricity, a continual rising of vapor, especially in the tropics, carrying a great accumulation of electricity to the coldest parts of the earth, where the electricity again escapes to the earth, and rushes along its surface, with the vapor in the lower parts of the atmosphere, towards the equator, and is again carried back by rising vapor to the poles in constant succession, interrupted only in part, by the intense cold of the polar regions, causing the air to be then comparatively dry. The least disturbance then, taking place in the highly charged vapor, (either by part of the electricity being drawn off to the earth, or by vapor diffused from the more temperate regions, or by the accession of vapor either more or less charged with electricity,) must cause an instantaneous flash to pass through the whole mass of vapor, by the rush of electricity to restore the equilibrium, thus establishing the Aurora. Magnetism he ascribes to the constant circulation of electricity, and shows that this opinion will account for the leading phenomena, polarity, daily declination, variation, and constant oscillation of the magnetic needle.

Mr. Espy read a paper to show that the four fluctuations of the barometer which occur daily, are produced entirely by the increasing and diminishing elasticity of the air due to increasing and diminishing temperature. When the sun rises, the air begins to expand by heat; this expansion of the air, especially of that near the surface of the earth, lifts the strata of air above, which will produce a reaction, causing the barometer to rise; and the greatest rise of the barometer will take place when the increase of heat in the lower parts of the atmosphere is the most rapid, probably about 9 or 10 A. M. The barometer, from that time, will begin to fall; and at the moment when the air is parting with its heat as fast as it receives it, the barometer will indicate the exact weight of the atmosphere. The barometer, however, will continue to descend on account of the diminished tension of the air, and consequent sinking upon itself, as the evening advances; and its greatest depression will be at the moment of the most rapid diminution of temperature, which will be about 4 or 5 o'clock. At this moment the barometer will indicate a less pressure than the true weight of the atmosphere. The whole upper parts of the atmosphere have now acquired a momentum downwards, which will cause the barometer to rise above the mean, as the motion diminishes, which must have taken place some time in the night. This rise will be small, however, compared with that at 9 or 10 A. M. As the barometer now stands above the mean, it must necessarily descend to the mean at the moment when it is neither increasing nor diminishing in temperature, which will be a little before sunrise. If this is a true explanation of the four fluctuations of the barometer in a day, it will follow that the morning rise ought to be greater at considerable elevations, provided they are not too great, because some of the air will be lifted above the place of observation ; and such was found to be the case by Col. Sykes in India. As this morning rise of the barometer depends on the increasing elasticity of the air, and this increasing elasticity, on heat, Mr. E. proposed to the mathematicians to calculate how much the whole atmosphere is heated from sunrise till the time when the barometer stands highest, the actual rise of the barometer being given. In this way meteorology may assist astronomy. Prof. Forbes doubted the correctness of Mr. E.'s views of the great daily fluctuations of the barometer at elevated stations : for, towards 2 or 3 o'clock the heat being greatest, its effect in lifting up the inferior air to and above the elevated station, should then be greatest, whereas that time of the day was nearer to the time of minimum height of the barometer than of its maximum. After the announcement of the titles of a few other papers, the business of the Section was concluded.

Sect. B. *Chemistry and Mineralogy.*

A paper on *some electrical phenomena*, and on a *supposed new substance*, was presented by Dr. Schönbein. It is well known to electricians that in certain electro-chemical decompositions a peculiar odor is evolved, very analogous to that produced by common electric sparks, or by the working of an ordinary electrical machine in the air. Dr. S. has undertaken a series of experiments in order to ascertain the circumstances under which this electro-chemical odor is evolved, the causes which influence its production, and if possible, the principle to which its appearance is to be attributed. This peculiar odor is evolved at the anode or positive surface, when certain aqueous solutions are decomposed by the passage of a voltaic current. The oxygen gas which is then evolved has a strong and peculiar smell, which is perfectly similar to that which is always perceived when an electrical machine is worked or sparks passed through the air. M. Schönbein has observed that the odor is evolved on the decomposition of water, dilute sulphuric acid, solutions of phosphoric and nitric acid, potassa, and many oxysalts; dilute sulphuric acid yielding it in the greatest quantity, while no smell whatever was perceived on the decomposition of solutions of hydracids, chlorides, bromides, or iodides, which not only did not evolve it themselves, but by their presence, even in small quantity, prevented its evolution from solutions which would otherwise have produced it abundantly. He found, on collecting the oxygen gas evolved at the anode, from a solution capable of evolving the odor, that the odor might be preserved for some time, by enclosing the gas in well-stopped bottles. From the characters possessed by this oxygen, he was led to consider the odor due to the presence of a minute quantity of a new and hitherto wholly unknown substance, of considerable importance in many natural phenomena, and he has therefore named it from its most evident character, *ozone.* Its properties are briefly as follows: it is evolved only from solutions containing it, by perfectly clean electrodes of platinum or gold; whilst charcoal and the more oxidizable metals are unable to cause its appearance. It can be obtained only from a cold solution, as heat prevents its evolution. When a piece of one of the oxidizable metals, such as zinc, tin, iron, mercury, &c., or a few drops of solution of the protochloride of tin, or protosulphate of iron, are placed in a portion of oxygen impregnated with ozone, that peculiar substance is almost instantaneously absorbed; and the oxygen becomes inodorous. When perfectly clean and dry plates of gold or platinum are immersed in oxygen containing ozone, they acquire a negatively electric state of polarity: silver and copper also become thus electric, but in a far less degree than gold or platinum. The plates thus polarized retain their electric powers in air for a considerable time, but rapidly lose it when plunged into hydrogen gas, in which, if retained a sufficient time, they acquire an opposite state, becoming positively polarized. He then compares these effects with those produced by the odorous matter peculiar to common electric sparks and brushes. When a perfectly clean and dry plate of gold or platinum is exposed to an electric brush issuing from a charged and conducting point, it becomes positively polarized, and the degree of polarity depends on the nature of the point and the time which the plate has been exposed to the influence of the brush issuing from it. He shows that the power is not due to the mere current of electricity escaping from the point, but to some substance produced or evolved by it; because if the point be moistened, the electricity still continues to be given off as a brush, but the power of polarizing the gold or platinum plates is lost. A plate thus charged, is perfectly similar in its electrical powers to a plate charged or polarized by immersion in oxygen impregnated with ozone. Heat or exposure to hydrogen, which destroys or inverts the electricity of such a plate, exerts a precisely similar action on plates polarized by exposure to the brush; and likewise, if the plates are not perfectly clean and dry, it is equally impossible to charge them, either by exposure to the brush, or by immersion in oxygen containing ozone. He supposes that there exists, both in the air and water, a very minute quantity of an electrolyte or compound substance, which when decomposed by electricity, evolves, as one of its constituents, the peculiar odorous matter called ozone. He observes, that both from its electromotive power, and likewise from its strong affinity for metals, it is evidently similar to chlorine, bromine, and iodine. Its non-appearance when water is decomposed by electrodes of the more oxidizable metals, he attributes to its entering immediately into combination with those metals: and he considers that when the solution is heated, the affinity of the ozone for metals is so much increased, that it is even able to combine with gold and platinum, thus accounting for its disappearance when heated. By this theory, all the phenomena attendant on its evolution may be easily explained, and it hence becomes very interesting to search for traces of this widely diffused substance. M. Schönbein considers that the smell perceived whenever bodies are struck by lightning, is probably due to a small portion of ozone being set free; and relates a recent case within his own observation, of a church struck by lightning, in which the surrounding buildings to a considerable distance were filled with a bluish vapor having a peculiarly pungent odor. Even in this early stage of the inquiry, it will readily be seen that many curious and unexplained phenomena might be accounted for, if the existence of the supposed electrolyte be proved. Mr. S. proposes devoting all his leisure to the prosecution of this inquiry.

Mr. E. Solly proposed the following mode of *bleaching vegetable wax*. The wax must be melted, a small quantity of sulphuric acid is poured in, composed of one part of oil of vitriol to two of water, and then a few crystals of nitrate of soda stirred in, the whole is then agitated with a wooden stirrer and kept heated. Nitric acid is thus evolved in considerable quantity and purity, from a large surface, and in such a manner that all the acid evolved must necessarily pass through the melted wax. This method answers the purpose very completely; the process is cheap and rapid, and the residuum, being merely a little solution of sulphate of soda, is easily removed. When it is desired to employ chlorine in place of nitric acid as the bleaching agent, the same process may be adopted.

Prof. Gregory read a paper on the *preëxistence of urea in uric acid.* By the action of peroxide of lead on uric acid, Liebig and Wöhler obtained from it oxalic acid, allantoine and urea, and they considered the latter as existing in the uric acid, combined with urile. The author having found that urea, unlike most organic substances, resists the oxidizing agency of permanganate of potash, thought that if urea could be obtained from uric acid by the action of that salt, the argument for its preëxistence would be much strengthened; as, if only the elements of urea were present, the oxidizing agency of the permanganate would most likely prevent its formation. On trying the experiment, a large quantity of urea was obtained, along with oxalic acid, and a new acid probably formed by the oxidation of allantoine. The author further described the acetate of urea, a salt formed in his experiments.

Prof. Gregory then exhibited a new process, communicated by Prof. Liebig, for preparing the very singular and beautiful compound termed *murexide* by Liebig and Wöhler, and *purpurate of ammonia*, by Prout. The process is quite certain, and very productive. It consists in adding a boiling solution of 7 grains of aloxan, and 4 grains of aloxantine in 240 grains of water, to 80 grains of a cold and strong solution of carbonate of ammonia. The mixture instantly acquires a deep purple color, and on cooling, deposits the golden green crystals of murexide.

On the relation of form to chemical composition, by Dr. Schafhaeutl. The object of this paper is to explain the circumstances under which certain modifications of form take place in Graphite, (as also in others generally considered to be elementary,) and to prove their connexion with changes of an entirely chemical nature.

Account of a *new compound of arsenious and sulphuric acids;* by Dr. Schafhaeutl. This was obtained from the escaping smoke of copper calcining furnaces near Swansea, in South Wales. The new compound was another singular instance where an anhydrous crystallized body was deposited under the presence of water only, and was a remarkable proof of the unlimited number of different forms of combination which might be produced even in inorganic substances in contact under varying circumstances. The copper ores smelted in South Wales were chiefly copper pyrites, mixed with iron pyrites, gray copper ore, &c.; in fact, a mixture in which the sulphurets of copper, iron, arsenic, antimony, cobalt, nickel, zinc and tin were invariably found together. The sulphur and arsenic escape from these ores during the calcining process, as sulphurous and arsenious acids, and have been found to destroy all vegetation for miles around the copper works, without affecting animal life in the slightest degree. By bringing the escaping fumes in contact with steam, and forcing it through burning charcoal, or subjecting it only to a great pressure in contact with steam, the new solid compound was deposited on the cool surfaces of the chambers connected with the calcining furnace. It was deposited in beautiful crystallized leaves or tables, perhaps belonging to the same class as Wöhler's dimorphic modification of the crystallization of arsenious acid, the regular form of which belongs to the octahedron. It was found to consist of

Arsenious acid, - - - - -	68.250
Sulphuric acid, - - - - -	27.643
Protoxide of iron, - - - -	3.029
Oxide of copper, - - - -	0.420
Oxide of nickel, - - - -	0.656
	99.998

These crystals attracted moisture from the air with great rapidity and with evolution of heat, corroding animal and vegetable substances as powerfully as concentrated sulphuric acid. One of the remarkable changes during the formation of this compound, was the conversion of sulphurous acid into sulphuric acid, as well as the presence of iron, copper, and nickel, in a deposit from gaseous matter. No other definite compound of arsenic acid with another acid seems to be known, except those with the organic tartaric and paratartaric acids.

Prof. Thomas Thomson communicated an account of the most important chemical manufactures carried on in Glasgow and the neighborhood. 1. *Iron.* The quantity of iron smelted here cannot be much less than 200,000 tons, nearly a fifth part of all the iron smelted in Great Britain. The ore employed, is the carbonate of iron, or clay iron-stone. The rapid increase of iron-smelting has been the consequence of a discovery of Mr. Neilson, manager of the gas works. This is now universally known under the name of the *hot blast.* The air is heated to more than 607° before it enters the furnace, by passing through a range of heated pipes. Under this treatment, coals may be used without previous coking, and instead of seven tons of coals for every ton of cast iron, three tons or even two and a half, will suffice. 2. *Sulphuric acid.* The manufacture of this acid was begun by Dr. Roebuck, at Prestonpans, about 1763, but it is now more than 20 years since his manufactory was abandoned. The sulphuric acid works, at St. Rollox, on the banks of the Monkland canal, were begun about 45 years ago. They were at first upon a very small scale, but they are now probably the largest of the kind in Europe. The size of the leaden chambers was gradually increased, and the substitution of steam for the water formerly placed at the bottom of the chambers, was a vast improvement. The sulphur is burnt over a stove, and an iron cup containing the requisite quantity of saltpetre, mixed with the requisite quantity of sulphuric acid, is placed over the burning sulphur. By this contrivance the sulphur is completely converted into sulphurous acid, and the whole of the nitric acid carried along with it into the leaden chambers. The acid which collects at the bottom of the chambers, has a gravity of 1.75 ; or it is a compound of one atom anhydrous acid, and two atoms water. This acid is concentrated by heating it in a platinum still, till the second atom of water is driven off. When in full work, the quantity of sulphuric acid made in it, exceeds 300,000 pounds avoirdupois per week. Forty five years ago, it cost eight pence per pound; the present price is under a penny a pound. 3. *Bleaching powder.* One of the great purposes to which sulphuric acid is applied at St. Rollox, is the manufacture of bleaching powder, or chlorite of lime, as it is now called. The requisite mixture of common salt, binoxide of manganese, and sulphuric acid, is put into a leaden still, and the chlorine evolved passes through leaden tubes into air-tight stone chambers, the bottoms of which are covered with a stratum of slacked lime several inches thick. The lime absorbs the gas as it passes into the chamber, and the process is continued till the absorption is reckoned sufficient. Pure bleaching powder is a compound of

Chloride of calcium, - - - -	7.
Chlorite of lime, - - - -	10.
Water, - - - - - -	3.375
	20.375

Half the lime loses its oxygen and combines with chlorine, constituting chloride of calcium. The oxygen combines with chlorine, which in the state of chlorous acid, combines with the other half of the lime, constituting chlorite of lime. Two atoms of the water were in the slacked lime. The third atom must have come along with the chlorine gas, or been absorbed from the atmosphere. 4. *Carbonate of soda* is obtained from the semi-liquid mass remaining in the still, and consisting partly of the impurities of the manganese, and partly of sulphate of soda, and sulphate of manganese. 5. The manufacture of *alum* is carried on at two establishments, one at the Hurlet, about six miles S. W. by the Paisley canal; another at Campsie, about eight miles off, near Kirkintulloch, on the great canal, and near the foot of the Campsie Hills. The alum is made from the shale, which exists in great abundance in the exhausted coal beds. This shale is a clay mixed with some coal, and with that variety of iron pyrites which undergoes decomposition, and is converted into sulphate of iron, by exposure to the air. The sulphate of iron thus formed, acts slowly on the clay, and finally converts it into sulphate of alumina. The alum-maker washes this altered shale, and obtains a solution of sulphate of iron and sulphate of alumina. The liquor yields an abundant crop of sulphate of iron, which is removed, dried, and sold at a cheap rate. The sulphate of alumina does not crystallize till it is mixed with sulphate of potash or sulphate of ammonia; because ammonia is a double salt composed of three atoms of sulphate of alumina and one atom of sulphate of potash or sulphate of ammonia. 6. At Campsie alum-works, is prepared the *prussiate of potash*, a well known beautiful yellow salt, crystallizing in truncated octahedrons. The manufacture of this salt on the large scale began here. Before that time it was prepared only in laboratories for scientific purposes, and sold at a high price. Mr. Mackintosh introduced it to the calico-printers, who use it extensively, to produce very beautiful blues and greens. It is prepared by burning the hoofs and horns of cattle with potash, and some iron in iron pots. The hoofs and horns of a hundred head of cattle are consumed in the works every day. After combustion the residue is lixiviated with water, and when the solution is sufficiently concentrated, the prussiate of potash crystallizes. Connected with this manufactory is one of *Prussian blue.* It is made by mixing sulphate of iron, alum, and prussiate of potash, and precipitating the whole by an alkali. The precipitate is at first light blue, but it is washed with new portions of water every day for several weeks. At every washing the color deepens, and when it has acquired the requisite shade, the Prussian blue is allowed to subside, the water is drawn off, and the powder permitted to dry. The color varies according to the proportion of alum employed, and it has the finest color of all, with the coppery lustre which is so much admired, when no alumina whatever is mixed with it. 7. *Bichromate of potash*, a salt very much used by calico-printers, and forming the finest and most indelible yellows, oranges, and greens, is made at the manufactory of Mr. White, at Shawfield, near Rutherglen, about two miles from Glasgow. Its introduction constituted quite an era in calico-printing. It formerly cost a guinea an ounce; it is now sold at two shillings a pound. *Tartaric acid* is also made here. It is largely used by the calico-printers, chiefly to disengage the chlorous acid from bleaching powder, and enable it to destroy the color on particular parts of the cloth, either that these parts may remain white, or that some other color may be superadded. It is obtained from cream of tartar, by throwing down the tartaric acid by means of lime, and afterwards decomposing the tartrate of lime by means of sulphuric acid, and crystallizing the tartaric. At the same manufactory the carbonate of soda is converted by exposure in an atmosphere of carbonic acid, into sesquicarbonate. It is chiefly used by the makers of soda water. Among other manufactures and processes carried on at this place, were mentioned acetic acid from wood; iodine; soap; bleaching of cotton cloth; Turkish red dyeing; calico-printing; glass making; starch making, &c.

Arthur Connell communicated some additional observations on the voltaic decomposition of alcohol; his experiments appearing to him to prove that water exists as such in absolute alcohol.

On the compound or radical called *kakodyl*, by Prof. Bunsen of Marburg. This is a new radical resembling alcohol, in which arsenic replaces the oxygen of that compound. The oxide of kakodyl has so great an affinity for oxygen, that when exposed to the air it immediately inflames. The bodies produced by the combustion are arsenious acid, carbonic acid, and water. By the further oxidation of the oxide of kakodyl, kakodylic acid is produced. The sulphuret of kakodyl is similar in composition to the oxide, and participates in many of its properties. The telluret, selenuret, iodide and bromide of kakodyl were also examined. The danger attending these experiments is very great, and the poisonous effects produced by the inhalation of the vapor, were described as dreadful. Kakodyl is produced from the liquor of Cadet, and is extremely interesting as being a link connecting organic and inorganic chemistry. Prof. B. is engaged in further experiments on this subject, and has already obtained many new combinations.

New method of preparing Morphine and its salts, by Dr. Mohs, of Coblentz. This mode of separating morphine from narcotine, and all other heterogeneous substances, consists in dissolving it in an excess of caustic lime, and precipitating it by muriate of ammonia. This method of precipitation is in principle very similar to the precipitation of alumina, from a solution in caustic potash. The process is as follows: the opium is boiled in water, in which it readily dissolves; the decoction is strained through a linen cloth and the dregs are pressed; this operation of boiling and straining is repeated twice on the same quantity of opium, and the solution of the whole concentrated until its weight is four times that of the opium employed. The concentrated solution is, while still warm, mixed with milk of lime, prepared with a quantity of dry lime equal to the fourth part of the weight of the opium. The mixture is heated till it boils, and is filtered through linen while hot. The filtered liquor has a light brown yellow color. While still hot it is mixed with pulverized sal ammoniac in excess; the lime is saturated with muriatic acid, ammonia is set free, and the morphine is precipitated. When the solution is greatly concentrated, the precipitation is instantaneous, and is almost equal in volume to half the solution. When the solution is less concentrated, there is at first no precipitation, but as the liquor cools open needles appear, and at a certain point a large mass of precipitate is suddenly formed. The peculiarity of this process is, that it affords a well crystallized and fine product of morphine, without the use of alcohol. This is due to the fact that the ammonia is not added in a free state, but is generated in immediate contact with the substance to be acted upon. The morphine is nearly colorless: by dissolving it in muriatic acid, and crystallizing, we obtain pure muriate of morphine in white crystals. The milk of lime, it is to be observed, must not be added to a boiling hot solution of the crude opium, for then the precipitate adheres to the sides of the vessel, and does not afterwards redissolve perfectly. The liquor containing the morphine should either be cold or only lukewarm, when the milk of lime is added to it. If it is boiling hot, it must be added to the milk of lime, and not *vice versâ.*

Dr. Schäfhaeutl read a paper on a new method of photogenic drawing.—Prof. Graham gave an abstract of Prof. Liebig's new chemical views relative to Agriculture and Physiology.—Prof. Playfair read a statement of Prof. Liebig's new views on the subject of poisons.—Dr. P. gave an account of a new fat acid.—Mr. J. J. Griffin read a paper on a new method of crystallographic notation. He classes the planes of crystals into seven elementary sets, which he calls *forms;* and entered into various details to prove that the occurrence of planes not representable by one or other of these seven forms, is a mathematical impossibility, and that the proposed system of notation is amply sufficient for all the purposes of the chemist and mineralogist, while it has over other systems of crystallography, the advantage of requiring but a small amount of mathematical knowledge.

Prof. F. Penny communicated details and results of his experiments on the action of nitric acid on the chlorates, iodates, and bromates of potassa and soda.—Dr. R. D. Thomson gave his views on the tests for sulphuric acid when thrown upon the person.—Prof. Johnston stated his experiments on the resin of sarcocolla.—Prof. T. Thomson enumerated the minerals found in the neighborhood, among which were nine species of lead ore, and numerous minerals of the zeolite family. Sulphuret of cadmium had been lately discovered along with prehnite, at Bishoptown:

it was rare, and single crystals sold at £10 each.—Prof. Andrew Buchanan related his method of separating, by filtration, the coagulable lymph from liquid human blood.—Dr. Clark, of Aberdeen, stated his mode of detecting minute portions of arsenic.—Mr. McGregor read a paper containing experiments made by him while resident in the Royal Infirmary of Glasgow, which showed that the per centage of carbonic acid thrown off from the lungs is greater in disease than in health.—A paper by Messrs. Redtenbacher and Varrentrapp was read, on the constitution and products of the distillation of fat bodies.—Prof. Bunsen stated his new *mode of estimating nitrogen in organic analysis.* He introduces the substance to be analyzed, after having mixed it with oxide of copper, into a glass tube. A few slips of metallic copper are then added, and the tube is fixed to Dobereiner's apparatus for producing hydrogen. This gas is conducted through it until all the atmospheric air is expelled, giving the tube at the same time, a rotary motion, in order to dislodge any air which might be retained between the particles of the oxide of copper. The tube is now hermetically sealed, and introduced into an iron vessel, filled with moist gypsum. Thus prepared it is introduced into the common oven used for organic analysis, and surrounded with red-hot coals. If the tube is of strong green glass, it never bursts. When the combustion is completed, the tube is placed below a graduated glass receiver standing over mercury, and the point cut off. The gas, which had a pressure of several atmospheres, now rushes into the jar. The carbonic acid is absorbed by a ball of hydrated potash, which is introduced into it, and the remaining gas must be nitrogen, for all the hydrogen must have been converted into water by the oxygen of the oxide of copper. The results obtained by this method agree with theory to the second and often to the third decimal place.—Prof. Penny gave an account of a new salt obtained from iodine and caustic soda, which he names the sesquiodide of iodate of soda.—Prof. Johnston read a paper on *resins*, containing the following results. 1. The resins differ from each other in the quantity of oxygen they contain. 2. Those in which the atoms of oxygen is the same, may vary in the hydrogen, which is another cause of difference in properties. 3. In all the resins hitherto analyzed, the number of atoms of carbon is constant. 4. The resins, as a natural family, may be represented by a general formula containing two variables. 5. The known resins divide themselves into two groups, possessing unlike chemical and physical properties: of one, colophony is the type, and of the other gamboge, or dragon's blood.—Prof. Johnston exhibited some varieties of peat from the moss near Paisley, which he said illustrated a transition from the comparatively fresh vegetable matter to a substance resembling coal, but which he affirmed to be ulmic acid. He stated that the same substance might be obtained from peat by digesting it in ammonia, and afterwards precipitating the brown solution by an acid; while, on the other hand, caustic potash extracts another acid, which he proposed to term *humic* acid.—Mr. Alex. Bryson stated a new method of ascertaining the *refractive powers of minute bodies*, and its application to mineralogy. In ascertaining the refractive powers of crystalline substances, it is commonly thought necessary to procure pieces not less than a quarter of an inch in size, which are then to be ground into prisms. The microscope, with a slight alteration, is well suited to give minute differences in refractive powers. On the stage of the microscope is placed a piece of crown glass, with fine lines drawn on its first surface. If a piece of beryl, or any other mineral with parallel sides, is now placed on the glass, the lines will no longer be visible through the microscope, until it is raised above the crystal three hundredths of an inch. The difference of focus becomes an index of the difference of refractive power between the glass plate and the crystal. The means adopted to ascertain minute changes in focal length, is a scale of hundredths of an inch, with a vernier dividing it into thousandth parts.—Prof. Gregory communicated a paper on the preparation of *alloxan, alloxantine, thionurate of ammonia, uramile*, and *murexide*. To prepare alloxan from uric acid, Liebig and Wöhler used nitric acid of sp. gr. 1.42, and separated the acid liquid from the crystals by means of a porous brick, thus losing the whole of the mother liquid. The author uses nitric acid of sp. gr. 1.35. The action of this acid on uric acid must be kept moderate. When crystals of alloxan are formed, the whole is thrown on a filter, the throat of which is stopped with asbestus. That portion of the acid liquid which remains in the crystals is displaced by a few drops of cold water, and the crystals are purified by recrystallization. The liquid is again employed in the same way and the crystals collected as before. Five such operations may be performed with the same liquid, each yielding a large crop of crystals: while the mother liquid is preserved, and yields a large quantity of parabanic acid, or oxalurate of ammonia. By this process the author obtains from 100 parts of uric acid, 65 of anhydrous alloxan, or 90 of alloxan + 6 aq. From alloxan, alloxantine is easily obtained by the action of sulphuretted hydrogen. Thionurate of ammonia is easily formed, by boiling a solution of alloxan with sulphite of ammonia, and free ammonia. Uramile is also easily obtained, by boiling a solution of thionurate of ammonia, with an excess of dilute sulphuric acid. Murexide is obtained, as has been already described, (p. 46 of this No.) The theory of the formation of murexide is of great importance in reference to organic coloring matters.

Sect. C. *Geology and Physical Geography.*

Dr. Robb presented a communication on the *geology of the country around the River St. John*, in New Brunswick. The St. John is as large as any river of the first class in Europe. It drains a large portion of the province of New Brunswick, and the volume of water which it discharges into the Bay of Fundy is very great, especially during the spring floods. Yet in one place the river is so contracted that it is not more than 310 feet in width. In the Bay of Fundy, as is well known, the tides rise higher than in almost any other part of the globe, there being sometimes, in spring tides, a rise of from 40 to 60 feet. The tidal wave is then forced up into the narrow parts of the river, and causes a backward fall of water, against the natural current, of many feet in height. Dr. R. alluded to the singular configuration of the country in the vicinity of the river, it being shaped in *terraces*, one below the other. The river, from the distance, between the first or uppermost pair of terraces, appears to have been of much greater width, and subsequently to have gradually become contracted, until confined within its present narrow limits. The higher terraces slope towards the stream, the lower ones become more horizontal, and the lowest is with a slope turned from the river; an appearance which Dr. R. explained by the successive depositions of alluvial matter near the bed of the stream, and extending no farther, so that depressions resulted behind these depositions, which often contain water. In no part of the world can the phenomena of rivers be studied better than in America, from the little alteration produced upon them by the hand of man. In many parts of the new continent the rivers present these terraces; they may be observed near the St. Lawrence, the Mississippi, and in many other places; and had the rivers of the Old World been examined before their banks became cultivated, in all probability they would, in many cases, have displayed similar terraces. Dr R. adverted to the theory which explains the formation of these terraces by the bursting of the barriers of lakes through which the river had passed. He conceived that in a few cases, the phenomena might be so explained, but that in general we must have recourse for the solution of the problem to an upheaving of the land, and that at a comparatively recent period. In these terraces, but few organic remains had been discovered, which he accounted for by the paucity of the limestone rocks from which mollusca could obtain matter for the formation of shells; the long cold winter, too, might have had its influence. He had, however, found in the lower terraces, *Uniones* and *Anodontes* resembling those now existing in the river. Some bones had been discovered, but they seemed to be recent, probably belonging to the spermaceti whale. In the different terraces there is a difference in the quality of the soil, there being most alluvium on the lowest terraces. The middle terraces, being so nearly horizontal, are well fitted for roads, and are used accordingly. At Frederickton, where wells had been sunk, the vegetable soil was three inches deep, after which 14 feet of sand was gone through, when water was reached, retained by a bed of clay, underlaid by a slate rock. The terraces were composed wholly of detrital matter, the upper ones being coarser, and often having boulder stones, some of which were 15 feet in diameter, and seemingly derived from parent rocks to the N. W., inferred from scratches on the ground, coinciding with the major axes of the boulders. The rocks in the neighborhood of the river, are slates, with some limestone, the whole disturbed by sienite.

Prof. Johnston submitted an account of the first part of his *Report on Chemical Geology*. He referred to the combination of science requisite to promote geology. It had drawn upon the labor of the zoologist, the comparative anatomist, the botanist, the historian, the natural philosopher, and had now called in the assistance of the chemist. Prof. J. referred to what had been done by the late Sir Humphry Davy, and the late Dr.

Turner, in their application of chemistry to geology; and he had been requested by the Association to draw up a report of this application. He now brought forward his investigations on the most important of our mineral productions,—*coal.* Although some geologists may entertain a different opinion, he assumes for granted, the vegetable origin of coal; and although it may be classified in various ways for economic or geological convenience, as into caking or not caking, bituminous or not bituminous, the true basis of the classification must depend on the chemical composition. Carbon, oxygen, and hydrogen, are the components of living vegetables, and the same elements compose coal, but in different proportions. In the decomposition of vegetable matter there are two agents always at hand, viz. air and water, which resolve it into carbon, oxygen, and hydrogen, forming with one another these combinations; carburetted hydrogen, carbonic acid, and water. In the change from lignin to fossil wood, we find that carbonic acid is parted with; and this continues without variation in all the kinds down to cannel coal. In mines of lignite and cannel coal we find only carbonic acid, (or choke-damp;) while in mines of coal lower in the scale, we find in addition, carburetted hydrogen, (or fire damp;) the hydrogen diminishing in each variety as we approach the anthracite. In regard to the question whether the vegetable matter that formed coal had been drifted or generated on the spot, he was inclined to the latter opinion.

Mr. Mathie Hamilton presented "Observations on *great earthquakes on the West coast of South America*, particularly the great one of Sept. 18, 1833, which destroyed the city of Tacna, and other places in Peru." Tacna, an Indian town of some antiquity, now capital of the province of the same name, lies in the midst of a desert tract of about 50 miles broad, between the mountains and the sea. The port of Arica, about 40 miles distant, had, since the first arrival of the Spaniards, been five times destroyed by earthquakes, while Tacna had enjoyed a happy immunity, and was supposed beyond the reach of this calamitous visitation. After 1826, however, very frequent and severe shocks were felt, particularly a few weeks previous to the great one of Oct. 8, 1831, which reduced Arica to a heap of rubbish; yet it continued nearly uninjured till the evening of Sept. 16, 1833, when there occurred a single loud report, with an upward movement of the ground. On the morning of the 18th there was a much more violent movement, the earth heaving at once up and down, and also laterally, accompanied by a frightful subterranean noise. The falling of houses all around, the cries of the people, the howlings of animals, produced a scene that cannot be described. The agitation seemed to have reached the utmost possible height, when suddenly the earth, as if striving to get rid of some mighty load, made a movement more terrible than ever, in every direction, and in one minute the work of destruction was completed. The cathedral in falling, destroyed a numerous congregation of females who had assembled there, and were endeavoring to escape; but the priests, who remained under an arch, were saved. It is remarkable that while some quarters had nearly every house demolished, others were comparatively uninjured. Rain, (here a rare phenomenon,) fell almost every day during six weeks; and at Arica, on the first week of October, there came down a deluge, such as had not been witnessed for half a century. The river which supplies Tacna with water, remained undisturbed, but others were changed in their courses, and one altogether disappeared. The earthquake was felt many hundred miles to the south, as far as the desert of Atacama. At Suto, about 40 miles distant, fissures were made in the ground, whence issued a dark colored fluid. In the province of Tarapaca, villages were overthrown; and one, which stood in a ravine, was buried with all its inhabitants. To the north its ravages were equally extensive. The villages of Samo, distant 30 miles, and of Coquimbo, distant 60, were both destroyed. Moquehua, 120 miles off, suffered severe damage; and Arequipa was violently shaken, but with little injury. The effects extended even to the lofty peaks of Upper Peru. Tacora, 15,000 feet above the sea, had its church thrown down. When the atmosphere cleared after the calamity, that mighty range, as seen from Tacna, presented in many parts, a new outline. Large masses had been detached or slid down into the valleys or ravines, leaving many elevated peaks denuded of their most prominent features. Mr. Scott, engineer, then employed at Achozumio, about 14,500 feet high, describes the shocks there as terrific, and the noise as if an immense mass of porcelain had, after being raised in the air, been then let fall, and dashed to pieces. By his telescope, he saw the masses falling from the mountains, one of them leaving a space as large as St. Enoch's

Square, Glasgow. On the 20th of January, 1834, a terrible earthquake occurred in New Grenada, by which the large towns of Popayan and Pasto, were entirely demolished, and many thousands perished. On the 21st of September, 1834, Mr. Hamilton experienced a most severe shock, in which the movements of the earth were entirely vertical, and seemed to take place twice every second. He mentions also, the terrible earthquake on the coast of Chili, February 5, 1835, by which the seaport of Conception and Talcahuano, the capital of the province, were totally destroyed. The sea then retired several times to a great distance, and returned in immense billows. It is believed that new banks were then thrown up from its bottom, and that it was on one of these, that the *Challenger*, ship of war, was wrecked a few months after.

M. Agassiz made a communication on the subject of *glaciers and boulders in Switzerland.* He particularly drew attention to facts relative to the mode of the movements of the glaciers, which he attributes to the continual introduction of water into all their minutest fissures, which, in freezing, constantly expands the mass. The effects of the movement produced by this expansion, upon the rocks beneath the ice, are very remarkable. The bases of the glaciers, and the sides of the valleys which contain them, are always polished and scratched. The fragments of the rocks that fall upon the glaciers are accumulated in longitudinal ridges on the sides of the ice, by the effects of the unequal movement of its middle and lateral masses. The result is longitudinal deposits of stony detritus, which are called *morains;* and as the glaciers are continually pressed forwards, and often in hot summers melted back at their lower extremity, it results that the polished surfaces, occasioned by friction on the bottom and sides, are left uncovered, and that the morains, or curvilinear ridges of gravel, remain upon the rocks formerly covered by the ice, so that we can discover by the polished surfaces and the morains, the extent to which the glaciers have heretofore existed, much beyond the limits they now occupy in the Alpine valleys. It even appears to result from the *facts* mentioned by Prof. A., that enormous masses of ice have, at a former period, covered the great valley of Switzerland, together with the whole chain of the Jura, the sides of which, facing the Alps, are also polished, and interspersed with angular erratic rocks, resembling the boulders in the morains; but so far different, that the masses of ice, not being there confined between two sides of a valley, their movements were in some respects different,—the boulders not being connected in continuous ridges, but dispersed singly over the Jura at different levels. Prof. A. conceives that at a certain epoch, all the North of Europe, and also the North of Asia and America, were covered with a mass of ice, in which the elephants and other mammalia found in the frozen mud and gravel of the arctic regions, were imbedded at the time of their destruction. He thinks that when this immense mass of ice began quickly to melt, the currents of water that resulted, transported and deposited the masses of irregular rounded boulders and gravel which fill the bottoms of the valleys; innumerable boulders having at the same time been transported together with mud and gravel, upon the masses of the glaciers then set afloat. Prof. A. announced that these facts are explained at length in the work which he has just published, 'Etudes sur les Glaciers de la Suisse,' illustrated by plates.

Mr. Jeffreys detailed an experiment he had made on a very great scale, to decide the question, whether silicious matter could be dissolved largely by water, or what is the same thing, by its vapor. This experiment formed the subject of a paper, read some months ago before the Royal Society, and by the experiment a solution of more than 200 lbs. of silica was effected in steam, at a heat exceeding that of fused cast-iron. The steam was not under pressure, but was conducted into a large kiln used for stone pottery. The silica was not only dissolved, but carried away in the vapor, and some pounds weight of it were deposited from the vapor, before it issued from the kiln, like a hoar frost, upon some articles in the kiln, where the temperature was not above a red heat.

The following papers were also read, but our limits permit us to give only their titles.

On the relative level of land and sea, and on the alteration of the east coast of England; by Mr. Stevenson.

On the superficial beds in the neighborhood of Glasgow; by Mr. Smith of Jordan Hill.

On the Geology of Canada; by Capt. Baddeley.

On the Silurian rocks of Llangollen, and on a plateau of igneous rocks on the east flank of the Berwyn range; by Mr. Bowman.

On ancient sea cliffs and needles in the chalk of the valley of the Seine, in Normandy; by Charles Lyell.

On the geology of the island of Arran; by A. C. Ramsay.

On the geology of Castle Hill, Ardrossan; by Wm. Keir.

On the value of topographical maps and models, with a map of the county of Mayo, in Ireland; by Mr. Bald.

Additional Notes on the Wadi el 'Arabah, in Syria; by Rev. E. Robinson, of New York.

Some observations on relief maps; by Mr. A. Ravenstein.

On the stratified deposits which constitute the Northern and Central regions of Russia; by Mr. Murchison and M. E. de Verneuil.

On the coal formations of the West of Scotland; by Mr. Craig.

On earthquakes in Scotland; by Mr. Milne.

On the occurrence of two species of Shells of the genus Conus, in the lias or inferior oolite, near Caen, in Normandy; by Charles Lyell.

On the yellow sandstone of the carboniferous limestone series of Ireland; by Mr. Griffith.

On a Pleistocene tract in the Isle of Man, and the relations of its Fauna to that of the neighboring sea; by Mr. Edward Forbes.

On the old red sandstone of the Northern Counties of Scotland; by Mr. Murchison.

On the geology of Ceará, North Brazil; by J. E. Bowman.

On the Vale of Solway and Closeburn Basin; by Mr. Knipe.

Sect. D. *Zoology and Botany.*

A paper was read, entitled "Queries respecting the Human Race, to be addressed to travellers and others, drawn up by a Committee of the British Association for the Advancement of Science, appointed in 1839." At the meeting at Birmingham, Dr. Prichard read a paper on *the extinction of some varieties of the Human Race.* He pointed out instances in which this extinction had already to a great extent taken place, and showed that many races now existing are likely, at no distant period, to be annihilated. Science must sustain an irretrievable loss, if so large a portion of the human race, counting by tribes instead of individuals, is suffered to perish before many interesting questions of a psychological, physiological and philological character, as well as many historical facts in relation to them, have been investigated. The Association voted £5 to be expended in printing a set of queries to be addressed to those who may travel or reside in parts of the globe inhabited by declining races. The paper now presented was the result. The subjects embrace a wide field of inquiry, and the queries alone fill thirteen closely printed octavo pages. They refer specially to the stature and weight of the people,—any prevailing proportion between different parts of the body—the complexion—the color and character of the hair and eyes—the formation of the head and face—the skull and all physical peculiarities—the effect of intermarriage where it prevails—health, longevity, physical and intellectual character—language—ceremonies—superstition—education—dress—treatment of sick

—nature of sickness—inferior animals associated with man—ceremonies connected with marriages, births and burials—notions of a future state—habitations of the people—monuments—remains of skeletons—tools and instruments—form of government—food—mode of cooking—clans or castes—laws—geographical limits and character of the region—population—religious observances.

A memoir was read on the *Pollen and Vegetable impregnation;* by Dr. Aldridge, of Dublin. The author having discovered that nitric and other inorganic and organic acids produce the dehiscence of pollen-grains, in the same manner as if placed on the natural stigmatic surface, instituted experiments, of which the following are the general results. 1. The spore of cryptogamic vegetables, which some botanists consider analogous to pollen, do not dehisce under the influence of acids. 2. The pollen of the grasses is spherical, both when dry and when placed in water; with acid it bursts, protruding one long, cylindrical mass, which remains afterward unacted upon by the liquid. 3. The pollen of the Aroideæ, Colchicaceæ, Smilaceæ, Liliaceæ, Commelinaceæ, Bulomaceæ, Amaryllidaceæ, Iridaceæ and Conaceæ, are, when dry, oval, and marked with a dark neutral line; but become, when placed in water, more broadly oval or circular, the long diameter remaining the same, and the opake line disappearing, after the addition of acid; the external membrane of the pollen or peripollen dehisces by a chink or suture sufficiently broad to permit the contents or endopollen to escape without any alteration in its form, after which the endopollen remains unacted upon by the liquid. 4. In the Salicineæ, Salicariæ, Leguminosæ, Rosaceæ, Crassulaceæ, Saxifragaceæ, Hypericaceæ, Rutaceæ, Hippocastaneæ, Resedaceæ, and the tribe Helleboreæ of the Ranunculaceæ, the pollen when dry, oval and marked with a dark central line, becomes when placed in water, round or nearly so, the dark line disappearing; and when acted upon by acids, assumes a triangular form, and protrudes at three equidistant points cylindrical or club-shaped masses very similar at the origin to tubes, and presenting the appearance of being enveloped by a membrane. 5. In the greater number of the remaining Dicotyledons examined, the dry pollen is opake, and either broadly oval or spherical. 6. In the Ericaceæ and Epacridaceæ, the pollen grains when dry, appear triangular or oval in some instances, triangular or rhombic in others, according to the position in which they are examined. Having ascertained the results of acids on the pollen, the author was induced to examine the stigma, and in every case found that the stigmatic tissue gave indications of an acid reaction upon litmus paper. The next question is, in what manner is the fertilizing influence of the male organs communicated to the ovale? After quoting the opinions of Amici, Brown, Fritzche, Corda, Treviranus, Brongniart, and others, the author came to the conclusion that the *boyaux* or intestine-like protrusion from the pollen-grains is the result of the action of acids upon the fluid which contained the fovilla in the pollen-grain; and he inferred this from the fact that this tube or *boyau* is never found when the pollen-grain is placed in water, but is constant when the grain is placed in acid. After describing and explaining the anomalous character of the pollen-grains in Orchidaceæ, Fumariaceæ, Asclepias, &c. the author gave the following conclusions as the results of his researches. 1. The stigma is invariably acid. 2. It is in consequence of this acidity that the pollen bursts. 3. By the same means the fluid contents of the pollen become coagulated, enveloping the fovilla, and assuming, according to the method of dehiscence, different and very remarkable forms.—The memoir was illustrated by an extensive series of drawings.

A paper on the *Alpaca* was read by W. Danson. Since the meeting at Birmingham, about twenty of these animals had been imported into Liverpool. The alpaca is remarkable for its extraordinarily long wool, the staples being from twenty to twenty four inches in length. The wool is naturally free from grease, in which respect it differs materially from the sheep, attributable to its not perspiring through the skin, and consequently not requiring the artificial protection of smearing with tar and other substances injurious to the wool, as far as the manufacture is concerned; and in shearing, the animal requires no washing preparatory to the operation. The alpaca is hardy, flourishing under the line of perpetual snow, in the mountains of the Andes, (Peru,) and has a peculiar coat of silky wool, which proves a complete protection against an atmosphere at all times excessively humid, and against the deluging rain which continues to fall more than four months in the year. The animal is not only capable of undergoing great fatigue, but moreover lives on mountain herbage, little better than a kind of withered grass, and in times of scarcity has been sustained several days without water, taking only a handful of maize. Their flesh is considered equal to venison, being commonly eaten by the Peruvians, who state the slaughter of them for food to be about four millions annually. The importation of the alpaca wool in 1839, is stated at one million pounds, and within the last year at three millions. It seemed, however, doubtful whether the animal could be made to thrive in Great Britain.

Prof. Agassiz made a communication on *animals found in Red Snow*. He stated that Shuttleworth had lately demonstrated that besides the *Protococcus nivalis*, the red snow contained several species of infusoria. The results of Prof. A.'s observations led him to conclude that the red snow was altogether an animal production, and that the so-called *Protococcus nivalis* is the ova of a species of rotiferous animal called by Ehrenberg *Philodina roseola*. This animalcule he had found dead in the red snow, and abundantly in ditches in the neighborhood, at the bottom of which its ova produced a red deposit. Under the microscope the colored ova in the ovaries could be distinctly observed. He had also seen the infusoria described by Shuttleworth.

The following papers were also communicated to this Section.

On the preservation of animal and vegetable substances; by Prof. Henslow and the committee.

Further researches on the British Ciliograda; by Edward Forbes and John Goodsir.

On the plants and animals found in the sulphureous springs of Askern and Harrowgate, Yorkshire; by Dr. Lankester.

On the structure of fishes; by Dr. Macdonald.

On the regeneration of lost organs discharging the functions of the head and viscera, by the Holothuria and Amphitrite, two marine animals; by Sir John G. Dalyell.

Report of a committee appointed to dredge various parts of the Coasts of Great Britain for marine animals.

On an anomalous form of the plum observed in the gardens of the province of New Brunswick; by Prof. Robb.

On the identity of the fish called the *parr* with the fry of the salmon; by Messrs Shaw and Wilson.

On the true method of discovering the natural system in Zoology and Botany; by Hugh Strickland.

On the organs of sense in the Salmon; by Dr. Lizars.

On a new Salmon-stair, with a model; by Mr. Smith, of Deanston.

On the development of the embryo in the ova of fishes, more especially of the family Salmonidæ; by Prof. Agassiz.

On a new genus of Ascidians, viz. *Pelonaia;* by Messrs. Forbes and Goodsir.

On the Medusæ; by Mr. Patterson.

On the various modes of fishing employed by Indians in the West of Guiana; by Mr. Schomburgk.

Report on the Irish Fauna, including only the Vertebrata; by Mr. Thomson.

Report of the committee for engraving skeleton maps, for recording the distribution of plants and animals.

On the migration of birds on certain parts of the coast of Ireland; by Sir Thos. Phillips.

Remarks on the synonyms and affinities of some South African genera of Plants; by Dr. W. Arnott.

Further remarks on the bones of a whale found in 1839 in the keep of Durham Castle; by G. T. Fox. The letter dated 1661, which appeared to clear up this fact, proves to be a fabrication. (See Vol. xxxviii, p. 130.)

On the growth of Cotton; by Dr. Lankester and Mr. Felkin.

On India Cotton; by Dr. Alexander Burn.

On the first changes in the ovum in mammiferous animals; by Dr. Martin Barry.

Sect. E. *Medical Science.*

The following communications were presented to the Section.

Report of the London Committee on the motions and sounds of the heart.

On the use of the conglobate glands; by Dr. Jeffrey.

On the diffusion of contagious fevers, the laws which govern them, and the mode by which they are communicated; by Dr. J. Perry.

On the properties, chemical and therapeutic, of the Matias Bark; by Dr. McKay.

On the therapeutic effects of Croton oil in certain affections of the nerves, by Dr. Newbigging.

On the results of amputations; by Dr. Lawrie.

On the medicinal action of bromine and its compounds; by Dr. R. M. Glover.

On opacity of the cornea, produced by sulphuric acid; by Dr. R. D. Thomson.

On dislocations of the ankle-joint backwards and forwards; by Mr. Douglass.

On the physiology of the lungs and bronchi; by Dr. C. J. B. Williams.

Investigation of the physiology of the organ of hearing; by Dr. Sym.

On the manner in which vital actions become suspended in Asphyxia; by Dr. John Reid.

On the subserviency of the fifth pair of nerves to the adjustments of the organs of sense, and of a retransmission through its nasal branch and the lenticular ganglion to the iris; by Dr. R. Fowler.

Notice of Bonten d'Alleppe and Bagdad boil in the East; by Dr. Charles W. Bell.

On the effects of air when injected into the veins; by Dr. J. R. Cormack.

On the anatomy of the Medulla Oblongata; by Dr. John Reid.

On the connexion between the nervous system and muscular contractility; by Dr. John Reid.

On the structure of the Gastro-intestinal mucous membrane; by Prof. Allen Thomson.

On local inflammatory action, the effusion of coagulable lymph, formation of pus, &c.; by Dr. Perry.

On Pertussis; by Dr. Hannay.

On Mnemonics; by Dr. McDonald.

Sect. F. *Statistics.*

The following communications were presented to the Section.

Report on the state of crime within Glasgow and city police jurisdiction, with observations of a remedial nature; by Capt. Millar.

On the population, trade and commerce of the city of Glasgow; by Dr. Cleland.

On the Glasgow Asylum for the blind; by Mr. Alston.

On the application of Statistics to moral and economic science; by Dr. Chalmers.

Illustrations of the practical operation of the Scottish system of the management of the poor; by Dr. Alison.

On the comparative vital statistics of Edinburgh and Glasgow; by Mr. Watt.

On the bill circulation of Great Britain; by Mr. Leatham.

On the excess of population, and on emigration as a remedy for it in the Highlands of Scotland; by Prof. Ramsay.

On the vital statistics of Glasgow; by Dr. Cowan.

On the state of education and crime in England and Wales; by Joseph Bentley.

On Pawnbroking in Ireland; by Mr. Porter.

Tables containing a comparative view of the state of crime in London, Dublin and Glasgow.

On the state of crime in the borough of Calton; by Mr. Rutherglen.

On the state of crime in the district of Gorbals; by Mr. Richardson.

On the state of crime in the suburban burgh of Anderston; by Mr. Findlater.

Report of the Manchester Statistical Society on the state of education in the borough of Kingston-upon-Hull.

On the population of certain parts of Africa; by Mr. Saxe Bannister.

Sect. G. *Mechanical Science.*

Mr. Wallace offered a paper on *extinguishing fire in steam vessels.* This he proposes to effect by steam itself. The plan has been some time before the public, and many successful experiments have been made in the presence of scientific men. Among the most important was the following, made on board the *Leven* steamboat:—On the cabin floor, a space of 10 feet by 14 was covered with wet sand, on which were laid iron plates, and on these a fire was kindled with about $4\frac{1}{2}$ cwt. of very combustible materials, such as tar barrels, &c. A hose 34 feet long and $2\frac{1}{2}$ inches in diameter, extended from the boiler of the engine to the cabin, and when the fire had been sufficiently kindled, so that the panes of glass in the windows of the cabin began to crack by the heat, the steam was let in, and the doors of the cabin shut. The fire was extinguished in about four minutes. Several trials were made, and all with like success. On another trial, a metal pipe of a greater diameter than the hose was connected with the steam-boiler, and extended into the cabin. A small square hatch was cut in the deck, immediately above the cabin, and through this opening were lowered down into the cabin two movable grates, each containing a blazing fire, well kindled, of about 1 cwt. of coals. The hatch on the deck, and the cabin doors were then shut, and the steam let in, and in fifteen minutes the small hatch was opened, and one of the grates hoisted up, when the whole mass of coal and cinders, which had before formed a powerful fire, was found to be completely extinguished. This experiment was twice repeated, with equal success.

On the application of *Native Alloy for Compass pivots*, by Capt. E. J. Johnson, R. N.

Among those portions of a ship's compass which most affect its working, are the pivots and caps on which the needle and card traverse, and which like the balance of a chronometer (but of far more importance to the practical navigator) should not only be fitted with the most scrupulous attention to accuracy, but be made of materials capable of maintaining a given form under the trials to which such instruments are necessarily exposed. Having examined a great variety of compasses which had been used at sea, wherein Capt. J. noticed that the pivots were generally injured, and often by rust, he searched numerous records of experiments for its prevention, and for improving the quality of steel in other respects, by means of alloys of platinum, palladium, silver, &c., (alluding particularly to the experiments of Dr. Faraday and Mr. Stoddart,) and Mr. Pepys having obligingly supplied Capt. J. with specimens of similar kinds of steel to those used by them, these examples, together with pivots made of the ordinary kind of steel, and hardened and tempered in the manner recommended by eminent instrument makers, were placed in a frame for experiment; and to these again Capt J. added certain contrivances of his own, such as rubbing a steel pivot with sal-ammoniac, then dipping it into zinc in a state of fusion, and afterwards changing the extreme point. Some specimens he coated with a mixture of powdered zinc, oil of tar, and turpentine; and others again were set in zinc pillars having small zinc caps, through which the extreme point of the pivot protruded, after the manner of black lead through pencil tubes. The whole of the specimens were then placed in a cellar, occasionally exposed to the open air, examined from time to time during more than half a year, and their several states, as respected oxidation, duly registered. The general result was, that not any of the kinds of steel pivots used in this trial, except such as were coated with zinc, remained free from rust, while the pivot made of the *native alloy* which is found with platinum, completely retained its brilliancy. Capt. J. then applied to this singular substance a more severe test, first by placing sulphuric acid, and then nitro-muriatic acid upon it; but even under this trial he could not observe that any change had been effected, although the blade of a penknife subjected to a similar process, was rusted to the centre. Having enumerated the facts respecting the trials to which he had subjected this curious material, Capt. J. stated the conclusion that he had come to, viz. that it is sufficiently tough not to break, and hard enough not to bend, under the trials to which it would be *fairly* exposed: and that being alike free from magnetic properties and liability to oxidation from exposure to the air, it possesses the qualities requisite for the pivot of the mariner's compass: and he anticipated that when fitted with a ruby cap to correspond, it would be found greatly to improve the working. Besides the use of this substance for compass pivots, Capt. J. stated that it might probably be found advantageous for other instruments, and especially for the points of the axes of the dipping needles, fitted on Mr. Fox's plan, for use on shipboard.—Mr. Hawkins has used this *native alloy* for several years in tipping the points of pens, and not a single instance exists in which any of these pens shows the least symptom of wear. He tried native alloy on a cap, in comparison with ruby, when he found that in the same circumstances, the ruby was ground away with diamond dust twice as rapidly as the native alloy. He had made engravers' tools of the same metal, and when made too sharp they cannot be blunted on the Turkey stone, but only by diamond dust.

The following papers were also read.

On safety-valves for steam-boilers; by Mr. Galline.

On wheels of Locomotive engines; by Mr. Grime.

On the temperature of most effective condensation in steam vessels; by J. Scott Russell.

On warming and ventilating buildings; by Mr. Ritchie.

On timber bridges of a large size, in special reference to Railways; by Mr. Vignoles.

Experimental inquiry into the strength of iron, with respect to its application as a substitute for wood in ship-building; by Mr. Fairbairn.

On raising water from low lands; by Mr. Fairbairn.

Additional notice concerning the most economical and effective proportion of engine power to the tonnage of the hull in steam vessels, and more especially in those designed for long voyages; by J. Scott Russell.

On the action of air and water on iron; by Mr. Mallet.

On the turbine water-wheel; by Prof. Gordon.

On producing true planes or surfaces on metals; by Mr. Jos. Whitworth.

Report of the Committee on the best form of Vessels.

On the economy of railways in respect of gradients; by Mr. Vignoles.

On the fan-blast as applied to furnaces; by Mr. Fairbairn.

On new rain-gauges; by Jas. Johnston, and also by Mr. Thom.

On a Revolving Balance; by Mr. Lothian.

On the combustion of coal and the prevention of the generation of smoke in furnaces; by Mr. Williams.

Art. VIII.—*Remarks relating to the Tornado which visited New Brunswick, in the State of New Jersey, June* 19, 1835, *with a Plan and Schedule of the Prostrations observed on a section of its track;* by W. C. Redfield, Esq.*

[From the Lond. Phil. Mag. and Jour. of Science. Revised by the Author.]

In a paper printed in the American Journal of Science, in which I referred to the support given by Prof. Bache to Mr. Espy's theory of storms, at the meeting of the British Association in 1838, founded upon observations made on the New Brunswick tornado, I have stated, that in my own examinations I had observed numerous facts which appear to demonstrate the *whirling* character of this tornado, as well as the *inward* tendency of the whirling vortex at the surface of the ground; and further, that the direction of rotation was *towards the left*, as in the North Atlantic hurricanes.† It was due to Prof. Bache that my observations should be brought forward; a task which has been too long delayed, partly from a desire that he would revise his former conclusions. The facts now presented form part of the evidence to which I then alluded.

If the effects which I present for consideration be due to "a moving column of rarefied air without any whirling motion at or near the surface of the ground," as maintained by Professor Bache,‡ we might expect to find a relative uniformity in the effects on the two opposite sides or margins of the track. How far this is the case may be seen by inspecting the observations which are found upon the annexed plan of prostrations.

The occurrence of these tornadoes appears to have been noticed from the earliest antiquity; and their violence has been considered as the effect of an active whirling motion in the body of the tornado; this peculiarity of action having often been supported by the testimony of eye-witnesses.

---

* [Communicated by Sir John F. W. Herschel, Bart.] This paper was intended by its author to have been read at the late meeting of the British Association in Glasgow, but was unfortunately detained.—J. F. W. H.

† Amer. Jour. of Science, Oct. 1838, vol. xxxv, pp. 206, 207.

‡ Transactions of Amer. Phil. Society, vol. v, p. 417, New Series.

The whirling motion, however, has not been recognized by Prof. Bache, Mr. Espy,* or Prof. Walter R. Johnson,† in their several accounts of the New Brunswick tornado; these writers having been led to adopt or favor a theory of ascending columns in the atmosphere, founded on the supposed influence of calorific expansion accompanying the condensation of vapor.

It is remarkable that previous to this period the evidences of the rotation or other characteristic action of tornadoes appear not to have been duly examined and recorded, nor even to have received the distinct consideration of scientific observers. We are therefore left to seek out the peculiarities of their action, by examining the direction of the prostrations and other effects of the wind; and from a careful induction from the effects which are thus registered as by the finger of the tornado, we may hope to arrive at satisfactory conclusions.

If the numerous prostrations of trees and other objects, which may be observed in the path of a tornado, be the effects of a violent whirlwind, it appears most reasonable to infer that this whirl had the common properties which may be observed in all narrow and violent vortices, viz. *a spirally descending and involuted motion* of the exterior and lower portions of the vortex, rapidly quickened in its gyrations as it approaches toward the centre or axis of the whirl, and thence continued (in the case of the whirlwind) spirally upward, but gradually expanding in its spiral course by an evolute motion in ascending towards the extreme height of the revolving mass.

If we now contemplate the action of this whirling body, while in a state of rapid progression, on the several objects found in distinct portions of its path, we may expect to witness effects of much complexity, particularly as regards direction; and, also, that amid this apparent complexity, some clue may be obtained that will serve to indicate or establish the true character of its action. Some of the effects which may be expected or observed, will be here considered.

1. We may expect to find, in the path of the whirlwind, strong evidence of the inward or vorticular course of the wind at the earth's surface; the violence of which inward motion is clearly

* Trans. Amer. Phil. Society, vol. v, New Series.

† Journ. Academy Nat. Sciences of Philadelphia, vol. vii, part ii.

indicated by the force with which various objects, often of much weight, are carried spirally upward about the axis of the revolving body.

Now the effects of this inward vorticular motion at the surface of the ground, are clearly manifested in the cases before us; and are also well illustrated by Prof. Bache, in his paper on this tornado, although referred by him to a different action.*

2. As the effects which may be observed at various points in the track were produced at different moments of time, and by forces acting in different directions, as well as of various intensities, we may expect to find great diversities in the several directions of the fallen trees and other prostrated bodies; and further, as all the forces, in addition to their inward tendency, have likewise a common tendency in the direction pursued by the tornado, we may expect to find, also, full evidence of this progressive force in the direction of the fallen bodies.

These effects, I need hardly state, are distinctly observed in the case before us; and appear likewise from the observations of Prof. Bache. The results already noticed have been observed also in the tracks of other tornadoes: so that a general inclination, both inward and onward, amid the various and confused directions of the fallen bodies, is distinctly recognized by all parties to this inquiry.

3. It has been often noticed, that where two fallen trees are found lying across each other, the uppermost or last fallen points most nearly to the course pursued by the tornado.

In view of the facts above stated, much pains have been taken to establish, by induction, a central and non-whirling course in the wind of the tornado; first inward and then upward, like that resulting from a common fire in the open air. I do not propose to notice the insuperable difficulties which appear to attend this hypothesis. It is important to state, however, that all the above mentioned effects, when theoretically considered, are, at least, equally consistent with the involute whirling action of an advancing vortex. This important consideration I have not seen recognized by the advocates of the non-whirling theory; and it seems proper, therefore, to point out, as we proceed, other and more distinguishing effects of the whirling action.

* Transactions of American Philosophical Society, vol. v, N. S.

4. It has been noticed, also, that the directions given to broken limbs and other bodies, by the successive changes in the direction of the wind as the tornado passed over, have been found in opposite courses of change, on the two opposite sides of the track.

This fact, too, has been strongly urged as disproving a rotary motion. But, unfortunately for the objection, this effect accords fully with the rotary action of a progressive mass of atmosphere; as is well known to all who clearly understand the *theory* of rotary storms.

In all such whirling masses the successive changes in the direction of the wind, *result solely from their progressive motion*, and necessarily take place in opposite directions or courses of change on the two opposite sides of the advancing axis. This indication fails, therefore, as a theoretic test; and I now proceed to notice others, which are peculiar to a progressive whirling action.

5. In considering further the effects of such action, we may expect to find that the greatly increased activity of gyration which is always observed near the centre of a vortex, will be indicated by a more violent and irregular action in and near the path pursued by the axis of the whirlwind, than is found under its more outward portions.

This effect is often strikingly exhibited in the path of tornadoes; while, in the supposed ascent of a non-whirling column, it would seem that no part of the surface would be so much exempted from its action, and particularly from its power of prostration, as that lying near its centre.

6. As the effect of rotation must be to produce, on one side of the advancing axis, a reverse motion which is contrary to the course of the tornado, it is evident that on this side the prostrating power will be much lessened; that the cases of prostration, therefore, will be here less numerous; and that some of these, at least, will be produced in a backward direction, more or less opposite to the course of the tornado. By this criterion, not only the whirling movement, but the direction of the rotation also, may be clearly ascertained.

This effect is best observed by comparing the two opposite margins of the track, and is strongly exemplified in the case before us. Here we find, that most of the trees prostrated within five chains (110 yards) from the northern or left-hand margin of the track, lie in directions which are more or less backward from the course of the tornado. The prostrations in this part of the track are also for the most part less general than on the opposite side of the axis,* a greater portion of the trees being left standing.

It sometimes happens, owing perhaps to the inward or involute motion having exceeded the progressive motion at a particular point, that some inclination backward will be found in the prostrations on the progressive side of the whirl, as seen on the sketch, Nos. 77 to 80. But these unfrequent cases by no means compare with the numerous backward and sometimes *outward* prostrations, found on the reverse side of the whirl, as illustrated by Nos. 1, 3, 4, 7, 9, 10, 12, 13, &c. on the left side of the track. Thus we find here a satisfactory indication that this tornado was a whirlwind; and that the course of its rotation was to the *left* in front.

7. It is also apparent, that the prostrating power of a whirlwind on the side of its reverse motion as just considered, will be limited to a shorter distance than on the opposite or progressive side of its axis.

This is seen in the more limited *extent* of the prostrations on the north or left margin of the track, as compared with the extent of those which incline inward on the right side of the apparent axis. There were many trees standing beyond the northern border of the track, but none had fallen.

8. It follows, in like manner, that on that side of a whirlwind in which the rotary motion coincides with the progressive movement, the prostrating power will not only be increased in its intensity, but will also be effective over a wider space; and that few, if any, of the prostrated bodies will be found to have been thrown backward.

In the case before us, as may be seen in the sketch, the prostrations are found to extend on the southern or right side of the apparent axis to a distance nearly twice as great as on the left side. The same general result has also been noticed in the tracks of other tornadoes which I have examined.

* There was a vacant space in the belt of wood, immediately to the right of the line *c c* or axis of the tornado, owing to which the effect mentioned does not appear so obvious in the figure.

The facts here considered are too important to be overlooked, and seem fully to establish both the whirling action and the course of rotation.

9. If a rotative action be exhibited, the mean directions of all the prostrations, on each of the two opposite sides, will differ greatly in their respective inclinations to the line of progress, and the mean direction of those on the reverse side will be found more backward than on the opposite side, where the rotative course coincides with the progressive action.

In the case before us, the mean direction of all the prostrations on the right side of the track is found to incline 52 degrees inward from the line of progress. The course of the tornado is here taken to be east; although for the last half mile its course had been a little north of east. On the left side, the mean direction is found to be S. 3° W., or 93 degrees inward and backward; a difference in the mean inclination from the course on the two sides of 41 degrees.*

If we now take the indications afforded by the two exterior portions of the track, to the width of five chains on each side, where the effects are more distinctive in their character, we find on the right side a mean inward inclination of 46 degrees, the mean direction being N. 44° E.; while on the left side of the track the mean inclination is not only inward but 48 degrees backward, the mean direction on this side being S. 48° W. We have thus a mean difference in the inclination of the fallen trees, on the two exterior portions of the track, of no less than 92 degrees.

These indications seem conclusive, also, in favor of the whirling action in the direction from right to left.

10. Although of less importance, it should be mentioned that the diminished action of the tornado which is commonly observed on the hillsides and summits over which it passes, and the greatly increased action in the bottoms of the valleys, and even in deep ravines, afford a strong argument against ascribing the effects to the ascent of a non-whirling rarefied column; as the latter, it would seem, must act with greater force on the hillsides and summits than in the bottoms of valleys. The general correctness of the observation above stated cannot justly be questioned.

* The inclinations of the fallen trees from the course, on both sides the axis, are reckoned inward and backward.

11. The sudden and extraordinary diminution of the atmospheric pressure which is said to take place at the points successively passed over by a tornado, causing the doors and windows of buildings to burst outwards, seems to afford strong confirmation of a violent whirling motion; for an effect of this kind is necessarily due to the centrifugal and upward force of the vorticular action in the interior portion of the whirlwind. There are no other means known by which such an abstraction of pressure can be effected in the open air. An increase of calorific elasticity, if such were produced, either generally or locally, would not greatly disturb the equilibrium of pressure, being resisted by the surrounding and incumbent weight of the entire atmosphere. Besides, the immediate effect of such increased elasticity might rather be to burst *inward* the windows and doors of buildings exposed to its action.

Some of the more important indications mentioned above appear also from an examination of Prof. Bache's observations; although the latter are not definitely located by him, as regards the extreme borders of the track. Thus, in Fig. 7 of Professor Bache's paper, assuming the course of the tornado to be east, and rejecting a few observations near the centre, to avoid error, we find in twenty observations on the right side of the track, a mean inward inclination of 64 degrees; and for nine observations on the left side, a mean inclination, reckoned inward and backward from the course, of 104 degrees, being 14 degrees backward.

It is stated by Prof. Bache, "that the trees lying perpendicular to the track of the storm, are not those furthest from the centre of that track." This generalization accords with my own observations; but can hardly be reconciled with an inward non-whirling motion in the tornado.

It may appear to some, that in the case of a whirlwind the greater portion of the prostrations on the reverse side of the axis should be found in a backward direction; and so they would undoubtedly be found, were it not for the inward and the progressive action. But the force is here so far lessened by the reverse action above noticed, that in most cases only a small portion of the trees exposed will be thus prostrated; while the greatest force of the whirlwind, on this side, is felt near its last or closing portion and towards the apparent axis, where the inward, together with the rotative and progressive forces, seem to combine their influence in the closing rush towards the heart of the receding vortex. This appears to account for the nearly opposite directions of prostration found on this side, and it is apparently by this more violent closing action, that many trees which were first overthrown in a direction nearly across the centre of the path, were again moved from their position, or swept onward nearly in the course of the tornado. It is proper to remark here, that an attentive examination of these effects has served to convince me that on the right and more central portions of the track the prostrations for the most part take place either at the outset or under the middle portions of the whirlwind; while on the left or reverse side, up to the line of the apparent axis, and even across the latter, they occur chiefly under the closing action of the whirl, as above described. The violent effects of this central and closing action are more clearly seen as we advance from the left-hand margin towards the centre or apparent axis of the path.

From the causes to which I have just alluded, the effects are usually more violent on and near the line passed over by the axis, than in other portions of the track. This line of greatest violence is found to coincide nearly with the line which separates the inwardly inclined prostrations of the two opposite sides of the track.* The latter line or apparent axis of the track is sometimes called the line of convergence, and is indicated on the figure by the line and arrow *c c*. Along this line, from the causes just mentioned, aided also by the elevating forces about the axis, many of the trees are swept onward, and left with their tops in a direction nearly parallel to the course of the tornado; forming an apparent, but not a just exception, to the more lateral direction which pertains to most of the trees prostrated by the onset of the whirlwind, near the central portions of the track. Indeed, the central or closing violence of the advancing whirl is here so great, that the trees are not unfrequently torn out of the ground and carried onward to considerable distances.

It is proper to state here, that in the tracks of all the tornadoes which I have had opportunity to examine, and in some, at least,

* The line of greatest violence, for the most part, is found somewhat to the right of the line of convergence.

of those examined by others, the course of rotation has been found the same as in the case before us.*

In order to make a just and satisfactory examination of the effects of a tornado, it appears necessary to select portions of the track where the extension of wood or single trees, on each side, is found sufficient to mark clearly the exterior limits of the prostrating power, and where the effects on both sides of the axis are also clearly developed. Our next care should be to ascertain, as near as may be practicable, the line which separates the opposite convergence of the two sides, noticed above as the axis or line of convergence. We should then determine the general direction of this line and of the track at the place examined; which being done, we may proceed to measure the distance to which the prostrations are extended on each side, and then carefully to take the position and direction of prostration of each and of all of the fallen bodies, noting with care, also, any other phenomena which may serve to aid our inquiries. We may thus obtain valuable materials for future analysis; and this course of investigation, if faithfully pursued, will, it is believed, remove all reasonable doubt of the rotative action of these tornadoes. An examination of their probable origin, and the causes of their enduring activity and violence, belongs not to the present occasion.

New York, February 5, 1841.

---

* As in the tornado which passed through Allegany county, New York, July 25th, 1838; described by Mr. Gaylord in the American Journal of Science, Vol. xxxvii, p. 92.

SCHEDULE OF THE PROSTRATIONS

Observed on a Section of the Track of the New-Brunswick Tornado, of June 19th, 1835.

No.	Direction of Prostration.	Inclination.
	TABLE I. Left side of the track to the line *b b*—5 chains.	Inward and backward.
a	Tree lies S. 20° W. . .	110°
b	.. .. S. 80 W. . .	170
1	.. .. N. 67 W. . .	203
2	.. .. S.	90
3	.. .. W.	180
4	.. .. S. 80 W. . .	170
5	.. .. S. 40 W. . .	130
6	.. .. S. 40 W. . .	130
7	.. .. S. 80 W. . .	170
8	.. .. S. 10 E. . .	80
9	.. .. S. 50 W. . .	140
10	.. .. S. 50 W. . .	140
11	.. .. S. 26 W. . .	116
12	.. .. S. 50 W. . .	140
13	.. .. S. 65 W. . .	155
14	.. .. South . . .	90
	Mean direction, 16 cases, S.48°W. Mean inclination from course, inward and backward, 138 degrees.	
	TABLE II. Left side of the axis, from the line *b b* to *c c*—1½ chains.	
15	Tree lies S. 2° E. . .	88
16	.. .. S. 12 W. . .	102
17	.. .. S 35 E. . .	55
18	.. .. S. 62 E. . .	28
19	.. .. S. 25 E. . .	65
20	.. .. N. 80 W. . .	190
23	.. .. S. 20 E. . .	70
24	.. .. S. 80 E. . .	10
25	.. .. S. 45 E. . .	45
26	.. .. S. 10 E. . .	80
27	.. .. S. 45 E. . .	45
28	.. .. S. 20 E. . .	70
29	.. .. S. 60 E. . .	30
30	.. .. S 60 E. . .	30
31	.. .. East. . . .	00
32	.. .. S. 75 E. . .	15
33	.. .. S. 56 E. [Included as belonging by its inclination to this table.]	34
35	.. .. East . . .	00
	Mean direction, 18 cases, S. 37° E. Mean inclination inward, 53 deg.	
	TABLE III. Right side of apparent axis from line *c c* to W E—3 chains.	
21	Tree lies N. 56° E. . .	34
22	.. .. N. 60 E. . .	30
34	.. .. N. 80 E. [Included as belonging by its inclination to this table.]	10
36	.. .. N. 85 E. . .	5
37	.. .. East (two) . .	0
..	.. .. East . . .	0
38	.. .. East . . .	0
39	.. .. N. 30 E. . .	60
40	.. .. N. 70 E. . .	20
41	.. .. N. 55 E. . .	35
42	.. .. N. 50 E. . .	40
43	.. .. N. 78 E. . .	12
44	.. .. N. 45 E. . .	45
45	.. .. N. 45 E. . .	45
46	.. .. N. 25 E. . .	65
47	.. .. N. 35 E. . .	55
48	.. .. N. 40 E. . .	50
	Mean direction, 17 cases, N.60° E. Mean inclination inward, 30 deg.	
	TABLE IV. Right side of axis, from line W E to line *a a*—4½ chains.	Inward and backward.
49	Tree lies N. 67° E. . .	23°
50	.. .. N. 45 E. . .	45
51	.. .. N. 22 E. . .	68
52	.. .. N. 3 E. . .	87
53	.. .. N. 30 E. . .	60
54	.. .. N. 10 E. . .	80
55	.. .. N. 35 E. . .	55
56	.. .. North . . .	90
57	.. .. N. 10 E. . .	80
58	.. .. N. 3 E. . .	87
59	.. .. N. 45 E. . .	45
60	.. .. N. 10 E. . .	80
61	.. .. N. 35 E. . .	55
62	.. .. N. 60 E. . .	30
64	.. .. N. 40 E. . .	50
65	.. .. N. 20 E. . .	70
66	.. .. N. 10 E. . .	80
67	.. .. N. 20 E. . .	70
68	.. .. N. 40 E. . .	50
69	.. .. N. 70 E. . .	20
70	.. .. N. 50 E. . .	40
71	.. .. N. 35 E. . .	55
72	.. .. N. 30 E. . .	60
73	.. .. N. 50 E. . .	40
74	.. .. North (two) .	90
75	.. .. North " .	90
76	.. .. North " .	90
77	.. .. N. 20 W. (clump of 3)	110
78	.. .. N. 35 W. . .	125
79	.. .. N. 30 W. . .	120
80	.. .. N. 10 W. . .	100
81	.. .. N. 65 E. . .	25
82	.. .. N. 70 E. . .	20
	Mean direction, 33 cases, N. 24° E. Mean inclination, 66 degrees.	
	TABLE V. Right side of track, from line *a a* to outward limit of prostration—5 ch.	
63	Tree lies N. 65° E. . .	25
83	.. .. N. 45 E. . .	45
84	.. .. N. 40 E. . .	50
e	.. .. N. 55 E. . .	35
85	.. .. N. 70 E. . .	20
86	.. .. N. 23 E. . .	67
87	.. .. N. 31 E. . .	59
88	.. .. N. 20 E. . .	70
89	.. .. N. 22 E. . .	68
90	.. .. N. 10 E. . .	80
91	.. .. N. 55 E. . .	35
92	.. .. N. 70 E. . .	20
93	.. .. N. 55 E. . .	35
94	.. .. N. 68 E. . .	22
95	.. .. N. 25 E. . .	65

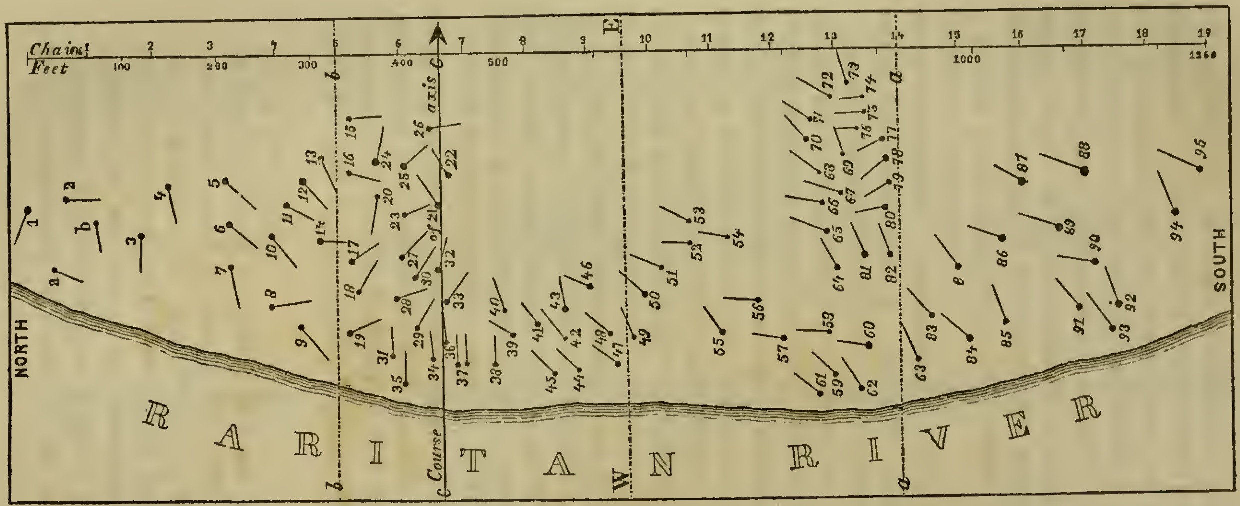
Mean direction, 15 cases, N. 44° E. Mean inclination inward from course of tornado, 46°.

Mean direction of all the prostrations on *left* of axis, 34 cases, S. 3° W.—being 3° backward, or 93° inward and backward.

Mean direction on *right* of axis, 65 cases, N. 38° E.—being 52° inward from course.

Difference of mean inclination on the two sides, 41 degrees.

Difference of opposite marginal sections (Tables I and V) 92 degrees.



Sketch of the Prostrations found on a section of the Track of the Tornado of June 19, 1835, on the bank of the Raritan, opposite the City of New Brunswick, in the State of New-Jersey.

Explanations.—The east bank of the river is here covered with a belt of wood; the latter having a very irregular outline on the east, where it is bounded by a clear field. The line *c c* represents the apparent course of the axis of the tornado: W. west, E. east. The large dots on the several figures show the root ends of the trees, which were chiefly a species of cedar. In all these cases of prostration, part of the roots were still fast in the ground. Course of the tornado east. The approximate positions of the several trees are in many cases slightly changed in the sketch, for the purpose of a distinct exhibition of each.

Note.—This bank of the river is intersected by small ravines with wooded margins, one of which is nearly opposite to chain 5, and another is near chain 13, and which cause most of the irregularity in the wooded outline.

Art. IX.—*Notice of a Model of the Western portion of the Schuylkill or Southern Coal-Field of Pennsylvania, in illustration of an Address to the Association of American Geologists, on the most appropriate modes for representing Geological Phenomena;* by Richard C. Taylor, Member of the Amer. Phil. Soc.; Fellow of the Geol. Soc. of London, and of other Societies in Europe and the United States. Read 9th of April, 1841.

On the 18th of June, 1830, I had the honor of presenting to the Geological Society of London, and of reading a concise description of two models and sections of part of the mineral basin of South Wales, in the vicinity of Pontypool.

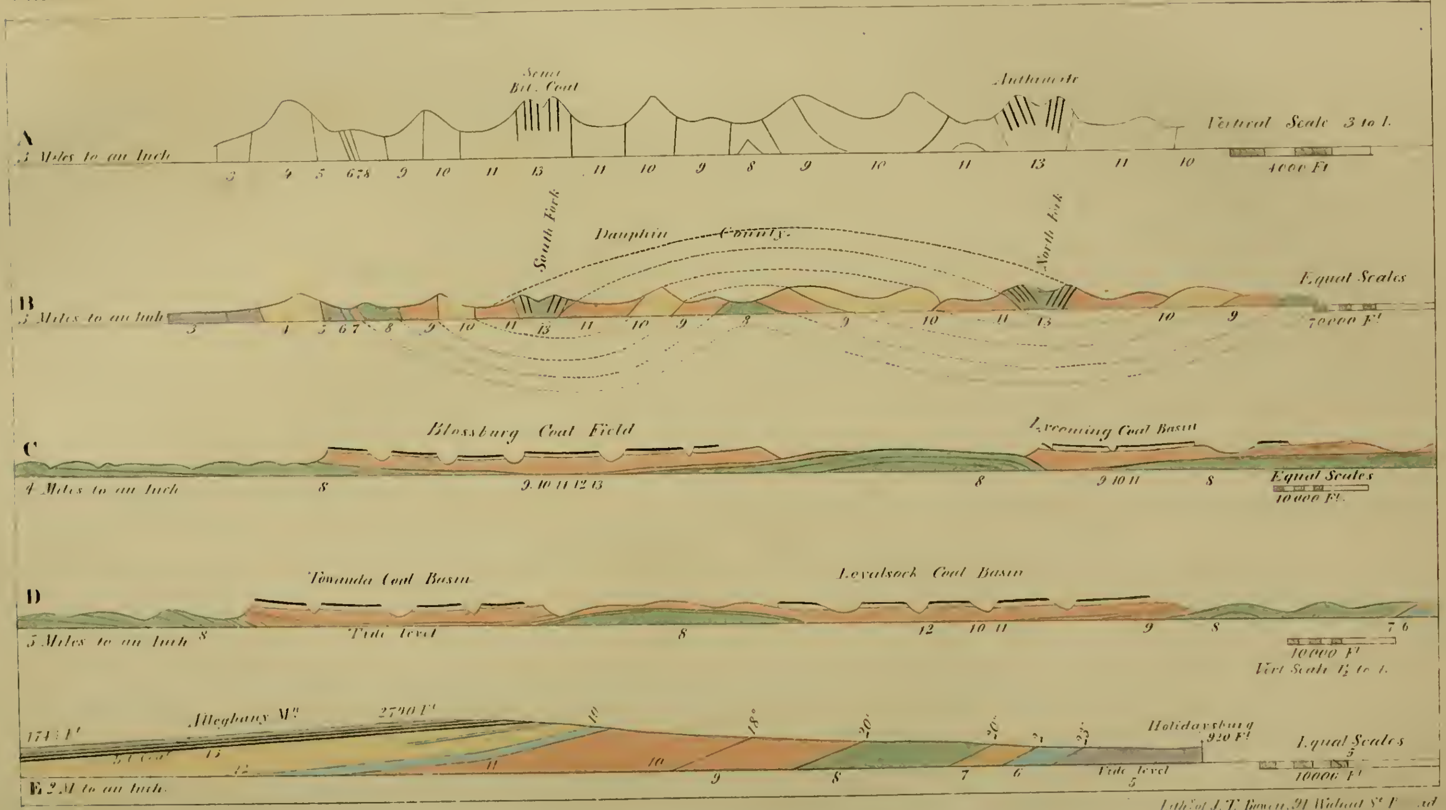
On the present occasion I take the liberty of exhibiting to the Association of American Geologists, at their second annual meeting, a model of the western half of the Schuylkill coal-field, in Pennsylvania. This is, in all probability, the first geological model that has been constructed in the United States; as was, I believe, that of the Welsh mineral district, the earliest of its kind; and as such was received in the exhibition of the Society of Arts.* I have felt anxious, I may say ambitious, to introduce the first American geological model to this Association. It seems necessary to the occasion to make a few explanatory observations, and I desire especially to address some general remarks to this meeting, on the available methods of geological illustration.

Before proceeding to the descriptive details of the present work, and of the region which it represents, I would advert to the extreme applicability of the science of modelling to the purposes of geological elucidation.

During a somewhat active life, embracing thirty six years of occupations connected either with the superficial features of our earth's surface, in various climates, or with investigations of the positions of rock formations, the modes of representing the principal phenomena, and the different systems resorted to for practical illustrations, have, of course, been long and frequently under deliberation. The result, it need scarcely be added, is an increasing conviction of the vast superiority of that method which ad-

* The gold Isis medal being awarded to the exhibitor in 1830.

PLATE 1



Lith. of J. T. Bowen, 94 Walnut St. Phil.

mits of shewing the objects solidly, in relief; and according to their actual proportions, whenever practicable. I mean the process of modelling areas of country, in preference to any other method of representation; whether by drafts, diagrams, tables, maps, sections or other customary means. Under this impression, and with a view to convey these sentiments in a useful direction, I have made some exertion to complete, for this occasion, a specimen of the art, illustrative of several hundred square miles of interesting country in the interior of this state; but have only during the intervals of the present meeting of geologists, found time to prepare and commit to paper the following observations.

With the best assistance which art can confer, by means of horizontal or vertical or concentric shading, or by the most elaborate arrangement of lines upon a plane surface, to produce the effects of light, shadow, height, depth and perspective, such processes, it is universally conceded, fail to accomplish what is simply effected by modelling. If to these desiderata in geological illustrations, and to those other lines which are indispensable to topographical or local delineation, we add those which are intended to represent the courses and the inclination of strata, and the breadths and separate characters of formations, the difficulties attendant on lucid illustration are heightened, in any process short of modelling.

Whenever the scale, upon which a given area is protracted, is sufficiently large to permit an approximate correspondence between the horizontal and the vertical admeasurements, the effect is perfect. The utility of the work is enhanced, in as much as it combines the exhibition of both transverse and horizontal sections on the field of survey; and illustrates not only the external features and physical geography of the district, but enables the interior structure, the grouping of its mountain masses, the inclinations, bearing, direction, contortions and dislocations of the strata into which those formations and masses are subdivided, to be exhibited in a simple yet very striking and appropriate manner. With the addition of superficial coloring, the pictorial character of the region represented, can be as accurately depicted as in a highly finished landscape. Perhaps even more so; in as much as the positions of all surrounding objects, and of all accessory details, are defined with geognostic accuracy in the one case, rather than imperfectly traced in the other, however experienced may be the hand and the eye of the artist. The interesting and accurate effect incidental to a picture, thus formed in relief, is apparent enough when the observer brings his eye to the level of any point whatever on the model;—the summit of a mountain, the point of a bluff, or the curve of a river, for instance,—from whence all that he needs in obtaining at ease and convenience a view of the surrounding scenery, is accomplished.

For topographical observations, for rapid reconnoissances, for tracing routes for railroads, for canals, or for ordinary roads and communications, the model system presents facilities for numberless practical purposes, and may be the means of saving a great deal of preliminary labor and expense, on such occasions, in a mountainous or forest district.

In all such regions, it is common to adopt as the best, because they are the most natural and the most permanent, lines of demarcation, the elevated chains, the elongated ridges, the ranges of highlands or platforms which divide the sources of rivers and influence the descent of drainage; or to constitute the rivers themselves, as they flow between these ranges, the boundaries of local and territorial jurisdictions. All of these are particularly and necessarily prominent features in a model; and these, the most sublime and most imperishable monuments in all countries, have with propriety been selected as the most fitting for such conventional purposes. Had a model, however roughly constructed, been in existence to illustrate the physical geography of what is termed "the disputed territory" in the northeast, half a century of embarrassment and conflicting opinions, and local difficulties, might have been saved to the interested parties. It is not too late, even at this hour, to exhibit in this way, all the topographical characters of that region; to represent those great natural features, suggested for lines of international boundary. All the details applicable for this purpose are but now in progress of collection. From their arrangement we may expect to result the clearing up of existing obscurities; a more accurate construction of terms, and the adjustment of important points, now at issue.

Models are peculiarly adapted to the exhibition of geological phenomena. For ordinary convenience of transportation and portability, no doubt maps are best adapted, for the library, or for the use of the traveller. But for public and more enlarged objects, and for scientific institutions, the more permanent and ponderous form of representation, such as that we have now under consideration, does appear to possess stronger recommendations. We would desire to extend the principle so far as to introduce it in the final elucidation of state geological surveys; convinced that the greatest advantage would result from it. There exists no remarkable or insurmountable difficulty in thus exhibiting, in the distinctest form, the most prominent geological features of the states around us. There appears to be no practical or scientific reason, (pecuniary considerations aside,) why the results of those labors which have proceeded so successfully, and are still prosecuted so satisfactorily, in most of the States in the Union, by gentlemen of high professional eminence, a large proportion of whom are now assembled here,—there appears no reason, I remark, why the vast mass of facts which they have thus so industriously accumulated, should not finally receive this mode of representation. The capitols of Harrisburg, of Richmond, or of Albany, and other seats of local government, might be honorably adorned with instructive geological models of their respective states; and in due time when the great work has so far proceeded in advancement, the Capitol of Washington itself might be enriched by one superb model, in which shall be concentrated those results which so much combined talent has brought to light and reduced to order, and the usefulness of which has been demonstrated.

The illustration of physical geography by means of models has long been practiced in the admirable representations of mountainous regions in Southern Europe, by skillful artists. Many of the European museums contain extremely beautiful models of alpine districts. Some of these even embrace a large portion of Southern Europe; constituting, in fact, maps in relievo, elaborately executed and truly valuable as works of art. For the most part they are designed as pictorial representations of highly interesting regions; without particular reference to their geology, or to the interior structure and arrangement of their elevated masses. Although these much prized models obtained a place in the English collections, as splendid specimens of a peculiar art, the application of that art to economic geology, and to kindred subjects, for which it is especially adapted, has been but little employed in England and its introduction is of comparative recent date. The geological model, for which the Society of Arts conferred their gold medal, in 1830, was the first which had been exhibited in that excellent institution. A recommendation to adopt the more frequent application of the system, has been occasionally urged by prominent geological authorities. Since the date referred to two most elaborately executed models, on a very large scale, have been exhibited in London; the one represents the field and battle of Waterloo, the other depicts the beautiful lake scenery of the north of England; both of them the result of vast labor and singular perseverance. Those models which, in Germany and some other mineral countries, represent the internal economy of the mines and mining operations, belong to a class extremely useful, but different to that which has given rise to the present memoir.

We come now to the consideration of the model before us. In point of mineral value, of geological peculiarities, of statistical intricacy, and of highly picturesque features, the district here represented in miniature, yet with sufficient faithfulness as regards characteristic distinctness, has perhaps no equal, within a similar area, in America. Its approximation to the tide waters of the Atlantic coast, moreover, confers upon it a commercial value, in connection with the sources of industry and of remuneration for labor, manifestly within its limits. We are justified in adverting to these circumstances, because the useful results, and the beneficial application of science in economic geology, form legitimate objects of associations like that which I have the honor of addressing.

The area here illustrated comprehends seven hundred and twenty square miles; being in length forty five miles, and in width sixteen miles. It extends in breadth from four miles above Harrisburg, northward, to Millersburg on the Susquehanna, at the junction of the Wiconisco railroad. In length it reaches from the western extremity of the Cove mountain, on the west side of the Susquehanna to within eight miles of Pottsville. It comprises the two forks into which the Schuylkill coal-field separates, opposite to Pinegrove, in the Swatara region. The northern fork or branch extends to the Wiconisco Coal Company's mines at Bear Gap, and the southern branch stretches towards the Susquehanna in a southwest direction, to within about a mile of that river. The coal formation along several miles of the western portion of this lower fork, is reduced to a narrow ridge, which can scarcely be expected to contain coal to any valuable extent. Both the branches alluded to are bounded or enclosed by corresponding mountain ridges; the strata of which, composed of the inferior red shales and of a numerous series of sandstones and conglomerates, underlie the coal measures and the upper red shales. The coal strata in these separate branches or basins dip, for the most part, to their respective centres.

The horizontal area is protracted as a scale of two inches to a mile; the data for which have been derived from a variety of public and local surveys. In the vertical scale we have been enabled to approach so near to the horizontal as two of the former to one of the latter; an approximation which is more close than is usual in such works. And here it may be permitted to apply some remarks on the construction of diagrams.

It has been customary with most geologists, and I believe almost universally with civil engineers, where the bases of their sections are considerably extended, to adopt a much larger scale for the perpendicular than for the longitudinal dimensions. Consequently, the diagrams, so drawn, amount to absolute distortions, and manifestly convey very inaccurate ideas. The proportions of relative heights and lengths are thus so grossly caricatured, that they bear but distant resemblances to what is intended to be represented. The inclinations of strata are changed from moderate angles almost up to vertical; the altitudes of hills are stretched to the eminence of lofty peaks; rounded secondary mountains assume the form of attenuated spires; gentle undulations become craggy steeps, and the ordinary surface of a country is thus metamorphosed into a region harshly broken into pinnacled spires and alpine crests, and steep and fathomless gulfs—a hideous burlesque upon the actual aspect of the district represented, or rather misrepresented.

In constructing geological diagrams I have, for some time, ceased to make any difference between the horizontal and vertical scales. At any rate I have endeavored, as closely as may be, to adhere to that principle. If the drawings be executed with delicacy they rarely require a deviation from the rule; and I would respectfully recommend an adherence to it, among my geological friends, particularly in relation to the state surveys, where comparisons of sections are continually needed. We shall then, and not till then, possess something like uniformity in the representations of similar things. So long as the distorting principle is tolerated we shall continue to convey and to view every thing under a false medium, and shall describe objects under every shape but their real one. Geological sections, if drawn with suitable care and with the nicety that such works demand, particularly if they be engraved rather than lithographed, may be made perfectly distinct at a very small vertical scale. Detailed sections of particular portions, on a larger scale, can readily accompany and elucidate the general section. The present writer has constructed sections of many hundred miles in this country upon a scale, both vertical and horizontal, or very nearly corresponding, as small as five miles to an inch, and yet has exhibited all important features therein. The system is clearly the right one, and ought to be followed. It is the only one, in fact, which can be made to exhibit the true inclination of the strata, the real bearing, position and magnitude of the formations and their relation to each other, and furnishes the means of measuring the thickness of those masses. In modelling also, although not always attainable, it would be equally desirable to approach as nearly as possible to the same rule.

I have dwelt the more strenuously upon these methods of illustrating geological phenomena, with a view to attract the attention of gentlemen who are about to place before the public, for the benefit and instruction of us all, the result of their respective labors in the field. I would take the liberty of earnestly soliciting their attention to a matter which we all admit is extremely desirable,—namely, uniformity in the process and modes of representation. Wherever it is possible, let similar scales be employed for the geological sections of different states. Wherever it is practicable, and to a very great degree it already is, let similar colors represent similar formations, wherever they occur. If at this stage we cannot yet settle that extremely difficult point, that "consummation devoutly to be wished," a common nomenclature, let us approach it as near as we can, by the use of common symbols, as a temporary substitute for a common language.

I think these are matters on which the present meeting might, with perfect propriety, enter. Let it be borne in mind that the accumulation of facts is one thing—a desideratum of primary importance, certainly. But the science of putting together those materials—the exemplification of those facts—to effect the purposes of geological elucidation, to reach the understanding, to impress on the mind and memory, is no mean part remaining to be performed. Let it be remembered that facts are comparatively useless without arrangement; that they are valueless if they are not presented to the senses in an intelligible and accurate form. The elements wherefrom to erect a geometric figure may be before us, but until we have truly constructed that figure from those elements, our impressions as to its form and proportion are necessarily vague and feeble. The materials wherewith to construct a house or a ship may be all prepared with strict regard to their individual dimensions, but as separate members they convey to us no idea of the actual form of that house or that ship. It is the art of the builder then to put together those materials; and, in like manner, the geologist, or the geological draftsman, or the modeller, has to exercise his art, in putting together and exhibiting in correct forms, the details he has labored so hard to collect. Above all things, let him avoid distortions in drawing. It is incumbent upon those who undertake to enlighten and instruct others by diagrams, to exhibit those diagrams in true, and not in false proportions. The master can no more hope to convey to his pupil a right idea of a cube or a square, for instance, by representing in his diagram its height four or five times its breadth, than can the draftsman in our science, expect to convey correct notions of geological arrangement by a similarly defective process.

Returning to the model before us. The local *elevations* above the level of tide, have been ascertained at a sufficient number of points, particularly in the coal districts, to convey the prevailing characters of the country. A number of these heights, are marked on their proper sites upon the model. All of these were found by spirit level and positive admeasurements. The Pennsylvania canal on the east bank of the Susquehanna river, where it cuts through the second mountain, is three hundred and twenty seven feet above tide level in Chesapeake Bay. The Swatara river, above Pinegrove, passes through the same mountain, thirty one miles to the eastward, at the height of six hundred and nine feet above tide. The prevailing elevation of the ridges which form the north and south edges of the southern coal basin, is sixteen hundred or sixteen hundred and fifty feet above tide water. As a general remark, when casting a glance over the area here represented, we cannot but be struck with the comparative uniformity in their elevations, and the extensive maintenance of those levels along the crests of the ridges, when not broken by transverse fissures. The Blue or Kittatinny Mountain, the southernmost of these nearly parallel ranges, is probably the highest. The coal range is next in elevation, and there is some lofty ground, forming Short Mountain, between Peter's and Berry's mountains.

Geological Features.—Under this head we shall here be very brief; because that subject is not the primary object of this address; and because the region has received or will receive, ample investigation by the state geological survey, with all the combined advantages resulting from the official resources, the science and the experience of its able conductor. The results of that great work it would be premature to anticipate. The positions of the various formations and of the respective members of the groups of strata, within these limits, have already been indicated in the annual reports of Professor Rogers.

In contemplating this region, it appears to us that its most interesting features are attributable to the undulating and broken or upheaved character of the formations, by which process some of them are repeatedly brought to the surface, in long elevated ridges, and again dip at high angles and form basins which enclose or support the superior strata—the carboniferous series being of course the highest. These circumstances confer a remarkably picturesque character upon the scenery, particularly where these parallel ridges are intersected by the Susquehanna, the Juniata, and the Swatara rivers. No part of Pennsylvania is so rich in pictorial beauty as the borders of the noble Susquehanna, or has furnished so many subjects for the skill of the painter.

The spectacle here presented, by this river, cutting across in its singular passage, nearly at right angles, through so many ridges of extremely hard rocks, would of itself furnish a theme for geological speculation. Phenomena like these are well illustrated by the mode of representation we have adopted. The numerous cross fractures marked by the frequent gaps through the mountains and by the remarkable ramifications of the Swatara, in the Pinegrove coal region, could by no other process of exhibition be rendered so intelligible.

The Coal Formation.—It forms no part of the plan of the writer to encumber this communication with minute details. With regard to the southern branch, more especially, it is the less necessary, as they have been recently published, at considerable length, in the form of reports to the proprietors of the soil.* What remains to be added here, under this head, will occupy a brief space.†

Ranging along the southern margin of this coal-field, appear nine principal transverse sections, "gaps" as they are locally termed, which cut through Sharp mountain. Through these ravines, many hundred feet in depth, the drainage of the coal area descends southward; and by the same avenues the highly inclined coal seams are intersected. The height at which these coal seams can now be reached within the gaps, without expensive tunnelling, varies from eight hundred to eleven hundred feet above the level of the sea; and as the summit attains an elevation of sixteen hundred and fifty feet, there are therefore from four hundred to eight hundred and fifty feet, measuring perpendicularly, of coal, capable of being worked, above those points of intersection.

The number, thickness, compactness and density of these coal seams increase as we pass eastward along the counties of Dauphin, Lebanon and Schuylkill. At the same time, and in a corresponding degree, or rather in a reverse ratio, the amount of bituminous and volatile matter, contained within the coal, diminishes; passing from a bituminous or semi-bituminous coal, which yields a bright blazing fire, and at some points is convertible into coke of good quality, to a compact anthracite, on the borders of Schuylkill county. This fact is exemplified in the series of analyses made on behalf of the proprietors and published in 1840, and subsequently by another series, recently embodied in the state geological report.‡ The prevailing breadth of this southern fork, measuring from red shale to red shale, is about a mile; except towards the western termination, where it is only about one thousand feet to one thousand and two hundred feet wide, for three or four miles. In this range the lower conglomerate, interposed in great thickness towards the eastern extremity of the Schuyl-

* Vide report on the coal lands, mines, &c., of the Dauphin and Susquehanna Coal Company, by Richard C. Taylor, president of the board of directors. Report of the geological examinations, &c. of the Stony Creek Estate, in Dauphin and Lebanon counties, by Richard C. Taylor. Philadelphia, 1840.

† At the request of the author, we have, in printing the memoir, omitted many details which were embodied in the original; because without the assistance of maps and diagrams, they could not be rendered sufficiently intelligible to the reader.

‡ See also a series of analyses of coals from this region, published in Vol. xl, p. 373, of this Journal, by Mr. M. C. Lea.

kill coal basin, between the coal beds and the red shale, has thinned out and at some points appears to be altogether absent. The greatest amount of coal which has been proved at this branch, is at Blackspring gap, where eight southern and seven northern seams have been explored, the aggregate thickness of which is ninety feet.

Passing over to the *northern branch* of the main coal-field, a fine series of beds occurs, some of them being of considerable thickness. The anthracite here is of excellent quality. From the Swatara region eastward for several miles, there has been but little exploration of the numerous coal seams known to exist there; there being no convenient mode of communication, by canal or railroad, completed in that quarter. Argillaceous carbonate of iron, in beds and detached masses, prevails in this coal region; to what extent, however, we cannot say, as their investigation has hitherto been but a secondary object.

The foregoing notes are, it is conceived, sufficiently explanatory of the prevailing characters of the district. Did it possess no other peculiarity or attraction than that derived from the remarkable arrangement, or rather derangement, of the formations which it comprises, it might still claim your attention, as an area of high geological interest. Viewing it with reference to its growing importance as a mineral country, favorably circumstanced, we have little cause for apprehension that the labors of the artist have been employed on a barren and profitless field. Already have nine or ten chartered companies for coal and mining operations located themselves here. Already several furnaces and forges have been established in its vicinity. The Pennsylvanian, the Wiconisco and the Union canals, traverse within its limits. Two or three railroads are already in full communication with its collieries, and charters for five or six others have been procured from the legislature. As relates to the region we have been considering, we are but on the threshold of improvement. The industry of man has but recently been put in requisition within its borders. But experience has already informed him that the once despised, chaotic, impassable wilderness, teems with treasures more precious to him, perhaps, than gold. The labors of the geologist, be they local or general; be they for private or for public objects; for individuals, for associations, or for the community at large,—cannot fail to develop new and beneficial results wheresoever directed, in such a field. To have had some share in the attaining and the distribution of this knowledge, and to have contributed any aid to the great cause of economic geology, is a gratification which is worth no small exertion to acquire. It has proved, let me add, in all sincerity, the strongest inducement to perseverance in the work now before the association.

At the request of some members of the association, I have annexed to the foregoing memoir a few illustrative sections constructed on a variety of small scales, with the intention of exhibiting the practicability of using even minute vertical scales, in geological demonstrations. They are as follows:

Fig. A, is a section whose horizontal scale is three miles to an inch, and the vertical scale 5280 feet to the inch, being in fact in the proportion of 3 to 1. This, although less distorted than is occasionally the case, it being easy to point out examples where the proportions are 6, 8, and 10 to 1, is drawn to show the contrast to the section B beneath it, where the proportions are equal, the horizontal line and the area illustrated being similar.

Fig. B. Section at three miles to an inch, both vertical and horizontal, of the same ground as Section A, and in fact a transverse section of the model, which has been described in the foregoing paper.

Fig. C. Section protracted at four miles to an inch, both vertical and horizontal It shews the position of two of the detached Pennsylvania bituminous coal basins.

Fig. D. Section at five miles to an inch. Here there is a trifling increase, amounting to one half only, in the vertical scale, viz. $1\frac{1}{2}$ to 1. It also exhibits two detached coal basins in Pennsylvania.

Fig. E. Section at two miles to an inch, on equal scales. This projection is sufficiently large to admit of characteristic details. It is a profile of the Alleghany mountain, descending eastward; also in this state.

As I have not conveniently at hand, examples of sections having the altitudes above tide level, drawn by other authorities, it was necessary to resort to the materials which happen to be in my possession, and for the most part prepared from personal observation. I hope they are sufficiently accurate for the purpose designed.

To render these experimental drawings more useful for comparison I have inserted Professor H. D. Rogers's numbers of the respective formations. With regard to the colors adopted, they are not proposed as standards, but are simply those which I have been accustomed to employ; differing very little, I perceive, from those used by the gentleman last mentioned.

Before closing this subject, permit me to allude to the new geological map of England and Wales, by Mr. Greenough. As a finished specimen of art, it is probably the most beautiful production of the age, and may with great advantage, be consulted, for the extreme clearness of its details.

Art. X.—*A Notice of "Indian Cyprinidæ," being the second part of the nineteenth volume of Asiatic Researches, by Mr. John M'Clelland.* Read Feb. 17, 1841, to the Boston Society of Natural History, by D. Humphreys Storer.

We are almost entirely ignorant of the fresh-water fishes of many of those countries, whose marine species are tolerably well known to ichthyologists. The reason is obvious—the smallest sea-port has its market constantly supplied with those species which are used for food—the great proportion of such species, are taken along the shores of the different countries, and but very few fluviatile fishes are considered of sufficient value to be procured—so that, we are compelled to learn from the zealous naturalist, who is actuated by other motives than the expectation of pecuniary reward, the characters and habits of such species as inhabit the streams and rivers and lakes. He therefore who attempts to elucidate a subject so desirable to be known, recommends himself by the mere effort, to our regard—should he succeed in his attempt, we ought not to withhold our gratification.

Agassiz, whose name is a guarantee of the value of his labors, is at this moment preparing a magnificent work upon the "Poissons d'Eau Douce de l'Europe Centrale." In our own country, Dr. Kirtland, like a true naturalist, with an enthusiasm proportionate to the obstacles he encounters, is endeavoring to present you with the "Fishes of the Western waters"—and I feel confident, when his entire paper shall be published, although future research may, and undoubtedly will point out errors, that it must be invaluable to the American ichthyologist.

While these observers are prosecuting their enquiries, we are surprised to receive an elaborate paper upon the "Indian Cyprinidæ," read to the Asiatic Society in Sept., 1838, and published in their "Researches" the following year. This paper, to which I would now call your attention, was prepared by Mr. George M'Clelland, assistant surgeon in the Bengal medical service. Our author was induced to undertake the elucidation of this subject, by perceiving that Cuvier had adopted only such of the Indian Cyprinidæ as were figured in Dr. Buchanan's work on Gangetic Fishes—leaving the remaining three fourths of the species described in that work, as not well determined—and feeling satisfied that these descriptions of Buchanan were so general that they could not by any one be distinguished, he resolved to make the attempt to identify them, by collecting all these species and minutely studying their characters. "After perseverance for the better part of three years," to use the words of our author, "occasionally giving it up in despair, I succeeded in identifying most of the species unfigured by Buchanan, as well as in having made two series of finished drawings of them, one set for England and one for India." After his paper was ready for publication, our author learned that some of Buchanan's drawings of his Gangetic Fishes, were in the government house at the botanic garden in Calcutta—and upon investigation, found a collection "amounting to one hundred and fifty beautifully executed, and including nearly all the unpublished species on which my painters had been so long employed, with the specific names in Buchanan's hand-writing marked under the figures, so as to leave no doubt or difficulty in referring them to corresponding descriptions in the Gangetic Fishes." Fortunate indeed was it for science, although gross injustice to Buchanan, that these drawings should have been thus long concealed; had all the figures appeared in his "Gangetic Fishes," they would have supplied the deficiency in his descriptions, and the rich volume before us, would have never been undertaken. Now, after having for years examined the swamps and stagnant pools, and the mountain streams of India—after having enlisted his numerous friends in his service, and possessed through their efforts and his own, not merely all the species described by Buchanan, but many previously unknown—Dr. M'Clelland is not satisfied merely to cry out ευρηκα, but embodies here a great amount of information obtained during his researches, and throws new light upon the ichthyology of the east.

The Cyprinidæ, are arranged by Cuvier in the "Regne Animal," as the first family of the Malacopterygii abdominales—and are characterized thus—they are "recognized by the slightly cleft mouth; the weak jaws, generally edentated, and whose border is formed by the intermaxillaries; by the deeply dentated pharyngeals which compose the trifling armature of the jaws, and by the small number of the branchial rays. Their body is scaly, and they have no adipose dorsal, such as we shall find in the Siluri and in the Salmons. Their stomach has no cul-de-sac, neither are there any cæcal appendages to their pylorus. Of all the fishes, they are the least carnivorous." This family is divided by Cuvier into seventeen genera, characterized for the most part by the form of the mouth, and the position of the dorsal fin. After a minute examination of the digestive apparatus of these fishes, Dr. M'Clelland has pointed out a natural arrangement, which goes far to simplify their study. He ascertained that upon the greater or less development of the intestinal canal, he could determine the food taken by the different genera—whether it was animal or vegetable; and that the position of the mouth corresponded with this arrangement of the canal. That such of the family as lived entirely upon vegetable food, possessed the greatest development of the intestines—and their mouths were horizontal or directed downwards; and that those which lived upon insects, had the least development of these organs—and their jaws were directed upwards.

He has accordingly formed three subfamilies, into which he divides the Cyprinidæ. The first, he calls *Pæonominæ*, or herbivorus Cyprins—from *poionomos*, that feeds on herbs. This subfamily is thus characterized—"mouth slightly cleft, either horizontal or directed more or less downward. The stomach is a lengthened tube continuous with a long intestinal canal; colorus plain; three rays in the branchial membrane. *Obs.* Their food consists chiefly of confervoid plants and other productions of the vegetable kingdom." The *Pæonominæ* contain five long established genera; viz. *Cirrhinus*, *Barbus*, *Cyprinus proprius*, *Gobio*, and *Gonorhynchus*. From the Barbels, our author has formed a subgenus, which he calls *Oreinus*, from *Oreinos*, pertaining to mountains.

The second subfamily is called *Sarcoborinæ*, from Σαρκοβορος, carnivorous. This subfamily is composed of five genera. Two of these genera were previously established, viz. *Leuciscus*, (Klein,) and *Abramis*, (Cuv.)—three other genera are formed by our author. The first of these, he calls *Systomus*, from *Systomos*, that has a narrow mouth. *Characters.* "Intermaxillaries protractile, dorsal and anal short, the former opposite to the ventrals and preceded by a spinous ray; body elevated, and marked by two or more distinct dark spots, or diffuse spots either on the fins or opercula, prominence on the apex of the lower jaw obscure."

The genus *Perilampus*, from Πεϱιλαμπω, to irradiate, or shine brilliantly, is thus distinguished. "Head small, obliquely raised above the axis of the body; dorsal placed opposite to a larger anal; apices of the jaws raised to a line with the dorsum, which is straight; the ventral margin is much arched; sides usually streaked with blue; fins without spinous rays. *Obs.* In this genus the intestine is small, and very little longer than the body. The species all subsist exclusively on insects, which they seize by leaping above the surface. They vary from two to four inches in length."

The third genus of our author, is *Opsarius*, from οψαϱιον, *pisciculus*, a small fish; its characters are thus defined. "Mouth widely cleft; body slender, and usually marked with transverse green streaks or spots; dorsal small without spines, and placed behind the middle, and long, lower margin of the body more arched than the upper. *Obs.* Intestine very short, and extends almost straight from the stomach to the vent."

In the genera of this subfamily, the mouth is situated directly opposite to the position it had in the former. They are all insectivorous—the *Opsarions*, which also devour smaller species of fishes, particularly *gudgeons*, are so voracious, that "it is no uncommon thing to find an Opsarion so overgorged that the tail of its prey remains protruding from the mouth, to be swallowed after that portion which is capable of being received into the capacious stomach is sufficiently digested to admit of the introduction of the remainder." Another striking distinction between the individuals of these two subfamilies, is pointed out to us in their colors. "The whole of the subfamily *Pæonominæ* are remarkable for their uniformly plain colors, consisting of olive green, bluish gray, or brown, extended along the back, and softened off on the sides so as to leave the lower surface of the body an impure white, partaking more or less of the colors of the back." "Of the species, not one possesses a brilliant spot of any pure color." But as we leave the herbivorous group and enter the carnivorous, we find numerous bright, dark spots, and the opercula and fins to be stained with yellow and red, in deep and natural tints; and the more carnivorous the genera the more remarkable is the brilliancy of their colors.

The third subfamily, is called *Apalopterinæ*, from απαλος, soft, and πτεϱον, a fin or wing. It "consists of those genera, the species of which have either elongated cylindric bodies or flat heads, as the *Loaches* and *Pœciliæ.* They are without spinous rays in any of the fins, the intestine is short, and enveloped in a copious mucous secretion; three to six rays in the branchial membrane." There are four genera.

The first genus of this subfamily is called *Platycara*, from *platus*, broad, and *kara*, head. Its characters are "head flat, with the eyes placed on the upper surface, fins thick and opaque, pectorals large, anal small, caudal bifid, mouth without teeth and placed on the lower surface of the head, three rays in the branchial membrane. *Obs.* The stomach and intestine form a continuous fleshy tube, not much exceeding the length of the body; they are found in elevated mountain streams."

The second genus is *Psilorhynchus*, from *psilo*, thin or attenuated, and *rynchus*, a snout or beak. It is distinguished by "muzzle elongated and flattened, eyes placed on the edges of the head, mouth small and suctorial without cirri, opercula small, caudal bifid, dorsal opposite to the ventrals."

The third genus *Pœcilia*, includes the genera *Pœcilia*, *Lebias*, *Fundulus*, *Molinesia*, and *Cyprinodon*, contained in the "Regne Animal." A single subgenus is formed from this genus, which is called *Aplocheilus*, from *Απλος*, simple or single, and *χειλος*, the lip, and having the following characters—"intermaxillaries fixed, apices of the jaws broad, flat, and directed upwards; five rays in the branchial membrane; fins transparent. *Obs.* A short dorsal is placed opposite to the last ray of a long anal, the ventrals are very small; the intestine and stomach form together a small tube scarcely longer than the body."

The fourth genus *Cobitis*, (Linn.) is divided into two subgenera; the first, our author calls *Cobitis propria*—here, we find the "caudal entire, large, and ornamented as well as the dorsal, with bars or spots; prevailing color of the body, various shades of brown, disposed in more or less dense nebulæ." To the second subgenus, our author has given the name of *Schistura*, from *Σχιστος*, split or separated, and *ουρα*, cauda. Its characters are "caudal bilobate, dorsal and ventrals opposite, and short; with or without suborbitar spines; sides ornamented with fasciated bars, mostly green. *Obs.* The intestine is somewhat longer than that of the true Loaches, (*Cobitis propria*,) being usually reflected once upon the stomach."

We have thus exhibited a mere outline of the important improvements proposed by Dr. M'Clelland in the natural arrangement of the *Cyprinidæ;* but besides his classification, our author has presented us a general synopsis of the species, and illustrated them with lithographic figures, from drawings mostly his own, all of which are sufficiently well executed to convey accurate ideas, and many of them are highly creditable. Besides furnishing nineteen colored plates, containing nearly one hundred figures, the volume before us is also enriched by a single plate, exhibiting the peculiar forms of the jaws in several genera of the subfamily *Pæonominæ*, and the differences of form and proportion of the alimentary canal in each of these great subfamilies.

Almost one half of the entire volume is devoted to an "account of the species," and it constitutes by far the most interesting portion. We here find, not merely a scientific description of each fish, with its Latin and native name and its geographical locality, but its habits are elucidated with great clearness and evident acquaintance; its value as an article of food, from its delicacy or abundance, and the possibility and importance in many instances of its being transported from one locality to another for economical purposes, are pointed out with the zeal of a philanthropist; while the perseverance and fidelity with which our author has studied the minute anatomy of his subjects, must claim from all readers their admiration.

Published as this treatise is, by a society whose "Transactions" have become so voluminous, and are with difficulty procured, it cannot be extensively circulated or known among American naturalists; I would notice therefore a few points of general interest, selected from the portion of this paper just referred to.

In some parts of India, many of the species of fishes are found in immense quantities. Our author, after describing *Cyprinus-chola*, remarks: "Casting a net into a pond in Middle Assam, not presenting any remarkable appearance of containing fish, about one hundred and twenty were brought up at a single draught"—(nine species captured are here mentioned). "The extent of the pond may have been four hundred yards, and that of the net three yards; and supposing half the fish to have escaped from under the net, the number in the pond would have been thirty two thousand. When we consider the vast extent of surface occupied by waters equally productive, both in Bengal and Assam, we may form a conception of the inexhaustible supply of fish that might be procured for consumption in other parts of the country where they are less plentiful." p. 384. Several species are useful not merely as an article of food; speaking of *Cyprinus cursis*, Dr. M'Clelland observes, "It is a beautiful fish, common in Bengal and Assam as high as Sudyah, but being full of bones, is little valued as an article of food. If it be less useful in this respect than other Cirrhins, it is more serviceable than we are aware of, in common with the numerous Gudgeons, in clearing the indolent waters of the plains from a redundancy of vegetation with which they would otherwise be choked up." p. 330. Many of the species are of great value as excellent and nutritious food, and might be rendered infinitely more so, could they be salted when taken, or be transported and allowed to propagate in other portions of the country. Thus the *Barbus megalepis*, which is an admirable fish, is "peculiar to remote, unpopulated districts, where no attempts are made to cure; they are consequently lost to the wants of other places, where an unlimited demand for dried fish must prevail at all seasons, but particularly during the rains and hot weather, when fresh fish become scarce. There can be no doubt that if some relaxation of salt duties could be made in favor of those who would embark in such a business, a profitable and useful trade might be established to a far greater extent than we can at present form any notion of. The season for fishing is short, and without the means of saving more than can be consumed when fresh, the fishermen have nothing to stimulate them to any exertion beyond that of earning during their brief season, a sufficient sum to support them during the rest of the year. Had the fisherman the means of preserving the results of his labor, his chief market would commence when the fishing season ends, and his industry would then become a permanent benefit to himself and to the country at large. Sea fisheries would be of still higher importance, although neither should be neglected. The cold season, from November to February, is the time at which fishes are chiefly taken; the waters being then low, the fish are confined to narrow channels, and are often completely cut off from the larger streams and left in pools, in which they are easily secured. When passing Solano Mookh with the Assam deputation in January, I saw boats laden with most of the five kinds of Barbels just described, from one to two and a half feet in length; but as Suddyah, the nearest market at which they could probably be disposed of, was thirty miles distant, and a strong current to be opposed in reaching that place, and no means of curing the fish, the owners entertained little hopes of realizing any thing whatever by them." p. 339.

To the important subject of transporting fishes from one pond or river to another, our author has given much attention, and his suggestions are worthy the notice of the government of India. We are told, that "throughout the Mysore country, as well as in many of the western provinces, large tanks or reservoirs occur, many of them from three to thirty miles in circumference, and being indispensable for irrigation, may be supposed to be nearly universal in all populous districts not watered by rivers. These reservoirs are considered by the honorable Col. Morison, C. B. as among the greatest national monuments to be found in India. They are capable, according to Buchanan, of supplying water for from eighteen months to two years, and thus of maintaining the surrounding crops should no rain fall within that period.

"They are drained by an ingenious system of sluices and aqueducts of the most simple but complete construction, which afford a perfect control over the distribution of the water. During the dry season they are all pretty much exhausted, and may, if necessary for repairs, be left perfectly dry. This would afford an excellent opportunity for destroying crocodiles and all the various destructive fishes, sparing only the more profitable kinds, which are limited to two or three species only; and by repeating this operation for several seasons, or as often as may be necessary, all but those we wish to propagate would soon be exterminated.

"By a wise law of nature, the carnivorous animals of every class are less prolific than the harmless, and may therefore be the more easily subdued. Nearly all the destructive fishes are viviparous, bringing forth comparatively few young; whereas, the more profitable kinds, or those which should be the objects of our care, are all oviparous, and bring forth their young from spawn.

"A single female carp weighing only nine pounds, has been found by Dr. Locke to contain no less than six hundred thousand ova; and by Schneider, one, ten pounds weight, was found to contain seven hundred thousand ova or eggs.

"The fecundity of the *Ruee*, *Catla*, and *Mrigala*, has not yet been ascertained, but from their close affinity to the carp we may suppose them to correspond in this respect with that species; the question, however, is one that may be easily ascertained by weighing a grain of the roe and ascertaining the number of globules it contains, while these will be to the whole roe what one grain is to its entire weight. The result will show that these species are capable of yielding, by their extraordinary fertility, a source of food as inexhaustible as the sands of the ocean, could we only bring their propagation and the safety of the young sufficiently within our control.

"In the reservoirs above described, we have every facility for effecting this object on a scale of great magnitude, without in any way interfering with the other uses of the water." p. 458, et seq. "The only alteration in the present form of the reservoirs to adapt them to the purposes in view, would be to enclose the lowest portions of the bottom of each with stakes long enough to reach above the highest surface of the water, and close enough together to prevent the entrance of crocodiles, otters, and the like, should any such exist in the neighborhood. The spawning season of the *Ruee* and other *Cirrhins*, appears to be in the dry weather; the contrivance here suggested would therefore protect them at that time, and if there should be any danger of the whole of the water drying up, wells of sufficient size and depth might be formed within the enclosure, to which the fishes would retire during droughts, while the shallow waters around the wells would afford space enough for the deposit of spawn.

"Much of our success would depend on keeping those enclosures as free as possible from all but the species we desire to propagate. At the commencement of the dry season, before the fish begin to enter the enclosure, the interval between the stakes might be closed with straw, and as the water becomes sufficiently low without, most of the rapacious kinds may be removed or destroyed; none should be allowed to remain but that species alone which may be the object of our care. This done, the only further attention necessary, would be to save the fish in the enclosure from birds during the remainder of the dry season.

"Should our success be complete, from every moderately sized female *Ruee* we should have, on the commencement of the rains, from five to ten hundred thousand fry, which, as the waters rise, would be quite able to take care of themselves till the next season, when it would be necessary again to destroy the rapacious kinds as before." "On the fishes of Bengal, Assam, and other provinces subject to the inundations of the larger rivers, we can exercise no control, nor is it desirable that we should, even were it in our power, the supply of fish being plentiful and constant enough; but in the higher parts of the plains, near the foot of the mountains, where the larger *Cirrhins* and *Barbels* retire during the dry season for the purpose of spawning, fisheries might be carried on with advantage to a considerable extent." p. 461.

As some species of the Indian fishes are found only in clear and rapid streams, they would not probably thrive well, if at all, in tanks where the water would be still. Should it be considered desirable to propagate to any extent such species, our author suggests, that "the most suitable *vivarium* for such species might be formed by stopping up a clear mountain stream to a certain depth, and filling the irregularities of the bottom with sand, gravel and stones; there should be a current in the water, and to prevent the escape of the fish, a grating should be fixed below; at the opposite end a stronger grating, if necessary, to prevent the introduction of rubbish during floods, as well as the escape of the fish, should the fall not be sufficient for the latter purpose." p. 347. Occasional remarks are found relating to the altitude at which fishes are known to live in India. Speaking of the *Oreinus guttatus*, Dr. M'Clelland observes, it is found "in rivers in different parts of Boutan, between the elevation of two and five thousand feet. It may occur higher, but Mr. Griffith remarks, that in valleys above five thousand feet, though fine, clear streams are common, yet fishes of any kind do not occur in them, and the natives assured the mission to which he was attached, that no fish existed at such elevations." p. 345. The *Gonorhynchus petrophilus* "inhabits streams in Kemaon at an elevation of six thousand feet above the sea, and has been observed by Lieut. Hutton at similar elevations in the mountains north of Simla, as well as by Dr. Campbell in Nipal." p. 371.

And again, "whether any other kind of fishes may yet be found in still higher altitudes than those at which the Gonorhynchs and Mountain Barbels disappear, is a problem in the distribution of this class of animals, that travellers in the Himalaya and other lofty regions must decide. In the limpid streams which Mr. Griffith passed with Capt. Pemberton, at elevations of from six to eight thousand feet in Boutan, no inhabitants were found;

and both here and at Simla, as well as in Kemaon, the Gonorhynchs and Mountain Barbels have not been found at greater altitudes than six thousand feet above the level of the sea, where we may presume they disappear; but from that altitude downwards to the plain, they constitute the prevailing forms that have hitherto been met with in the waters." p. 369.

It is well known that several of the marine fishes, under peculiar circumstances, produce derangement in the systems of those who eat them; it appears that similar affections are the result of feeding upon some of the Indian Cyprinidæ. Thus we are told "that many of the natives abstain from the use of the *Cyprinus cursis*, imagining that if eaten on the same day with milk it will occasion a disease called elephantiasis." p. 329.

The *Oreinus progastus* "is said by the natives of Assam to occasion swimming of the head and temporary loss of reason for several days, without any particular derangement of the stomach. It is the most herbivorous of the Barbels, and like some of the Gudgeons, tends rapidly to decay after death, and in the abdominal cavity a copious oily secretion is found, which is probably the cause of its bad effects." p. 344.

Our author observes, that in some species "the whole of the abdominal viscera float in a dark, oily kind of fluid;" and he remarks, "that either this fluid, or the great proportion of vegetable matter contained in the intestines of the gudgeon and Gonorhynchs, tends rapidly to putrefaction; to which cause, as well as to the neglect of removing the viscera from those species immediately after they are caught, I ascribe the bad effects which have by some been observed to result on certain occasions from their use." "Mr. Bruce, of Assam, also mentioned to me, that he knew of instances of indisposition supposed to be occasioned by a variety of Bangon. All Bangons and Gonorhynchs should therefore have the viscera removed soon after they are taken, and the dark, oily fluid washed away; when, if it be necessary, they will keep fresh as long as any other kind of fish; but if this be neglected, the stomach rapidly putrefies, in which state, if it be necessary to use these fish, the thin parts adjoining the ventral fins should be removed." p. 371.

I might proceed to point out much, which could not fail to interest you, but it was not my intention to present an elaborate paper. I wished merely by glancing generally at the work before me, to show you how creditably the author has performed his task; he deserves, and will I trust, receive in the pages of the scientific journals of his native country, a faithful critique.

Before closing this report, I would revert to a single circumstance which is highly honorable to Dr. M'Clelland. It appears that Dr. Buchanan, during a long residence in India, had made a large collection of papers and drawings illustrating the natural history of that country, which were taken from him, as he was about to leave India, by the Marquis of Hastings, and deposited in the government house at the botanic garden in Calcutta—so that he was obliged when he reached Edinburgh, to publish his Gangetic Fishes without most of his plates. Dr. M'Clelland, when he finally knew of the existence of these drawings, was surprised to ascertain that Hardwicke in his "Illustrations" had freely copied many of them, without giving the slightest credit to Buchanan, and had even annexed to them new names, although the names of their discoverer were attached to the drawing, and in his own handwriting. But the names were not merely changed; "during the twenty years Buchanan's drawings lay at the botanic garden, before they were transferred to Hardwicke's Illustrations, many of the colors appear to have undergone a change, such as light blue and green becoming dark brown; not aware of this, the copyist has not only imitated the altered colors, but added a little to their intensity; the consequence of which is, that the figures thence obtained in the expensive work referred to, are made to appear in black, when they should only be a pale grey or green." p. 355, *note.* Mortified to find, that a departed naturalist had thus been robbed of a portion of his well earned fame, and proud to be the means of rescuing from oblivion the labors of Buchanan, Dr. M'Clelland has not only in every instance, when practicable, referred to his plates in the most generous manner, but has even presented us with some of the original plates, although he had made similar drawings previous to their discovery. His whole conduct in this transaction is noble and disinterested; and while the naturalist, after reading this volume, acknowledges his obligations for the information received, he will also feel for the author a deep and abiding esteem.

Art. XI.—*Des Moulins' General Considerations on Restricting the number of Species of the Genera Unio and Anodonta;* translated from the French, by Philip H. Nicklin, A. M.

Prefatory Remarks by the Translator.

The paper of Mr. Charles des Moulins, now offered in an English translation, possesses merits of a very high order. It criticises without acrimony, and praises without flattery; it is scientific without pedantry, and philosophical without obscurity. It is imbued throughout with the enlightened spirit of catholic Christianity, which does honor to the writer, and is a favorable symptom of the moral condition of the scientific world.

Mr. Des Moulins was the predecessor of Dr. Graleloup in the presidency of the Linnæan Society of Bordeaux, and is doubtless a naturalist of a high order. On the presentation of the 2nd volume of Mr. Isaac Lea's Conchological Observations to the Linnæan Society, Mr. Des Moulins requested of the Society to be charged with the '*Rapport*,' on account of his familiar knowledge of the English language, acquired during a long residence in Great Britain. In a letter to Mr. Lea he observes, "as I felt obliged to speak my thoughts plainly in the *rapport*, I ventured to give an explanation of the principles and method on which I would rely, for an enucleation of the *true* species from amidst the mere forms and differences of sex and age, and in short from all sorts of varieties. This theoretical part of my paper, the Linnæan Society have judged fit to be printed in their transactions." "As for the second part of my work, which was plainly the Report on your volume, it was not admissible in such a kind of review as our transactions, where original writings only can be admitted, and it is not worth my sending a manuscript copy. Let me only tell you that I gave a numerical account of the species of each genus you described, that I criticised your changing the Lamarckian generic name *Iridina** for *Platiris*, and the specific one of *Iridina rostrata*, Rang, for *cœlestis*, while it remains evi-

---

* If 'Iridina' had been retained for the genus, it would have been necessary to strike from Lamarck's generic description the following phrase, "*per longitudinem tuberculosus, subcrenatus; tuberculis inæqualibus, crebris;*" because the *Iridina rubens* and its fellows, now forming Mr. Lea's subgenus *Spatha*, do not fulfil that cardinal condition: it was therefore manifestly proper to adopt a new name for the

dent by your own plate, that the shells are exactly the same. I did specially point to your excellent anatomical researches on the animals of Naïades, and to that valuable Synoptical Table, on which you must have toiled so long and so hard, to enubilate the tremendous synonomy of those poor shells. Lastly, after some other analytical details, I ended my report as follows:

"Tel est Messieurs, le bel ouvrage dont votre bibliothèque vient d'être enrichie par la générosité d'un des plus laborieux de vos correspondans. Favorisé par toutes les circonstances qui facilitent et encouragent les travaux du naturaliste, Mr. Lea vous donne l'espoir de voir les siens se multipliér chaque année, ainsi que ses titres à la reconnaissance des savans. Témoignez-lui la vôtre, Messieurs, par l'organe de M. le Secretaire Général, car elle est bien méritée, et je le demande comme justice, a la Société Linnéenne, en terminant ce rapport."

The paper of Mr. Des Moulins, though critical, is highly honorable to the labors of our countrymen, shewing that they are casting their mites into the general treasury of the world's knowledge. The tribe of the Naïades is one of the most interesting of Nature's families, dwelling in cells of pearl, adorned within, with all the hues of flowers and brilliant rainbow dyes, inhabiting every crystal lake and running stream of this terraqueous globe. Man doth press with wandering foot no river bank, nor margin of translucent lake, but there a lovely Naïs doth wait and woo his admiration;

—"candida Naïs
Pallentes violas et summa papavera" *ferens.—Virg. Ecl.* 2, *l.* 46.

This is more abundantly the case in our own youthful country, which, as Mr. Des Moulins justly observes, is "évidemment la patrie privilégiée des Unios et des Anodontes;" it is therefore not surprising that our young naturalists should have directed their researches with great enthusiasm towards that interesting branch of natural history, in the beautiful subjects of which our country abounds so much more than all other parts of the world. Mr. Des Moulins arrives at a just conclusion, when he says, "Il vaut probablement mieux lâcher encore les rênes, pendant quel-

---

genus, and to use Lamarck's name *Iridina* for the *subgenus*, in which are arranged all the species which conform to his generic description. It would not have been expedient to make two genera, because the animals of *Iridina* and *Spatha* are supposed to be nearly identical.—*Translator's Note.*

ques années, aux collecteurs, aux observateurs, aux faiseurs d'espèces même, pour ne pas les decourager; et pour laisser ainsi s'augmenter notablement la masse des matériaux;" so that, when the heap of knowledge shall be thus "*notablement augmentée*," some future classifier, endowed perhaps with a Linnæan power of generalizing, may extinguish the superfluous species, and arrange the remainder in exact scientific order.

Mr. Des Moulins wears his Christian mantle with so much grace and courage, that he has even quoted Saint Augustine for a profound generalization of the works of God. And perhaps in this matter the saint has excelled the *savant;* for the latter offers the following exposition of the three kingdoms:

Mineralia crescunt,
Vegetabilia crescunt et vivunt,
Animalia crescunt et vivunt et sentiunt;

but the former says:

Quædam sunt, i. e. Mineralia;
Quædam sunt et vivunt, i. e. Vegetabilia;
Quædam sunt, vivunt et sentiunt, i. e. Animalia.

Mr. Des Moulins seems to be in error in making *growth* a predicate of minerals. He applauds Linnæus for having borrowed "*presque textuellement à Saint Augustin les trois premiers termes de cette magnifique progression d'institution divine*," and yet himself departs from the Augustinian text by substituting *crescere* for *esse;* and as he departs, so far he errs.

The Linnæan scheme is as follows:

Lapides corpora *congesta*, nec viva, nec sentientia.

Vegetabilia corpora *organisata et viva*, non sentientia.

Animalia corpora *organisata et viva et sentientia*, sponteque se moventia.

Minerals are bodies *aggregate*, not living nor perceiving.

Vegetables are bodies *organic and living*, not perceiving.

Animals are bodies, *organic, living and perceiving*, and moving of their own accord.

The schemes of the two modern philosophers, when compared with that of the ancient divine, seem to be deficient in severity and precision.

Young naturalists will find Mr. Des Moulins' paper an interesting and improving study; and the application of its principles need not be restrained to the branch of natural history in discussion in its pages. The important question, "*what is species?*" which he asks but does not answer, seems to present itself at the beginning of every arrangement; and that naturalist would be canonized who should answer it in a manner acceptable to all.

The definition given by Milne Edwards in his Elémens de Zoologie, seems to be sufficient for the purpose of arrangement: "On donne le nom d'espèce à la réunion des individus qui se reproduisent entre eux avec les mêmes propriétés essentielles."

P. H. N.

Philadelphia, April 10, 1841.

General Considerations on the Reform which ought to take place in restricting the number of Species of the genera Unio and Anodonta. Presented to the Linnæan Society of Bordeaux, at its sitting of the 5th of July, 1839, on the occasion of the publication of Mr. Isaac Lea's second volume of Conchological Observations. By Ch. Des Moulins, Member of the Linnæan Society of Bordeaux, &c.

Gentlemen—An able designer had arranged, in a hundred adjoining compartments, a hundred figures, of which each one differed so little in appearance from its neighbor, as to be indistinguishable at the first glance. The first was the figure of the Apollo Belvidere, the last was that of a frog. This curious comparison shadows forth a relation little flattering to the physical man, but of much importance in a *philosophical** point of view, in the sense now attached to this word. It is the graphic representation of the celebrated maxim of Linnæus, *Natura non facit saltum*—'Nature makes no leaps.'

But really, gentlemen, what is nature? Nature, according to the notion of Linnæus, in the opinion of Christians, in truth is nothing else than the *whole of those laws*, or *material order*, which God imposed on physical things when he created them. Now gentlemen, this saying of Linnæus is admirable, it is profound, it is true in the *material order*, the object at once of the study and teaching of the Professor of Upsal; and the more worthy it is of our respect, the greater is the necessity of adding to it a commentary, restraining its scope within the bounds contemplated by its immortal author.

---

* The use of this word often appears to me grammatically absurd; sometimes, instead of it, we ought to say *physiological*, but oftener *metaphysical*; but unfortunately the latter frightens children, even children of all ages.

This saying is true; observe its justness from kingdom to kingdom; behold the close connexion of the mineral with the vegetable kingdom, in those innumerable crystals which form the microscopic quasi skeleton of the stem of the Cactæa, and of so many other plants; behold it also, though in a different form, in those *Chara*, so delicate as to be always protected by a stony covering; observe the relation between the mineral and the animal kingdoms, in those curious madrepores, of which the frame-work, entirely mineral, is secreted by a feeble living jelly; behold, finally, that of the animal with the vegetable kingdom, in those corals which unite the essential composition of the former with the flexibility and the appearance of the laws of growth of the latter: *Natura non facit saltum.*

Let us proceed, and this maxim will shed more light. It is not a simple line of junction that marks the boundary between the inorganic and the two organic kingdoms; they go even to a superposition, or if I may invent a term to express my idea, to a *superaddition*, so constant as to form one of the essential laws of the physical world. Nothing is purely animal, nothing is purely vegetable, since every organic substance is reduced, by analysis, to inorganic elements. The mineral or inorganic kingdom is, then, the primary matter of the physical world—matter which its Creator has set in action and modified by *superadding* other principles entirely foreign to it: *Natura non facit saltum.*

But observe, gentlemen, this wonderful concatenation: the inorganic kingdom, which enters into the composition of all physical things, is subject to *all universal* laws, and is limited by *one only* of those which govern more *complicated creatures*: MINERALIA CRESCUNT; behold the basis of the physical world. God works, and *superadds* the principle of vegetable life, hidden from our feeble intellect: VEGETABILIA CRESCUNT ET VIVUNT. God works again, but in another direction, and *superadds* the principle still more mysterious and sublime, of animal life, of sensation, of volition: ANIMALIA CRESCUNT, VIVUNT ET SENTIENT. Here you still perceive, *Natura non facit saltum.*

The naturalist stops here; he stops where stopt the mind of the great classifier of material things, at the bound which necessarily limited the extent of his immortal maxim. Linnæus, after having borrowed, almost in very phrase from Saint Augustine, the first three terms of this divinely instituted magnificent progression, stops before the fourth term,* which did not concern him at that moment, or rather he transfers to another place its expression, and guided by his sublime genius, and still more by the bias of his religious heart, places it in the specific name which he has given to man: HOMO SAPIENS.

And now, gentlemen, behold the curtain is raised; for in this double name is fully expressed the double substance of the privileged being. By one of his two essences, he stands on the sum-

---

* Quædam enim tangis† ut sint, non tamen ut vivant, sentiant et discernant; quædam verò tangis ut sint et vivant, non tamen ut sentiant et discernant; quædam verò tangis ut sint, vivant et sentiant, non tamen ut discernant; quædam verò tangis ut sint, vivant, sentiant et discernant.—AUGUST. *Medit. Cap.* 29, § 3.

Some things thou commandest *to be*, but not to live, perceive, and understand; some thou commandest *to be and live*, but not to perceive and understand; some thou commandest *to be, live, and perceive*, but not to understand; and some thou commandest *to be, live, perceive, and understand.*

And lest it should be supposed that such a passage could flow from his pen unintentionally, and without sufficient reflection and long meditation, I quote here what Saint Augustine says upon the same subject in another of his works.

Non eram, et creasti me; nihil fueram, et de nihilo me aliquid fecisti. Quale autem aliquid? Non stillam aquæ, non ignem, non avem vel piscem, non serpentem, vel aliquid ex brutis animalibus; non lapidem vel lignum, non ex eorum genere quæ tantum habent esse, vel ex eorum quæ tantum esse possunt, et crescere; non ex eorum genere quæ tantum esse et crescere et sentire possunt; sed super omnia hæc, voluisti me esse ex his quæ habent esse, quia sum; et ex his quæ habent esse et crescere, quia sum et cresco; et ex his quæ sunt, crescunt et sentiunt, quia sum, cresco et sentio: et paulò minùs parem me creasti angelis, quia rationem te cognoscendi, cum ipsis à te communem accepi.—AUGUST. *Soliloq. Cap.* 7, § 4.

I was not, and thou createdst me; I had been nothing, and of nothing thou madest me something. But what? not a drop of water, not fire, not a bird, nor a fish, not a serpent, nor any irrational animal; not a stone, nor a stock, nor any of that kind which can only *be*, nor of that which can only *be* and *grow;* nor of that which can only *be*, and *grow*, and *perceive;* but above all these, thou hast willed me to be of those which can *be*, because I *am;* and of those which can *be* and *grow*, for I *am* and *do grow;* and of those which can *be, grow, and perceive*, for I *am, do grow, and do perceive;* and thou hast created me a little lower than the angels, because in common with them I have received reason, by which I am able to know thee.

Saint Augustine is one of the most astonishing phenomena in the history of the human mind. With what power of observation and reasoning must this African bishop have been gifted, who being neither evangelist nor prophet, but merely a doctor of the church, yet aided by the Bible, advanced fourteen hundred years beyond his age in scientific discovery!

† I have not rendered the verb *tangere* by its common English equivalent *to touch*, because it is plain that things must first be created before they can be touched, and because the Bible informs us that God created all things by his word. Saint Augustine uses it to express the action of creative power.—*Translator's note.*

mit of the ladder whose descending steps degrade him to a frog; and by a further descent even to a monad: by the other, he is placed on the lowest rung of the sublime ladder of created intelligences. But, let us speak to our senses: there is a point, in the hundred compartments of which I first spoke, where the *homo sapiens* finishes, a point where the *brute* begins; there is the impracticable *leap*, *the gulf* without a bridge, which separates the immaterial from the physical creation: there is the immense, the impenetrable abyss, which the hand of the Almighty has left between the Apollo and the frog.

It may seem that these reflections are too high, or too abstruse if you will, to serve as the preamble to a critical examination of certain new species of shells. I do not think so, gentlemen; we can only judge piecemeal; and when the subject is intellectual, the pieces are principles. I thought a full explanation necessary, that you might the better appreciate the elements of the discussion which I propose to submit to your consideration. From what I have stated, I now proceed to draw the single rule that will serve as the basis of my labors.

Between the first and the last of our hundred compartments, there is a point where *the man* ends, where *the brute* begins; a point at which, notwithstanding the imperceptible deterioration of external form, there is a change of nature, a change of material classification, a change of order, a change of class, a change of family, a change of genus, a change of species. This consideration is enough for my purpose; without illustrations, its announcement is here sufficient.

But, gentlemen, the most eminent zoologist now living in France, (M. de Blainville,) stated twelve or fifteen years ago, that in considering the embarrassing variety of forms then known in the genera *Unio* and *Anodonta*, he felt the necessity of enquiring, whether, if the two genera* were joined again into one, as their comparative anatomy requires, *that one* should present to the classifying naturalist, an almost endless series of really distinct species, or only *one species* infinitely variable in its forms.

---

* In March, 1829, the translator expressed the opinion, "that the seven genera, now referred to the family of *Naïades*, are founded in artificial distinctions, and not in nature; and that in fact the family contains but one genus."—*Trans. of the Amer. Phil. Soc., Vol.* 3, *p.* 398.

I do not think that M. de Blainville, seeing the rich materials since published by Mr. Isaac Lea, would now feel a similar hesitancy. It may be supposed, that in a series of specimens more or less numerous, selected with care from the different domestic and foreign forms, the passage may be almost insensible from the immensely large cardinal teeth of the *Unio crassissima* to the toothless hinge of the *Anodonta cygnea*. But if a hundred specimens be necessary to pass through all these successive gradations; I dare say another series of different specimens quite as numerous, would be required to pass insensibly from the very round form of the *Unio circulus* to the linear form of the *Unio rectus*, exhibiting successively all the modifications of form contained between these extremes; and I warrant that still new series almost as numerous, would be necessary to form a communication between the most solid and stony species of the United States, and the *Unio lævissimus;* which is so thin that atmospheric drought makes it split; or to connect those whose form is almost as compressed as that of certain *Lutrariæ*, with those whose three diameters are nearly equal. In fine, I do not think that a single series, however numerous, can exhibit at once all these combinations, with all these various conditions of gradation. And since it required ninety eight designs, almost entirely imaginary, to make an uninterrupted chain between the Apollo and the frog, what would the number be, when we should find ourselves engaged with nature herself, so rich in resources, so abounding in ornament, so inexhaustible in the variety of forms that she exhibits to our admiration.

But in the same manner, (and even oftener than the ladder of ninety eight steps which separates the Apollo from the frog,) comparative anatomy, (which also offers us a clear scale of perceptible gradations,) discovers to us the successive and gradual changes of *orders*, of *classes*, of *families*, of *genera* and of *species;* and it is enough to cast the eye over a rich collection of *Naïades*, or over the magnificent figures contained in Mr. Lea's two volumes, to perceive there at the first glance, a certain number of typical forms, distinct, and each surrounded by derivative forms, more or less numerous; which are too closely connected with them to be separated from them, or even to form connecting links between them and other types entirely different.

The truth, in this matter, seems to me then, intermediate to two extreme propositions, between which M. de Blainville seemed to pause some years ago. Yes, I can confidently say, that there really exist several species, nay, many species in the great genus formed by the union of the *Anodontæ* with the *Uniones.* But I may add with equal confidence, that authors have too greatly multiplied these species. Does this criticism apply also to our honorable correspondent, Mr. Isaac Lea? in my opinion it does. The more of peculiar importance that I attach to these types, so well defined, so perspicuous, so distinguished by their forms, of which, the American authors above all, and Mr. Lea more than any of them, have discovered to us the existence, the more I am convinced of the necessity of degrading to the rank of varieties, and often to that of deviations, (as our learned colleague, M. Casimir Picard of Abbeville says,) those variations of form typically the same, those specimens from different localities, which are not identical in the detail of parts, which are neither precisely this, nor exactly that, but which possess a clear and undeniable relationship with such or such a type perspicuously defined. I think that a certain number of Mr. Lea's species would be extinguished by a reform based upon this principle.

But to propose this reform in detail, we should have under our eyes the collection of Mr. Lea itself, probably the richest in the world; or at least a collection very numerous in species and specimens, identified by comparison with all the published figures of authors. To establish such reform irrevocably, still more is necessary; we must have what at this moment may be considered as impossible, *the comparative anatomy of all the pretended species* established by authors. Then only could we be absolutely sure of viewing the question in all its phases. I think I may affirm that we should then find anatomical differences between all true species, and we would rank without scruple among *forms* or *varieties* all those diversities as to length, shortness, &c. which exert no influence upon the internal structure, the number, or the functions of the organs and integuments. Thus, better than in any other way, should we approach the solution of that important question, *what is species?* a question which so much occupies, and still more divides philosophers. Every body demands a definition of species, but a definition *assented to by all.* Definitions, indeed, are not wanting; but common assent sanctioning one exclusively, is entirely wanting, and will perhaps always be so, if I am right in classing among *impossibilities*, the only means of bringing about such assent.

What is to be done, then, in the midst of all these difficulties? How shall we escape the horns of this dilemma; *reform is wanted, but permanent reform is at this time impossible;* nevertheless, on the other hand, *it is absolutely necessary to classify the objects already known, and those which are discovered every day.* There is nothing but provisional arrangement, nothing but system, (and *system* is *artificial* method,) which can rid us of this embarrassment. Let us, then, consult theory, experience, analogy, that we may labor to come at the truth.

Theory says, that there are essential differences between genus and genus, between species and species.

Experience tells us, that dissimilar animals are sometimes found in similar shells, and animals almost identical in shells, apparently very different.

Analogy proposes, under these premises, to draw inductions from the mass of discoveries already made.

Let us take then this immense group, such as MM. de Blainville and Deshayes understood it a few years ago; this group, whose animals they then believed to be *generically identical;* let us begin by lopping off, finally, with M. Deshayes, the *Iridinæ*, with M. D'Orbigny the *Castaliæ*, of which the shell alone did not permit a rational distinction. Then let us reduce, with Mr. Lea, all the other genera of the *Naïades*,* to the simple importance of subgenera or sections, waiting always to withdraw from these, all those species whose anatomical characters when known, shall be found to differ from those of the studied type, in like manner as the *Iridinæ* and *Castaliæ*, and we shall have a genus which will possess only a *supposed unity* in its essential characters, but which on that very account will be conditionally, and therefore theoretically, exact.

Arriving at species, we would proclaim as irrevocably autonomous, those which offered no specific anatomical differences, such as the form of the ovaries studied by Mr. Lea in the *Unio irroratus*, *ochraceus*, *cariosus*, *retusus*, in the *Anodonta undulata* and

---

* I say nothing here of the genus *Mycetopoda* of M. d'Orbigny, which I have not at present an opportunity of studying. Mr. Lea does not admit it, any more than that of *Castalia*.

fluviatilis; such as the form of the foot or of the appendices of the mantle in the *Unio radiatus*, &c.

Secondly, we will take for our guide that very important publication which Mr. Lea is at present making of observations made by Dr. Kirtland and himself on the dioïcity of the Naïades, and on the variations of form which it produces in individuals of the same species, and we should thus arrive at the certain and rational extinguishment of many species that authors have established, as Mr. Lea admits, solely upon their external character.

Thirdly, we will apply most rigorously the luminous and solid principles that M. Casimir Picard, of Abbeville, is going to publish, I hope, in a short time, upon the *deviations* or *pathological malformations* of the *Naïades*; and many more species would disappear in the sieve of this physiological scrutiny.

Fourthly, we will examine the laws of the geographical distribution of animals in the different classes, and, governed by analogy, we will not admit cosmopolitan species, except with the greatest reserve; but we will take equal care not to give too much specific importance to a difference of localities or of *habitat* within the limits of the same *zoological region.*

By means of these four considerations, we shall have exhausted all that *experience* and *analogy* offer as regards theory; and it will remain for us,

Fifthly, to resort to artificial method to finish conditionally, with all proper reservations, this great work of reform.

Here we shall be helped in the choice of types by the aid of consideration and combination; 1st, of the general form regarded as the generatrix of the modifications which will constitute simple varieties; 2dly, of the pallial and muscular impressions, and of the ligament; 3dly, of the general system of structure of the appendices of the shell, whether *null*, *pliciform*, *nodulous*, *spinous*, *symphynote* or *not*, (these ingenious divisions are due to Mr. Lea;) 4thly, of the essential and constant character of the structure of the hinge; 5thly, of the thickness of the shell; 6thly and finally, of the general system of coloring; for we must, as Mr. Lea has done in many cases, deny *specific* value to epidermal coloring, and still more to rays so variable in form, in magnitude, and in the different ages, and especially to the superficial coloring of the nacre.

I shall cite as examples of this mode of judging, the entirely peculiar system of coloring of the *Unio irroratus* and *cylindricus;* the unique form of thc appendices in the *Unio spinosus* and in the *Symphynota bialata* (Lea, 1st volume;) the peculiar form of the tubercles in the *Unio lacrymosus* and *apiculatus*, &c.

By combining all these means of exact judgment on the one part, and of logical induction on the other, I am satisfied that we shall recognize in the group of the Naïades, as experience has taught us in others, the truth of that observation, whose expression is recorded in the maxim, *Natura non facit saltum*, viz. that exterior unlikeness does not exclude autonomy, and *vice versâ;* or, in other words, that two beings inseparably connected by their essential characters, are often much more unlike in their external appearance, than two beings entirely different in essential characters. The demonstration of this proposition is found by the zoologist, in the *races*, so abounding in varieties, of the dog, the ox, the cock, of man himself compared with the *real* species but little removed, of the quadrumana, the rodentia, the motacillæ, the accipitres, etc. The botanist finds it, in comparing the *races* so prodigiously varied of the pears, the roses, the carnations, of the cereal plants and pot-herbs, with the true species, although differing little in appearance, of the genera *Ranunculus*, *Galium*, *Artemisia*, *Senecio*, *Carex*, *Bromus*, etc. The mineralogist and the geologist find it, as every one knows, at every step of their investigations.

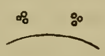
Such, gentlemen, are the principles, upon which, (in default of absolute certainty, perhaps not attainable,) I would reform the demarcation of the species of the family of the Naïades. And I must say, that no philosopher seems to me at the same time more able, and better situated to accomplish it than Mr. Lea himself. But has the time arrived in which he can thus occupy himself usefully? I think not yet. Possessing the richest of *special* collections, Mr. Lea has not yet, however, had the opportunity of inspecting *all* the nominal species. America, where he lives, is evidently the privileged country of the Uniones and Anodontæ, considered with respect to the number of species; but America has not yet been so entirely explored in this point of view, as not to leave a great number of links wanting in that chain, (which should be unbroken if possible,) upon which the reformer should mark his divisions. It would probably be better, still to loose the reins for some years, to collectors, to observers, and even to makers of species, in order to encourage them, and thus induce them to increase greatly the mass of materials. In this latter relation, no author has rendered more eminent services to science than Mr. Lea, in his first volume, which is already in your library, and also in his second, of which I am going to exhibit to you briefly, the contents. CH. DES MOULINS.

ART. XII.—*Description of an American Spider, constituting a new sub-genus, of the tribe Inæquitelæ of Latreille;* by Prof. N. M. HENTZ, Florence, Ala.

[Read before the Yale Natural-History Society, April 28, 1841.]

THE genus Aranea of Linnæus, like most of the genera established by that great man, is now in fact an extensive family of the animal kingdom. Walckenaer and Latreille subdivided it, and at once classified the numerous species known to them, in an admirable order. We may add the species since discovered and such subgenera as were not known to those authors, without materially altering their superstructure. But when the work is accomplished, and all nature is described by man, the number of species included in the common word *spider*, will be truly amazing. Walckenaer enumerated 260 species thirty four years ago, and Latreille could easily have doubled the catalogue, if the number of species had been mentioned in the last edition of the *Règne Animal.* The writer of this paper, in the course of twenty years, has, at stolen hours, collected and described 147 species; but he is convinced that *fifty* more could be added; as he has not explored the vast peninsula of Florida, nor any portion of Louisiana. Two hundred species, therefore, would be a low estimate of the number of spiders inhabiting the United States, not including the territories yet unoccupied by civilized men. It is obvious that the number of species throughout the world will amount to more than *two thousand*, when the natural history of all countries is complete. It is equally obvious that the rapidly increasing number of new species requires subdivisions, when it is practicable to make them. The subgenus now proposed is indispensable, as the species cannot be classed under any existing generic name. It will be placed in a natural order immediately after Pholcus.

Subgenus Spermophora.

Eyes, six, in two clusters, one on each side of the cephalothorax, thus 

Legs, the *first* pair longest, then the *fourth* and *second*, nearly equal, the *third* pair shortest. Length moderate, slender.

Lip, wide, triangular.

Maxillæ, tapering towards the point, inclined over the lip.

Mandibulæ, short, conical, with very small fangs.

The characters derived from the trophi, are nearly those of Pholcus, but the absence of the two eyes in front of the cephalothorax, would alone, remove this spider from that subdivision. Moreover, the legs, which in Pholcus are excessively long, are here of a moderate length. This spider, which is wholly of a pale hue, makes its very loose web in dark places, under rubbish. The female carries in its mandibles its eggs glued together without any silk, until they are hatched. Inhabits Alabama.

This species, the 137th of my MS. catalogue, is there named *Spermophora meridionalis*. Of the 147 species comprised in this catalogue, there are not ten mentioned in European works, besides those described by Bosc, whose manuscript was never printed.*

Florence, Ala., September 2, 1839.

Art. XIII.—*Contributions to Electricity and Magnetism.—No. IV. On Electro-Dynamic Induction;* by Joseph Henry, LL. D., Professor of Natural Philosophy in the College of New Jersey, Princeton. Read June 19, 1840.†

(Continued from p. 243, Vol. xxxviii, No. 2, of this Journal.)

INTRODUCTION.

1. In the course of my last paper, it was stated that the investigations which it detailed were not as complete in some parts as I could wish, and that I hoped to develop them more fully in

---

* A valuable catalogue and description by Prof. Hentz, of the genera of Araneides found in this country, was published in this Journal, Vol. xxi, p. 100—109, 1832.—*Eds.*

† From the Transactions of the American Philosophical Society, Vol. 8, N. S.

another communication. After considerable delay, occasioned by alterations in the rooms of the physical department of the college, I was enabled to resume my researches, and since then I have been so fortunate as to discover a series of new facts belonging to different parts of the general subject of my contributions. These I have announced to the Society at different times, as they were discovered, and I now purpose to select from the whole such portions as relate particularly to the principal subject of my last paper, namely, the induction at the beginning and ending of a galvanic current, and to present them as a continuation, and, in a measure, as the completion, of this part of my researches. The other results of my labors in this line will be arranged for publication as soon as my duties will permit me to give them a more careful examination.

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

3. The principal articles of apparatus used in these experiments are nearly the same as those described in my last paper, namely, several flat coils and a number of long wire helices. (III. 6, 7, 8.)* I have, however, added to these a constant battery, on Professor Daniell's plan, the performance of which has fully answered my expectations, and confirmed the accounts given of this form of the instrument by its author. It consists of thirty elements, formed of as many copper cylinders, open at the bottom, each five inches and a half in height, three inches and a half in diameter, and placed in earthen cups. A zinc rod is suspended in each of these, of the same length as the cylinders, and about one inch in diameter. The several elements are connected by a thick copper wire, soldered to the copper cylinder of one element, and dipping into a cup of mercury on the zinc of the next. The cop-

* When the numerals II or III are included in the parenthesis, reference is made to the corresponding Nos. of my contributions.

per and zinc as usual are separated by a membrane, on both sides of which is placed a solution of one part of sulphuric acid in ten parts of water; and to this is added, on the side next the copper, as much sulphate of copper, as will saturate the solution. The battery was sometimes used as a single series, with all its elements placed consecutively, and at others in two or three series, arranged collaterally, so as to vary the quantity and intensity of the electricity as the occasion might require.

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

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

Section I.

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

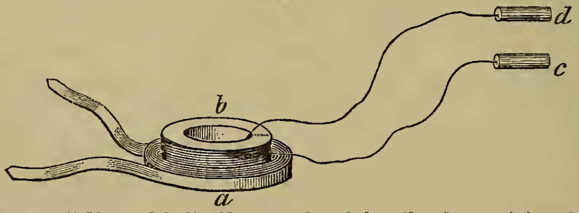
6. It will be recollected that the arrangement of apparatus employed in my last series of experiments gave a powerful induction at the moment of breaking the galvanic circuit, but the effect at making the same was so feeble as scarcely to be perceptible. I was unable in any case to get indications of currents of the third

or fourth orders from the beginning induction, and its action was therefore supposed to be so feeble as not materially to affect the results obtained.

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

8. In the first experiment, coil No. 2, which it will be remembered (III, 7) consists of a copper riband of about sixty feet long, and coiled on itself like the main spring of a watch, was connected with the compound battery, and helix No. 1, (III. 8,) formed of one thousand six hundred and sixty yards of fine copper wire,

Fig. 3.



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

was placed on the coil to receive the induction, as is shown in figure 3, which is again inserted here for the convenience of the reader. This arrangement being made, currents of increasing intensity were passed through the coil, by constantly retaining one of its ends in the cup of mercury forming one extremity of the battery, and successively plunging the other end into the cups which served to form the connections of the several elements of the battery. With the current from one element, the shock at breaking the circuit was quite severe, but at making the same it was very feeble, and could be perceived in the fingers only or through the tongue. With two elements in the circuit, the shock at beginning was slightly increased; with three elements the increase was more decided, while the shock at breaking the circuit remained nearly of the same intensity as at first, or was comparatively but little increased. When the number of elements was increased to *ten*, the shock at making contact was found fully equal to that at breaking, and by employing a still greater number, the former was decidedly stronger than the latter, the difference continually increasing until all the thirty elements were introduced into the circuit.

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

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

shocks are mentioned as being compared, in the other parts of this paper.

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

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

13. In the experiment given in paragraph 11, the intensity of the shock at the *ending* of the battery current diminishes with each diminution of the length of the coil; and this is also due to the decrease of the number of the spires of the coil, as is evident from an experiment similar to the last, in which the helix was placed on a coil consisting of only two turns or spires of copper riband; the shock at the ending, with this arrangement, was comparatively feeble, but could be felt in the hands. Different lengths of coil No. 2 were now introduced into the same circuit, but not so as to act on the helix; but although these were varied from four or five feet to the whole length of the coil, (sixty feet,) not the least difference in the intensity of the shock could be per-

ceived. We have, therefore, the remarkable result, that the intensity of the ending induction of each unit of length of the battery current is not materially altered, at least within certain limits, by changing the length of the whole conductor. From this we would infer that the shock depends more on the intensity of the action than on the quantity of the current, since we know that the latter is diminished in a given unit of the conductor by increasing the length of the whole.

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

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

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

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

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

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

19. The feebleness of the shock at the beginning of the current, with a single battery and a long coil, is not entirely owing to the cause we have stated, (17,) namely, the resistance to conduction offered by the long conductor, but also depends, in a considerable degre, if not principally, on the adverse influence of the secondary current, induced in the primary conductor itself, as is shown by the result of the following experiment. Helix No. 1 was placed on a coil consisting of only three spires or turns of copper riband; with this, the shock both at making and breaking the circuit with a single battery could be felt in the hands. A compound coil was then formed of the copper ribands of coils No. 3 and 4 rolled together so that the several spires of the two alternated with each other, and when this was introduced into the circuit so as not to act on the helix by its induction, and the battery current passed through, for example, coil No. 3, the shock at making contact with the pole of the battery was so much redu-

ced as to be imperceptible in the hands, while the shock at breaking the contact was about the same as before this addition was made to the length of the circuit. The ends of coil No. 4 were now joined so as to produce a closed circuit, the induced current in which would neutralize the secondary current in the battery conductor itself; and now the shock at making the contact was nearly as powerful as in the case where the short conductor alone formed the circuit with the battery. Hence, the principal cause of the feebleness of the effect at the beginning of the battery current is the adverse action on the helix of the secondary current produced in the conductor of the battery circuit itself. The shock at the breaking of the circuit, in this experiment, did not appear affected by joining or separating the ends of coil No. 4.

20. Having investigated the conditions on which the inductive action at the beginning of a battery current depends, experiments were next instituted to determine the nature of the effects produced by this induction; and first, the coils were arranged in the manner described in my last paper, (III, 79,) for producing currents of the different orders. The result with this arrangement was similar to that which I have described in reference to the ending induction, namely, currents of the third, fourth, and fifth orders were readily obtained.

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

22. Likewise, the same screening or rather neutralizing effect was produced, when a plate of metal was interposed between two consecutive conductors of the series of currents, as was described (III, section 4,) in reference to the ending induction. In short, the series of induced currents produced at the beginning of the primary current appeared to possess all the properties belonging to those of the induction at the ending of the same current.

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

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

25. The direction of the different currents produced at the making of the battery current, as determined by these instruments, is as follows, viz. the direction of the secondary current is, as stated by Dr. Faraday, adverse to that of the primary current, and also, the direction of each succeeding current is opposite to that of the one which produced it. We have, therefore, from these results, and those formerly obtained, (III, 92,) the following series of directions of currents, one produced at the moment of beginning and the other at that of the ending of the battery current.

	At the beginning.	At the ending.
Primary current, ...	+	+
Secondary current, . . .	—	+
Current of the third order, .	+	—
Current of the fourth order, .	—	+
Current of the fifth order, .	+	—

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

27. In connexion with the results given in the last two paragraphs, it is due to Mr. Sturgeon that I should state that, in a letter addressed to me, and published in the Annals of Electricity, he has predicted, from his theory, that I would find on examination, the series of alternation of currents for the beginning induction which I have here given. I may however add, that

it appears to me that this result might have been predicted without reference to any theory. There was no reason to suppose the induction at the beginning would be different in its nature from that at the ending, and therefore the series which would be produced from the former might be immediately inferred from that belonging to the latter, by recollecting that the direction of the induction at the beginning should be opposite to that at the ending. I do not wish it to be supposed, however, from this remark, that I had, myself, drawn any inference from my experiments as to the alternations of currents which might be produced by the beginning induction; the truth is, that this action was so feeble with the arrangement of apparatus I employed, that I supposed it could not produce a series of currents of the different orders.

---

28. In the course of the experiments given in this section, I have found that a shock can be produced without using a coil, by arranging about ten elements of the battery in the form of a circle, and placing the helix within this. The shock was felt in the hands at the moment of closing the circuit, but the effect at opening the same was scarcely perceptible through the tongue. An attempt was also made to get indications of induction by placing the helix within a circle of dilute acid, connected with a battery instead of a coil, but the effect, if any, was very feeble.

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

30. In this connexion I may mention that the idea has occurred to me that the intense shocks given by the electrical fish may possibly be from a secondary current, and that the great amount of nervous organization found in these animals may serve the purpose of a long conductor.* It appears to me, that in the present state of knowledge, this is the only way in which we can conceive of such intense electricity being produced in organs imperfectly insulated and immersed in a conducting medium. But we have seen that an original current of feeble intensity can induce, in a long wire, a secondary current capable of giving intense shocks, although the several strands of the wire are separated from each other only by a covering of cotton thread. Whatever may be the worth of this suggestion, the secondary current affords the means of imitating the phenomena of the shock from the electrical eel, as described by Dr. Faraday. By immersing the apparatus (Fig. 3,) in a shallow vessel of water, the handles being placed at the two extremities of the diameter of the helix, and the hands plunged into the water parallel to a line joining the two poles, a shock is felt through the arms; but when the contact with the water is made in a line at right angles to the last, only a slight sensation is felt in each hand, but no shock.

31. Since the publication of my last paper, I have exhibited to my class the experiment, (No. III, Sec. 3d,) relative to the induction at a distance on a much larger scale. All my coils were united so as to form a single length of conductor of about four hundred feet, and this was rolled into a ring of five and a half feet in diameter, and suspended vertically against the inside of the large folding doors which separate the laboratory from the lecture room. On the other side of the doors, in the lecture room, and directly opposite the coil, was placed a helix, formed of upwards of a mile of copper wire, one sixteenth of an inch in thickness, and wound into a hoop of four feet in diameter. With this arrangement, and a battery of one hundred and forty seven square feet of zinc surface divided into eight elements, shocks were perceptible in the tongue, when the two conductors were separated, to the distance of nearly seven feet; at the distance of between three and four feet, the shocks were quite severe. The exhibition was

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

rendered more interesting by causing the induction to take place through a number of persons standing in a row between the two conductors.

Section II.

On apparently two kinds of electro-dynamic induction.

32. The investigations arranged under this head had their origin in the following circumstances. After the publication of my last paper, I received, through the kindness of Dr. Faraday, a copy of the fourteenth series of his researches, and in this I was surprised to find a statement which appeared in direct opposition to one of the principal facts of my communication. In paragraph 59, I state, in substance, that when a plate of metal is interposed between the coil transmitting a galvanic current, and the helix placed above it to receive the induction, the shock from the secondary current is almost perfectly neutralized. Dr. Faraday, in the extension of his new and ingenious views of the agency of the intermediate particles in transmitting induction, was led to make an experiment on the same point, and apparently, under the same circumstances, he found that it "makes not the least difference, whether the intervening space between the two conductors is occupied by such insulating bodies as air, sulphur, and shell-lac, or such conducting bodies as copper and other non-magnetic metals."

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

34. After some reflection, however, the thought occurred to me that induction might be produced in such a way as not to be affected by the interposition of a plate of metal. To understand this, suppose the end of a magnetic bar placed perpendicularly under the middle of a plate of copper, and a helix suddenly brought down on this; an induced current would be produced in the helix by its motion towards the plate, since the copper, in this

case, could not screen the magnetic influence. Now if we substitute for the magnet a coil through which a galvanic current is passing, the effect should be the same. The experiment was tried by attaching the ends of the helix to a galvanometer,* and the result was as I expected: when the coil was suddenly brought down on the plate, the needle swung in one direction, and when lifted up, in the other; the amount of deflection being the same, whether the plate was interposed or not.

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

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

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

38. The experiment was also tried with magneto-electricity. For this purpose, about forty feet of copper wire, covered with silk, were wound around a short cylinder of stiff paper, and into this was inserted a hollow cylinder of sheet copper, and into this again, a short rod of soft iron; when the latter was rendered

---

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

magnetic, by suddenly bringing in contact with its two ends the different poles of two magnets, a current, of course, was generated in the wire, and this, as before, was found to affect the galvanometer to the same degree, when the copper cylinder was interposed, as when nothing but the paper intervened.

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

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

41. In reference to this surmise, it became important to examine again all the phenomena of induction at suddenly making and breaking a galvanic current.* And in connexion with this part of the subject, I will first mention a fact which was observed in the course of the experiments given in the last section, on the direction of the induced currents of different orders. It was found that though the indications of the galvanometer were the same as those of the spiral, in reference to the direction of the induced currents, yet they were very different in regard to the intensity of the action. Thus, when the arrangement of the apparatus was such that the induction at making the battery circuit was so feeble

* See *Contributions*, No. III.

as not to give the least magnetism to the needle, and so powerful at the ending as to magnetize it to saturation, the indication of the galvanometer was the same in both cases.

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

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

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

45. Another fact of importance belonging to the same class has been mentioned before, (24,) namely, that the action of the currents of the third, fourth, and fifth orders produces a very small effect on the galvanometer, compared with that of the secondary current; and this is not alone on account of the diminishing power of the successive inductions, as will be evident from the following experiment. By raising the helix from the coil, in the arrangement of apparatus for the secondary current, the shock was so diminished as to be inferior to one produced by the arrange ment for a tertiary current, yet, while with the secondary current the needle was deflected twenty-five degrees, with the tertiary it scarcely moved more than one degree, and with the currents of the fourth and fifth orders the deflections were still less, resembling the effect of a slight impulse given to the end of the needle.

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

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

48. In order to increase the number of facts belonging to this class, the last experiments were varied in several ways; and, first, instead of the hard steel needle, one of soft iron wire was placed in the spiral, with a small quantity of iron filings almost in contact with one of its ends. The plate being interposed, the small particles of iron were attracted by the end of the needle, indicating a feeble, temporary development of magnetism. Hence the current which moves the needle, and is not neutralized by the interposed plate, also feebly magnetizes soft iron, but not hard steel.

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

50. Another result was obtained by placing in the circuit of the helix, (Fig. 3d,) at the same time, the galvanometer, the spiral, and a drop of distilled water; with these the magnetizing power of the spiral was the same as without the water, but the deflection of the galvanometer was reduced from ten to about

four degrees. In addition to these, the body was also introduced into the same circuit; the shocks were found very severe, the spiral magnetized needles strongly, but the galvanometer was still less moved than before. The current of low intensity, which deflects the needle of the galvanometer in these instances, was partially intercepted by the imperfect conduction of the water and the body.

51. To exhibit the results of these experiments with still more precision, an arrangement of apparatus was adopted similar to that used by Dr. Faraday, and described in the fourteenth series of his researches, namely, a double galvanometer was formed of two separate wires of equal length and thickness, and wound together on the same frame; and also, a double magnetizing spiral was prepared by winding two equal wires around the same piece of hollow straw. Coil No. 1, connected with the battery, was supported perpendicularly on a table, and coils Nos. 3 and 4 were placed parallel to this, one on each side, to receive the induction, the ends of these being so joined with those of the galvanometer and the spiral that the induced current from the one coil, would pass through the two instruments, in an opposite direction to that of the current from the other coil. The two outside coils were then so adjusted, by moving them to and from the middle coil, that the induced currents perfectly neutralized each other in the two instruments, and the needle of the galvanometer and that in the spiral were both unaffected when the circuit of the battery was made and broken. With this delicate arrangement the slightest difference in the action of the two currents would be rendered perceptible; but when a zinc plate was introduced so as to screen one of the coils, the needle of the galvanometer still remained perfectly stationary, indicating not the least action of the plate, while the needle in the spiral became powerfully magnetic. When, however, a plate of iron was interposed instead of the one of zinc, the needle of the galvanometer was also affected.

52. From the foregoing results it would seem that the secondary current, produced at the moment of suddenly beginning or ending of a galvanic current, by making and breaking contact with a cup of mercury, consists of two parts, which possess different properties. One of these is of low intensity, can be interrupted by a drop of water, does not magnetize hardened steel needles, and is not screened by the interposition of a plate of any

metal, except iron, between the conductors. The other part is of considerable intensity, is not intercepted by a drop of water, develops the magnetism of hardened steel, gives shocks and is screened or neutralized by a closed coil, or a plate of any kind of metal. Also, the induced current produced by moving a conductor towards or from a battery current, and that produced by the movement up and down of a battery in the acid, are of the nature of the first mentioned part, while the currents of the third, fourth, and fifth orders partake almost exclusively of the properties of the second part.

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

Section III.

Theoretical Considerations relating to the Phenomena described in this and the preceding communications. Read November 20, 1840.

54. The experiments given in the last No. of my contributions were merely arranged under different heads, and only such inferences drawn from them as could be immediately deduced without reference to a general explanation. The addition, however, which I have since made to the number of facts, affords the means of a wider generalization; and after an attentive consideration of all the results given in this and the preceding papers, I have come to the conclusion that they can all be referred to the simple laws of the induction at the beginning and the ending of a galvanic current.

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

importance in the way of suggesting new forms of experiment, or as a first step towards a more perfect generalization.

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

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

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

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

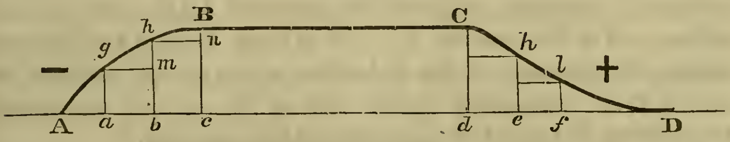
---

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

the same. During the time of the descent of the battery into the liquid, the conductor connected with it is constantly receiving additional quantities of current electricity, and each of these additions produces an inductive action on the adjoining secondary conductor. The amount, therefore, of induced current produced during any moment of time will be just in proportion to the corresponding increase in the current of the battery during the same moment. Also, the amount of induction during any moment while the current of battery is diminishing in quantity will be in proportion to the decrease during the same moment.

60. The several conditions of this experiment may be represented by the different parts of the curve, A, B, C, D, Fig. 17, in which the distances, A*a*, A*b*, A*c*, represent the times during which the battery is descending to different depths into the acid,

Fig. 17.



and the corresponding ordinates, *ag*, *bh*, *c*B, represent the amount of current electricity in the battery conductor corresponding to these times. The differences of the ordinates, namely, *ag*, *mh*, *n*B, express the increase in the quantity of the battery current during the corresponding moments of time represented by A*a*, *ab*, *bc;* and since the inductive actions (59) are just in proportion to these *increases*, the same differences will also represent the amount of induced action exerted on the secondary conductor during the same moments of time.

61. When the battery is fully immersed in the acid, or when the current in the conductor has reached its state of maximum quantity, and during the time of its remaining constant, no induction is exerted; and this condition is expressed by the constant ordinates of the part of the curve BC, parallel to the axis. Also, the inductive action produced by each diminution of the battery current, while the apparatus is in the progress of being drawn from the acid, will be represented by the differences of the ordinates at the other end, CD, of the curve.

62. The sum of the several increments of the battery current up to its full development, will be expressed by the ordinate *c*B,

and this will, therefore, also represent the whole amount of inductive action exerted in one direction at the beginning of the primary current; and for the same reason, the equal ordinate, C*d*, will represent the whole induction in the other direction at the ending of the same current. Also, the whole time of continuance of the inductive action at the beginning and ending will be represented by A*c* and *d*D.

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

64. It is evident from the last paragraph, that the greater or less intensity of the inductive action will be immediately presented to the eye, by the greater or less obliquity of the several parts of the curve to the axis. Thus, if the battery be suddenly plunged into the acid for a short distance, and then gradually immersed through the remainder of the depth, the varying action will be exhibited at once by the form of AB, the first part of the curve, Fig. 17. The steepness of the part A*g* will indicate an intense action for a short time A*a*, while the part *g*B denotes a more feeble induction during the time represented by *ac*. In the same way, by drawing up the battery suddenly at first, and afterwards slowly, we may produce an inductive action, such as would be represented by the parts between C and D of the ending of the curve.

65. Having thus obtained representations of the different elements of action, we are now prepared to apply these to the phe-

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

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

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

67. The results mentioned in the last paragraph cannot be obtained by plunging a battery into the acid; the formation of the current in this way is not sufficiently rapid to produce a shock. The example was given to illustrate the manner in which the same effect is supposed to be produced, in the case of the more sudden formation of a current, by plunging one end of the conductor into a cup of mercury permanently attached to a battery already

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

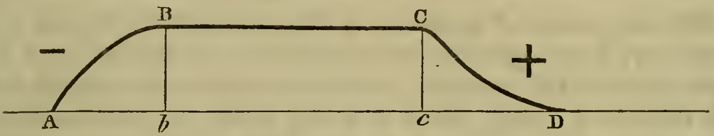
in the acid, and in full operation. The current, in this case, rapid as may be its development, cannot be supposed to assume *per saltum* its maximum state of quantity; on the contrary, from the general law of continuity we would infer, that it passes through all the intermediate states of quantity, from that of no current, if the expression may be allowed, to one of full development; there are, however, considerations of an experimental nature which would lead us to the same conclusion, (18,) (90,) and also to the farther inference that the *decline* of the current is not instantaneous. According to this view, therefore, the inductive actions at the beginning and the ending of a primary current, of which the formation and interruption are effected by means of the contact with a cup of mercury, may also be represented by the several parts of the curve, Fig. 17.

68. We have now to consider how the rate of increase or diminution of the current, in the case in question, can be altered by a change in the different parts of the apparatus; and, first, let us take the example of a single battery and a short conductor, making only one or two turns around the helix; with this arrangement, a feeble shock, as we have seen, (11,) will be felt at the making, and also at the breaking of the circuit. In this case it would seem that almost the only impediment to the most rapid development of the current would be the resistance to conduction of the metal; and this we might suppose would be more rapidly overcome by increasing the tension of the electricity; and, accordingly, we find that if the number of elements of the battery be increased, the shock at making the circuit will also be increased, while that at breaking the circuit will remain nearly the same. To explain, however, this effect more minutely, we must call to mind the fact before referred to, (17,) that when the poles of a compound battery are not connected, the apparatus acquires an accumulation of electricity, which is discharged at the first moment of contact, and which, in this case, would more rapidly develope the full current, and hence produce the more intense action on the helix at making the circuit.

69. The shock, and also the deflection of the needle, at breaking the circuit with a compound battery and a short coil, (9,) appear nearly the same with a battery of a single element, because the accumulation just mentioned, in the compound battery, is discharged almost instantly, and, according to the theory (71) of

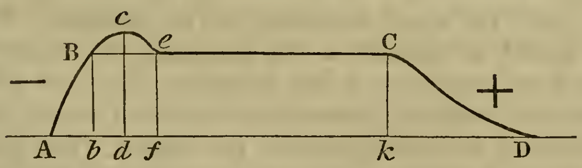
the galvanic current, leaves the constant current in the conductor nearly in the same state of quantity as that which would be produced by a battery of a single element; and hence the conditions of the ending of the current are the same in both cases. Indeed, in reference to the ending induction, it may be assumed as a fact which is in accordance with all the experiments, (9, 13, 73, 74, 75, 76, &c.,) as well as with theoretical considerations,* *that when the circuit is broken by a cup of mercury, the rate of the diminution of the current, within certain limits, remains the same, however the intensity of the electricity or the length of the conductor may be varied.*

Fig. 18.



70. The several conditions of the foregoing examples are exhibited by the parts of the curves, Figs. 18 and 19. The gradual development of the current in the short conductor, with a single battery, and the gradual decline of the same, are represented by the gentle rise of AB and fall of CD, Fig. 18; while, in the next Fig. (19,) the sudden rise of AB indicates the intensity which produces the increased shock, after the number of elements of the battery has been increased. The accumulation of the electricity, which almost instantly subsides, is represented by the part B*ce*, Fig. 19, and from this we see at once, that although the

Fig. 19.



shock is increased by using the compound battery, yet the needle of the galvanometer will be deflected only to the same number of degrees, since the parts B*c* and *ce* give inductive actions in

* See the theory of Ohm.

contrary directions, and both within the time of a single swing of the needle, and, consequently, will neutralize each other. The resulting deflecting force will, therefore, be represented by *ef*, which is equal to C*k*, or to *b*B, in Fig. 18.

The intensity of the shock at the breaking is represented as being the same in the two figures, by the similarity of the rate of descent of the part CD of the curve in each.

71. We have said (69) that the quantity of current electricity in a short conductor and a compound battery, after the first discharge, is nearly the same as with a single battery. The exact quantity, according to the theory of Ohm, in a unit of length of the conductor is given by the formula $\frac{nA}{rn+R}$. In this, n represents the number of elements; A, the electromotive force of one element; r, the resistance to conduction of one element; and R, the length of the conductor, or rather its resistance to conduction in terms of r. Now, when R is very small, in reference to rn, as is the case with a very short metallic conductor, it may be neglected, and then the expression becomes $\frac{nA}{rn}$ or $\frac{A}{r}$; and since this expresses the quantity of current electricity in a unit of the length of the circuit, with either a single or a compound battery, therefore, with a short conductor, the quantity of current electricity in the two cases is nearly the same.

72. Let us next return to the experiment with a battery of a single element, (68,) and instead of increasing the intensity of the apparatus, as in the last example, let the length of the conductor be increased; then the intensity of the shock at the beginning of the current, as we have seen, (14,) will be diminished, while that of the one at the ending will be increased. That the shock should be lessened at the beginning, by increasing the length of the conductor, is not surprising, since, as we might suppose, the increased resistance to conduction would diminish the rapidity of the development of the current. But the secondary current, which is produced in the conductor of the primary current itself, as we have seen, (19,) is the principal cause which lessens the intensity of the shock; and the effect of this, as will be shown hereafter, may also be inferred from the principles we have adopted.

73. The explanation of the increased shock at the moment of breaking the circuit with the long conductor, rests on the assumption before mentioned, (69,) that the velocity of the diminution of a current is nearly the same in the case of a long conductor as in that of a short one. But, to understand the application of this principle more minutely, we must refer to the change which takes place in the quantity of the current in the conductor by varying its length; and this will be given by another application of the formula before stated, (71.) This, in the case of a single battery, in which n equals unity, becomes $\frac{A}{r+R}$; and since this, as will be recollected, represents the quantity of current electricity in a unit of length of the conductor, we readily infer from it, that by increasing the length of the conductor, or the value of R, the quantity of current in a unit of the length is lessened. And if the resistance of a unit of the length of the conductor were very great in comparison with that of r, (the resistance of one element of the battery,) then the formula would become $\frac{A}{R}$, or the quantity in a single unit of the conductor would be inversely as its entire length, and hence the amount of current electricity in the whole conductor would be a constant quantity, whatever might be its length. This, however, can never be the case in any of our experiments, since in no instance is the resistance of R very great in reference to r, and therefore, according to the formula, (73,) the whole quantity of current electricity in a long conductor is always somewhat greater than in a short one.

74. Let us, however, in order to simplify the conditions of the induction at the ending of a current, suppose that the quantity in a unit of the conductor is inversely as its whole length, or, in other words, that the quantity of current electricity is the same in a long conductor as in a short one; and let us also suppose, for an example, that the length of the spiral conductor, Fig. 3, was increased from one spire to twenty spires; then, if the velocity of the diminution of the section of the current is the same (69) in the long conductor as in the short one, the shock which would be received by submitting the helix to the action of one spire of the long coil would be nearly of the same intensity as that from one spire of the short conductor; the quantity of induction, however, as shown by the galvanometer, should be nearly twenty

times less; and these inferences I have found in accordance with the results of experiments, (75.) If, however, instead of placing the helix on one spire of the long conductor, it be submitted at once to the influence of all the twenty spires, then the intensity of the shock should be twenty times greater, since twenty times the quantity of current electricity collapses, if we may be allowed the expression, in the same time, and exerts at once all its influence on the helix. If, in addition to this, we add the consideration that the whole quantity of current electricity in a long conductor is greater than that in a short one, (73,) we shall have a further reason for the increase of the terminal shock, when we increase the length of the battery conductor.

75. The inference given in the last paragraph, relative to the change in the quantity of the induction, but not in the intensity of the shock from a single spire, by increasing the whole length of the conductor, is shown to be true by repeating the experiment described in paragraph 13. In this, as we have seen, the intensity of the shock remained the same, although the length of the circuit was increased by the addition of coil No. 2. When, however, the galvanometer was employed in the same arrangement, the whole quantity of induction, as indicated by the deflection of the needle, was diminished almost in proportion to the increased length of the circuit. I was led to make this addition to the experiment (13) by my present views.

76. The explanation given in paragraph 74, also includes that of the peculiar action of a long conductor, either coiled or extended, in giving shocks and sparks from a battery of a single element, discovered by myself in 1831; (see Contrib. No. II.) The induction, in this case, takes place in the conductor of the primary current itself, and the secondary current which is produced is generated by the joint action of each unit of the length of the primary current. Let us suppose, for illustration, that the conductor was at first one foot long, and afterwards increased to twenty feet. In the first case, because the short conductor would transmit a greater quantity of electricity, the secondary current produced by it would be one of considerable quantity, or power to deflect a galvanometer; but it would be of feeble intensity, for although the primary current would collapse with its usual velocity, (69,) yet, acting on only a foot of conducting matter, the effect (74) would be feeble. In the second case, each foot

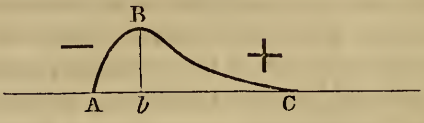
of the twenty feet of the primary current would severally produce an inductive action of the same intensity as that of the short conductor, the velocity of collapsion being the same; and as they are all at once exerted on the same conductor, a secondary current would result of twenty times the intensity of the current in the former case.

77. To render this explanation more explicit, it may be proper to mention that a current produced by an induction on one part of a long conductor of uniform diameter, must exist, of the same intensity, in every other part of the conductor; hence, the action of the several units of length of the primary current must enforce each other, and produce the same effect on its own conductor that the same current would if it were in a coil, and acting on a helix. I need scarcely add, that in this case, as in that given in paragraph 74, the whole amount of induction is greater with the long conductor than with the short one, because the quantity of current electricity is greater in the former than in the latter.

78. We may next consider the character of the secondary current, in reference to its action in producing a tertiary current in a third conductor. The secondary current consists, as we may suppose, in the disturbance, for an instant, of the natural electricity of the metal, which, subsiding, leaves the conductor again in its natural state; and whether it is produced by the beginning or ending of a primary current, its nature, as we have seen, (22,) is the same. Although the time of continuance of the secondary current is very short, still we must suppose it to have some duration, and that it increases, by degrees, to a state of maximum development, and then diminishes to the normal condition of the metal of the conductor; the velocity of its development, like that of the primary current, will depend on the intensity of the action by which it is generated, and also, perhaps, in some degree, on the resistance of the conductor; while, agreeably to the hypothesis we have assumed, (69,) the velocity of its diminution is nearly a constant quantity, and is not affected by changes in these conditions; hence, if we suppose the induction which produces the secondary current to be sufficiently intense, the velocity of its development will exceed that of its diminution, as in the example of the primary current from the intense source of the compound battery of many elements. Now this is the case with the inductions which produce currents of the different or-

ders, capable of giving shocks or of magnetizing steel needles; the secondary currents from these are always of considerable intensity, and hence their rate of development must be greater than that of their diminution, and, consequently, they may be represented by a curve of the form exhibited in Fig. 20, in which there is no constant part, and in which the steepness of AB is

Fig. 20.



greater than that of BC. There are, however, other considerations, which will be noticed hereafter, (89,) which may affect the form of the part BC of the curve, Fig. 20, rendering it still more gradual in its descent, or, in other words, which tend to diminish the intensity of the ending induction of the secondary current.

79. It will be seen at once, by an inspection of the curve, that the effect produced, in a third conductor, and which we have called a tertiary current, is not of the same nature as that of a secondary current. Instead of being a single development in one direction, it consists of two instantaneous currents, one produced by the induction of AB, and the other, by that of BC, in opposite directions, of equal quantities, but of different intensities. The whole quantity of induction in the two directions, will each be represented by the ordinate B*b*, and hence they will nearly neutralize each other, in reference to their action on the galvanometer, in the circuit of the third conductor. I say, they will *nearly* neutralize each other, because, although they are equal in quantity, they do not both act in absolutely the same moment of time. The needle will, therefore, be slightly affected; it will be impelled in one direction, say to the right, by the induction of AB, but, before it can get fairly under way, it will be arrested, and turned in the other direction, by the action of BC. This inference is in strict accordance with observation; the needle, as we have seen, (24,) starts from a state of rest, with a velocity which, apparently, would send it through a large arc, but before it has reached, perhaps, more than half a degree, it suddenly stops, and turns in the other direction. As the needle is first affected by the action of AB, it indicates a current in the adverse direction to the secondary current.

80. Although the two inductions in the tertiary conductor nearly neutralize each other, in reference to the indications of the galvanometer, yet this is far from being the case with regard to the shocks, and the magnetization of steel needles. These effects may be considered as the results alone of the action of AB; the induction of BC being too feeble in intensity to produce a tertiary current of sufficient power to penetrate the body, or overcome the coercive power of the hardened steel. Hence, in reference to the shock, and magnetization of the steel needle, we may entirely neglect the action of BC, and consider the tertiary excitement as a single current, produced by the action of AB; and, because this is the beginning induction, (56,) the tertiary current must be in an opposite direction to the secondary. For a similar reason, a current of the third order should produce in effect a single current of the fourth order, in a direction opposite to that of the current which produced it, and so on: we have here, therefore, a simple explanation of the extraordinary phenomenon of the alternation of the directions of the currents, of the different orders, as given in this (25) and the preceding paper.

81. The operation of the interposed plate, (32, 47, 48, &c.,) in neutralizing the shock, and not affecting the galvanometer, can also be readily referred to the same principles. It is certain, that an induced current is produced in the plate, (III, 64,) and that this must react on the secondary, in the helix; but it should not alter the total amount of this current, since, for example, at the ending induction, the same quantity of current is added to the helix, while the current in the plate is decreasing, as is subtracted while the same current is increasing. To make this more clear, let the inductive actions of the interposed current be represented by the parts of the curve, Fig. 20. The induction represented by AB will react on the current in the helix, and diminish its quantity, by an amount represented by the ordinate *b*B; but the induction represented by BC, will act in the next moment, on the same current, and increase its quantity by an equal amount, as represented by the same ordinate B*b*; and since both actions take place within a small part of the time of a single swing of the needle, the whole deflection will not be altered, and consequently, as far as the galvanometer is concerned, the interposition of the plate will have no perceptible effect.

82. But the effect of the plate on the shock, and on the magnetization of tempered steel, should be very different; for although the quantity of induction in the helix may not be changed, yet its intensity may be so reduced, by the adverse action of the interposed current, as to fall below that degree which enables it to penetrate the body, or overcome the coercive force of the steel. To understand how this may be, let us again refer, for example, to the induction which takes place at the ending of a battery current: this will produce, in both the helix and the plate, a momentary current, in the direction of the primary current, which we have called *plus;* the current in the plate will react on the helix, and tend to produce in it two inductions, which, as before, may be represented by AB, and BC, of the curve, Fig. 20; the first of these, AB, will be an intense action, (78,) in the *minus* direction, and will, therefore, tend to neutralize the intense action of the primary current on the helix; the second, (BC,) will add to the helix an equal quantity of induced current, but of a much more feeble intensity, and hence the resulting current in the helix will not be able to penetrate the body; no shock will be perceived, or at least a very slight one, and the phenomena of screening will be exhibited.

83. When the plate of metal is placed between the conductors of the second and third orders, or between those of the third and fourth, the action is somewhat different, although the general principle is the same. Let us suppose the plate interposed between the second and third conductors; then the helix, or third conductor, will be acted on by four inductions, two from the secondary current, and two from the current in the plate. The direction and character of these will be as follows, on the supposition that the direction of the secondary current is itself *plus:*

The beginning secondary .	intense and . .	*minus.*
The ending secondary . .	feeble and . .	*plus.*
The beginning interposed .	intense and . .	*plus.*
The ending interposed . .	feeble and . .	*minus.*

Now if the action, on the third conductor, of the first and third of the above inductions be equal in intensity and quantity, they will neutralize each other; and the same will also take place with the action of the second and fourth, if they be equal, and hence, in this case, neither shock nor motion of the needle of the galvanometer would be produced. If these inductions are not precisely equal, then, only a partial neutralization will take place, and the shock will only be diminished in power; and, also, perhaps, the needle will be very slightly affected.

84. If, in the foregoing exposition, we throw out of consideration the actions of the feeble currents which cannot pass the body, and, consequently, are not concerned in producing the shock, then the same explanation will still apply which was given in the last paper, (III, 94,) namely, in the above example, the helix is acted on by the minus influence of the secondary, and the plus influence of the interposed current.

85. We are now prepared to consider the effect on the helix (Fig. 3) of the induced currents produced in the conductor of the primary current itself. These are true secondary currents, and are almost precisely the same in their action as those in the interposed plate. Let us first examine the induced current at the beginning of the primary, in the case of a long coil and a battery of a single element; its action on the helix may be represented by the parts of the curve, Fig. 20. The first part, AB, will produce an intense induction opposite to that of the primary current; and hence the action of the two will tend to neutralize each other, and no shock, or a very feeble one, will be produced. The ending action of the same induced current, which is represented by BD, restores to the helix the same quantity of current electricity (but in a feeble state) which was neutralized by AB, and hence the needle of the galvanometer will be as much affected as if this current did not exist. These inferences perfectly agree with the experiment given in paragraph 19. In this, when the ends of the interposed coil were joined so as to neutralize the induced current in the long conductor, the shock at the beginning of the primary current was nearly as powerful as with a short conductor, while the amount of deflection of the galvanometer was unaffected by joining the ends of the same coil.

86. At first sight it might appear that any change in the apparatus which might tend to increase the induction of the primary current (16) would also tend to increase, in the same degree, the adverse secondary in the same conductor; and that hence the neutralization mentioned in the last paragraph would take place in all cases; but we must recollect that if a more full current be suddenly formed in a conductor of a given thickness, the ad-

verse current will not have, as it were, as much space for its development, and therefore, will have less power in neutralizing the induction of the primary than before. But there is another, and, perhaps, a better reason, in the consideration that in the case of the increase of the number of elements of the battery, although the rapidity of the development of the primary current is greater, yet the increased resistance which the secondary meets with, in its motion against the action of the several elements, will tend to diminish its effect. Also, by diminishing the length of the primary current, we must diminish (76) the intensity of the secondary, so that it will meet with more resistance in passing the acid of the single battery, and thus its effects be diminished.

87. The action of the secondary current, in the long coil at the *ending* of the primary current, should, also, at first sight, produce the same screening influence as the current in the interposed plate; but, on reflection, it will be perceived that its action in this respect must be much more feeble than that of the similar current at the beginning; the latter is produced at the moment of making contact, and hence it is propagated in a continuous circuit of conducting matter, while the other takes place at the *rupture* of the circuit, and must therefore be rendered comparatively feeble by being obliged to pass through a small portion of heated air; very little effect is therefore produced on the helix by this induction, (19.) The fact that this current is capable of giving intense shocks, when the ends of a long wire, which is transmitting a primary current, are grasped at the time of breaking the circuit, is readily explained, since, in this case, the body forms, with the conductor, a closed circuit, which permits the comparatively free circulation of the induced current.

88. It will be seen that I have given a peculiar form to the beginning and ending of the curves, Figs. 17, 18, &c. These are intended to represent the variations which may be supposed to take place in the rate of increase and decrease of the quantity of the current, even in the case where the contact is made and broken with mercury. We may suppose, from the existence of analogous phenomena in magnetism, heat, &c., that the development of the current would be more rapid at first than when it approximates what may be called the state of current saturation, or when the current has reached more nearly the limit of capacity of conduction of the metal. Also, the decline of the current may

be supposed to be more rapid at the first moment, than after it has lost somewhat of its intensity, or sunk more nearly to its normal state. These variations are indicated by the rapid rise of the curve, Fig. 17, from A to *g*, and the more gradual increase of the ordinates from *h* to B; and by the rapid diminution of the ordinates between C and *l*, and the gradual decrease of those towards the end of the curve.

89. These more minute considerations, relative to the form of the curve, will enable us to conceive, how the time of the ending of the secondary current, as we have suggested, (78,) may be prolonged beyond that of the natural subsidence of the disturbance of the electricity of the conductor on which this current depends. If the development of the primary current is produced by equal increments in equal times, as would be the case in plunging the battery (59) into the acid with a uniform velocity; then the part AB of the curve Fig. 17, would be a straight line, and the resulting secondary current, after the first instant, would be one of constant quantity during nearly the whole time represented by A*c*; but if the rate of the development of the primary current be supposed to vary in accordance with the views we have given in the last paragraph, then the quantity of the secondary current will begin to decline before the termination of the induction, or as soon as the increments of the primary begin to diminish; and hence the whole time of the subsidence of the secondary will be prolonged, or the length of *b*C, Fig. 20, will be increased, the descent of BC be more gradual, and the intensity of the ending induction of the secondary current be diminished: (see last part of paragraph 78.)

90. Besides the considerations we have mentioned, (88,) there are others of a more obvious character, which would also appear to affect the form of particular parts of the curve. And first we might perhaps make a slight correction in the drawing of Figs. 17, 18, &c., at the point A, in consideration of the fact that the very first contact of the end of the conductor with the surface of the mercury is formed by a point of the metal, and hence the increment of development should be a little less rapid at the first moment than after the contact has become larger; or in other words, the curve should perhaps start a little less abruptly from the axis at the point A. Also Dr. Page has stated* that he finds

---

* Vide this Journal, Vol. xxxiv, p. 166.

the shock increased by spreading a stratum of oil over the surface of the mercury; in this case it is probable that the termination of the current is more sudden, on account of the prevention of the combustion of the metal by means of the oil, and the fact that the end of the conductor is drawn up into a non-conducting medium.

91. The time of the subsidence of the current, when the circuit is broken by means of a surface of mercury, is very small, and probably does not exceed the ten thousandth part of a second, but even this is an appreciable duration, since I find that the spark at the ending presents the appearance of a band of light of considerable length, when viewed in a mirror revolving at the rate of six hundred times in a second; and I think the variations in the time of ending of the current under different conditions may be detected by means of this instrument.

92. Before concluding this communication, I should state that I have made a number of attempts to verify the suggestion given in my last paper, (III, 127,) that an inverse induction is produced by a galvanic current by a change in the distance of the conductors, but without success. These attempts were made before I had adopted the views given in this section, and since then I have found (80) a more simple explanation of the alternation of the currents.

93. In this number of my contributions, the phenomena exhibited by the galvanic apparatus have alone been discussed. I have, however, made a series of experiments on the induction from ordinary electricity, and the reaction of soft iron on currents, and I think that the results of these can also be referred to the simple principles adopted in this paper; but they require further examination before being submitted to the public.

Art. XV.—*A Brief, Preliminary Account of the Hessian Fly and its Parasites;* by Edward C. Herrick, Mem. Yale Nat. Hist. Soc.

[Communicated to the Yale Nat. Hist. Society, April 28, 1841.]

For several years past I have spent some time in the study of the habits of the Hessian fly, and of the various insects by which it is attacked. During a part of the period I enjoyed the important coöperation of my valued friend, Mr. James D. Dana, now absent from the country, as one of the scientific corps of the United States South Sea Exploring Expedition. It was, and still continues to be, my intention, to offer an extended paper on this subject. The investigation is not yet in every particular so complete as could be wished, but several circumstances seem to render it advisable to give at this time, a brief abstract of some portion of the results. The civil history of the insect, as well as the scientific descriptions, with many other details, are reserved for the final paper.

The Hessian fly, which has so long been conspicuous for its depredations on the wheat crops of this country, is a two-winged insect of the genus *Lasioptera* or *Cecidomyia*, (*Meig.* and *Latr.*) and was first scientifically described by the late Mr. Thomas Say, (*Jour. Acad. Nat. Sci. Phil.* 1817, i, 45,) who gave it the specific name of *Destructor*. The popular name was first used by Col. George Morgan, of Prospect, N. J., on the supposition that the insect was introduced into this country among the straw brought by the Hessian troops who came here in the service of Great Britain, during the war of the Revolution. This supposition has been rejected by most entomologists, chiefly perhaps, because an extensive and apparently thorough inquiry made in various parts of Europe a few years after, resulted in the uncontradicted conclusion that the insect was wholly unknown in that quarter of the world. I am not prepared to assert that this insect was introduced in the manner above supposed, but it may be shown that it is highly probable that it was unknown here before that time; that it now exists in Europe, and has probably been there for centuries.

In the *Eléments d'Agriculture, par Duhamel du Monceau, Paris*, 1771, 2 *tomes*, 12mo., is a statement from M. de Châteauvieux, of which the following is a translation:—"Our wheat [in the neighborhood of Geneva] has sustained the present month of May, 1755, an injury, from which the grain cultivated by the new husbandry has not been exempt. We found upon it a number of small white worms, which eventually turned to a chestnut color: they fix themselves within the leaves, and gnaw the stalks. They are commonly found between the first joint and the root: the stalks on which they fasten, grow no more; they become yellow and dry up. We suffered the same injury in 1732, when these insects appeared in the middle of May, and did such damage that the crops were almost annihilated." i, 299. This passage was quoted by Col. Morgan, (*Carey's Amer. Mus.* 1787, i, 530,) in the belief that the insect described in it was the Hessian fly. The description is too imperfect to authorize a positive assertion, but there seems to be little doubt that his opinion is correct.

In 1833, Mr. Dana sailed for the Mediterranean in the U. S. ship Delaware. An opportunity was thus afforded him to make personal exploration for the Hessian fly among the wheat fields of the old world; a work for which he was well prepared by his thorough acquaintance with this insect in its various stages. His examinations were rewarded with the most gratifying success, for they proved that *the Hessian fly is an inhabitant of Europe.* On the 13th of March, 1834, and subsequently, he collected several larvæ and pupæ, from wheat plants growing in a field on the island of Minorca. From these pupæ, were evolved on the 16th of March, 1834, two individuals of an insect which his recollections, (aided by a drawing of the Hessian fly with which he was provided,) enabled him to pronounce to be the *Cecidomyia Destructor.* More of the perfect insects were evolved in the course of the month, one of which deposited eggs like those of the Hessian fly. In letters dated Mahon, April 8 and 21, 1834, Mr. D. sent me five of the insects, and several of the pupæ. They arrived in safety, and after a careful examination, I saw no good reason to doubt the identity of this insect with the Hessian fly. The Mahonese asserted that the insect had been there from time immemorial, and often did great damage both there and in Spain. On the 28th of April, 1834, Mr. D. collected from a wheat field just without the walls of the city of *Toulon* in France, several pupæ and one larva like those before obtained. On the 4th of June, 1834, he obtained similar pupæ from a wheat field near *Naples*.—About the period of Mr. Dana's investigations in the south of Europe, attention was turned to the injury caused by certain larvæ among the wheat in Hungary. It appears now to be commonly believed, that their parent insect is either our Hessian fly, or an animal very closely allied to it.

I have searched in vain for any traces of the Hessian fly in this country before the Revolution. The Rev. Jared Eliot, in his "*Essays upon Field Husbandry in New England*," Boston, 1760, treats of the culture of wheat, but makes no allusion to any insect having habits like those of the Hessian fly; neither does Kalm, the naturalist, who travelled in this country about 1750. I am therefore inclined to consider the common opinion of the origin of the insect quite as probable as any other which has been advanced.

In this part of our country, wheat is usually sown about the first of September. Soon after the plants are up, the Hessian fly begins to lay her eggs upon them, and continues her operations for several weeks. She deposits her eggs on the upper surface of the leaf (i. e. the *ligula*, or strap-shaped portion of the leaf) of the plant. The number on a single leaf is often twenty or thirty, and sometimes much greater. In these cases many of the larvæ must perish. The egg is about a fiftieth of an inch long, and four hundredths of an inch in diameter, cylindrical, translucent, and of a pale red color. In about four days the egg hatches; the young larva creeps down the leaf, enters the sheath, and with the head downwards, fastens upon the tender culm or stalk, generally just above some joint. The larva appears to feed solely on the sap of the plant; it does not gnaw the stalk, and never enters it, but is gradually imbedded in it as the plant matures. Having taken its post, the larva is stationary; it gradually loses its reddish color, becomes translucent, and clouded with white spots, and when near maturity, the central part within is of a greenish hue. In about five or six weeks, (or longer if the season is cold,) the larva begins to assume a brownish tinge, and soon is of a bright chestnut color, when the insect may be said to have reached the state of *pupa*. It has then some resemblance to a flax-seed. The outer skin of the larva becomes the puparium of the pupa. The wheat plant is injured by the loss of sap, but principally by the pressure of the larvæ and pupæ upon the culm. A single larva will do little harm, (and may even be useful by stimulating the plant to throw out side shoots,) but five or six of them are sufficient seriously to check the growth of the plant, or perhaps to destroy it entirely.*

During the winter the insect is in the pupa state, near the root of the wheat plant, and usually a little below the surface of the earth. In April and May we again find the Hessian fly laying eggs on the young wheat, both that which was sown in the autumn previous, and the spring wheat, which is of course recently up. The larvæ from these eggs become pupæ about the middle of June.

There is no difficulty in tracing the insect as far as the state of pupa, and to this point its history is satisfactorily ascertained. Regarding the periods of the evolution of the perfect insect, there is, however, some obscurity, which numerous observations have not wholly cleared up. The difficulty results in part from the fact that in this region, a very large proportion, probably *more than nine tenths*, of every generation of the Hessian fly, is destroyed by parasites. A great part of the pupæ which may be collected will evolve some parasitic insect, instead of the Hessian fly. It is certain that sometimes, the pupæ, which became so in June, evolve the perfect insect in October following, and that other pupæ of the same date will not evolve the perfect insect until October of the year succeeding. The following seems to me the *probable* history of the matter. The pupæ, which became such in the autumn, evolve the perfect insect, partly during the next spring, and partly in the summer and autumn following.

---

* It has been repeatedly asserted that the Hessian fly lays her eggs on the ripening grain. This error has doubtless arisen from mistaking for the Hessian fly, other insects, which in various parts of our country attack the wheat. In the *Trans. of the Amer. Phil. Soc.*, 1771, i, 205, Col. Landon Carter has given some account of a pale brownish moth, called by him the *fly weevil that destroys the wheat*, which lays its eggs on the grain. A paper on what is probably the same insect, is published by John Lorain in Mease's *Archives of Useful Knowledge*, 1812, ii, 47. The insect is supposed to be that described by Duhamel du Monceau in his "*Histoire d'un Insecte qui devore les Grains de l'Angoumois*," Paris, 1762. 12mo.—the *Œcophora cerealella*, Oliv. I have little doubt that the *Tipula* (*Cecidomyia*) *Tritici*, Kirby, (*Trans. Linn. Soc.* iii, iv, v,) also inhabits this country.—In August, 1833, I received several stalks of wheat from West Chester, Penn., each containing, in the centre, a small larva of a wax-yellow color; but I failed to obtain the perfect insect. It is hardly necessary to say that this was not the larva of the Hessian fly. A critical investigation of all these insects is very much to be desired.

The pupæ, which became such in and about June, evolve the perfect insect, partly during the next autumn, and partly during the year succeeding.

Parasites.—There are in this region, *four* principal parasites of the Hessian fly, one of which attacks the eggs, and the other three the pupæ. They are all minute *Hymenoptera.*

1. The egg-parasite, is a species of *Platygaster*, Latr., and may prove to be identical with some one of the hundred species of this genus which are described. (Entom. Mag. Lond. iii, 217. Cont. Macl. Lyc. i, 81.) The insect is abundant in the autumn. I first saw it Sept. 23, 1833, in the act of depositing its eggs in the eggs of the Hessian fly. From subsequent observations it appears that four or five eggs are laid in a single egg of the Hessian fly. The latter egg hatches, and the animal advances to the pupa state as usual, but from the puparium no Hessian fly ever comes forth. This parasite forms within the puparium, a silky cocoon of a brownish color.

2. This is the chief parasite of the pupa. It is described by Mr. Say, (Jour. Acad. Nat. Sci. Phil. i, 47,) as the *Ceraphron Destructor.* It appears to me not to belong to the genus *Ceraphron*, (Latr.) but to fall within the genus *Eurytoma*, of Illiger. It pierces the sheath of the stalk, (making a hole too small to be detected by a powerful microscope,) and deposits an egg in the pupa within. This is done chiefly in June. The perfect insect is evolved in the summer and autumn succeeding, eating its way through the puparium and the sheath of the leaf. An insect (of which I have seen females only) very similar to the Eurytoma Destructor, but with mere rudiments of wings, is sometimes evolved from the pupæ of the Hessian fly. I am in doubt whether it should be considered a distinct species or only a variety. The winged individuals never throw off their wings.

3. The next parasite of the pupa, is an insect of the tribe Chalcidiæ, (*Latr.* in *Cuv. Règne An.*) whose genus I have not determined. Its habits are like those of No. 2, but it is evolved later. Apterous females of this species are also found.

4. Another parasite of the pupa is an insect of the tribe Oxyuri, (*Latr.* in *Cuv.*) whose genus I have not determined. In habits it agrees with Nos. 2 and 3, but it is evolved still later in the year. All of these parasites are likewise evolved in the spring, from Hessian fly pupæ of the summer previous.

A few suggestions may be made respecting the best modes of preventing the ravages of the Hessian fly. They have all been published before, by others, but they are of such a nature that there is little probability that any of them will ever exterminate the insect. The stouter varieties of wheat ought always be chosen, and the land should be kept in good condition. If fall wheat is sown late, some of the eggs will be avoided, but risk of winter-killing the plants will be incurred. If cattle are permitted to graze the wheat fields during the fall, they will devour many of the eggs. A large number of the pupæ may be destroyed by burning the wheat-stubble immediately after harvest, and then ploughing and harrowing the land. This method will undoubtedly do much good. As the Hessian fly also lays its eggs, to some extent, on rye and barley, these crops should be treated in a similar manner.

New Haven, Conn.

Art. XV.—*Proceedings of Learned Societies.*

I. *Association of American Geologists.*

The second annual meeting of this Association was held during the second week in April, at the rooms of the Academy of Natural Sciences in Philadelphia. The following is an abstract of the proceedings.

Monday, April 5th, 1841, 4 o'clock, P. M.—The Association met pursuant to the adjournment of last year. The regularly appointed presiding officers being absent, Prof. Henry D. Rogers was called to the chair. After the completion of some business arrangements, the Association adjourned until 10 o'clock, A. M. of Tuesday.

Tuesday, April 6th, 1841, 10 o'clock, A. M.—The Association met pursuant to adjournment. *Prof. Silliman* took the chair. *Dr. L. C. Beck* was appointed secretary. *Messrs. W. R. Johnson, Vanuxem, H. D. Rogers, Mather*, and *Locke*, were appointed a committee to report a plan of business for the meeting.

The subject of mineral manures having been referred at the last annual meeting, was proposed for discussion. Remarks were offered, and facts stated by *Mr. Martin H. Boyé, Drs. Charles T. Jackson, James B. Rogers, J. Locke*, and *Mr. B. Silliman, Jr.*

Mr. Boyé enquired whether the antacid powers of magnesia, and its effects on vegetation, had been noticed, as it exists in the dolomitic limestone.

Dr. Jackson inferred from his observations, that magnesia is injurious only when used in a caustic state, in the same manner as caustic lime is known to be injurious to vegetation by abstracting carbonic acid from the atmosphere, and from decomposing vegetable and animal matters. It also acts unfavorably in virtue of its hydraulic power; rendering in some cases, the soil very hard. He further stated, that when composted with peat and swamp muck it gained from these substances phosphoric acid, and thus became the means of conveying to wheat and other cereal grains the phosphate of magnesia, known to be always found in their ashes. Dr. Jackson further considered the combinations of lime with the organic acids of soils as deserving much attention. He had found subsoils to contain a larger quantity of crenates of lime than the soil, and that some streams in like manner contained a larger proportion of soluble crenates than others, and these former are most valuable for the purposes of irrigation. Dr. J. recommends the employment of a compost of lime, swamp muck or peat, and animal manure, and he attributes the beneficial effect of this in part to the evolution of ammonia consequent upon the decomposition of the organic matters.

The discussion then turned on the character of hydrated peroxide of iron on vegetation. It was thought by Dr. Jackson, that the injurious effects sometimes known to arise from it were to be attributed to free sulphuric acid contained in it from the decomposition of sulphuret of iron. A marl was cited which at first produced very luxuriant vegetation, but at a subsequent period was found to destroy the plants growing where it had been used, owing to the decomposition of sulphuret of iron contained in it, producing free sulphuric acid, which corroded the plants. *Prof. H. D. Rogers*, thought that some of the marls of *New Jersey*, contained so much sulphuret of iron as to require more alkaline matter than was to be found in them to neutralize the acid resulting from their decomposition. Still it was thought that *small quantities* of sulphuret of iron, in a marl, would by its decomposition be useful in agriculture.

The subject of potash in soils was next brought before the meeting. *Dr. Jackson* inquired if any experiments had been made on this subject by gentlemen present. He had digested soils from Maine, New Hampshire, and Rhode Island, with boiling water, without discovering more than a trace of potash; while the method proposed by Mitscherlich of digesting the soils in free sulphuric acid, always gave decided indications of potash. He was led to infer, therefore, that the mica and other minerals containing potassa were by this method decomposed.

Mr. B. Silliman, Jr. stated, that the soil of the Nile, when treated according to the method of Mitscherlich, gave abundance of potash, but not any appreciable quantity with boiling water; he was therefore led to believe that the mica, contained abundantly in the soil, was decomposed by the sulphuric acid.

Resolved, That a committee be appointed to prepare a detailed report upon the subject of soils and mineral manures, embodying as well the fruits of their own investigations as the results arrived at by others, and that the same be presented at the next meeting.

Drs. C. T. Jackson, *Robert Rogers*, *Mr. M. Boyè*, *Dr. L. C. Beck*, *Dr. W. Horton*, *Mr. B. Silliman, Jun.*, and *Prof. Booth*, were appointed on the above committee.

The committee appointed to prepare a plan of business, made a report, which was adopted.

Prof. Mather asked for and obtained leave to defer his report on "Drift," until the next meeting of the Association; in the mean time he was requested to make an oral communication on this subject during the present meeting.

Prof. Locke read a paper "On the Geology of some parts of the United States west of the Allegany Mountains."

In this paper the author exhibited particularly the points of agreement between the lead region of the upper Mississippi, and that of Derbyshire in England, and between the mountain limestone of Europe and the "cliff limestone" of the west. He showed that the two rocks agree in geological position, in external and chemical characters, in fossil remains, and in metallic veins; being both highly metalliferous and abounding in lead and zinc ores occupying vertical fissures. He dèscribed the upper, middle, and lower beds of the "cliff limestone" of the lead region of the west as differing somewhat in characters and in fossil remains, and suggested the inquiry whether these three beds, together with the blue fossiliferous limestone which underlies them, (the probable equivalent of the Trenton limestone,) and the alternations of the lower magnesian limestone with the saccharoid sandstone, found at Prairie du Chien, should be considered distinct formations, (as their fossil remains would to some extent indicate,) or as different members of one formation—the mountain limestone.

In reply to some remarks by *Prof. H. D. Rogers*, *Prof. Locke* observed, that he did not undertake to be the advocate of absolute equivalency, but merely to point out the agreement and disagreement of certain formations in America with similar ones in Europe. He was of opinion, however, that certain points of equivalency must be admitted, as for example granite, the great coal formations, &c.

Prof. Mather proposed the subject of "Joints of Rocks" for discussion during this session of the Association; and *Prof. H. D. Rogers* proposed that of "Fossil and Recent Infusoria."

The Association then adjourned until 4 o'clock this afternoon.

April 6th, 1841, 4 o'clock, P. M.—The Association met pursuant to adjournment, *Prof. Locke* in the chair.

Mr. William C. Redfield exhibited specimens of fossil shells, from the tertiary marl-beds at Washington, Beaufort county, North Carolina.

Mr. R. stated that these beds, which are about sixty miles from the Atlantic, are found from fifteen to twenty feet below the adjacent surface, and two or more feet lower than the usual level of Pamlico river and sound. The fossils are in a good state of preservation, and are supposed to belong to the miocene period.

Prof. Locke read a paper "on a new species of Trilobite, found at Cincinnati, Ohio," and called by him Isotelus maximus.

This species is characterized by its elliptical terminations, and by a thorn-like process about one tenth of the length of the animal, projecting backwards from each angle of the shield, similar to an Ogygia. He exhibited casts of one entire specimen, nine and three fourths inches in length, and of a fragment of another of double that size in linear dimensions, which of course must have been nineteen and a half inches long—the largest specimen hitherto known to have been found.

Dr. Jackson stated that trilobites had been found in the limestone at the mouth of St. Croix river. He then exhibited the following specimens of minerals and fossils, viz.

Fossils from the limestone belonging to the red sandstone group of Machias, Maine. A new mineral from Unity, New Hampshire, which he has analyzed and proposes to describe under the name of chlorophyllite; it was remarkable as containing a large amount of phosphoric acid. A new mineral from Natick, Rhode Island, described by him under the name of Masonite. Tin ore from Jackson, New Hampshire, near the celebrated gorge of the White mountains. Phosphuret of copper and iron mixed with tremolite, from the town of Warren, N. H.; the mass yields from 6 to 12 per cent. of metallic copper. Recent bituminous coal from the vicinity of Newfield, Maine, taken from a peat bed. New red sandstone from Tobig river, in New Brunswick, containing about one half its weight of gypsum. Syphonia, a fossil-like substance with tubulæ running through them and assuming various forms. They were supposed to be concretions formed around twigs and roots of trees or other organic matter.

Dr. L. C. Beck read a paper "On the Sulphur Springs of the State of New York."

In this paper the author noticed, 1st. The geographical range of these springs, their geological positions and associations. Under this head it was stated that they are found in almost every formation, from the slates of the Hudson river to the shales of Erie and Chatauque county, having a range over nearly the whole state, and being found in almost every county. 2d. The amount of gaseous matter evolved by these springs. This cannot be correctly ascertained, but from many facts stated by the author, there can be no doubt of its vast quantity. Some instances were mentioned in which large streams and ponds were impregnated with sulphuretted hydrogen. It was also remarked, that independently of the amount of gas which is held in solution by the waters of these springs, there is often a flow of gas which seems to be undissolved or uncombined. 3d. Some facts were stated in regard to the uniformity in the composition of these springs. In all cases in which they have been examined, they contain, in addition to the sulphuretted hydrogen, a small proportion of carbonic acid. The solid matters are almost invariably sulphates of lime and magnesia, with smaller proportions of carbonate of lime, and occasionally sulphate of soda. It was especially observed that sulphate of iron is very rarely found among the solid ingredients of these waters. Common salt is often found in the sulphur springs which occur in the vicinity of the Onondago brine springs. 4th. The author next adverted to the observations which had been made in regard to the temperature of the New York sulphur springs. Although these have not yet been very extensively conducted, those which have been made seem to warrant the inference that the temperature of these springs is somewhat higher (say 1° to 3°) than that of the mean temperature of the localities in which they are found.

The author then proceeded to examine the theories which have been proposed to account for the formation of sulphur springs. The decomposition of iron pyrites, often assigned as a cause, was objected to on the ground that it was not sufficiently general—that it did not meet those cases in which these springs are found in the older rocks—that from what is known concerning the decomposition of iron pyrites, it seems to be in-adequate to account for the enormous quantity of sulphuretted hydrogen which is evolved, and lastly, that the almost entire absence of sulphate of iron in the New York sulphur springs, is irreconcilable with this theory.

The author then noticed the two general theories which have been proposed in regard to the origin of these springs, and gave the preference to the chemical theory, or that which attributes them, as the products of the great volcanic focus, to a chemical agency, as most consistent with the facts hitherto observed in the State of New York. He proposed, however, to extend the chemical theory so as to include the action of water upon the sulphurets of the bases of the alkalies and alkaline earths assumed to exist in the interior of the earth.

The Association then adjourned until 10 o'clock, Wednesday morning.

Wednesday, April 7th, 1841, 10 o'clock, A. M.—The Association met pursuant to adjournment. *Prof. Silliman* in the chair.

The subject of sulphur springs was discussed by *Messrs. H. D. Rogers, Locke, M. H. Boyè, Johnson* and *Mather.* *Mr. Vanuxem* announced his intention of presenting his views in regard to the New York sulphur springs at the next meeting of the association.

Prof. Hubbard presented a specimen of the slate found at Waterville, Maine, containing impressions, which in the Geological Report of Maine, were described as resembling ferns and fuci, which they resembled more than any thing else that had been found at that time, and of course an error in regard to their nature was unavoidable. Having received Murchison's Silurian system about two years since, Prof. H. found that the impressions were true Annelides and belong to the two genera Myrianites and Nereites figured in that work; thus carrying the occurrence of organic life in the New England rocks, one step lower than had heretofore been observed, and showing a coincidence between the Waterville slate and the slate containing the Annelides described by Murchison and included by him among the Cambrian rocks.

Dr. Jackson observed that he had received information from other gentlemen, that impressions of ferns occurred in the Waterville slate and had stated this in his first annual report of the geology of Maine. He had, however, subsequently visited the locality and satisfactorily ascertained that the slates of Waterville do not belong to the coal formation, and this fact was stated by him in his second report. Dr. J. remarked, that in justice to the gentlemen referred to, it should be observed that Prof. Sedgwick and

Mr. Murchison's report was not then published, and consequently these fossils could not be identified with the Annelides there described. On seeing this work Dr. J. had been able so to identify them, and he now concurred in the views expressed concerning them by Prof. Hubbard.

Prof. Mather stated that he had found an entire analogy in the fossils of the slates on the Hudson river, in Rensselaer and Saratoga counties, and in the western parts of the state.

Mr. Vanuxem confirmed the statements of Mr. Mather in regard to the identity of these slates.

Mr. W. C. Redfield made some observations concerning the fossils in the flagging slates employed in the city of New York. These slates are generally obtained from the counties of Greene and Ulster, N. Y. He referred to the corner of Cedar street and Broadway, and to the walk in front of the Spring street church, near the Hudson, as exhibitions of these impressions.

Prof. H. D. Rogers observed, that the pavement in front of the United States Bank, (Philadelphia,) afforded a similar exhibition.

Dr. Jackson now offered some general remarks upon the geology of the states of Maine and New Hampshire.

At 12 o'clock the Association adjourned, as a mark of respect to the memory of General Harrison, late President of the United States, whose funeral took place at this hour.

April 7th, 1 o'clock, P. M.—The Association met, *Prof. Silliman* in the chair. After the transaction of some ordinary business,

Mr. W. C. Redfield laid on the table sundry specimens of fossil fishes found in the red sandstone formations of Connecticut, Massachusetts, and New Jersey.*

Of eight species from these formations comprised in the collection, five species are found to belong to the genus *Paleoniscus,* and three species to the genus *Catopterus.* It is remarkable that nearly all of these several species are common to most of the known localities of these fossils in the above mentioned states. The importance of this fact, as aiding to establish the cotemporaneous character of these formations, induced Mr. R. to place this collection before the Association.

Mr. R. stated that the lithological appearances of the shales in which the fossil fishes are found, as well as of the more minute and undeter-

* See Mr. Redfield's paper, published at length in the present number of this Journal.

mined fossils which they contain, are nearly alike in all the localities which he has visited in the above mentioned states. Slight contortions of the strata with small faults or dislocations, which in some cases affect the fossil specimens, are also common to the several localities, and seem to be referable to like causes.

In addition to the above, Mr. R. also exhibited specimens of a new species of *Catopterus* from the rocks which overlie the coal mines in Chesterfield county, Virginia.

Some remarks upon the elevation of trap dykes were made by the chairman, and *Profs. H. D. Rogers*, *Mather*, and *Hitchcock*. The Association then adjourned until 4 o'clock.

April 7th, 4 o'clock, P. M.—The Association met, *Prof. Silliman* in the chair.

Mr. Vanuxem, from a committee appointed at the last meeting of the Association, presented a report in regard to the "Ornithichnites or foot-marks of extinct birds in the new red sandstone of Massachusetts and Connecticut," observed and described by Prof. Hitchcock. This report confirms the opinion respecting these appearances now entertained by Prof. Hitchcock.

Report on the Ornithichnites or Foot Marks of Extinct Birds, in the new red Sandstone of Massachusetts and Connecticut, observed and described by Prof. Hitchcock, of Amherst.

The undersigned, forming the committee to whom the subject of the origin of the bird-tracks of Prof. Hitchcock was assigned, beg leave to present the following brief report.

It may be well previously to state, that the object of the meeting in appointing this committee, was founded solely upon the desire to produce, if possible, unanimity of opinion, there being a few of the members who dissented from the views, published by Prof. Hitchcock. In our country, the subject, as it undoubtedly ought, had attracted considerable attention. It had been very favorably received and republished in Europe, and from its great importance to Palæozoic geology, an attempt should be made to settle the question; for were the views of our highly respected member correct, we were made acquainted with the earliest period in which biped animals existed whose foot-marks were analogous to, if not identical with, those of the tread of birds. On the contrary, if wrong, we were presented with another class of facts, which show that certain appearances supposed to belong solely to animal life, were held and presented by the vegetable kingdom likewise.

We shall now state, in a few words, what we suppose are the general facts upon which Prof. Hitchcock's views were founded, and then the facts of those who assumed the opposite opinion.

The first and most obvious impression upon the mind, on looking at the indentations or marks, is their thin tripartite form, resembling the tread or foot-mark of those kinds of birds which show three toes, the fourth one being rudimental, and are referable to no other known kind of animal. The tracks or foot-marks in several localities are arranged in a determinate order, like those of a bird or fowl moving in a straight line, the toes or marks in all such cases being alternate; that is, if the right foot be presented on the rock, the left would next follow, and thus right and left in regular succession, sometimes with many repetitions. In other instances, the foot-marks presented no determinate direction or order, as might naturally be supposed of a bird or any other animal having no particular place or object in view.

In all cases where a succession of tracks was observed, there was an uniform correspondence as to size, and considerable regularity as to distance between the tracks. Whatever deviations were observed, they were not greater than might be supposed to take place in animals possessed of voluntary motion.

On some surfaces, not unfrequently one or more different kinds of track were exposed, belonging, as was reasonably conjectured, to different species and genera of ornithichnites.

That the slaty material of the rock showed that the impressing body possessed force or weight, for frequently the thin layers or laminæ were bent downwards for an inch or more, and that the mud of which the slate was formed was of a highly adhesive or tenacious character.

In all cases the foot-marks or part impressed, was the fixed part of the rock; the part removed when the lower side was turned upwards, shewed the cast or what corresponded with the toes or foot. That no trace of any organic matter could be perceived occupying the cavity or mould, the cast or part in relief being in all respects like the material of the rock of which it formed a part.

Finally, that the foot-marks belonged to a group of rocks which must be considered to have been produced by the same general causes which gave rise to the new red sandstone of Europe, and referable only to that sandstone. This sandstone presents foot-marks in many localties, though comparatively but a few years have elapsed since attention has been called to them. Some of the specimens have reached this country, and had they not, the information is well given by Dr. Buckland in his Bridgewater Treatise. The most remarkable of these foot-marks, is that of the chirotherium from the quarries of Hesberg, near Hildburghausen in Saxony, and greatly resembles a fleshy human hand. These, in the drawing and in the specimen which we have seen, are alternately right and left. Other foot-marks have been observed by Mr. Linse in the same sandstone, having made out four species of animals, some of which are conjectured to belong to gigantic Batrachians. Near Dumfries, the foot-marks of animals, probaly tortoises, were obtained from the same sandstone, but as yet no tracks like those of New England have been discovered.

The facts, &c. which led to a different conclusion are these. First, that the forms assumed by fucoidal plants were numerous and imitative, some resembling the tail of a rooster, the cauda galli; another, which was like unto a large claw or paw, and which may have been a Lusus naturæ, and the two specimens on the table of the Association, which present in relief a distinct tripartite form. These, as they all appertain to rocks of great antiquity in comparison with those of New England, it appeared more reasonable to believe that there might be resemblances as perfect, as the fossils with a tripartite character were approximations to the forms in question.

That no trace of organic matter could be discovered by the eye in the greater number of the fucoides. In some, such as the Harlani, they have been seen to be made up of small pebbles, presenting no little difficulty, not to the manner only in which the organic matter was replaced, the external form being complete, but the nature of this material, which could make so definite an impression and preserve its form entire.

There were other facts which showed resemblances, such as that the part in relief was the part removed when the fucoides was attached to the sandstone at its upper part. It may also be stated, that the appendages to the heel of some of the New England tracks, might have been caused by a bird whose legs were feathered, but not to a wader, and they favored their vegetable origin, for the appendages might readily be conceived to be either leaves or radicals, or both.

From a comparative examination of the facts on both sides, your committee unanimously believe, that the evidence entirely favors the views of Prof. Hitchcock, and should regret that a difference had existed, if they did not feel assured it would lead to greater stability of opinion. To liken things to what we know is the nature of mind, the error from this tendency increases with ignorance, and diminishes as knowledge increases, so that he that knoweth all things, as is self-evident, can commit no error when following this instinct of his being. The discoveries of Prof. Hitchcock were published at a period when the mind of those who embraced the negative side of the subject was preoccupied with the anomalous vegetation with which many of the Silurian rocks of New York abound, and to which provisionally the name of fucoides had been given. From this imitative character, and from finding a few specimens presenting a tripartite or trifurcate form, &c. it appeared not only possible but probable, that the impressions from Massachusetts and Connecticut, were with greater propriety referable to fucoidal bodies, than to those which Prof. Hitchcock had assigned them.

We may here remark how essential it is that truth or the facts which make manifest any truth, should first be presented to us; so readily is the mind impressed when not preoccupied, and when a strong impression is made, be it ever so false, it is no easy matter to free ourselves from it. From this circumstance we can readily foresee the advantage which future generations will possess over those of the present and especially those of former times. As the progress of knowledge is certain, each day will lessen error and enlarge the domains of truth, and should man be true to his permanent interests, error finally will cease to have existence.

Signed, Henry D. Rogers, Lardner Vanuxem, Richard C. Taylor, Ebenezer Emmons, T. A. Conrad.

Mr. Vanuxem read a paper "On the Ancient Oyster Shell Deposits observed near the Altantic coast of the United States."

Among the unsettled subjects of geology in our country, is the origin of the deposits of oyster shells, (*Ostrea Virginica,*) observed in many parts of the Atlantic seaboard, of which a few only of those near South Amboy have come under our notice. But the greater number of those of the largest dimensions are in the waters of the Chesapeake. Some of these southern deposits of shells are enormous, covering, it is said, acres of ground, adding no small weight to the truth of that belief that considers them *in situ,* as ancient oyster beds, raised from their original position by the uplifting of our coast, of which the fact of their generally holding, if not a real, an apparent similarity of level would seem to be ample confirmation. This theory of their being in place, is highly satisfactory, being in accordance with the less modern deposits beneath them, adding one more to the number of elevating movements to which our coast has been subjected, thus mutually confirming each other, making the certainty of these movements sure.

When the nature of their origin was advanced by Mr. Conrad, I confessed a decided bias; for I knew not the facts upon which Dr. Ducatel, the geologist of Maryland, maintained the opposite one. None were known to me adverse to the views of Mr. Conrad, for the history of our country afforded no light that could be recollected, either as to the origin of these oyster shell deposits, or to any extraordinary manifestation of gastromic power in the aborigines, in respect of this article of diet, which would lead me to infer their existence, and which the magnitude of some of the deposits required.

The eastern shore of Maryland presents many deposits of these oyster shells, hitherto until recently, unused and little examined, so far as knowledge has been received this way. Now, as many of the planters in that section of the country are waking from the deep slumber of the past, and turning their attention to the all-important subject of improving their lands by the use of lime, a few of these deposits have become the subject of investigation, furnishing facts, which, were the same discovered elsewhere, would settle the question of their origin, and in favor of the Maryland geologist.

At the mouth of Pickawaxent Creek, about eighty miles below Washington, there is an extensive deposit of oyster shells, at which an establishment has been formed, which, in a few months has converted many thousand bushels of them into lime. Before any excavation of the mass was commenced, I had directed the attention of Mr. Downing—one of the partners concerned—to the doubtful nature of their origin, requesting that all facts tending to throw light upon them should be carefully observed and preserved. When Mr. D. first went into the country, he was in favor of the views of Mr. Conrad ; it was only by the examination of the mass at the Pickawaxent, of another not remote from that one, and from subsequent observation in the city of Baltimore, showing the amount of shells which there accumulates, was he assured that their origin was to be referred to man, and not to other or more elementary powers of nature.

The first and most important fact there observed, was that neither he nor any of the hands employed in getting out the shells had been able to find any two valves which fitted each other, excepting in one instance ; a waterman having brought the specimen to him. The deposits having the nature of a mass or heap composed of shells whose valves were separated before being thrown together.

That in many parts of the mass arrow-heads and fragments of pottery have been found in the progress of excavation—these in no wise different from those found in old settlements of the Indians.

That the bottom of the bed is formed of the yellow loam or soil of the country, and that the roots and other parts of the cedar of the country have been met with at the bottom of the bed, showing a growth upon the surface, before the shells were deposited upon it.

That these deposits are at the mouths of the creeks, extending up the creeks, and rarely extending along the river shore, owing, as Mr. Downing conjectures, to the excellent fishing which the creeks furnish, and which would give to those who accumulated the shells, a twofold advantage.

That the shore is low on that side of the river where they are found, and the recent oyster in great abundance on that shore, whilst the channel is on the Virginia side, and no deposit of oyster shells existed in that section of country.

That these deposits are of some comparative antiquity, is to be inferred from the soil which is found upon them, and the existence of an exceedingly old cedar growing upon the top of a mass, and from the silence of history or tradition respecting them.

Against these facts which show an undoubted human origin for these deposits of oyster shells, there are others cited by Mr. Conrad which he has made me acquainted with, since this paper was written, which either I had not known or they had escaped my memory, and are equally conclusive as to the opposite opinion. The facts are, that masses exist composed of whole shells, as at Easton, on the eastern shore of Maryland. That in some localities fragments of older fossils are found with them, and which must have been thrown amongst the oysters by the waves of the estuary from their position below. And again that deposits of the shells exist in situations too remote from present oyster beds to have been removed by human agency, such as those in Cumberland county, in New Jersey; therefore it would appear that both causes have operated to produce them, and that no single generalization can comply with the requisition of the facts which they present, leading as they do to a twofold, one from opposite conclusions, one referable to human, the other, to natural causes, and that severally they must be examined in order to ascertain to which of the two causes any given mass is to be referred.*

LARDNER VANUXEM.

Prof. Booth stated that his observations upon these deposits had led him to the same conclusion as that which had been arrived at by Mr. Vanuxem, viz. "that they are sometimes referable to human agency, and at others to natural causes." In answer to an inquiry, Prof. B. observed that these shells reduced to powder had been used with great success in the State of Delaware, as a manure. *Prof. Hitchcock* stated that the fertilizing powers of these deposits of shells had also been tested by experiments on Cape Cod. Facts were stated by the chairman and *Prof. Mather* in regard to beds of oyster shells similar to those described by *Mr. Vanuxem*, on the Island of Nantucket, and on Long Island.

Prof. Bailey commenced his account of "Fossil Infusoria," by an exhibition and description of the microscope employed by him in his researches.

Messrs. Charles B. Trego and *B. Silliman, Jun.* were appointed assistant secretaries. The Association then adjourned until 9 o'clock, Thursday morning.

Third day of meeting, Thursday, April 8, 1841.—The Association met at 9 o'clock, A. M. *Prof. Silliman* in the chair. The minutes of yesterday were read and adopted.

---

* Since the meeting of the Association, I have found on conversing with Dr. Ducatel that the impressions which I had of his views were founded upon his first report, that of 1834, for in his subsequent ones, he makes known their twofold origin. We should withdraw this paper did we not believe that it would be of service; for it not only settles the point in question, which was its object, but it affords a useful lesson as to caution in an expression of the opinions of others.

Peter A. Browne, Esq. laid on the table, for the inspection of the members, a suite of specimens, chiefly fossils, from the chalk basin of Paris, collected and labelled by A. Brongniart.

Prof. Locke made some observations concerning the connection of magnetism with geology, mentioning an instance where he found an increase of the dip and intensity as he approached, from south to north, a certain point or meridian line, and a decrease as he receded from it; also remarking that a similar change is found upon crossing the Ohio river: querying from this, whether the water of large streams running east and west, has an influence on the magnetic relation.

Dr. Houghton remarked, that in the vicinity of the great northwestern lakes a change in the magnetic deflection was frequently found on approaching within a few miles of a large body of water.

Dr. R. E. Rogers called the attention of the Association to the subject of limestones, observing that he thought the magnesian character of these rocks generally had not received sufficient attention. He stated that he had found, upon analyzing some of the lower limestones of Pennsylvania, a larger proportion of magnesia than is requisite for the formation of a true dolomite, and threw out the query as a point of scientific interest, whether the carbonate of lime and carbonate of magnesia were chemically combined in the proportions to form dolomite, and this mingled throughout the excess of the carbonate which might be present, or whether the two carbonates were mechanically and uniformly intermingled.

Dr. Jackson stated, that he considered the granular or crystallized dolomite to be a regular chemical double salt, consisting of one equivalent of carbonate of lime and one equivalent of carbonate of magnesia. But he had never found any magnesian limestone to contain *more* than this proportion of magnesia, although he had frequently analyzed limestones containing a *less* proportion than one equivalent of magnesia. His published analyses will illustrate this remark.

Dr. J. inquired whether Dr. Rogers had ascertained if the limestones to which he alluded did not contain the hydrate or silicate of magnesia, mixed with dolomite. If the rock was of the compact variety, this might have been the case.

Dr. James Rogers thought we must consider dolomite as a true double salt: 1 atom carbonate of lime + 1 atom carbonate of magnesia; the excess of magnesia found in our limestones must be considered a mechanical mixture.

A communication was received from *Peter A. Browne, Esq.*, expressing a willingness to read before the Association "an Essay on Aërolites or Meteoric Stones," at the next annual session. Laid upon the table.

Prof. Mather made a verbal communication on the joints in rocks, particularly as they occur in the primary, transition and secondary of this country. He found two principal sets of joints prevailing; the first had a general direction of north by northeast, the second set were nearly perpendicular to the former—besides these, there were two other sets not so well defined. The joints in the primary were not so smooth and well marked as in other formations; this observation was not intended to apply to the joints of slate rocks.

Dr. Jackson cited the joints or fractures of the conglomerate around Boston, and particularly at Roxbury, Mass., and also in the island of Rhode Island, at a place called Purgatory, the large pebbles are broken by these fractures, without dislocation or loosening from their beds. He supposed the parallel and uniform cracks in the lime rocks and slates of that country to have connection with the different epochs of irruption of the trap, granite and porphyry.

Dr. Douglass Houghton inquired of Dr. Jackson if these cracks in the conglomerate had reference to the line of bearing, remarking that in Michigan they were nearly at right angles to the line of the longer diameter of the pebbles. Dr. Jackson replied that such was the case in the cases he had cited; that at Purgatory the pebbles were very large, ovate, and arranged with their longer diameters in one direction, and seemed to be joined together by very little cement, and yet they were broken at right angles to their longer diameter, without dislocation. He stated that Mr. A. A. Hayes had found that chloride of calcium would concrete pebbles of quartz into a firm mass—this fact might elucidate the present subject. Specular iron was generally observable among the interstices of the pebbles at Purgatory, and more or less of iron and lead ore was generally to be found at the juncture and fissures of the trap dykes.

Prof. Hitchcock thought that the steps of the new red sandstone of the Connecticut valley were the result of the fractures referred to by Prof. Mather—they were nearly coincident with the strike of the strata, as if caused by elevatory movements. He found difficulty in distinguishing between fissures produced by mechanical violence and joints properly so called; he viewed those of the conglomerate as mechanical, those of the slates as chemical. Two cases occurred to him as worthy of notice; the first was of a dyke of greenstone crossed by parallel transverse planes two or three feet apart and at right angles to the strike of the vein. The second case seemed to throw some light on the origin of this class of phenomena; it occurred in a bed of the common blue diluvial clay—the horizontal layers were unmoved, but some of them were divided into double rhombs. The experiments of Mr. Fox, of England, in the lamination of clay by galvanism, seem to explain this structure.

B. Silliman, Jr. had found this rhombic structure in great perfection in the argillaceous sandstone of the Connecticut valley at Hartford, in Connecticut, where this variety of sandstone is used for flagstone. Many of the joints parallel to this rhombic structure are filled with carbonate of lime.

Dr. C. T. Jackson stated that the great trap dykes of Nova Scotia had the perpendicular columnar structure in a high degree—in the smaller dykes this structure prevails from side to side of the dyke, perpendicular to the walls. He thought that in all cases these phenomena were referable to the way in which the dykes cooled—the structure being perpendicular to the cooling surface—thus the narrow dykes cooled from side to side, and the heavy ones from the upper surface downward.

Prof. Henry D. Rogers remarked, that the trap dykes of Pennsylvania and the magnetic iron ore of New Jersey were abundantly characterized by the columnal structure. He viewed the horizontal columnar structure of the magnetic iron ore, as a very important indication of its igneous origin.

This discussion was here suspended to give an opportunity for Prof. Bailey to read his paper on recent and fossil infusoriæ.*

---

* As this paper of Prof. Bailey is to appear in full, with plates, in the October No. of this Journal, it has been thought unnecessary to attempt here any abstract of his remarks before the Association.—B. S. Jr.

Dr. Jackson stated that the mass of infusorial deposit found under peat bogs is hydrate of silica, which loses by being heated to redness from 12 to 15 per cent., principally vegetable matter. Great abundance of this material occurs at Newfield, in Maine, where it covers many hundred acres, and is five or six feet thick. After burning, it is so white and beautiful that it has been fraudulently sold for magnesia alba. The ammonia which is evolved in its destructive distillation, is probably derived from the crenic and apocrenic acids which it contains. Phosphate of lime and manganese are found in it in small quantities. As a fertilizer of land, it is considered of use when containing in large quantity the juices of plants.

A memoir from M. Alexandre Vattemare was presented, proposing a general system of exchange of objects of nature and art among all nations. It was by motion laid on the table.

The subject of bowlders and diluvial scratches was then brought up for discussion by Prof. Mather, and a protracted debate ensued, in which many of the members joined.

Prof. Mather inferred from the facts in the case, that the bowlders and diluvial scratches had, in general, come from the north; those on the east of the Hudson from the northwest, those on the west from the northeast, as by the result of two forces.

The diluvial furrows are, in general, parallel to the valleys in which they are found—thus in the small transverse valleys, the scratches are found parallel to the direction of the valleys—and not coincident with those of the main valleys. All the bowlders seem to have been brought from the northwest, both at the east and beyond the river St. Peter's at the west, and very few are found below 38° or 39° of north latitude.

The chairman (*Prof. Silliman*) cited the recorded observations of Mr. C. Darwin, naturalist to H. M. ship Beagle, that in South America no bowlders occur nearer the equator than about 40° south latitude.

Prof. Mather had not seen any bowlders in the coal region of Ohio, and very few in Kentucky. He thought that the bowlders mentioned by Mr. Hodge in the gold region of North Carolina, were not transported masses, but were composed of granite which had suffered decomposition *in situ* by atmospheric agency.

Prof. Henry D. Rogers said there was need of much caution in the use of the term bowlder, as regards the size of the mass to which it should be restricted; he was inclined to give the term much latitude. Thus he conceived that a current of drift coming from the north and meeting the terraces of Pennsylvania, would there be arrested and deposit its larger masses—and so from stage to stage, until the onward current would carry forward only the smallest sand; in this way, we may find among the drift of the south, all the materials derived from the northern rocks.

He concluded that all the materials of a current of drift, find their resting place in accordance with gravity.

Prof. Mather doubted whether the large bowlders found in Long Island, resting on beds of sand or fine gravel, could be thus accounted for, because a current of sufficient force to move such large masses would have carried away the sand.

Prof. Rogers replied, that diluvial action could not be restricted to a single epoch.

We must find in secular and periodical elevation, the cause of the translation of the beds of infusorial earth recently found in the tertiary of Virginia, which are there covered by the quiet strata of the Meiocene. We have evidence of numerous slight elevatory movements on the eastern coast of North America, and the various terraces of our rivers seem to present the same phenomena; for the source of these elevatory movements we must look to the great volcanic foci of Greenland.

Prof. Locke mentioned a locality in Ohio, at which the limestone is ground down to a perfect plane, as if it had been done by a stone-cutter by grinding one stone on another, over an extent of ten acres. Upon this planished surface, lines have been engraved in systems perfectly straight and parallel, running from northwest to southeast. Some of these lines are fine, as if cut with the point of a diamond, and others perhaps half an inch broad, and one eighth of an inch deep, scaled rough in the bottom, as if they had been ploughed by an iron chisel properly set and carried forward with an irresistible force. Prof. L. inferred from the facts of the exact straightness and parallelism of these lines, that they had been formed by a body of immense weight, moving with a momentum scarcely affected by the resistance offered by the cutting of the grooves. Such a momentum and actions would be supplied by a floating iceberg, whose lower surface should present projecting sharp points of imbedded bowlders.

Prof. Mather further stated, that the bowlders of Ohio were in continuous lines and groups, and not scattered promiscuously. On the river St. Peter's, the bowlders may be seen extending for miles, as along a coast line; in some situations one might see them bounding the horizon as far as the eye could reach.

Dr. Locke, in conformity with Lt. Mather's statements, mentioned a region of bowlders in Ohio, extending from the town of Eaton quite across the state; five miles in width and over forty in extent.

Dr. C. T. Jackson remarked, that the phenomena of diluvial currents were well exhibited in the vicinity of Providence, at Cumberland, R. I. A large mass or mountain of porphyritic titaniferous iron of very peculiar character exists in that place; to the north of it no bowlders are to be found, but on the south, huge bowlders of it may be seen, and so abundant that the stone walls are built of them; and below, at Papoose Squash Neck, small bowlders of the same characteristic rock are found; south of Newport, and still further south, the same are met with of a smaller size, the whole extending from north to south forty miles, and from six to fifteen in width, diverging to the south.

The characteristic macle rock at Lancaster, Mass., presents similar phenomena, being found in loose masses to the south as far as Bolton, while none can be found to the north of the locality.

He considered the power of the diluvial currrent greater to the north than at the south, since the evidences of it in Maine are much greater than in Rhode Island; bowlders have been found on Mount Katadin as high up as four thousand feet; he thought there was no evidence of any elevation of the rocks after the diluvial current had passed.

Mr. Nicollet proposed, at a future meeting, to make some remarks upon, and to exhibit specimens from, the cretaceous formation on the upper Missouri.

Mr. Redfield expressed a wish that the attention of the Association should, at some convenient time, be called to the recent sand formation along the eastern coast of the United States.

Adjourned to meet this afternoon at 4 o'clock, at the rooms of *Mr. Richard C. Taylor*, for the purpose of viewing a model of the coal region of Dauphin and Lebanon counties, east of the Susquehanna.

Thursday, 4 o'clock, P. M.—The Association assembled at the rooms of Mr. Taylor, where that gentleman exhibited a highly interesting model in plaster of the Dauphin and Lebanon coal region, embracing, altogether, an area of seven hundred and twenty square miles, showing the range of the mountain elevations, with their relative height and position; also their elevation above tide level; the dip of the rocks, the position of the coal seams, and much other useful information.

Mr. Taylor accompanied this exhibition with remarks explanatory and statistical, in relation to this coal region, and made some observations on the importance of this mode of exhibiting the geological features of a country, expressing the hope that the day would come when models of this kind, representing the several states, and even the whole United States, shall be constructed. He also enlarged upon the propriety of following, as closely as possible, the actual conformation of the country in drawing sections, and of adopting uniform modes of illustration by colors, &c., and the importance of an equal scale of extension and elevation as far as practicable in such sections.*

Prof. H. D. Rogers followed with observations upon the Pennsylvania coal formations and the range of their underlying rocks, detailing what he conceived to be the cause of the inverted dip observable along the southern border of the Kittatiny series, ascribing it to a great force acting laterally, and folding and crushing the axes so as to produce this inverted dip by tossing the strata many degrees beyond the perpendicular, and thus producing the present apparent dip of the lower stratified or sedimentary rocks *beneath* the primary.

Adjourned to half past nine o'clock to-morrow morning.

In the evening, the members of the Association had the pleasure, in common with a number of citizens, of listening to a very interesting and appropriate address from Prof. Hitchcock, embracing all the points at present most interesting to the American geologist.†

---

* See Mr. Taylor's article published entire, with a colored section, in the present number of this Journal.—B. S. Jr.

† As this address is to be published entire by the Association, and it is intended to give an abstract of it in the next number of this Journal, no farther notice of it is inserted here.—B. S. Jr.

Fourth day of session, Friday, April 9, 1841.—The Association met, pursuant to adjournment, at half past nine o'clock, A. M. *Prof. Silliman* in the chair.

After the minutes of yesterday had been read and adopted, *Dr. Beck* moved a series of resolutions, of which the first was adopted, as follows:

Resolved, That the thanks of the Association be presented to Professor Hitchcock for the interesting and valuable address delivered last evening; and that a copy of the same be requested for publication.

The committee on business reported the following resolutions, which were all adopted excepting the first,—it being laid on the table.

Resolved, 1. That the committee recommend to the Association the first Monday of May as the period for the next annual meeting.

2. That the Association adjourn its present annual session this week.

3. That a committee of five be appointed to draft a constitution and by-laws for the regulation of future proceedings of the Association, and that each member of the committee be recommended to draft a plan of organization, to be discussed by the committee.

4. That at each meeting a local committee of three members, resident at the place of the next annual meeting, be appointed, for the purpose of making arrangements for the reception of the Association.

5. That the members of the "Academy of Natural Sciences" be invited to attend the present session of the Association, and to participate in its proceedings.

Prof. Renwick, *Mr. Nuttall*, and *Dr. Hayden* of Baltimore, were recommended as members of the Association.

Dr. Harlan exhibited models of the fossil remains of the *Dinotherium giganteum.*

The first specimen presented to the view of the Association, was the cast of a small model of the *Dinotherium giganteum* or the great fossil *Tapir* of Cuvier—the only model of the kind, which, as far as Dr. Harlan is aware, has yet reached America. The Paris Garden of Plants, possesses a model of the skull of the size of nature, which is sold by the German naturalists, Messrs. Klipstein and Kaup, for $100. The dimensions of this skull are four feet in length, three feet in width, and two feet in height. In peculiarity of structure and colossal dimensions, the

Dinotherium constitutes one of the most curious and interesting animals of an antediluvian Fauna. M. Klipstein, Professor in the University of Giessen, a few years since, discovered a perfectly preserved specimen of the skull on the borders of the Rhine. Baron Cuvier had many years previously described, in his Fossil Animals, some remains of this animal as allied to the genus *Tapir*. The fragments subjected to his observation consisted only of two imperfect pieces of the lower jaw, and some molar teeth. From such data alone, he was able to represent them as belonging to two distinct species, *Dinotherium giganteum* and *D. Cuvieri*, and to estimate the size of the larger species at eighteen Paris feet, which was subsequently proved to be correct. In 1829, Mr. Kaup, director of the museum at Darmstadt, discovered and described numerous portions of this animal, all obtained from the same strata of the tertiary sand of Eppelsheim.

The whole animal creation, fossil or recent, presents no parallel to the structure of the lower jaw and tusks of this animal. The anterior portions are recurved downwards, and from which depend two enormous tusks, in a direction downwards and backwards. The upper jaw is destitute of incisors. The configuration of the anterior nares and their vicinity, demonstrates that the animal was supplied with a *proboscis*, and like the hippopotamus and tapir, the habits of the animal were evidently aquatic; and the peculiar arrangement of the tusks was evidently adapted to the nature of the animal's food and the means of attaining it—they would be very useful in unison with its powerful claws, in eradicating from the mud the thick and succulent roots of aquatic plants, which probably constituted its principal nourishment. A correct notion of the enormous dimensions of this animal may be obtained by a view of the models of the tusk after nature, as well as by a series of the molars of one side of the lower jaw. It evidently attained a size far exceeding that of the hippopotamus of our day.

The last or ungual phalange, presents so close analogy to that of the Manis or scaly ant-eater, that Cuvier, at first sight, referred this species to an animal of that genus, and named it *Manis gigantea*. In offering you my own views of this peculiar specimen of a departed type, it should be stated that various notions exist among different naturalists, as to the real nature and habits of the animal in question. Some German naturalists place it among the *Phocæ*. Blainville took it for a pachydermatous animal, closely allied to the elephant. Kaup considered that it might range as a fifth and last family of the class Edentata. Others referred it to the herbivorous Cetacea, &c. &c.

Dr. H. also made some observations upon the remains formerly described by him as belonging to the "Basilosaurus," but which he is now satisfied, from the microscopic examinations of a section of one of the teeth by Prof. Owen, should be referred to a genus of the aquatic mammalia, and which is now named "Zygodon," —specimens of the vertebræ of which, from the tertiary deposits of Alabama, he exhibited to the Association.

Mr. Nicollet then made some highly interesting remarks upon the geology of the region on the Upper Mississippi, and the cretaceous formation of the Upper Missouri.

He referred to his arrival in this country for the purpose of making a scientific tour, and with the view of contributing to the progressive increase of knowledge in the physical geography of North America. After spending several months in Philadelphia, Baltimore and Washington, he proceeded through the southern states; explored the south Allegany range, the states of South Carolina, Georgia, Kentucky, Mississippi, Alabama, Florida, Louisiana, Arkansas Territory, and Missouri; ascended the Red-River, Arkansas river, and to a great distance the Missouri river. Having thus made himself well acquainted with the lower half of the Mississippi, he undertook the full exploration of that celebrated stream, from its mouth to its very sources; the latter of which he succesfully reached near the close of the month of August, 1836. During five years of unremitted exertions, he took occasion to make numerous observations calculated to lay the foundation of the astronomical and physical geography of a large extent of country, and more especially of the great and interesting region between the Falls of St. Anthony and the sources of the Mississippi. With these labors was connected the study of the customs, habits, manners and languages of the several Indian nations, that occupy this vast region of country.

Mr. N. acknowledged, in feeling terms, the generous hospitality, on the part of our citizens generally, of the agents of the American Fur Company, the civil and military officers, as well as the kind protection of the government, extended to him on all occasions, so as greatly to facilitate his operations and second the accomplishment of his designs. At the expiration of this long and arduous journey, Mr. N., broken in health, and his means exhausted, returned to Baltimore, where he soon received a flattering invitation from the war department and topographical bureau to repair to Washington. The result of his travels having been made known to these departments and appreciated by them, he was intrusted with the command of a new expedition, to enable him to complete to the greatest advantage of the country, the scheme which he had himself projected in his first visit to the far west; namely, the construction of a map of the region explored by him. This map having been recently submitted to Congress, the senate of the United States has, unanimously, ordered its publication under the direction of the topographical bureau. It is to be accompanied by a report embracing an account of the physical geography of the country represented, together with the most prominent features in the geology and mineral resources of other sections of our western states not embraced within the limits of the map.

Mr. N. then went on to give a succinct account of his geological researches, which, modestly disclaiming any pretensions to be considered a professed geologist, he had felt an irresistible inclination to engage in, as a subject of general and growing interest. This account he offered as a more appropriate theme, in view of the objects contemplated by the present meeting.

Mr. N. said he had traced a magnesian limestone—the cliff limestone of Dr. Owen—which is probably referrible to the mountain limestone of European geologists, over a vast extent of country, within the valley of the Mississippi. Connecting his own researches with the facts brought to light in the survey of the Iowa and Wisconsin Territories by Dr. D. D. Owen and Prof. John Locke, and with the observations of Dr. Henry King, during an exploration of the country watered by the Osage river, Mr. N. thought himself warranted in assigning the Falls of St. Anthony on the Mississippi river, as the northern limit of this formation, which to the west, extends to Fort Leavenworth on the Missouri river, and to the south, embraces the metalliferous region of the state of Missouri. This limestone, containing *trilobites*, *catenipora*, and other coralline fossils, is the metalliferous rock not only in Missouri, but in Iowa and Wisconsin, from which the lead and copper ores are extracted. The rock intervening between it and the coal formation is characterized by the occurrence of the *Pentamerus oblongus*. In this relative position, also are found thin beds of oolitic limestone, that are perhaps referrible, geologically, to the oolitic limestone of Tennessee, described by Dr. Troost, who indicates the *pentremites* as their characteristic fossil; a large number of these fossils, in a loose state, was collected in the vicinity of these rocks. Shallow coal basins frequently occur in Missouri and the south part of Iowa Territory; but on the Mississippi river, the coal disappears, about thirty miles above St. Louis; thence, ascending the river as far as the great Platte river, the cliff limestone and the coal rocks present themselves in alternate succession. In the vicinity of the Platte river, as well as at Council Bluff, a limestone containing *cyathophylla* of large size, *encrinites*, and other fossils, appears in a position seemingly between the cliff limestone and the coal. Near the confluence of the Sioux river and the Missouri, there occurs a formation overlaid by a thick deposit of clay, containing, in abundance, several species of *ammonites* and *baculites*, *belemnites*, *inocerami*, &c. &c., beautifully raised on their exterior and sparry in their interior. Some of them were exhibited to the meeting. These fossils were identified with similar ones belonging to the green sand deposit of New Jersey, a member of the chalk series; but no true chalk or flint (silex pyromage) was observed. The occurrence of this formation had already been indicated, by some fossils that Lewis and Clark and Mr. Thomas Nuttall had brought along with them from their travels, and which were described by Dr. Morton. Mr. N. exhibited farther, some fossil bones which had been submitted to the inspection of Dr. Harlan, who describes them as belonging to *vertebræ* of a Squalus and of a nondescript crocodile, also articulated vertebræ of an animal referrible to the order *Enalio-sauri* of Conybeare. The surface presented by a transverse section of these vertebræ, Dr. H. thinks peculiar, as also the mode in which the ribs are attached to a small process in the middle of the inferior surface of each vertebra. From their size and unique character, it is quite probable that these vertebræ form a part of the skeleton of the *Sauro-cephalus lanciformis*, (Harlan,) an animal possessing still more of the fish than the lizard, than exists in the organization of the *ichthyosaurus*, in which respect these vertebræ correspond. According to Dr. Harlan, similar fossils have been found in the green sand of New Jersey and in the chalk of England.

Mr. Nicollet concluded by remarking, that he had followed up and described this formation, along an extent of upwards of four hundred miles, and from information received and from fossils that had been furnished to him, thinks that it extends to the west at least as far as the sources of the rivers Running Water, White, Shayeune, &c. and northwest along the Missouri probably to the Yellow Stone, being an extent in length of about one thousand miles.

Mr. Hodge followed with some observations concerning the secondary and tertiary deposits of the Carolinas.

The remarks of Mr. Hodge regarding the secondary and tertiary deposits of the Carolinas, will be found embodied in the next number of this Journal. He next noticed the deceptive appearance of the bowlders of quartz and primary rocks, scattered over the country north of Columbia, S. C., and extending throughout the gold region of North Carolina, all *seemingly* referrible to a similar cause with that which covered the hills of the northern states with their bowlders. But according to the previously expressed opinions of Messrs. Vanuxem and Mather, these are considered not to have been transported to any distance, but to belong to the rocks in their immediate neighborhood.

He asked attention to the subject of the deposit gold mines; whether these were not still in progress of formation, notwithstanding the opinions to the contrary found in many of the foreign treatises; mentioning their occurrence always near the veins of the ore, and of the fact of veins having been discovered by working the deposits up to them, above which the gold suddenly ceased. Of the power of the freshets, the discovery of the little buried village in Nacochee Valley, Ga., was mentioned as a remarkable evidence. His opinion was, that though many of the deposits referred themselves far back to the period when the whole country was overspread with diluvium, still that the deposits have been going on ever since. Specimens were shown from, and some remarks made, concerning the gold and copper ores of Davidson and Guilford counties, N. C. Veins originally worked for the former, gradually passed into lodes of sulphuret of copper and iron, though these formed a very small part of the veins at the surface. Rich specimens of the double sulphurets from the Harlan mine, Guilford county, were exhibited, in which mine the lode is over ten feet thick, or the depth of one hundred and five feet, and consists almost entirely of these ores.

Some account of King's silver mine, Davidson county, was given, and specimens of the varieties of the silver ore shown. The mine was originally worked for lead, the ore being a carbonate, very rich, and in beautiful crystals. Native silver was discovered, and the pig lead already made, found to contain a considerable amount of that metal. Phosphate of lead, copper, zinc and sulphuret of iron, were also mentioned as occurring in the lode, which was twelve feet thick. Some of the ore was of a soft light magnesian character, and though its specific gravity could not be twice that of water, yet it was considered a rich silver ore.

The lode lies between granite and a magnesian rock above. All the metalliferous veins, it is believed, are found at the point of contact of these two rocks.

Peter A. Browne, Esq. presented to the Association a section of the rock strata on the Schuylkill above Philadelphia, drawn about the year 1825, being the first geological section made in the state of Pennsylvania.

Dr. Houghton then made some remarks upon the subject of the metalliferous veins of the northern peninsula of Michigan.

He began by remarking, that that portion of Michigan lying between Lakes Huron and Michigan on the south, and Lake Superior on the north, is known as the upper or northern peninsula, while that portion of the state lying south of the Straits of Mackinac, is more usually known as the southern or lower peninsula.

The rocks of the easterly portion of the upper peninsula, for a distance of one hundred and fifty miles, consist of a series of fossiliferous limestones and shales, resting upon sandstones, the whole dipping a few degrees east of south. The limestones appear only on the southerly portion of the peninsula, while the underlying sand-rocks form the immediate coast of Lake Superior.

At a point very nearly one hundred and fifty miles west from the easterly extremity of the peninsula, and near to the immediate coast of Lake Superior, several low ranges of granitic hills make their appearance, which hills are flanked on the south, by quartz rock, alternating with mica, talcose and clay slates. These hills have a general easterly and westerly direction.

Northerly from these, other ranges of hills occur, having a similar direction, but in the several ranges as we proceed north, the granitic character becomes less and less perfectly defined, being first sienitic, after this altered sienite, and finally the outer or northern range is made up of well defined trap. This range of trap hills continues very nearly unbroken for a distance of one hundred and thirty five miles within the limits of Michigan.

The trap rock, which chiefly appears as a compact greenstone, is nevertheless, quite uniformly bounded on the north by an amygdaloid, against or upon which rests a very coarse conglomerate, and upon this a series of alternating strata of conglomerate and sandstone, the whole being capped by an extensive formation of red sandstone.

The group of stratified rocks referred to, which have an entire thickness of several thousand feet, dip very regularly, and usually at a high angle, into the basin of Lake Superior; and since the same is the fact in regard to the rocks upon the north coast, that lake may be said to occupy a synclinal basin.

After some remarks upon the successive elevation of the several ranges of hills referred to, together with the long intervals of time that would appear to have elapsed between the several uplifts, Dr. H. proceeded to say, that with our present imperfect maps, it would be nearly impossible to convey a clear conception of its geographical geology, and that in fact he had made these references, only to render more intelligible what he wished to say upon the subject of the metalliferous veins of the district.

It is a fact well known, that south from the district referred to, transported masses of native copper are occasionally met with, in the diluvial deposits which are so abundantly spread over the country; and these loose masses are distributed over an area of many thousand miles, including southern Michigan, Wisconsin, Illinois and Indiana. In northern Michigan they are still more frequently met with.

The great transported mass of native copper on the Ontonagan river, so frequently alluded to by travellers, and which he, Dr. H., estimated to contain about four tons of native metal, was stated to have all the characters of the other loose masses referred to.

The source of these transported masses has, heretofore, been somewhat obscure, although there has been good reason to believe, that most of them had their origin from the trap rocks, but whether from true veins or from the mass of the rock itself, was not known. He said that after examining the country with care, he was enabled to state, that without doubt a very considerable portion of them had their origin from what may be regarded as true veins.

Those which were regarded as true veins, were uniformly noticed to originate in the trap rock, but they were frequently traced across the superimposed sedimentary rocks, to and including the red sandstone. The direction of the veins across the upper rocks most frequently corresponds to the dip of those rocks.

Dykes of trap, traversing the conglomerate and sandstone, were stated to be of frequent occurrence ; but these dykes very rarely cut *across* the strata of the upper rocks, or in other words, they mostly occupy places corresponding to the lines of stratification, for which reason the veins referred to, cut across the dykes at very high angles.

So far as we are enabled to judge from the examinations which have been made, those veins originating in the outer range of trap hills are the only ones in the district deserving the name of metalliferous veins. Not only do the separate veins vary from a mere line to several feet in thickness, but those traversing the several rocks above the trap, are usually very much expanded in their passage across the upper rocks.

By far the most important minerals contained in these veins are the several ores of copper. The metal occurs in a native form associated with the grey and red oxides, carbonate and silicate, together with several mixed compounds. Sulphuret of copper is exceedingly rare, and pyritous copper has not been found in what was regarded as a true vein, though this last named mineral, associated with the sulphuret and carbonate of lead, was noticed in small ramifying veins, in what may perhaps be regarded as a distant portion of the range under consideration. Native silver was very rarely seen in the form of specks and strings associated with the native copper.

Most of the ores of copper occur in the greenstone, amygdaloid and lower portions of the conglomerate, or at points in near proximity to the dykes before referred to, and they are most abundant at, or near to the junction of the trap and conglomerate, or in immediate vicinity of the dykes, thus following the general laws respecting the deposits of the metallic minerals.

As the veins recede from the trap, the place of the copper is frequently supplied by the silicious oxide and carbonate of zinc, together with calcareous spar, which latter usually fills the entire vein in its passage across the sandstone.

The veinstone in those portions of the vein most rich in the ores of copper is chiefly quartz, and this is frequently filled with minute specks and filaments of the native metal.

Dr. H. conceives these to be veins of sublimation, or in other words to be simple fissures filled from below by the metal in a vaporous state, and that all the compounds had their origin from copper in a native form. The conglomerate was stated to have been noticed where the cement consisted to a large extent of ores of copper, and even of copper in a native state. This was observed only in close proximity to considerable veins.

The veins, as well as different portions of the same vein, are very variable in their metalliferous character, portions being apparently rich, while others are completely barren. With the present knowledge upon the subject, we can scarcely arrive at safe conclusions as to the value of these veins for the purposes of mining, but upon the whole they may be looked upon favorably rather than otherwise.

Adjourned to 4 o'clock this afternoon.

Friday, 4 o'clock, P. M.—*Prof. Silliman* being absent, *Dr. Locke* was called to the chair.

Dr. Jackson gave his views in relation to the construction of geological maps, suggesting the importance of concert and uniformity in design and execution, as regards *scale*, *coloring*, *symbols*, &c. on the part of the various state geologists employed throughout the Union. The subject was further discussed by Dr. Locke and Prof. Johnson, who concurred in the views of Dr. Jackson.

On motion of *Prof. Mather*, the subject was referred to a committee consisting of *Dr. Jackson*, *Dr. Locke*, and *Prof. Mather*, who are to report at the next annual session of the Association.

Prof. Johnson exhibited a section drawn across the Frostburg coal basin, extending between the Little Alleghany and Savage Mountains, a distance of about four miles. He offered some observations concerning this coal-field, and enlarged upon its value and importance as a coal and iron region.

Mr. Hodge and *Mr. Trego*, who had explored that portion of this basin which extends into Pennsylvania, also made some remarks in which they differed from the views of Prof. Johnson; particularly with regard to his opinion that some of the upper strata of the carboniferous rocks near the Savage Mountain, rest *unconformably* upon the lower ones.

Mr. Hodge placed on the table some clay concretions from Kennebec river.

On motion of *Dr. Beck*, *Resolved*, That when this Association terminates its present session, it adjourn to meet in Boston, on the last Monday in April next.

Dr. Jackson, *Prof. Hitchcock*, and *Mr. Moses B. Williams*, were appointed a local committee, (pursuant to a resolution reported by the committee on business,) for the purpose of making suitable arrangements for the next session of the Association.

The secretaries were intrusted with the preparation and publication of an abstract of the proceedings of the Association.

Resolved, That the chairman of the present session be requested to open the next session by an address.

Dr. S. G. Morton was then appointed chairman, and *Dr. Jackson* secretary, for the next session.

Dr. L. C. Beck, *Prof. H. D. Rogers*, *Prof. Hitchcock*, *Dr. Locke*, and *Dr. Jackson*, were appointed a committee to prepare a constitution, by-laws, &c. for the government of the Association, according to a resolution of the committee on business.

Dr. Griscom made a communication respecting the Duane "steel ore" of New York.

Adjourned to 9 o'clock to-morrow morning.

At 8 o'clock in the evening the Association had the pleasure, in common with a respectable audience of ladies and gentlemen of Philadelphia, of hearing a most interesting and instructive address from *Prof. Silliman*, on the general principles of geology, and subjects connected with its progress in America.

Fifth day of session, *Saturday*, *April* 10, 1841. The Association met this morning according to adjournment of yesterday. *Dr. Locke* in the chair.

The minutes of yesterday's proceedings were read and adopted.

Dr. Morton opened before the Association a vessel of earthen ware taken from the Pyramids of Sakhara in Egypt, and forwarded to him by the American consul at Cairo, which contained an embalmed body of the *Ibis religiosa*, or sacred bird of the ancient Egyptians. The earthen vessel containing this relic of the most remote antiquity, was of a cylindrical or rather a conical shape, having a lid or cover fixed on the larger end, closely fitted on and luted with a composition resembling common mortar of lime and sand.

Dr. M. remarked upon the interest attending these relics, owing to their complete preservation—the bones, feathers, and even animal matter being frequently found almost unchanged, except by desiccation. The specimen opened was one of uncommon interest on account of the great perfection and almost interminable number of the bandages of linen cloth in which it was enfolded, and the high preservation of the most delicate parts of the plumage. The position of the bird in the embalmed specimens is found to be invariably the same. The neck and head are drawn down between the legs, the lower mandible being presented outward and downward, and the legs drawn forward beneath the body of the bird, as if in a sitting posture, with the wings folded over the neck and legs. Some specimens less perfectly bandaged seem to have undergone a process of carbonization, and on the removal of the linen folds crumble into a dark powder, in which the bones appear, though reduced to a brittle state.

Dr. Morton referred to the very recent appearance of the pottery ware in which these specimens were contained; notwithstanding their extremely ancient date, which is at least three thousand to four thousand years. The pyramids of Sakhara are among the most ancient monuments of human art. These cases containing the embalmed ibis are still found in great numbers, though the traveller, Dr. Pococke, gave his opinion one hundred years ago, that they would probably soon become extinct.

Dr. Morton then proceeded to open another envelope containing some unknown embalmed object, which he conjectured to be a mass of snakes or serpents. This was less carefully enclosed than the ibis, being coarsely enveloped in rags rather than bandages, though still covered by hundreds of folds of linen. These being at length removed, disclosed *the wing of a bat!*

The next embalmed object unfolded was a young crocodile, (*Crocodilus niloticus*,) about a foot in length, and in good preservation. Dr. M. observed that this animal is found in embalmed specimens of all sizes, from the apparently just hatched young to those of five feet in length, one of the latter size being at present in his collection.

Mr. Quinby exhibited specimens of silver, lead, and other ores from the Andes in Peru, accompanying them with some observations upon their product, situation, &c.

Prof. Johnson showed specimens of magnetic iron ore from the State of New York, which he had found to contain titanic acid, combined with iron and manganese.

Dr. Locke made some observations on the application of magnetism to the discovery of metallic veins and deposits.

The following resolutions being moved by *Prof. Rogers*, were unanimously adopted.

Resolved, 1. That the thanks of the Association be presented to *Prof. Silliman*, for the interesting lecture delivered by him last evening.

2. That the thanks of the Association be presented to the Academy of Natural Sciences for the use of their rooms during the present session.

3. That the sincere thanks of the Association be presented to *Prof. Silliman*, for the highly able manner in which he has discharged the duties of chairman at the present annual meeting.

4. That the thanks of the Association be presented to *Dr. Beck* for the able and laborious manner in which he has discharged the duties of secretary throughout the first year, and at the present meeting. Also that the thanks of the Association be given to *Messrs. B. Silliman, Jr.* and *Charles B. Trego*, for their valuable services as assistant secretaries.

On motion of *Prof. Rogers*, amended by *B. Silliman, Jr.*, it was *Resolved*, That the Association publish five hundred copies of the address of Prof. Hitchcock, under the direction of the secretaries, which shall be distributed, as soon as practicable, to all the members of the Association ; and that the expense of publication be defrayed by a pro rata charge on each member, to be paid at the next meeting of the Association in Boston. Such copies as are not distributed to members under this resolution, to be sold for the benefit of the Association.

On motion of *Prof. J. C. Booth*, it was also *Resolved*, That the names of all the officers of the Association, of the local committee, and the names and addresses of all the members of the Association, be appended to the address of Prof. Hitchcock.

The Association then adjourned to meet in Boston on the last Monday in April next, (1842.)

B. Silliman, *Chairman.*

L. C. Beck, *Secretary.*

B. Silliman, Jr.
Charles B. Trego, } *Assistant Secretaries.*

II. *Boston Society of Natural History.*

At a regular meeting of the Society, held on the 21st day of April, 1841,

Dr. C. T. Jackson having announced that at the late meeting in Philadelphia of the Association of American geologists, it had been determined to hold the next meeting of that Association in this city, in the month of April, 1842, it was

Voted, That this Society invite the Association of American geologists to make use of the hall of this Society for the meetings of that Association, contemplated to be held in this city in April, 1842, and tender the use of the cabinet and library for the purposes of the Association. A true copy of record. Attest,

T. Bulfinch, *Sec. pro. tem.*

MISCELLANIES.

FOREIGN AND DOMESTIC.

1. *Proceedings of the Geological Society, June 10th*, 1840.—A paper was read on the polished and striated surfaces of the rocks which form the beds of glaciers in the Alps, by Prof. Agassiz.

This paper was accompanied by a series of plates, intended to represent the effect of glaciers upon the rocks over which they move.

These effects, consisting of surfaces highly polished, and covered with scratches, either in straight lines, or curvilinear, according to the direction of the movement of the glacier, are constantly found, not only at the lower extremity, where they are exposed by the melting of the glaciers, but also, whenever the subjacent rock is examined, by descending through deep crevices in the ice. Grains of quartz, and other fragments of fallen rocks, which compose the *moraines* that accompany the glaciers, have afforded the material which, moved by the action of the ice, has produced the polish and scratches on the sides and bottom of the Alpine valleys, through which the glaciers are constantly but slowly descending. It is impossible to attribute these effects to causes anterior to the formation of the glacier, as they are constantly present and parallel to the direction of the movement of the ice. They cannot be considered as the effects of an avalanche, for they are often at right angles to the direction in which an avalanche would descend; they are constantly sharp and fresh beneath existing glaciers, but less distinct on surfaces which have for some time been exposed to atmospheric action by the melting of the ice. In the valley of the Viesch, the direction of the scratches is from north to south, or towards the Rhone; the direction of those which accompany the glacier of the Rhone is from east to west; that of those beneath the glacier of the Aar is first from west to east as far as the Hospice of the Grimsel; and then from east to north, from the Grimsel to the Handeck. If we could account for these scratches by the action of the water, we must imagine currents of enormous depth filling these highest Apine valleys, and descending in opposite directions from the narrow crest that lies between them. In the upper part of the valley of the Viesch, is a glacier, beneath which runs a rapid torrent, coextensive in length with the great current to which the above hypothesis would attribute the polish and scratches on the rocks of the valley.

This small torrent corrodes the bottom of the valley into sinuous furrows and irregular holes, and polishes the sides of its bed; but the polish is of a different aspect from that produced by the action of the ice, and of the stones and sand which it carries with it. The polished surfaces beneath the ice are often salient and in high relief. The sides also of the valleys adjacent to the actual glaciers, are frequently polished and scratched at great heights above the ice, in a manner identical with the surface beneath it, but different from the polish of the bed of the torrent.

The amount of polish and scratches varies with the nature of the rocks. In the valley of Zermalt and Riffelhorn, rocks of serpentine are most exquisitely polished; so also are the granites on the sides of the glacier of the Aar, where they have not been long exposed to the action of the air. Gneiss and limestone do not preserve their polish under similar exposure, but retain it while they are protected by ice or a covering of earth.

These facts seem to show that the striated and polished condition of rocks beneath and on the sides of glaciers, is due to the action of the ice, and of the sand and fragments of stone forming the *moraines* which accompany it.—*Extracted from the Annals and Magazine of Natural History, for January,* 1841.

2. *The former existence of Glaciers in Scotland.*—The late visit of M. Agassiz to Scotland, during the meeting of the British Association, seems to have set all our geologists off upon a new scent,—glacier hunting. That distinguished zoologist and geologist, by his interesting work and illustrations on the glaciers of Switzerland,* has, we think, proved that they formerly existed at a much lower level than they do now on the Alps of the continent, and anxiety to examine a country where glaciers no longer existed, was the immediate motive of his visit to Scotland during the last autumn. In company with accomplished English and Scotch geologists, the examination was accordingly made, and the same appearances which characterize the rocks under the European glaciers being observed in various parts of the higher mountain ranges of Scotland, induced M. Agassiz to believe that they formerly existed in these mist-clad regions, and that many of the phenomena attributed to the action of water, such as the parallel roads of Glenroy, &c., were caused by their influence; and he writes thus on the subject to Prof. Jamieson:

* Etudes sur les Glaciers, par L. Agassiz, dessinées d'après Nature et Lithographies, par J. Bettannier, 1840, Neuchatel.

"After having obtained in Switzerland the most conclusive proofs, that at a former period the glaciers were of much greater extent than at the present; nay, that they had covered the whole country, and had transported the erratic blocks to the places where these are now found, it was my wish to examine a country where glaciers are no longer met with, but in which they might formerly have existed. I therefore directed my attention to Scotland, and had scarcely arrived in Glasgow, when I found remote traces of the action of glaciers; and the nearer I approached the high mountain chains, these became more distinct, until, at the foot of Ben Nevis, and in the principal valleys, I discovered the most distinct *moraines* and polished rocky surfaces, just as in the valleys of the Swiss Alps, in the region of existing glaciers; so that the existence of glaciers in Scotland at early periods can no longer be doubted. The parallel roads of Glenroy, are intimately connected with this former occurrence of glaciers, and have been caused by a glacier from Ben Nevis. The phenomenon must have been precisely analogous to the glacier lakes of the Tyrol, and to the event that took place in the valley of Bagne."

At one of the early meetings of the Geological Society of London, M. Agassiz read a paper, illustrating his views and their application to Scotland. This was followed on the 4th of November by a long paper from Dr. Buckland, on the same subject, and which was concluded on the meeting of the 18th, but reserving its more minute details for a subsequent evening; while Mr. Lyell has also commenced the reading of a paper "On the Geological evidence of the former existence of glaciers in Forfarshire." Dr. Buckland, in his paper, gives a general account of his late tour in Scotland, and among the more remarkable parts of his communication is the announcement that the traces of ancient glaciers are apparent in Crickhope Linn, in Nithsdale, Dumfriesshire, upon the rocks of Stirling and Edinburgh castles, and upon Corstorphine, the Calton, and Law Hills, near Edinburgh. This theory, as applied to Scotland, is comparatively new; and in the animated discussions to which it has just given rise, has been combatted, or strictly scrutinized, by Messrs. Greenough, Sedgwick, Murchison, Whewell, Phillips, and De la Beche. We have no doubt that the traces, as stated by the Professor, exist in all the above named localities; but while such is the case, it becomes most important to ascertain if these appearances, at so low an elevation, *could have been produced by any other action than that of ice.—From Annals and Magazine of Natural History, for Jan.* 1841.—On this subject, see also a paper in the January number of the same Magazine, "*On the Natural Terraces of the Eildon Hills, being formed by the action of Ancient Glaciers,*" by Mr. Bowman; from which we extract a single paragraph. "In conclusion, Prof. Agassiz informed me that he had traced repeated instances of the various descriptions of *moraines* in different parts of Scotland; also in Ireland, and between Sharp and Kendal, in Westmoreland;

and he does not doubt that they will be recognized, now that the attention of geologists is directed to the subject, in North Wales, in the Pyrenees, the Appenines, and other high mountain chains. Indeed, he believes, from strong evidences scattered over different countries, that at a recent geological period, and not long before the creation of the human race, the whole of Europe, and those parts of Asia and America which lie north of the parallel of the Mediterranean and Caspian seas, were enveloped in snow and ice; in short, consisted of a series of immense glaciers, above which, only the highest hills appeared as islands; presenting a character of scenery to be found in our day only in Greenland or Iceland."

3. *Red Color of the Salt Marshes of the Mediterranean.*—The red color of these marshes, often of a very deep tint, has been for a long time attributed to the presence of a minute crustaceous animal, *Artemia salina*, Leach. Mons. Joly has last year attended to this subject, and has come to the conclusion that the color is produced, not by the *Artemia*, but by minute animalcules, occurring in incalculable numbers, and to which he has given the name of *Monas dunalii*. This is fed upon by the Artemia, to which it communicates its brilliant red color, &c.—*From Annals and Magazine of Natural History, for Dec.* 1840.

4. *New observations on the Infusoria of Rock Salt.*—In the '*Comptes Rendus*,' (March 16,) mention is made of a note received by the Academy of Sciences by M. Marcel de Serres, relative to the observations which he is making on this subject along with M. Joly. In the specimens of rock salt of a tolerably decided greenish color, brought from Cardona, (Spain,) the infusoria appear more rare, smaller, and less distinct than in the specimens of a red color before examined. This, says M. Marcel de Serres, finds an explanation in M. Joly's previous observations on the changes of tint which the infusoria that color our salt marshes undergo by age. These animalcules, which are white at their birth, become green in their middle age, and do not till their adult age, take the purple tint which makes them so remarkable. In general, the green infusoria are not so often seen in salt marshes as the red, which seems to indicate that these monads remain but a short time in their middle state. We have found the same infusoria in the argillo-calcareous marls of Cardona beneath the rock salt. There they have their beautiful purple tint, but they are in too small numbers to communicate it to the mass of marl, which has remained grayish. This fact also proves, that in the ancient world, as in the present one, the animalcules were precipitated after their death to the bottom of the waters in which they previously lived.—*From the Annals and Magazine of Nat. Hist. for Sept.* 1840.

5. *Ornithological Gallery of the British Museum.*—The eastern gallery of the British Museum, which was formerly occupied by the collec-

tions of minerals, having undergone a complete repair, has been lately re-opened to the public, with the collection of birds and shells. Only the passerines, gallinaceous, and wading birds are as yet arranged; but the remainder will be exhibited in the course of the spring, when this room, which is three hundred feet long and fifty wide, will contain one of the richest ornithological collections in Europe. The cases are all glazed with large panes of plate-glass, with very narrow brass bars; and the smaller birds are arranged on a new plan, on box shelves, each bird having a background close behind it, so as to show its outline distinctly and relieve its colors; and the shells, which will occupy forty table cases, are exhibited on black velvet, which gives them admirable relief.—*Ann. and Mag. of Nat. Hist. for Dec.* 1840.

6. *Prof. Agassiz and his Works.*—In former volumes of this Journal, we have mentioned the labors of Prof. Agassiz on the fossil fishes, the echinodermata, and the living fresh-water fishes of Central Europe.

1. His great work on Fossil Fishes. 2. Monograph on the Echinodermata, living and fossil. 3. On the fresh-water Fishes of Central Europe, Part I. And we now announce the reception from him of 4. His Description of the Fossil Echinodermata of Switzerland, Parts I and II. 5. Memoir on the moulds of Molluscs, both living and fossil, Part I on the moulds of the living Molluscs. 6. Critical Study of Fossil Molluscs, Part I, containing the Trigoniæ of the Jura and of the Chalk of Switzerland. 7. Systematic Catalogue of the Echinodermata. 8. Study of the Glaciers, 1 vol. 8vo. with a folio Atlas containing thirty two plates.

The Swiss formations prove to be unexpectedly rich in fossil echinodermata, hitherto undescribed, as appears from the extensive catalogue of those which M. Agassiz has collected. It is his intention (as announced to us in a letter dated Jan. 8, 1841) speedily to publish monographs of all these species as well as of those that are living. Two of these parts have already appeared, and a third part is ready prepared. In his critical study of the Molluscs, the author intends, in like manner, to describe the species of these genera that have hitherto been the most neglected.

The first monograph of the Trigonias, now before us, affords a striking example of the great amount of materials which the author has collected. In the memoirs upon the moulds of Molluscs, we may confidently expect that a new era will open upon conchology, by proving the possibility of determining, with rigorous exactness, the fossil moulds so frequent in certain geological formations, and which have hitherto remained undetermined.

The study of the glaciers will probably excite a more vivid interest than any of the works of Agassiz, especially since the discovery made by him, during a residence of some months in the British Islands, of marks indicating, that in former ages a very extensive shell or crust of ice extended over all the chains of mountains in Scotland, Ireland, and England, a discovery which brings strong confirmation to his theory. The Athenæum, Literary Gazette, reports of the proceedings of the Geological Society, &c. have given a particular account of his observations, and of those of Dr. Buckland and Mr. Lyell on this subject. We have inserted a notice from the hand of a friend, but had not room in this number for a much fuller account in the reports of the Geological Society.

It will be very important that American geologists, guided by the work of Prof. Agassiz, should attentively study the similar phenomena which may doubtless be observed in North America. The works of the author abound with fine plates, and whether of minute molluscs or stupendous icy peaks, and domes or seas of ice, they are highly illustrative and beautiful. His works being very expensive, he has been repeatedly aided by funds from the British Association, but several of them have been undertaken at his own risk. Most of them are quartos, but the plates are in some instances folios. We invite American naturalists and institutions to encourage these arduous and responsible labors by purchasing the works, which may be obtained through Mr. Augustus Mayor, of New York, No. 63, William st.

The first livraison of the fossil echinodermata of Switzerland, costs fifteen francs; the second, twelve francs; the fossil molluscs, twelve francs; the moulds of molluscs, twelve francs; the glaciers with the splendid atlas, forty francs; the catalogue of the echinodermata, one franc. We are astonished at the variety, precision, and extent of the labors of this illustrious naturalist. He proves in his own case the possibility of cultivating successfully, at the same time, several different divisions of a great subject, and of bringing them out with almost unprecedented rapidity, and still without confusion or inconvenient delay. He has already pledged himself to so many important sequences of his begun labors, that we feel equally obligated and disposed cordially to wish him health and long life.

7. *Sketch of the Geology of North America, being the substance of a memoir read before the Ashmolean Society, Nov.* 26, 1838; by Charles Daubeny, M. D., F. R. S., &c. Oxford, 1839.—Although some time has now elapsed since the reading and publication of this memoir and since its reception by us, yet as it contains matter of considerable interest, it may not be amiss (although this notice has been longer delayed than we could have wished) briefly to review its contents, especially as it comes to us from one to whom science is under very many obligations.

From the short time which the writer passed in this country, having been absent from home scarcely a year, including a visit to Cuba, it cannot be expected that personal observation alone would have enabled him to give even a general view of the geology of our country, and we accordingly find, that he acknowledges his indebtedness to naturalists here, and more particularly to the reports which have been made upon the geology of the states, for many of the facts presented.

After some preliminary remarks in relation to the space occupied by the country, the advantages to be derived from the geological surveys now making of the several states, &c. the subject of diluvial action or rather of the indication of the action of water upon the continent is taken up, and the phenomena ascribable to this cause, brought to light by the observations of Prof. Hitchcock, Dr. Jackson, and others, are dwelt upon at some length. Reference is also made to an article in the first volume of the Transactions of the Literary and Historical Society of Quebec, by Major Bonnycastle, and extracts are given, from which we learn that the same evidence is presented by an examination of the country north of the United States, as has been furnished from within our borders and elsewhere, of the violent action of running water or moving ice, or of that agent, whatever it was, by which the large masses of rock were moved, and by which the dry land has been generally strewn over with bowlders and debris.

The view presented by Mr. Hayes, a few years since, in an article upon the geology of western New York, that the action of the tides and waves alone upon the rocks subsequent to their elevation above the ocean, would account for all the effects usually attributed to the action of running water, does not meet the favor of our author. He thinks that Mr. H. in adopting it, "overlooked the fact, that the force of submarine currents extends but a little way beneath the surface of the ocean, and that even the Gulf Stream could have no power to move a block of stone along the bed of the sea underneath it so soon as it was once safely deposited upon its bottom."

From the consideration of the changes caused upon the surface of the country by the effects of water, the memoir next refers to those that have resulted from fire, and which by subterraneous action uplifted from beneath the waves, the great chains of the Alleghany and Rocky mountains, and gave rise to the thermal springs associated with them. In this connection is presented an account of the unstratified primary rocks. Commencing with those of the Blue Ridge of the south, they are described as extending north and east to New York, and into New England, where they are the predominating rocks. The two groups of mountains composed of these rocks, east and west of Lake Champlain, are mentioned as uniting in Canada, and there blending in a low chain which crosses the continent northwesterly, the primary strata being visible until approaching Lake Winnipeg, where they are lost sight of beneath the cretaceous group. This class of formations is also referred to as prevailing in the Rocky mountains, travellers there, wherever observations have been made, having noticed granite, gneiss, quartz rock, &c.

In a wide sense then, says the memoir, it may be stated that the central portion of the continent is surrounded on the north, east and west, by a chain of primary formations, and that the entire space included betwixt the Alleghany and Rocky mountains, constitutes the great valley of the Mississippi. In so terming the immense country intermediate between these two ranges of mountains, the reader is guarded against the idea, that this tract is filled up with alluvial or tertiary deposits, or of supposing that it maintains an uniformly low level. This, as stated, can only be said of that portion situated between the Ozark and other hills, and the western side of the Alleghanies, about one hundred miles east and west of the Mississippi.

The Ozark hills, Dr. Daubeny, who had an opportunity of examining them in person, describes as consisting of clay slate, quartz rock, and sandstone, except near the mining district of Missouri, where porphyries and specular iron, in immense masses, have been protruded.

The several ores of the unstratified rocks are noticed, as are also the other minerals with which they abound. The dykes so common in New England, are mentioned as establishing a distinction between the rocks on the east, and those on the west of the primary chain, and as furnishing evidence of frequent igneous action subsequently to the elevation of. the Alleghany mountains.

The memoir next proceeds to describe some of the later formations; i. e. the fossiliferous rocks of New York, and those of the same relative position in the more southern states, with their mineral and paleontological contents. This, although mostly drawn from the able reports of the gentlemen engaged in the surveys of these formations, is yet interspersed with much interesting matter, drawn from the observation of the writer. Of this character are the remarks upon the Falls of Niagara, which are well worth the careful perusal of all who feel at all desirous of information in relation to their origin. The difference of level between the country below and that above Queenstown, has, it is well known, been generally ascribed to a subsidence of the former. Dr. Daubeny, however, from noticing that the strata from this place to Lake Erie appeared to dip somewhat south, (a fact, which, as stated in a note annexed, has been since verified by Mr. Hall,) is inclined to the opinion that this portion has been elevated. In relation to the falls, in other than a geological view, Dr. Daubeny, expresses himself as having been at first somewhat disappointed, though subsequently this feeling gave way to others of a different character. His own account is as follows:

" With respect to the fall itself, considered in a picturesque point of view, or as influencing the imagination and feelings, it may seem an odd confession to make, that my first feelings, on visiting it, were not unmixed with disappointment. Of an object so long known by report, each person before he reaches the spot conjures up in his mind some sort of idea, which in certain respects must differ from the reality, and may therefore lead him to imagine the latter has fallen short of his previous conceptions. Thus I had imagined that the fury of the waters after they had been launched over the cataract would have been more terrific, and was surprised at seeing the ease with which an insignificant ferry-boat crossed the stream within a very short distance below. The noise produced by the waterfall itself, I had also conceived would have been more stunning; and it was with a feeling nearly allied to what one might entertain at hearing a person of solid weight and character, talked down by a noisy upstart of yesterday, that I found the roar of this stupendous natural phenomenon overpowered by the hissing of a locomotive which was letting off its steam at the railroad station adjoining.

"The presence of these evidences of human ingenuity was in other respects also very unpropitious to the feelings which the scene itself was calculated to inspire; and though no enemy to railroads or manufactories in their proper place, I could have wished all vestiges, both of the one and of the other, banished from a spot where nature ought to be allowed to reign undisturbed and alone.

"But after a time, these first prepossessions wore away, and I then began to feel more impressed with the solemnity of the sound which the cataract produces in its descent, than I had expected to have been, by the deafening tumult of waters for which my imagination had prepared me.

"In surveying it too under various aspects and at different distances, I found new sources of admiration and astonishment continually presenting themselves, of which I had previously no conception; nor did the interest of the scene appear to flag, when I turned to contemplate the phenomena presented in the course of the river both above and below, which may be regarded, either as concomitants, or as consequences of the cataract itself."

This is followed by a graphic account of the various features exhibited by the majestic waterfall at the different points from which it may be observed, as also of the scenery connected with it.

In relation to the sandstone of the Connecticut river, Dr. Daubeny in giving Prof. Hitchcock's opinion, that it is equivalent to the new red sandstone of England, merely remarks, that if this be true, it is singular that no salt is found within it, this being its most appropriate position. He visited one of the localities of bird tracks, and compared the specimens on the surface of the rocks with those in the cabinet of Prof. Hitchcock, and fully satisfied himself that they could have been produced only by "birds of various sizes treading upon soft and plastic material."

An account is given of the coal fields and brine springs of the United States, as also of the thermal waters; the last being treated quite at length.

In conclusion, it may not be amiss to state, that the whole is written in the same easy and agreeable style as the other works of its distinguished author.

8. *Outlines of Anatomy and Physiology, translated from the French of H. Milne Edwards, M. D. &c., by J. F. W. Lane, M. D.* Boston, Charles C. Little and James Brown, 1841. pp. 312.—This treatise was intended as an introduction to a work on zoology by the same author. It gives us a general outline of the anatomical structure and physiological action of the different systems of organs of which animal bodies are composed; also those general characters and properties of living beings which could not with propriety be introduced into the body of a zoological treatise. It is written in a popular style, and is sufficiently free from technicalities to render it interesting and intelligible to a non-professional reader.

Before entering into a description of the individual functions and their organs, those properties are enumerated which are common to all living beings, and by which they are distinguished from common inert matter, viz. their mode of origin, structure and chemical composition, powers of nutrition, reproduction, and the definite duration of their existence; also the characteristics by which animals are distinguished from vegetables, the former having, in addition to the properties possessed by the latter, sensibility and voluntary motion.

The different functions, and the organs by which they are performed, are arranged in three great divisions, viz. those of nutrition, relation, and reproduction. Under the first head, are described the blood, its properties, the apparatus and mechanism of its circulation, exhalation, secretion, respiration, animal heat, and digestion: under the second head, are arranged those organs and functions by which animals are made acquainted with, and are enabled to act upon external objects, viz. the sensorial, intellectual, motory, and vocal; and under the third head, those which are subservient to the preservation of the species, viz. those of generation.

In treating of the different functions first enumerated, it is the object of the author to describe them as they exist in man, and compare with them those of which the lower orders of animals are the seat. In so doing, he has placed the subject of physiology under a very attractive form; and we trust that the work will be extensively circulated, and that it may conduce still farther to develop that taste for the science of which it treats, among the unprofessional public. The translation of the work is in some respects objectionable, the technical words being, in many cases, literally rendered, instead of substituting for them the corresponding English technical expressions. We have no hesitation, however, in saying, that the high merit and reputation of Dr. Edwards as a physiologist and zoologist, and the interest which is felt by the public in anatomical and physiological sciences, will render the work, at the present time, highly acceptable.

9. *Volcanic Phenomena in Hawaii.*

To Prof. B. Silliman—*My dear Sir:* You expressed a wish that I might send you an extract from the letter of Capt. Couthouy; and as my way of doing business is never to delay, if avoidable, I send it you. The letter is dated Honolulu, Oahu, Oct. 24, 1840.

"Visited the great crater at Kirauea or Ka Lua Pele, which is an immense pit one thousand feet deep and six miles in circuit, with perpendicular walls, except at one point, where it is reached by a steep descent, and the whole of this vast cauldron, full of boiling, bubbling, and spouting lava. The surface at one moment black as ink, and the next exhibiting rivers and pools and jets of a hideous blood-red fluid, that was sometimes thrown up to a height of fifty or sixty feet, and fell back with a sullen plashing that was indescribably awful. The aspect of the whole was *hellish*—no other term can express it. By night it was grand beyond description. The frequent lightings up, the hissings and deep muttering explosions, reminded me of some great city in flames, where there were magazines of gunpowder or mines continually exploding. Vesuvius is a fool to it. Just previous to my visit, the lava had burst out at a new place, about six miles northeast of the crater, and flowed down to the sea in a stream of forty miles in length, by from one to seven in breadth. I saw the light one hundred miles off. It reached the sea in five days, threw up three hills (I send a rude sketch, but literally correct, and interesting as the work of a native) of from one hundred and twenty to two hundred and fifty feet high, gained two thousand feet out seaward from old line of coast, by three fourths of a mile in width, and heated the water for fifteen miles either side, to such an extent, that the fishes were heaped up in myriads on the shore, scalded to death. Its falling into the sea, was accompanied with tremendous hissings, and detonations like constant discharges of heavy artillery distinctly heard at Hilo, twenty minutes distant."

With great respect, yours, truly, D. H. Storer.

10. *Manilla Hemp.*

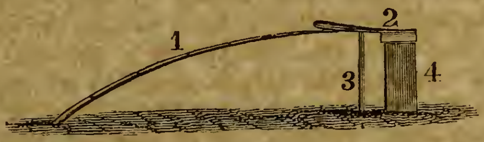
Singapore, May 29, 1840.

To Prof. B. Silliman—*Dear Sir:* Having had my attention called to the subject of Manilla hemp, and the mode in which it is prepared, I have had the good fortune to receive from a friend on the spot such an account of the matter as may be interesting to some of your readers. My correspondent is T. M. M., Esq., a British merchant in Manilla. His paper is chiefly a translation of Blanco's Flora Filipina, from page 247 to 250. He has enjoyed the best opportunities for obtaining information on the subject, and has freely made his remarks as the various topics in the translation gave occasion for them. He has sent me a specimen of some of the hemp which he made himself, while inspecting the operation; a portion of which I send you. As a part of his paper is taken up in controverting some of the opinions of Blanco, I shall omit this, and give that part of the process on his authority, he speaking from personal observation, and Blanco from report. Having obtained his consent, you are at liberty to make such use of the paper as you judge most suitable.

Yours, &c. Jos. S. Travelli.

This article is obtained from a species of plantain, the "Musa Trogloditarum textoria," and is probably a variety of the Musa Trogloditarum errans; the lower edge almost without incision, stamens five, without rudiments of sex, fruit with three ribs and many perfect seeds. It is one of the most useful plants and is cultivated with care in the province of Camarines, and in other parts. At first sight it is not different from others of the same species. The fruit is very small, seldom exceeding two inches in length. The use made of this plantain is immense; of it are made ropes, cables, and woven cloth of extreme fineness.*

In preparing the hemp they cut the tree near the ground at the time it is about to produce fruit, and also cut off the upper extremity or head, removing the leaves. They tear off one by one the layers, and with a knife remove the skin from the inner surface. The layer or roll being now stript of its inner skin, is torn into strips of about two fingers' breadth. These strips are placed, one at a time, on a table or piece of wood, below the edge of a knife, which is pressed down upon the table by a long bamboo fastened in the earth.



1, long bamboo pressing the handle of the knife *upwards* to keep its edge closely down. 2, knife—perhaps somewhat like a small cleaver or butcher's chopping-knife. 3, pivot or support for the knife. 4, the table, or stake of wood or bamboo on which the strip is placed under the edge of the knife.

The strip of the layer being placed beneath the knife, the outer skin or surface uppermost, they pull it with force by one end. The knife re-

* The quantity exported annually from Manilla to Europe and the United States, is about 80,000 piculs, (i. e. of 140 lbs. each,) making about 11,200,000 lbs. English; of which, about four-fifths is said to go to the United States.

moves the skin, and the pressure on the table enables the table to remove the fleshy substance. It is usually pulled but once; that is to say, when the strip is placed under the knife; about two feet remain on the side of the operator, which he grasps to pull it through; this done, he turns it, placing the other end under the knife, and grasping the end which now shows only the fibre, he by the second pull finishes this part of the process. The handful of threads or fibres is then placed with others on a branch of a tree, or peg, in the side of his hut, till a quantity is accumulated. Hung up in this way for an hour or two, the fibres are dried and fit for merchandise. T. M. M. thinks that very little, at the most not a fourth of the fibre, is lost by this process. Blanco imagines one half, he also thinks it undergoes a second process, being passed under a sort of saw. T. M. M. supposes this to be only a bad way of performing the process just described, it being much easier to draw the strip under a saw than under a knife. But as a much larger portion of the skin is left adhering to the fibres in this way, it is much less valuable. The process by the knife is so tedious, that three men will make only thirteen and a half pounds in an ordinary day's work.

The fibres thus prepared are separated into various qualities by the women. They weave them with great dexterity, even in the dust. If the hemp is to be used for making cloth, it is first wound into a ball about the size of a child's head, which is then placed in the wooden mortar used for husking rice, and is pounded for some time with the wooden pestle. This operation renders it very flexible, and less liable to break. This being done, the fibres are tied together, which is usually done by women and girls. The weaving process is the same as for cotton fabrics; but if the hemp (i. e. plantain) fibres are very fine, they place the women who weave it under a cover or shade, because the wind easily breaks the threads. The cloth being woven, is placed for a day and night in water, with a little lime made from sea-shells. It is afterwards washed and stretched out. Although the hemp is strong, yet it is inferior to linen thread, and to thread of European hemp, both in respect of strength and in the gloss and touch. Hemp (i. e. of the plantain) is produced of good quality in the province of Batangas, and in other parts, but not equal in quality to that of Camarines province; and this latter again appears inferior to that of Panai and Marinduque Islands. On this point, however, opinions differ.

The fruit of the hemp plant is eaten. The water (sap or juice) collected in a hole cut in the trunk of the tree, at the foot, is said to be a remedy for contraction of the *membrum virile*, a singular complaint, which ceases not to be frequent in the Bisayas provinces, and which is generally accompanied by a contraction of the tongue. The Englishman, Dampier, was mistaken when he said, that the hemp plant was known only in the island of Mindanao.

The hemp is easily dyed of blue and pink colors. To dye it blue, they still employ, as in the olden times, the leaves of a shrub or twining plant, which, in Camarines, is named *payanguit* and *aringuit*, the *Marsdenia akar.* (*Flora Filipina*, p. 118.) An intelligent description of this twining plant was made to the Politico-Economical Society of Manilla, by that curious and diligent observer, Padre Jose de Mata, of the order of San Francisco; and it is he, who, in these our days, has brought this plant to the notice of the European dwellers in the Philipines. The leaves of this plant give blue color in abundance.

To dye the hemp pink, I have heard that in Camarines province they boil the bark of the root of the Morinda (*Flora Filipina*, p. 148) with a little lime or alum, till the desired color is obtained, and with this they proceed to dye. But it is better to dye in the same manner as used for cotton thread, which is with a solution of wood-ashes and oil of *Sesamum.* (*Flora Filipina*, p. 507.) Additional note by T. M. M., 30 Mar. 1840.

The hemp of commerce is brought to Manilla chiefly in native rigged boats of fifty to one hundred tons burthen, termed *pontines*, from Camarines and Albay provinces, and from Zebu Island. The price paid to the actual producer in the interior of the country, I do not know; but considering that it is a bulky article, the cost of transportation must be heavy, as it is brought to the ports of shipment for Manilla, chiefly on horseback, through a country where there are few roads, and must also be collected from house to house; the price, therefore, that can be afforded to the native laborer must be very trifling. At the ports of shipment, when roughly made up into bales, it costs generally about —— per picul of 140 English pounds, on board the pontines. The general price at which they sell it in Manilla to the exporters is from \$3,75 to \$4,25 per picul. It is delivered from the pontine, at some of the various hemp screws, where it is opened out, examined, and again made up into well-shaped bales, each weighing two piculs, or 280 English pounds, net, and measuring about ten and a half cubic feet. In this state it is ready for transmission on board ship, and is generally known in commerce. The expenses of screwing, shipping, and two per cent. export duty, amount to about half a dollar per picul of 140 pounds.

11. *National Institution at Washington for the promotion of Science. Organized, May* 15, 1840.—Constitution adopted and objects declared, being to promote science and the useful arts, and to establish a National Museum of Natural History, &c. First list of officers.

Directors—Hon. Joel R. Poinsett, late Sec. of War, Hon. James K. Paulding, late Sec. of the Navy.

Councillors—Hon. John Q. Adams, Col. J. J. Abert, Col. Jos. Totten, Dr. Alex. McWilliams, A. O. Dayton.

Treasurer—William Stone.

Secretaries—Francis Markoe, Jr., Cor. Secretary, Pishey Thompson, Rec. Secretary.

A very able discourse was delivered before the Institution by the Hon. Joel R. Poinsett, at the first anniversary.

He states that the Institution "embraces every branch of knowledge." It is divided under the following sections, namely: Astronomy, Geography and Natural Philosophy, Natural History, Geology and Mineralogy, Chemistry, the application of Science to the Useful Arts, Agriculture, American History and Antiquities, and Literature and the Fine Arts. Gratuitous lectures to be given by eminent men. Collections of objects of natural history and of documentary information to be made from all quarters of this country, and of specimens from all foreign countries. Efforts to be made to rear a school of mines, which would be of great importance.

The ordnance officers lately returned from Europe have brought with them many specimens of iron ores used in foreign foundries, and those of the United States will be obtained and brought into comparison. The South Polar Exploring Expedition have already sent home one hundred and fifty boxes of specimens, which are stored away until a proper building can be provided to receive them.

Mr. Alexander Maclure, of New Harmony, Indiana, has offered a suite of geological specimens from the cabinet of his late brother, William Maclure, the pioneer of American geology, the munificent patron of the Academy of Natural Sciences at Philadelphia, and of many other institutions, recently deceased in Mexico.

The Institution has issued its first bulletin of proceedings, from June to December. Several of the sections are organized, being filled with able men. (We hope they will prove *workers*, as no machine will go without impulse.) Many donations are announced of minerals, books, documents, maps, curiosities, works of art, &c.;—and examinations of, and reports on various scientific and practical subjects are suggested and projected.

The bulletin contains a letter from the venerable Mr. Duponceau, on the prospects and interests of the society; it is exceedingly wise and kind, and its suggestions should be kept constantly in view.

There are instructive letters also from other eminent individuals at home and abroad—copious extracts from the admirable discourse of Mr. Poinsett, which shows enlarged views and extended knowledge, with the best spirit; also extended communications from Captain George W. Hughes, of the corps of topographical engineers, containing very instructive and important communications respecting the mines and metallic works and manufactures of South Wales, Cornwall and Devon, and many other important subjects.

Captain Hughes communicated also, as appears in the bulletin, a spirited sketch of the meeting of the British Association which he attended at Glasgow, in the autumn of 1840, with the views of many of its great men in scientific and practical geology.

On the whole, we are very favorably impressed by the plan and first inception of the National Institution at Washington, and we cordially wish it success. We trust that the government will respond to, and fulfill their high obligations in regard to the munificent half million bequest of Mr. Smithson, and will bring it to bear either in aid of this Institution, or in some other form, *without delay*, as neglect and procrastination which would be as little to our honor as to our advanage.

The frequent changes among our public men connected with the general government, creates no small anxiety as to the fate of any efforts to promote good knowledge and liberal arts, so far as a local institution at Washington may depend upon those who are oppressed with political duties, and whose action is so liable to receive a disastrous bias from the malign atmosphere of party. Liberal institutions at Washington, under the influence of enlightened and truly patriotic minds, could not fail to exalt the character, both of the government and of the country—of every such institution we would devoutly say, *esto perpetua!*

12. *Fossil Turtle.*—Dr. Mantell has discovered a small fossil turtle in the chalk of Kent—a very beautiful thing, and he has prepared a memoir on it for the Royal Society.

13. *Fossil Saurians.*—Dr. Mantell has communicated to the Royal Society an elaborate memoir upon the iguanodon, hylæosaurus, and other reptiles of Tilgate Forest, embodying all our knowledge of the osteology of those saurians, derived chiefly from his own researches.

14. *Microscopical Observations and Microscopes.*—Dr. Mantell, under date of London, March 29, 1841, writes to the senior editor of this Journal:

"Microscopical observations are now all the rage in every department of science. If you go to the Royal Society, you are sure of the microscopical investigation of the embryo—at the Medico-Chirurgical, microscopical observations on the blood corpuscules—at the Geological, microscopical observations on fossil teeth, and at the Linnæan, microscopical observations on vegetable organization. At the soiree of the president of the Royal Society, (the Marquis of Northampton,) microscopes are the great subject of attention, and then we have a microscopical society and a microscopical Journal.

"My young son has been very busy with his microscope in examining some American water from West Point, sent by Dr. Bailey, and we have seen some of your infusoria alive and as active as if they had not had a sea voyage, and had not been bottled up for weeks. Prince Albert was at Lord Northampton's soiree on Saturday, and I showed him many fossil teeth and bones and infusoria with the microscope. Prof. Owen, who has been following up his odontography, that is, microscopical examination of fossil teeth, has discovered a remarkable structure in the large conical teeth of Jaeggar's Mastodonsaurus—the calcigerous tubes—the medullary or pulp canals, and the dentine are distributed in the most intricate and labyrinthine manner you can conceive; and in consequence, Mr. Owen has named the animal the Labyrinthodon. A part of the skull has been found in Warwickshire, in the red sandstone, and although but a fragment, presents anatomical characters which prove the original to have belonged to the Batrachian family, and not to the Saurians. He believes that it is to some reptile of this kind that the impressions (the so called Chirotherium) on the red sandstone, are to be attributed. The sections of fossil teeth are made in the same manner as those of fossil vegetables, as figured and described by Mr. Witham; and they present the most beautiful objects imaginable; but you will doubtless see Mr. Owen's book, in which they are admirably represented. Mr. Owen's memoirs, whether he be right or wrong in ascribing the foot-marks of the red sandstone to the reptiles that Jaegger describes as Mastodonsaurus, are admirable essays on a most interesting department of palæontology. You will be delighted with his beautiful elucidation of the form and structure of the original animals from a new fragment of the upper jaw and a few teeth. He has, perhaps prematurely, given a restoration of the Labyrinthodon, representing it as a Batrachian, with a head like a crocodile.

"Ross and Powell are the two rival opticians whose microscopes are in demand. From ten to forty or fifty guineas is the price of the best."

Another correspondent* says, "I have one which, in its simple form, has cost me eighteen guineas, and will cost fifty to complete it. I learn that they are made much cheaper at Berlin and at Paris, where Mr. Donne has constructed a very good one for thirty-six francs."

The correspondent first named, adds: "Fossil infusoria in flint are now greatly in vogue, particularly Xanthidia, like those figured in Mantell's Wonders of Geology. You have them living in the water at West Point. But Ehrenberg's grand folio work has induced an active search after living animalcules, and my son has discovered in a stream near us, almost every form figured by Ehrenberg."

15. *Geological Drawings.—Schorf* has been painting in distemper representations of some of the plesiosauri and ichthyosauri in the British museum on a large scale, from six to eight feet long, for public lectures. The price is from forty shillings to three guineas each. A copy of the

---

* Mr. G. F. Richardson, curator of the British Museum, London.

Wealden reptiles would be five or six pounds—size nine feet by five or six. The hylæosaurus would be about three guineas. Even the Maidstone iguanodon might be drawn on the same scale.—*Dr. Mantell.*

16. *Glaciers, Moraines, &c.*—You will have seen that Agassiz's "Etudes des Glaciers" has excited some of our principal geologists and others to hunt after moraines, and they instantly found proofs of former glaciers, and of a cap of ice over the whole northern hemisphere, from the bowlders and scratches on the rocks and beds of gravel and mud, which erst were considered indubitable proofs of diluvial action. As a consequence, several memoirs on the former existence of glaciers in England and Scotland were read at the Geological Society, and gave rise to many animated discussions. This is now over, and Mr. Owen's footmarks and labyrinthine tracks are on the ascendant. But we are steadily advancing in knowledge. Although Agassiz's theory, to the extent to which he carries it, cannot be admitted, yet modified, it explains many hitherto obscure phenomena.—*Dr. Mantell.*

17. *British Association—Mr. Murchison—His Journey to the Ural Mountains—Opinions, &c.*—A letter from Roderick Impey Murchison, Esq. to the senior editor, dated Paris, April 4, 1841, announces "*on authority*, that the next meeting of the British Association will be at Plymouth, (England,) on the 29th of July, and not on the 12th as was first promulgated."

Mr. Murchison was at Paris, on his way to the Ural mountains in Russia, in which tour he was to be accompanied by his friend, M. E. De Verneuil, the companion of his journey in Northern Russia in the summer of 1840. Of the geological results of this journey, an interesting account is given in a printed memoir of Messrs. Murchison and Verneuil, which we have just received. Their journey, the present summer, is for the purpose of settling some important questions in Russian geology, with reference to a geological map of that country. The travellers hoped to reach St. Petersburgh the first of May, and to be in the Ural mountains or their flanks in the first week in June.

It appears from Mr. Murchison's last memoir alluded to above, that "the evidences in Russia are most complete as regards a triple descending fossiliferous order of carboniferous, old red or Devonian and Silurian." The Rhenish provinces and Germany furnished, it is true, to Mr. Murchison and Prof. Sedgwick, the proofs of a similar order, but Russia alone has, as yet, supplied the union of proof required, viz. the order of superposition and the collocation in the same bed of the *Ichthyolites* of Scotland with the *shells* of Devonshire.

Mr. Murchison expresses his conviction that North America may offer the fullest and most perfect sequence of paleozoic strata in the world. He adds, "To your magnificent region I look with intense interest, and I live in the hope of being able to explore its paleozoic rocks. Already, however, your countrymen are preparing all the elements for the complete classification of the older deposits of America. I have for some time been gratified to observe the steps which Conrad, Vanuxem, Hall, and others have been taking, and very recently I was delighted to receive from Mr. Hall a suite of specimens which leave no doubt of the descending succession of old red sandstone or Devonian rocks into the Silurian types. I sent his section to be laid before the Geological Society, with a notice from myself. Mr. Featherstonhaugh had furnished me with carboniferous and Silurian fossils, but the suite sent by Mr. Hall is the first that combines evidences of the existence of the great intermediate formation. The American old red sandstone with fishes is absolutely undistinguishable in hand specimens from certain beds with coccosteus, holoptychias, &c. which I have collected in the Highlands of Scotland, and largely in Russia."

We are authorized by Mr. Murchison to hope that his projected visit to the United States may not be postponed beyond the spring of 1843. Another eminent English geologist will visit us in the current summer—Mr. Lyell, as we learn by a recent letter from himself, may be expected in this country in August, and he will remain until the next year, travelling and engaged in professional objects. All our geologists will unite in welcoming these distinguished men, and they will receive a cordial welcome from many other intelligent persons.

18. *Dr. Brown of Edinburgh on the Production of Silicon from Paracyanogen.*—Much excitement has prevailed of late among the scientific world of Edinburgh, owing to the discovery claimed by Dr. Samuel M. Brown, a young chemist of that place, that one element may be derived from another by causing atoms of the *same* element to combine with each other under peculiar conditions, giving rise to bodies dissimilar in all respects to the properties of the original matter, and corresponding in character with the attributes of some other elementary body. In other words, one element (as we at present consider them) may be transmuted into another, and he conceives that all matter may proceed from one simple elementary body, which by union with *itself* under different conditions, gives rise to other dissimilar bodies possessing characters which have, we think, been considered elementary. He can combine but cannot separate the atoms.

It is now some months since we were first informed, through a correspondent in Edinburgh who is well known in this country, of this novel, and, as it seemed to us, extravagant claim, and we should never have thought of bringing the subject before our readers, had we not within a few days, through the same kind hand, received a copy in proof of Dr. Brown's second paper, the title of which is at the head of this notice, read by Dr. Christison before the Royal Society of Edinburgh, May 3, 1841. In this paper the author claims to have decomposed paracyanogen by heat in a glass tube away from contact with atmospheric air, and to have obtained as a result nitrogen, and a body possessing all the known properties of *silicic acid*, (*silex.*) He attempts to prove that the silex could not be derived from the tube or crucible in which the experiments were performed, both on account of the quantity produced, the difficulty of conceiving of any act of substitution by which the silicon could be brought out of the glass or porcelain, and the fact that as nearly as could be ascertained no loss of weight was suffered by the containing vessel.

Dr. Brown's paper is in great detail, occupying about twenty pages of the Transactions of the Royal Society. "Dr. Christison," says our correspondent, "who read the paper, (the author not being a member of the Society,) added that he had seen the experiments performed, had studied every objection, and had exhausted his ingenuity in endeavoring to detect sources of fallacy, but without success; if there is error, it is unknown to him. The processes, he said, are delicate and difficult, and he will not renounce his faith in Dr. Brown until opposite results are obtained by more than one chemist equally skillful in manipulation, and equally patient and truth-loving in spirit. The Society unanimously thanked the author, and admitted his paper into their Transactions."

The present communication consists of five parts. The first treats of the production of silicon from paracyanogen; the second of the formation of amorphous mixed compounds of silicon with copper, iron, and platinum, by the reaction of paracyanogen on these metals; the third of the quantity of nitrogen separated from paracyanogen when it is changed into nitrogen and silicon; the fourth contains processes for the preparation of amorphous, semi-crystalline, and crystalline disiliciurets of iron from the paracyanide of iron and the ferrocyanide of potassium; and the fifth gives an easy process for the preparation of silicic acid on any scale of operation, by the reaction of the ferrocyanide of potassium on the carbonate of potassa.

We hope in our next to find space to publish this extraordinary communication entire, and it will then be in the power of all interested to verify or annul the author's results.—B. S. Jr.

19. *Artesian Boring at Paris.*—Many years ago, near the Barriere de Grenelle, one of the highest points in Paris, a boring was begun to obtain water. It was discontinued after some years, and again resumed about seven years ago. The result has been successful. We have room only for the following facts, recently communicated by a friend in Paris. The water was at last obtained below the chalk, at the depth of nearly eighteen hundred feet. The torrent of water, about three cubic yards per minute, rises in a copious fountain in the grounds of an *abattoir*, (slaughter-house,)

and is very pure. The column rises from a source one third of a mile below ground, and it spouts thirty feet above the surface. The temperature at the bottom of the boring* was nearly 83° of Fahrenheit, (that of a hot summer's day, such as is rarely known there on the surface,) thus confirming fully the increase of heat in the interior of the earth, by the average generally observed in similar cases of about 1° for fifty feet of descent, which, at the same rate of increase, would give a fountain of boiling water at two miles from the surface—full ignition of rocks at ten miles, and fusion at two hundred miles; thus leaving a firm crust to preserve the good citizens of Paris from being disturbed by the fear of breaking through, or by the danger of the immediate outburst of the fire.

20. *The Theory of Horticulture: or an attempt to explain the principal operations of Gardening upon Physiological Principles;* by John Lindley, Ph. D. F. R. S., &c. &c. First American edition, with notes, &c., by A. J. Downing and A. Gray. New York, Wiley and Putnam. Boston, C. C. Little and Co. 1841.—Few authors, at least in the English language, have been equally successful with Dr. Lindley in rendering *real science* intelligible and attractive to the general reader. This work, "written in the hope of providing the intelligent gardener, and the scientific amateur, correctly, with the rationalia of the more important operations of horticulture," has supplied a very important desideratum, and is deemed indispensable to every gardener and amateur cultivator in Great Britain. We presume it will be equally prized in this country, where a taste for horticulture is so widely diffused, and where a work, which conveys this kind of information, is so greatly needed. This edition is very neatly printed in the 12mo. form, and is afforded at an extremely moderate price.

21. *Corresponding Magnetic Observations, by Prof. A. D. Bache of Philadelphia, and Prof. Lloyd of Dublin.*—It is with much pleasure that we republish from the Proceedings of the Royal Irish Academy, (for June 22, 1840,) the following account,† by Prof. Lloyd, of Dublin, of a series of simultaneous magnetic observations made in November, 1839, by Prof. Bache at Philadelphia, and Prof. Lloyd at Dublin, with a view to determine whether any deductions could be drawn from them for determining differences of longitude. The results are very satisfactory, inasmuch as they prove definitely that "no correspondence whatever exists between the smaller changes of declination at Dublin and at Philadelphia, and that the determination of differences of longitude by means of the magnet at such distances is impracticable."—Eds.

* Which is over eighteen inches wide at the top, and from seven to eight at the bottom, and lined with a metallic tube.

† This account has been slightly abridged, to accommodate it to the crowded state of our pages.—Eds.

It is well known that the magnetic declination, at a given place, is subject to frequent and irregular variations, and that corresponding changes occur, at the same instant of time, at very distant places. The first recognition of this remarkable phenomenon seems to have been made by Arago, while comparing the observations of declination made by himself, at Paris, in the year 1818, with the contemporaneous observations of M. Kupffer, at Casan. Not long after, the subject underwent a fuller investigation in the hands of Humboldt; and, in the year 1827, an extensive system of simultaneous observations was organized by that illustrious philosopher for the purpose of elucidating it. At length, in 1834, it was taken up by Gauss, and received a much greater development. Gauss discovered that the irregular changes of the declination were of *continual* occurrence; and that the synchronism, which had been previously observed only in the larger changes, extended to the minutest movements. In order to investigate the law of these synchronous changes, and the locality and other circumstances of the acting forces, Gauss arranged the extensive plan of simultaneous observations at short intervals, which has been already four years in operation, and in which almost every country in Europe has been represented by some one or more observers.

The rapidity with which these changes thus appeared to follow each other, held out the hope that they might be employed in determining differences of longitude; and it only remained to ascertain, for that purpose, whether variations so inconsiderable in magnitude corresponded at great distances. If such should prove to be the fact, it would be only necessary to project on a large scale the results of the observations made about the same absolute time at the two stations, and to compare the times of the corresponding maxima and minima. In the observations already referred to, the maxima and minima succeeded each other (as has been said) at intervals of about forty seconds, and the epoch of their occurrence was probably known to six or seven seconds. By shortening still further the interval of observation, it is manifest that this error may be much diminished. The corresponding error of the difference of longitude resulting from a single comparison (supposing the probable error of epoch to be the same at the two places) will be greater in the ratio of $\sqrt{2}$ to 1; but this error, owing to the multitude of the maxima and minima compared, must necessarily be greatly reduced in the final mean.

In order to put this question to the test on the largest scale, it was agreed between Mr. Bache and Mr. Lloyd, to make a series of corresponding observations in Philadelphia and in Dublin. Some difficulties occurred in concerting a plan, and Prof. Bache underwent, in one instance, the labor of an extensive series of observations, without any counterpart in Dublin. At length, however, it was agreed to observe during the week commencing the 11th of November, 1839; the observations being taken during two hours on each day—namely, from 12 to 1 P. M., and from 8 to 9 P. M., Greenwich mean time.

Prof. Bache's account of his observations is contained in the following extract of a letter which accompanied them, dated November 29, 1839.

"The place of observation is a room in one of the out buildings for the dwellings of the professors of the Girard College. As the materials used in the construction of the house must produce considerable local attraction, no absolute measures have been attempted. All movable magnetic substances were removed from the vicinity of the needle. A window near the needle was carefully closed by a shutter of wood, and by two curtains fastened to the window frame, and with an interval between them. There is no fire in the room; and a double door is between the observing room and an adjoining one where there is a fire.

"The instrument is one of Gauss's declination magnetometers, made by Mayerstein of Gottingen. The arrangement of it agrees exactly with that described in the *Resultate,* which has been followed as nearly as possible. The reading telescope is supported upon a small wooden shelf fastened to one of the side walls of the room; the scale is attached to a wooden frame before the shelf. One of the smallest divisions of the scale, which is divided by estimation in the observations to tenths, is 25″ .975 nearly in value. The zero of the scale did not vary sensibly in position during the observations.

"The observations were made every eight seconds, an assistant striking two seconds before the time of each observation. The ticks of the half-seconds chronometer being distinctly audible, the observation was made at the fourth beat after the signal given by the assistant, and thus the time was independent of the minute accuracy of the signal. Checks were adopted, to prevent or detect large errors in giving the signal. The interval of eight seconds is very nearly one third of the time of oscillation of the magnet bar.

"The time was observed by a chronometer beating half seconds. This was compared, before and after each set of magnetic observations, with one, and after the morning of the 14th with two chronometers. One of these was carried from the Girard College to the city after each set of observations; but the others remained during the night at the Girard College, and were removed to the city after the morning series, to compare them with the stationary chronometer belonging to the High School Observatory, the rate of which was ascertained by observations of transit of the sun and stars on the 6th, 9th, 11th, 13th, and 16th of November. Girard College is about 1770 feet west, and 8050 north of the High School by the city map."

The Dublin observations were made in the Magnetical Observatory. The instrument employed is of the form described some time since to the Academy.* It is a magnetic collimator with a graduated scale of glass,

* See Proceedings, No. 18, p. 330, *et seq.*

each division of which corresponds to 43″.22 of arc. The visual angle under which each division is seen is so considerable, that the divisions can readily be subdivided into tenths by estimation. The time of vibration of the magnet is 17″.78. The apparatus containing the magnet, as well as the reading telescope, are supported on stone pillars resting on solid masonry and insulated from the floor.

The observations having been undertaken by Prof. Lloyd without assistance, it was found impracticable to observe at intervals shorter than the time of vibration of the magnet bar, of which each successive elongation was accordingly noted. The time shown by the chronometer was usually noted every tenth or twelfth vibration; and thus the time of the intervening observations could be interpolated with much exactness. The error of the chronometer was obtained on the nights of the 11th, 14th, and 19th of November, by transit observations with the four-foot transit of the Observatory.

Of these observations, those made on Wednesday, Nov. 13, (8—9,) P. M. Greenwich mean time, were the most favorable for the purpose contemplated. The changes, though small, (from 5″ to 50″,) were marked and rapid, the intervals of the successive maxima and minima averaging thirty six seconds. The epoch of their occurrence seems to be determinable to between four and five seconds.

When the two sets of observations were reduced and laid down in curves, it was found that they presented no similarity; in other words, that there was *no correspondence whatever between the smaller changes of the declination at Dublin and at Philadelphia.* The determination of differences of longitude, by means of the magnet, is therefore, impracticable at such distances; but the attempt has revealed the important fact, that the irregular changes of declination, which have exhibited so marked a correspondence at the most distant stations at which simultaneous observations have been heretofore made, do not correspond on the American and European continents. Prof. Lloyd observed that much light would, ere long, be thrown upon this curious subject, by a comparison of the observations made at the Magnetical Observatory of Toronto, in Upper Canada, with those of Europe.

22. *Fossil Foraminifera in the Green Sand of New Jersey.*—Prof. J. W. Bailey, in a recent visit to the cretaceous formations of New Jersey, has brought to light the interesting fact that a large portion of the calcareous rock defined by Prof. H. D. Rogers as the third formation of the upper secondary, is made up, at the localities where he examined it, of great quantities of microscopic shells, belonging to the Foraminifera of D'Orbigny, which order includes those multilocular shells which compose a large part of the calcareous sands, &c. of Grignon and other localities in the tertiary deposits of Europe. Since the minute multilocular shells

above alluded to were discovered, Dr. Torrey and Prof. Bailey have together examined specimens of limestone from Claiborne, Alabama, and have found in them Foraminifera, of forms apparently identical with those occurring in New Jersey. None of this order except the genus Nummulite have heretofore been noticed in our green-sand formation. In this connection we may also announce the interesting discovery recently made by Prof. Wm. B. Rogers, of

A vast Stratum of Fossil Infusoria in the Tertiary Strata of Virginia.—It occurs about twenty feet in thickness, beneath Richmond, and is found to be filled with new and highly interesting forms of *marine* siliceous infusoria.* It would be interesting to have the specimens of the green-sand formation of the far west, collected by Mr. Nicollet, examined, to see if infusoria or Foraminifera may not be found in them.

An interesting paper on these subjects, by Prof. Bailey, with two plates, will appear in our next number.

23. *Discovery, in Virginia, of the regular Mineral Salt Formation.*—As salt springs and fountains are very numerous in the western and south-western parts of the United States, it was natural to expect that mineral salt would, sooner or later, be discovered. Indeed, strata of salt, in regular position, and roofed and floored by beds of sandstone, were, some few years since, reported by the Rev. Mr. Parker,† a missionary among the Indians of the far west, as existing in abundance, in a mountain on the Salmon river, among the Rocky mountains; but no mineral salt, in the solid form, had ever been discovered in the United States proper. Now, however, we have the pleasure of announcing this discovery on the authority of the Rev. Stephen Taylor, of Abingdon, Va. His letter to us is dated March 4, 1841, and was soon followed by a large box of salt of the most indubitable character.

It was taken from a well which is "still in the process of being dug, at the salt works about eighteen miles from Abingdon, in the county of Washington," now perhaps within the bounds of Smyth county. From Prof. Wm. B. Rogers, as the geologist of Virginia, we must expect a full scientific account of this salt formation, which, however, appears to have been discovered, as we are informed, since he visited that part of Virginia. The following particulars are derived from a correspondent of Mr. Taylor, and they may not be uninteresting in anticipation of the geological report of Prof. Rogers.

Different persons vary in their report of the depth of the different wells. That of the salt-rock well does not vary much from two hundred and sixty

* See Prof. Rogers's Report on geology of Virginia, 1841, pages 38 to 42.

† In conversation with the senior editor, Mr. Parker mentions (see his travels, p. 108) seeing the salt a mile off on the left; he was too ill to climb up to it, but the Indians procured some for him, which was pure and crystallized.

feet. At about thirty feet, gypsum has been struck in all the wells, and in the salt-rock well has continued down to the solid bed of salt-rock, occasionally changing to slate and thin veins of blue soft clay. At about two hundred and thirty feet, they arrived at the first symptoms of salt, which from plaster, slate-rock and salt, gradually changed, the salt-rock assuming the predominant part.

The writer remarks, "we have for thirty feet, continued through this substance, and now find the slate-rock gradually intermixing with it." Several wells have been bored at different times, within short distances of each other. It is said, that from one of them, some years ago, salt borings were extracted; salt water was found at two hundred and twenty six feet deep.

From a single well, sufficient salt water is now extracted in twenty four hours, to afford one thousand bushels of merchantable salt, which more than supplies the demand in that region of Virginia.

The salt forwarded to us by Mr. Taylor, is highly crystalline in its structure, and except a red color, (obviously derived from iron,*) and occasional fragments of rocks mixed with it, appears to be very pure. Its taste is decidedly and purely saline without bitterness; when pulverized, the red color almost disappears and it is tolerably white. Some small pieces were perfectly white. Specimens of gypsum were enclosed in our box; they are of a very decided character—are finely granular in structure, and of a grayish white. We have no account of the other rocks found with the salt, but from the fragments intermixed presume, that sandstone and marly clays are among them.

P. S. A second letter from Rev. S. Taylor, dated May 31, states, on the authority of Mr. W. Findley, who is the proprietor of the well in which the salt is found, that in sinking the well, they penetrated earth and rock about fifty or sixty feet, when they came upon the plaster, through which they passed about one hundred and fifty or one hundred and seventy feet. They then struck upon the bed of salt, and penetrated it about fifty or sixty feet without reaching the salt water; they then abandoned the digging, applied the auger, and bored about ten feet more, as he supposes, through the salt, but the mixture of salt water renders it uncertain. The roof then appears to be gypsum, but the floor is unknown.

24. *Proceedings of the Academy of Natural Sciences of Philadelphia.* Nos. 1 and 2, for March, April, and May, 1841.—This long established and useful institution has commenced the publication of a bulletin in monthly parts, containing notices of all the important doings of the Society, with a list of donations to the library, museum, &c. This plan was adopted by the American Philosophical Society, about two years since, in

* It is now ascertained that rock-salt is sometimes colored by animalcules.

accordance with the practice of all the important European societies, and our readers have had an opportunity of judging of its effects by the full abstracts from them which have been published in this Journal.* The usefulness and energy of scientific societies depends very much on the frequency of their contact with the public, and the announcement of their proceedings at short intervals, assures the distant members and the public, of their activity in the cause of science. We shall have occasion undoubtedly to refer to these proceedings again.

25. *A Treatise on the Theory and Practice of Landscape Gardening, adapted to North America; with a view to the improvement of country residences; comprising historical notices and general principles of the art, directions for laying out grounds and arranging plantations, the description and cultivation of hardy trees, decorative accompaniments to the house and grounds, the formation of pieces of artificial water, flower gardens, etc.; with remarks on Rural Architecture;* by A. J. Downing. New York and London, Wiley and Putnam. 1841. pp. 451, 8vo.—The nature and scope of this work may be learned from the copious title-page, which, indeed, would seem almost to supersede the necessity of a table of contents. We notice, with much gratification, the unusually beautiful manner in which this interesting volume is "got up," as the phrase is. It is printed on paper of excellent quality, (for this country,) and illustrated with seventy eight wood engravings, besides the vignettes at the commencement of each chapter, and an engraving on copper in the frontispiece. Some of the wood cuts, as the figure of the Kentucky Coffee Tree, the Charter Oak at Hartford, the residence of Washington Irving, and of the author's own residence at Newburgh, New York, are very creditable specimens of the art. We regret that our limits forbid our entering, at present, even upon a cursory examination of this highly interesting and useful work, which, while it presents an encouraging view of what has already been accomplished in this country, will doubtless greatly contribute to the diffusion of correct taste, and the promotion of this elegant art amongst us. The "hundreds of individuals who wish to ornament their grounds and embellish their places, but who are at a loss how to proceed, for the want of some *leading principles,*" may here meet with the information they need; and Mr. Downing's volume, the first American treatise on landscape gardening, will at once take the rank of *the* standard work upon the subject.

* These abstracts are complete in this Journal to the beginning of this year, and the Society have published them as late as June.

Painted by S.F.B. Morse P.N.A.

Engraved by Daggett, Hinman & Co.

Sheldon Clark

THE

AMERICAN

JOURNAL OF SCIENCE, &c.

Art. I.—*Notice of the late Sheldon Clark, Esq. of Oxford, Connecticut;* by Prof. Silliman.

The individual whose portrait is annexed, was an efficient friend of learning; he fully appreciated the value of a superior education, both in literature and in science, and to that end contributed, munificently, of his means, thus acting as a benefactor to his country and to mankind. He is therefore entitled to a memorial in this work, and the humble farmer of a quiet agricultural village, is thus enrolled among those to whom future generations will ever be owing an uncancelled debt, whose interest they must pay in worthy deeds.

Mr. Sheldon Clark was born in the town of Oxford, fourteen miles west of New Haven, January 31, 1785, and died April 10, 1840, aged 55 years. His parents and friends were of respectable standing among the farmers of that region, and he having lost his father when very young, was early adopted by his grandfather, the late Thomas Clark, Esq. of Oxford, of whose family he then became a member, and with whom he remained until the death of this venerable relative, at the age of 82, April 5, 1811.

The grandson, during his minority, manifested a decided inclination for a liberal education, but his grandfather, wishing to confine his attention to rural labors, and the rudiments of a common school education, would not consent to a course which appeared to him to involve a waste of time and money; and he does not appear to have granted him any extraordinary opportunities for education, except a period of instruction at Litchfield, South Farms, in 1805 and 1806. But the active mind of young Clark could not be restrained, entirely, within the prescribed bounds; he diligently read such books as he could find, and early indulged in habits of intellectual exercise—reflecting and reasoning upon a wide range of topics, and often adopting peculiar views of his own, in neglect of those more generally received, or in opposition to them. This independence of thought he carried through life, and it appears to have been the more firmly established, because his solitary walks did not lead him to contend with intellectual rivals and competitors; he measured himself by himself, and thus became confident in the correctness of his own opinions.

This independence appears to have led him to seek those opportunities of obtaining knowledge which were denied him by his aged friend. Soon after his grandfather's death, he first called on me, and introduced himself, as one desirous of acquiring learning. The impression made by his first appearance was pleasing. He was a florid, handsome young man, well dressed, of plain but respectful manners, very intelligent,—having the appearance of one conscious of mental power, and extremely desirous of making additional advances in knowledge. He stated, that by the prudential views of his grandfather, he had been debarred from obtaining a liberal education, and eagerly inquired, whether there was any thing in Yale College to which he could have access, without being a regular member of the institution. He was, of course, encouraged to expect the use of the libraries, with access to the lectures on science, and a hope was held out that he might, perhaps, by special favor, be admitted to the recitations and discussions of the senior class, under the direction of President Dwight. The death of his grandfather a short time before, left him the liberty to pursue his own course, and the means also of doing it, as he gave him his estate, except a few legacies. Accordingly, in the succeeding season of 1811–12, resorting to New Haven, he passed the autumn and winter and part of the spring in a course of study, connected with the recitations and discussions of President Dwight, and with the lectures in the departments of natural philosophy and chemistry. He took notes of what he heard, read and saw; the questions agitated in the discussions of the senior class, with the decisions of the president upon them, are recorded in his note-book, as are the texts and doctrines of the sermons in the college chapel; and there are memoranda, but less extensive, of the topics canvassed in the lectures on science.

Among his numerous manuscripts, recording the lucubrations of his own mind, there is a remarkable tract, dated at New Haven, January, 1812, during this winter's residence here, giving an account of a dream or vision of the general judgment. It is not impossible that it may have been suggested by Bunyan's Pilgrim's Progress. Without its quaintness, it is written in the same style of sustained allegory, and it is carried on with remarkable unity of design. The language is elevated and beautiful, the imagery splendid and sublime, and with slight corrections, it would form an interesting literary fragment. It is remarkable also for deep seriousness,—the most reverential exhibition of the heavenly world, and of the antecedents, concomitants, and results of the judgment of the great day.

Ten years rolled on their course, and I knew nothing more of Mr. Clark than that he was occupied with his rural labors in the summer, and with teaching the district school in the winter. He was occasionally seen in town, and sometimes brought me minerals for inspection; but no hint escaped him of what, as afterwards appeared, was passing in his mind, and which he brought forth in an intelligible form towards the close of 1822, when he called on me, and solicited a private interview.

In this interview, which took place in the office of the laboratory of Yale College, no one being present but Mr. Clark and myself, he stated, that the death of his grandfather had put him into possession of about twenty thousand dollars, which, by his industry and economy, he had increased to twenty-five thousand —that he had no family, and might never have one—that his relations were numerous, that were his property divided among them, the dividend of each would be small, and that he was therefore disposed to appropriate, at least a part of his estate, to the encouragement of learning. In further explanation of his views, he expressed himself in terms similar to those which he used, ten years afterwards, in reply to the senior class of 1833, on the occasion of their presenting him a letter of thanks for his telescope.* The letter of the class is annexed. I cannot do better, therefore, than to allow him to speak for himself by a quotation from his answer ;—omitting his glowing acknowledgments to the class, he says,

"Oxford, Nov. 29th, 1832.

"Respected Friends—Man is a child of circumstances. While some are born to ease and plenty, seldom meet with disappointments, are surrounded by benevolent friends, always ready to assist, to comfort, and to afford them the most ample means of enjoying the highest degree of mental culture; others are born to poverty and servitude, unassisted, even by their nearest relatives, and denied the privilege of obtaining a good common school education, and are often dispirited by disappointments.

"It was my destiny to belong to the latter class. Early in life I had a tender father, who was in possession of a large amount of property. He intended, and often promised, that I should have a liberal education—but, alas, before I was old enough to prepare to enter College, he died, and the *estate proved to be insolvent.*

"Thus all my fond hopes of having a liberal education were frustrated, and I was left fatherless and penniless in a hard, unfeeling, selfish world, to provide, by my own industry, to satisfy those positive wants congenial to poor human nature. It fell to my lot to live, till I was of age, with my grandfather, a hard working, parsimonious farmer, but I was allowed the privilege of reading occasionally, on Sundays, stormy days, and in the long nights of winter. From these opportunities of reading, I was soon convinced that the power, the honor, and

---

* "Yale College, Nov. 17th, 1832.

"Respected Sir—After enjoying the rare privilege of looking through the large telescope, some acknowledgment to the liberal and high-minded donor, was deemed proper and requisite.

"Not because we supposed the simple expression of our feelings would add lustre to the deed; but, that you, Sir, might know that your generosity was not bestowed on individuals ungrateful for the gratification which they have experienced, and the enlargement of their views in "the sublime science."

"Actions distinguished for generous munificence, and designed to accomplish a useful and exalted object, claim the admiration of all; but from those who receive the benefit, they demand not only admiration but gratitude. And considering the circumstances, where shall we look for one which displays this noble quality in a stronger light? Where for one intended to effect a nobler object than the extension of scientific knowledge?

"These, Sir, are the sentiments the undersigned committee for the senior class would express, at the same time wishing you all manner of happiness.

Cornelius Van Santvoord,
Samuel Wolcott,
John C. Beach.

"To Sheldon Clark, Esq."

glory of nations, consisted in, and depended upon, their great men. What has Greece, or Rome, or any nation of antiquity transmitted to posterity, worthy of esteem and admiration, but the achievements of their heroes, and the productions of their artists, poets, and philosophers? And what else can we transmit to succeeding ages, to distinguish us from the unlettered savages that roamed at large in the uncultivated wilds of America when discovered by our fathers? Full of this idea, and animated with an ardent desire to promote the honor and happiness of my own native country, I felt determined to do all I could to patronize and encourage literature and science, to provide the means of affording our literary and scientific genius a finished education.

" Oft when toiling with ceaseless assiduity to accomplish that object, I have been pointed at, by my fellow-citizens, with the finger of scorn, and taunted by the tongue of ridicule. But for all this I felt a reward in the anticipation of promoting the honor, and glory, and happiness of my beloved country. I never dreamed of personally receiving the grateful acknowledgments of one of the most respectable collegiate classes in the world. This I assure you, my dear friends, is a full, a rich compensation for all the labor, the hardships and privations I have suffered.

As honor, and glory, and happiness, are the only objects worthy of the attention of wise and intelligent beings, I have no doubt that they will be the chief objects of your pursuit. From the sentiments expressed in your kind and interesting letter, I presume that some of you are highly gratified with the study of the " sublime science."

Mr. Clark informed me, that the first conception of his plan took place during the season of his residence in Yale College, when he was attending in several of the college class-rooms, and that he had been maturing it ever since. In a rugged country of stony hills, he had followed the plough—he had fattened droves of cattle—he had taught school in winter, and loaned money at all times—not to accumulate wealth for himself, but to promote the good of others. He appealed to me as to the propriety of his views, and it is quite unnecessary to say that I encouraged them, remarking at the same time, that he alone must be the judge of his own obligations to his family friends, with which no one, and certainly not myself, would wish to interfere.

Mr. Clark having made up his mind, submitted through me, a proposition to deposit five thousand dollars, to be placed at compound interest, until it should become the foundation of a professorship. In his written communication, he requires, that at any time after the expiration of twenty-four years "from the time of receiving the money, he shall have the right to appropriate the sum of twenty thousand dollars for the establishment of a professorship, either of moral philosophy and metaphysics, of chemistry, or of natural philosophy, in the college, at his option."

A special meeting of the Corporation of the college was called at Hartford, May 8th, 1823, and the proposition being accepted, the money, or its equivalent, was conveyed, June 10th of the same year, to the Hon. James Hillhouse, the treasurer.

Eighteen years of the time have already passed away, and six more will finish the allotted term; in 1847, the "Clark professorship of moral philosophy and metaphysics," designated by him subsequently to his gift, must be established on his foundation. The duties of this professorship are at present fulfilled by the president of the college.

The community received with much satisfaction, the news of Mr. Clark's bequest;—he was of course hospitably received by the president and professors, in their houses, being introduced also to their literary and other friends, and to distinguished strangers. Without doubt, he was much gratified by the approbation and celebrity which his donation (then the greatest, save one, that the college had ever received from any private source) procured for him, and he must have differed from other men, if he had not been, on this account, the more confirmed in his liberal purposes.

Mr. Clark was so well satisfied with the provision which he had made for a professorship, that he soon followed up his first donation, by a second. At a meeting of the corporation of Yale College, in New Haven, Sept. 8th, 1824, it was reported by the treasurer, that Mr. Sheldon Clark had deposited with him the sum of "one thousand dollars for the purpose of establishing a scholarship, or scholarships, in the institution." It was stipulated, "that the thousand dollars deposited by said Clark shall be put to interest, upon good security, for twenty-four years from the 10th day of June, 1824, and at the expiration of that time the corporation of the college shall appropriate the sum of four thousand dollars for the purpose of founding a scholarship, or scholarships," under certain conditions and regulations. These are stated in detail by the donor; two scholarships are to be created—the first to take effect in the class that shall be graduated in 1848, the second in 1849—the boon to be conferred on the best scholar, as ascertained by examination, or by lot in cases of equal merit, among those who apply; each successful candidate to enjoy the income of two thousand dollars for two years, upon condition of pursuing a prescribed course of study, and to reside in New Haven nine months in each year,—upon failure of candidates, the income is to be appropriated in premiums for the encouragement of English composition, or other branches of learning among the undergraduates of the college. The donor, very wisely, adds a dispensing clause, "that the corporation of the college, in whom he reposes special confidence, may, from time to time, make such change in the foregoing regulations as they shall judge best calculated to promote the main purpose for which the donation has been made." Upon these conditions, the donation was accepted by the corporation, and will of course, when the prescribed term is completed, be carried into effect.

This new instance of liberality gave additional pleasure to the friends of learning and education, and produced an active interest in favor of the retired individual—the hard-working Oxford farmer, whose example had now placed him at the head of the benefactors of an ancient literary institution.

It appears to be incident to human nature, that he who has done either good or evil, especially in a signal manner, is much more prone to go and do likewise again. Mr. Clark, from his acquaintance with the officers of the college, took a particular interest in that painful catastrophe—the wreck of the Liverpool packet-ship Albion, at Kinsale in Ireland, in April, 1822, which deprived the college and the country of a man who, for his early years, left no superior behind him. With the lamented Fisher was lost the large telescope of the college, which he was taking with him to London, to have it put in the best order, for his observations on his return. Mr. Clark, on being informed of this loss, conceived the idea of replacing this valuable instrument with one still better. It was not for him, like a capitalist in a great emporium, simply to will, and then to draw a check for the amount. His contributions (since he did not, in making them, diminish his patrimonial estate derived from his grandfather) were drawn from the results of his own industry and economy, often rendered in small sums as he could obtain payment for his commodities, or collect the dues on outstanding notes. The treasurer's receipts for the money for the telescope lie before me, and may be worth copying, as an example of the manner in which an industrious and frugal farmer, by the use of moderate means, was able to accomplish an important end.

Payments by Sheldon Clark to the Treasurer of Yale College for a telescope.

February 22d, 1828,	- - - - -	$100
March 7th, "	- - - - -	100
April 16th, "	- - - - -	200
May 26th, "	- - - - -	50
October 28th, "	- - - - -	100
November 5th, "	- - - - -	142
November 11th, "	- - - - -	88
" "	- - - - -	100
December 1st, "	- - - - -	120
August 26th, 1829,	- - - - -	200
		$1,200

Of this donation, nineteen guineas were, by order of the donor, employed in the purchase of a pair of large globes by Carey, (twenty-one inches in diameter,) one celestial and the other terrestrial, elegantly mounted and covered. The telescope* was ordered of Dollond. Captain Basil Hall happened to be at the college at the time, and kindly volunteered to give his personal attention, with the maker, to the execution and arrangement of the instrument. Mr. Clark limited a period of two years, within which it was to be done or the money given by him was to be returned. It arrived in November, 1829, and was pronounced by Dollond to be "perfect, and such an instrument as he was pleased to send as a specimen of his powers." In a letter of September 3, 1835, Prof. Olmsted announces to Mr. Clark the dis-

* This telescope, (which, out of respect to the donor, is distinguished by the name of *Clark's Telescope*,) proves, says Prof. Olmsted, to be an excellent instrument. It has a focal length of ten feet, and an aperture of five inches. The object-glass is finely achromatic, and the light is very pure and abundant. For objects that require a fine light, as some of the nebulæ and smaller stars, this instrument exhibits great superiority, and its defining power is equally eminent. It has a good variety of eye-glasses, and a spider-line micrometer of the best construction. On favorable evenings, the views of the moon when in quadrature, of the planets, and of the milky way, are superb.

covery by his telescope of Halley's comet*—the first observation of this comet that had been made in this country, although astronomers in various parts of the United States had been on the look-out, without success; this discovery was justly regarded, "as proof that the instrument was a fine one for observations of this sort."

Mr. Clark was now fully before the public as a munificent patron of learning, and was justly so regarded, especially if his education, his position, his pursuits and his means are duly considered. His feelings were, in a good degree, identified with the prosperity of the college. He attended, occasionally, its quarterly exhibitions and its commencements; he passed some days or weeks, every winter in New Haven, frequenting the college halls and the society of its officers and their families, and returned with renewed zeal to his agricultural labors.

Mr. Clark was elected by his fellow-townsmen, a member of the State Legislature for 1825, as well as in several succeeding years, and served honorably during the sessions at Hartford and New Haven.

A portrait of Mr. Clark was requested for the College, and one was presented by him. It was painted by Samuel F. B. Morse, Esq., and was a very successful effort; the likeness was accurate, and although painted in 1825, sixteen years before his death, it remained good to the last, except that he had grown corpulent. It hangs in the south room of the Trumbull Gallery.

It appears that Mr. Clark had made up his mind how to dispose ultimately of his estate, before he could possibly know the effect of his actual donations on his fame, for his will was made and deposited with me, before the first of those donations had been accepted by the Corporation of Yale College, or made known to the public. The will, duly executed, bears date March 8th, 1823, just two months before the meeting of the Corporation, at which his first proposition, that for founding a professorship, was accepted. Here again his own language shall explain his views, as expressed in that solemn moment, when men look death in the face, and record the purposes that are to be fulfilled when they are in their graves.

---

* By Prof. Olmsted and Prof. Loomis, now of the Western Reserve College at Hudson, Ohio, then a tutor in Yale College.

"Knowing the uncertainty of life—thinking that we must always be prepared to die—feeling that it is our duty to do all the good in our power, and believing that part of my property will do more good if given to encourage literature than it would to descend according to law, I, Sheldon Clark, of Oxford, am voluntarily and of my own accord, disposed to make the following will.

"I wish to be buried in a decent manner, and to have decent grave-stones at the discretion of my executors. It is my will, that my just debts and my funeral expenses be paid out of my movable estate. I give and bequeath to the Corporation of Yale College in New Haven, all my homestead farm where I now live, with its buildings and appurtenances—also, all the land that was given to me by my grandfather, Thomas Clark, Esq., on the east side* of the road that runs north and south of Mr. Samuel Tucker, with its buildings and appurtenances—also, all my land that lies north of the road that runs by where George Drake now lives—also, my meadow that lies a few rods west of Rimmon school-house, and also, all my Red Oak farm, &c.

"Funds being so liable to be lost by bad security, it is my will, that the lands I have given to said Corporation shall never be sold, but that they shall be let or rented, in such way and manner, as the President and Fellows of said Yale College, and their successors, forever, shall judge to be for the best interest of said institution. It is my will, that the annual income of said lands shall be annually appropriated for the advancement of literature in said Yale College, in such a manner as its President and Fellows, and their successors forever, shall deem the best and most beneficial for said institution; but no part of said donation or income shall ever be appropriated to erect or repair buildings.

"I also give and bequeath to the Corporation of Yale College in New Haven, all the money I shall have on hand and all the notes I shall have due me at the time of my decease, (except three hundred and thirty four dollars for Chesnut-tree hill school district,) to be appropriated for the benefit of said Yale College, as its President and Fellows, and their successors forever, shall think shall be for its best good, and the most conducive to its prosperity and honor."

* The house and homestead farm were on the west side of the road.

He then gives in form, and with certain conditions, the above named sum to the Chesnut-tree hill school district. He gives also to his three sisters a valuable farm, which fell to him from his and their brother, besides other lands acquired after his will was made; also, all his personal estate* not otherwise disposed of; and on his death bed he expressed a wish, that the sisters should receive each one thousand dollars, and that the college should pay it.†

He named Abel Wheeler, Esq. of Oxford, and Benjamin Silliman of New Haven, his executors, and as Judge Wheeler was dead, the duties, of course, devolved on the surviving executor.

This will he brought to my house early in the spring of 1823, when he read it to me, and requested me to keep it *sacred and secure.* The lady of the house was also entrusted with this confidence, that the will might be found, if I were gone; by his direction I sealed it, in his presence, and wrote upon the envelope, "the last Will of Sheldon Clark, to be delivered to no one but himself in person, or in case of his death, to be opened by the President of Yale College."

It was now placed in a private drawer in my secretary, in the presence of us three only, and there it remained untouched and unknown, until the melancholy condition was fulfilled. His last visit at my house was in the evening of October 8, 1839, and when he was on the door-step taking his leave, I reminded him of the important document, which, sixteen years before, he had confided to me, and offered to surrender it to him, provided his purpose was changed. He replied, "No, I do not wish to make any alteration," and these (with a warm shake of the hand) were to me his words of farewell. I never saw him more, until I beheld him in his coffin on the 11th of April, 1840.

---

* Furniture, apparel, &c.

† It appears also that he had been liberal in private benefactions to his family. A memorandum in his hand writing now lies before me, dated April 22, 1811, a few days after his grandfather's death, entitled, "an account of money that I have given to my relations out of my own earnings." He then enumerates the sums of $200, $150, $100, $100, $53, and $55,75, amounting to $658,75, which, between April 20th, 1811, and March 20th, 1813, he had given chiefly to female relatives. Among his receipts also, is one for fifteen dollars, being a contribution for a church bell in a neighboring Episcopal society. He was very liberal to the Congregational society to which he belonged, as also to the town of Oxford, and he made frequent donations to individuals.

His death was tragical. Being, the preceding evening, on a scaffolding in his barn, moving on his hands and knees—an infirm pole, concealed by straw, suddenly broke;—being very heavy, a fall of fourteen feet upon a timber and stones, crushed in the sternum and some of the ribs, and he at once pronounced himself a dead man. With difficulty, aided by the arm of an attendant, he walked into the house, and lingered fifteen hours in agony, although with a clear disposing mind; he was able to command his hands to examine his pocket-book and papers—giving perspicuous directions as to his affairs, the college, and his sisters, and then he expired, in the full energy of a strong frame, not so much exhausted even by intense pain, as drowned by the extravasation and secretion of fluids, impeding, and at length arresting the play of his lungs.

Under his extreme sufferings, not a word escaped him as to his future prospects; he remarked only, that he had endeavored to do all the good in his power, and we trust the preceding pages have shewn, that his endeavors were not in vain.

A large concourse of friends and neighbors and people of the vicinage, with several of the officers of the college and the clergy attended him to his last home. A long retinue of rural vehicles wound slowly down the high hills and along the deep valleys to a secluded burying ground, which he had been instrumental in arranging, on a quiet and beautiful plain, shaded by pines and watered by the murmuring current of a branch of the Housatonick. A neat marble slab records his name as "a distinguished benefactor of Yale College." Such indeed he was. His benefactions to the institution, including the funded interest that had accumulated to the time of his death, amounted to full thirty thousand dollars—three times as much as any other individual ever gave.*

Allowing all that belongs to human nature, of the love of present or posthumous fame, (and there is no doubt that Mr. Clark fully participated in this common feeling,) still he has set a noble example, which it is equally our duty and our pleasure to present, in strong relief, to our country and the world. In proportion to his means, few have done as much, and it would not be easy to find those who have done more.

* Only one individual, the late Dr. Alfred Perkins of Norwich, gave $10,000 as a library fund, and there are a few living men who have given from $5000 to $8000 each.

The object was not accomplished without a long course of stern self-denial—with great industry and severe economy. Mr. Clark expended very little on his own personal accommodation. The plain farmer's house remained as his grandfather left it, without decoration and almost without repair; the furniture was of the humblest kind, but a warm welcome was given to his friends and to strangers, with ample provision not only of the produce of a farmer's cultivation and care, but occasionally, with a free hospitality in rarer things.

His policy was, to augment as far and as fast as possible, his productive capital; he attempted no improvements in his agriculture; he hardly preserved fences and buildings *in statu quo;* little return of manure was made to his hard worked soils, and even his wood and timber, were, to a certain extent, sold for money and cleared away for market, by other hands. He kept his money always at work—loaned all the cash he did not need, (and his personal wants were few)—required his interest and payments at the day—but was exactly just in his dealings—prompt to give his advice when desired, and kind in his treatment of all. His hoarding was not for himself; wife and children he had none, and he laid by his thousands—the results not of traffic or speculation, but of laborious thrifty industry—to furnish the means of a superior education to the children of others, and to generations yet unbórn.

His mind appears to have been constantly active, and he has left behind him numerous manuscripts—matter sufficient for sevral volumes. They are on various subjects: banks, the tariff, the sub-treasury, the currency, political economy, commerce, manufactures and agriculture, but above all, morals and metaphysics; the nature and immortality of the soul, moral responsibility, the agency of God in the affairs of the world, the nature of sin and future rewards and punishments, with numerous extracts from distinguished authors, and occasional poetical effusions:—all these, and many things more, are among his papers. Not a few of them are elaborate productions:—manuscripts, carefully copied, sometimes again and again, from less perfect notes and sketches,—in general, written out in a fair and legible hand, with correct orthography and punctuation, and an appropriate selection of words. The writings of Hume, Reid, Stewart, Edwards, Franklin, Jefferson, and other distinguished men, seem to have been familiar to him. The metaphysics of morals appear to have been his favorite field of speculation. His writings are often vigorous and acute, and his reasoning ingenious, but not always conclusive. Some of his speculations were printed, and transmitted by himself to eminent men. Among his letters are many replies, some of them of considerable curiosity on account of both the authors* and the sentiments; we shall venture to cite only one, and that from a man well known to fame.

To Mr. Sheldon Clark.

"Monticello, Dec. 5, '25.

SIR—I thank you for the pamphlets you have been so kind as to send me; but I cannot comply with your request to give my opinion of them; against this, I have been obliged to protest in every case. I should otherwise, for the last twenty years, have been constantly employed in the trade of a reviewer of books, for which I have neither taste, talent, nor time; and instead of reading according to my own choice, my course of reading would have been wholly under the direction of writers and printers, on all sorts of subjects. No mail comes without bringing me more than I could review before the arrival of the next.

A second reason is, that I revolt against all metaphysical reading, in which class your 'new pamphlet,' must at least be placed. Some acquaintance with the operations of the mind is worth acquiring, but any *one* of the writers suffices for that. Locke, Kames, Hartley, Reid, Stewart, Brown, Tracy, &c., these dreams of the day, like those of the night, vanish in vapor, leaving not a wreck behind. The business of life is with matter, that gives us tangible results; handling that, we arrive at the knowledge of the axe, the plough, the steamboat, and every thing useful in life; but, from metaphysical speculations, I have never seen one useful result.

Your second pamphlet, entitled 'Essays,' is certainly on important subjects, moral or physical, according to our individual creed. I dipped into them in several places, and found in them views both profound and instructive, and, but for my first reason above stated, I should say more on them. Persuaded that he who wrote them will perceive the reasonableness of my declining this office, I pray you to be assured of my great respect. TH. JEFFERSON.

His bounties to Yale College, procured for him applications from other institutions, and a number of students in different colleges, and other persons, as appears by his correspondence, were suitors for his largesses. As there are no records of answers, we

---

* In several remarkable cases, from persons still living.

are left to presume, that the applications were generally allowed to pass *sub silentio.*

As my object in this brief memoir is to present Mr. Clark to mankind as a liberal patron of good learning, I shall offer no opinions upon his views of moral and metaphysical subjects; but some judgment may be formed of them from the pamphlets which he printed and distributed.

It is however probable, that had his warm aspirations after a liberal education been indulged; had he been disciplined in courses of exact study, in literature and in science;—had he been brought into conflict with other minds, pushing forward on the same journey—had he been placed under the pressure of able instructors in the various branches of human learning, and beneath the sunshine of a kindly Christian influence and example, happy fruits would have been gathered from his gifted intellect.

In his laborious rural employments, he sighed for something more liberal and elevated than his daily toil—in his comfortless seclusion, (without that friend who, more than any other, cheers man's solitude, and influences his moral feelings and sentiments,) he found no favor to his mental efforts, and his vigorous mind shot in wild luxuriance, with little external culture and as little pruning.

While therefore his speculations always indicate ingenuity, and he seizes his subject with a vigorous grasp, it is not surprising that he, like many men of higher name, should sometimes have been bewildered in the mazes of his own metaphysics;—as a pilgrim threading a deep forest, wanders around and around, and emerges at last at a mistaken point, or emerges not at all, but plunges deeper and deeper, into the gloom of the dark and impervious wilderness.

ART. II.—*First Anniversary Address before the Association of American Geologists, at their second annual meeting in Philadelphia, April* 5, 1841; by EDWARD HITCHCOCK, LL. D., Prof. Chem. and Nat. Hist. Amherst College.

GENTLEMEN OF THE ASSOCIATION:—It may be expected on this occasion, that I should give some account of the origin and progress of this society. The history is short. A number of geologists had for years been employed in prosecuting geological surveys in many widely separated states of the Union, and as they were bringing their labors towards a close, they felt a strong desire to compare notes with one another, that they might clear up points obscure in the districts which they had examined, but which might perhaps be fully developed in others, and that more uniformity might be secured in the final results. The gentlemen of the New York survey at length issued a circular, inviting those engaged in similar surveys in the other states, to a meeting in this city a year ago. The number that responded to the invitation by their attendance, was not large. But I am sure that I shall speak the unanimous opinion of all present, when I say that the meeting was most pleasant and profitable. It was highly gratifying for those, who had long been engaged in the same work in widely separated fields, and who knew one another only by reputation, to be able to exchange salutations, and hear one another's voices, and share one another's sympathies. Particularly important was it for those of us who are very much insulated from geological society and counsel, to meet those who could solve our difficulties, and by detailing the phenomena of their own districts, could throw light upon obscurities that hung over our own.

Under these circumstances, it is not strange that the present meeting should have been appointed; nor that we should have ventured to invite others to join us, who are engaged in similar pursuits, although not in the state surveys; and some of whom are our seniors in cultivating the noble science of geology.

As to the ulterior plans of this Association, I am not aware that any have been concerted, whatever may be in the minds of individual members. It will be seen that their grand object is to develope American geology in a quiet and unostentatious manner. Whatever measures will promote this object, will meet, I presume, the support of the members;—and whoever has it so much at heart, that he is willing to engage in active and energetic labors to promote its advancement, will doubtless be welcomed to their fraternity. While they acknowledge their indebtedness to similar associations in Europe, for the example which they have set, and especially to the London Geological Society, the noble mother of them all; they do not aspire to be compared to any of them, until the fruits of their labors shall make such comparisons involuntary. They wish to be known only as an association of geologists, who love their favorite science so well, that they will pursue it with almost equal ardor, whether they are noticed or unnoticed, whether patronized or neglected. It is their motto,

Hoc opus, hoc studium, parvi properemus et ampli.

I propose, gentlemen, at this time, to sketch briefly the most important points in American geology, that require your special attention. In doing this, I must of course give some account of what has been already done in this wide field. And as far as possible, I shall treat both of these subjects together.

Until the commencement of the present century, almost nothing had been done by Americans to develope our mineralogy or geology. And until the year 1807, although mineralogy had begun to excite some interest, yet no effort worthy of notice had been made in geology. In that year, William Maclure commenced, single-handed, the Herculean task of tracing out and delineating the great features of our rock formations. This he at length accomplished, after crossing the Alleghany mountains in fifty places. This was certainly a most remarkable example of persevering devotedness to a favorite pursuit; and cannot but embalm his memory in the heart of every American geologist.

We must not presume from this isolated instance, that any correspondent knowledge of this subject existed at that time in our country. On this point we have the striking testimony of one, who is still among us in the vigor of ripe manhood, to witness the wondrous change which his own labors and those of others have produced. "We speak from experience," says Prof. Silliman, "and well remember with what impatient but almost despairing curiosity we eyed the bleak, naked ridges which impended over the valleys that were the scenes of our youthful excursions. In vain did we doubt that the glittering spangles of mica, and the still more alluring brilliancy of pyrites, gave assurance of the existence of the precious metals in these substances; or that the cutting of glass by the garnet, and by quartz, proved that these minerals were the diamond; but if they were not precious metals, and if they were not diamonds, we in vain enquired of our companions, and even of our teachers, what they were."—*Am. Journal of Science, Vol. I, p.* 36.

I cannot, on this occasion, go into minute details of the labors, or even of the names of those, by whom this state of things in a few years was entirely changed. In 1810, appeared the Mineralogical Journal of Dr. Bruce: in 1816, the work of Prof. Cleaveland on Mineralogy and Geology: in 1818, the American Journal of Science was commenced by Prof. Silliman: a work which has always been an efficient instrument in promoting a knowledge of geology as well as other sciences; and which, by great efforts, has now reached its forty first volume. In this connection, the Monthly American Journal of Geology and Natural Science, by Mr. Featherstonhaugh, which reached only its first volume, should not be forgotten. The transactions of several of our scientific societies, especially of the Academy of Natural Sciences in this city, of the Lyceum of Natural History in New York, and of the American Academy of Arts and Sciences at Boston, have contained many most valuable papers illustrative of the geological features of this continent. An American Geological Society was formed in 1818: but it has accomplished little, except that it has a valuable collection of specimens and books, chiefly through the liberality of its president, William Maclure. The Pennsylvania Geological Society was organized in 1832, and published two volumes of its transactions. Several other societies in the country, of a more local character, have contributed essentially to the promotion of geology; and the recent organization of the National Institution for the promotion of science at Washington, and its vigorous commencement, promise much for this branch of knowledge.

But the feature in the history of American geology, to which I feel bound to call special attention, is the institution of state geological surveys by the civil authorities. I regard this feature as peculiarly American, for I am not aware that any general survey of a large district, had been ordered in any other part of the world, till after it had been done in this country. At any rate, sure I am, that it was entirely original with those who introduced it here. North Carolina has the honor of having first directed a survey of her territory. This duty was committed to Prof. Olmsted, who made a report of one hundred and forty one pages, in 1824 and 1825, upon the economical geology of the state. The year following, South Carolina gave a similar commission to Prof. Vanuxem, whose report was published only in the newspapers. An interval of five or six years succeeded, before Massachusetts engaged in the work. In 1830, she ordered a survey;—in 1832, an annual report of seventy pages, and in 1833, one of seven hundred pages, with a second edition in 1835, were published. In 1837, a re-survey was directed; in 1838, an annual report of one hundred and thirty nine pages was printed, and the final report of eight hundred and forty quarto pages with fifty five plates, is just completed. Tennessee began the work only two or three years after Massachusetts, and committed it to Prof. Troost, who has published five annual reports in pamphlets of thirty to eighty pages, with a geological map of the state. In Maryland, the work was begun in 1834, and Prof. Ducatel was appointed to execute it, who has made seven annual reports of about fifty pages each, with numerous maps and sections. The survey of New Jersey was ordered in 1835; in 1836, Prof. Henry D. Rogers, the commissioned geologist, made a report of one hundred and eighty eight pages, with extensive sections; and in 1840, his final report of three hundred and one pages, with a geological map of the state and sections. The state of New York was divided into four sections; and Profs. Vanuxem, Mather, and Emmons, with Mr. James Hall, as geologists, Mr. Conrad as paleontologist, and Prof. L. C. Beck as chemist, were appointed in 1836, to survey them. Up to the present time, they have made five reports; the first of two hundred and twelve pages, the second of three hundred and eighty four pages, the third of three hundred and fifty one pages, the fourth of four hundred and eighty four pages, and the fifth of one hundred and eighty four pages. The work is now nearly completed; and the gentlemen are engaged in preparing their final report. The survey of Virginia was committed to Prof. William B. Rogers, who, since 1835, has made six reports: the first of thirty six pages, the second of thirty pages, the third of fifty four pages, the fourth of thirty two pages, the fifth of one hundred and sixty one pages, and the sixth of one hundred and thirty two pages.

Dr. Charles T. Jackson was appointed state geologist of Maine, in 1836, and he has since made three reports; the first of one hundred and twenty eight pages, the second of one hundred and sixty eight pages, and the third of three hundred and forty pages. He has also surveyed the public lands of Maine and Massachusetts, and made two reports. In 1839, the same gentleman was appointed to survey Rhode Island; and his final report, of three hundred and twelve pages, with a geological map and sections, appeared in 1840. In 1840, he was commissioned to survey New Hampshire, and his first annual report will soon appear. The survey of Connecticut has been made by Dr. J. G. Percival and Prof. Charles U. Shepard. The latter made a report in 1837, of one hundred and eighty eight pages, upon the economical mineralogy of the state. The report of the former gentleman has not yet been published, but is expected in the course of the ensuing year. The survey of Pennsylvania was begun in 1836, by Prof. Henry D. Rogers, who has made five annual reports; the first of twenty two pages, the second of ninety three pages, the third of one hundred and nineteen pages, the fourth of two hundred and fifty two pages, and the fifth of one hundred and seventy nine pages. The survey of Ohio was committed to Prof. Mather, as principal geologist, assisted by Dr. S. P. Hildreth, Profs. John Locke and J. C. Briggs, and J. W. Foster. Their first report of one hundred and thirty four pages, was made in 1837, and their second of two hundred and eighty six pages, with numerous drawings, in 1838. Delaware commenced this work in 1837, under the direction of James C. Booth, Esq., who has made two annual reports of a few pages, and his final report of one hundred and eighty pages, is nearly through the press. In Michigan, the survey was committed to Douglass Houghton, Esq., with assistants. His first report of thirty seven pages, was made in 1838, and his three subsequent ones of one hundred and twenty three, one hundred and twenty four, and one hundred and eighty four pages, in successive years. In 1837, Dr. D. D. Owen commenced a survey of Indiana, and he has since published two reports of thirty four and fifty four pages. In Kentucky, the work was begun in 1838, but has yet proceeded no farther than a reconnoissance by Prof. Mather. In Georgia, Mr. John R. Cotting was commissioned in 1836; he informs me, that about half the state has been surveyed; that three section lines, from three hundred to four hundred miles long, have been explored; that "a vast amount of interesting materials, both geological and agricultural, has been collected; and that it is in contemplation to publish a volume of six hundred pages the present year."

In 1834, the United States government directed Mr. Featherstonhaugh to examine, geologically, "the Territory of Arkansas, and the adjacent public lands." He has made two reports, one of ninety seven pages, and another of one hundred and sixty eight pages, with numerous sections. In 1839, Dr. D. D. Owen was commissioned to examine the Territory of Iowa, and his report, in connection with that of Dr. Locke, made in 1840, contains one hundred and sixty one pages. Mr. Nicollet has also been surveying, both astronomically and geologically, the northwestern portion of our country, including a vast region beyond the Mississippi; and his report is now in a course of publication, with a geological section tracing a distance of nearly two thousand miles.

I ought not to omit to mention an act of private munificence, which occurred before any of the state surveys were commenced. The late Hon. Stephen Van Rensselaer directed a geological survey to be made of the entire route of the Erie canal, at his own private expense. This work was executed by Prof. Amos Eaton, who in 1824, published a report of one hundred and sixty three pages, with a section from the Atlantic to Lake Erie. I might also mention with propriety, the surveys of several mineral districts by private companies; such as those of the coal-fields of Pennsylvania, by R. C. Taylor, Esq., and Prof. Johnson; of the gold region of Virginia, and of portions of the coal-fields of Pennsylvania, by Prof. Silliman; of the iron region of Missouri, by Prof. Shepard, &c. But time will not permit me to enter into fuller details.

From these statements it appears, that within the last sixteen or seventeen years, surveys have been commenced in no less than nineteen states, and two of the territories of this Union; embracing an area of nearly seven hundred thousand square miles. For the last four or five years, not less than twenty five principal geologists, and forty assistant geologists, have been constantly employed in the examination of this vast region, under the patronage of the state governments or of that of the Union. In three or four of the states, the surveys are for the present suspended; not, however, from a conviction of their being useless, but from peculiar circumstances. In Massachusetts, New York, Ohio, and Michigan, zoological and botanical surveys are connected with the geological; in Maryland, Ohio and Michigan, there is a topographical department;—and on these various subjects several valuable reports have already appeared, which this is not the proper place to notice.

I ought also to mention here that the British provinces of New Brunswick and Nova Scotia, have been geologically examined by Dr. Gessner, who has made reports. I am credibly informed, also, that the governor general of Canada, will recommend strongly to the House of Assembly at their next session, to order a geological survey of that territory.

Another very important feature of most of these surveys, is the chemical department. In the New York survey, one gentleman devotes himself to it exclusively. In some other states, also, as in Virginia, Pennsylvania, Maine and New Hampshire, laboratories for the sole purpose of analyzing the substances discovered, are fitted up, and one or two chemists are employed in them through the year. The number of analyses already executed in these establishments is immense, amounting to several thousands, and when they are all published, it will be seen that they have a most important bearing, not only in an economical, but also in a scientific point of view.

The annual reports have been confined chiefly to economical geology;—but it was understood from the commencement, that careful attention should be given to the scientific geology of the regions examined, and that the details should be given in the final reports. An immense mass of materials must now be in the hands of the gentlemen concerned in the surveys; and we may anticipate from their publication, most interesting disclosures respecting the geology of this country. Then too, the extensive and complete collections of our rocks, fossils, minerals and soils, which have been made and will be deposited in the capitals of the states, will prove an invaluable treasure. Another important result, which I trust only a few years will see consummated, will be the construction of an accurate geological map of the whole of the United States;—for it can hardly be doubted, that surveys will be soon ordered in the comparatively few states that yet remain unexamined;—or if they should not, if I form a right estimate of the spirit that actuates American geologists, a work of such importance will not be left incomplete. But the liberal feeling that has led so many of our state governments, within a few years, to do so much for geology, forbids the idea that any of this work will be left for volunteer labor. I cannot but feel, that the liberal governmental patronage which geology has of late received among us, and the fact that this patronage has come from all classes in the community, should make us justly proud of the enlarged views and extensive knowledge displayed by our countrymen. I speak advisedly, when I say, that probably our favorite science is now in this country twenty years in advance of what it would have been, if left to individual efforts.

Let us now enquire how much of American geology has been developed by all the efforts that have been made, with and without governmental patronage. As I must depend for these statements upon what has already been published, I shall of course fall far short of the actual knowledge that is possessed on this subject by individuals.

The primary rocks of this continent, both stratified and unstratified, correspond so exactly with those in other parts of the world, as to be easily identified. For the most part, also, they compose the principal axes of our extended chains of mountains. Thus, we find a range of these rocks, commencing in Alabama, and extending northeasterly, in a belt from eighty to one hundred miles broad, to New York; and thence through New England, occupying nearly the whole surface; and probably from thence to Labrador. In the northern part of New York, a range diverges from that just described, and extends in a westerly and northwesterly direction, till it approaches the Rocky Mountains, which are also primary. Thus the vast basin of the Mississippi, is bounded for the most part, on three sides by primary rocks, while the secondary and tertiary strata are found chiefly in that valley, and on the Atlantic slope, as far north as New York.

Only a small portion of these vast primary deposits has yet been carefully examined;—nor have many features been discovered in them that are very peculiar. The vast deposit of Labrador feldspar and hypersthene rock, in the north part of New York, as described by Prof. Emmons, is one of the most interesting. The same gentleman has, also, given us some remarkable details respecting the occurrence of genuine injected veins of limestone in granite, in the county of St. Lawrence. The facts have led him to discuss the question whether all primary limestones ought not to be classed among the unstratified rocks. This question, I apprehend, we have in this country abundant means of deciding, as we have the analogous question respecting serpentine; since we have numerous and extensive beds of both these rocks associated with the oldest of our strata. That they are metamorphic in a high degree, no one can doubt: nor is it less certain that serpentine connected with talcose slate and gneiss, exhibits numerous divisional planes; and often these are parallel to the planes of stratification in the adjoining strata;—but the question still remains, whether that divisional structure may not be the result of metamorphic agency instead of original deposition.

The northwestern border of the primary stratified belt of rocks, extending from Alabama to Canada, a distance of at least twelve hundred miles, is composed of interstratified beds of talcose and mica slates, gneiss, and granular limestone. I do not doubt (at least, from all that I can learn) that these rocks are continuous over this vast distance; forming perhaps the longest belt of limestone on the globe. A considerable part of this limestone is more or less magnesian; and in many places pure dolomite. It furnishes, therefore, a fine field for studying the phenomena and the origin of dolomitization. As to that portion of this field which has fallen under my observation, I find, that with one or two unimportant exceptions, all the cases of dolomitized limestone occur, either in the vicinity of a fault, or of unstratified rocks, or of the oldest gneiss. The pure dolomite is usually found where there is reason to believe extensive dislocations of the strata occur; and the marks of stratification in the limestone disappear, nearly in proportion to the amount of magnesia which it contains, so that the pure dolomite shows scarcely any traces of it. I doubt not that similar conclusions will follow an examination of other parts of this deposit, so remarkably uniform is the geology of this continent;—and moreover, these conclusions correspond to the history of dolomitization in Europe. They seem to render probable the theory of sublimation from the interior of the earth.

I have noticed another analogous and singular fact in connection with this limestone, and doubt not that it is common throughout its whole extent; although I have seen it mentioned by no one except Prof. Mather, in his account of the rocks of eastern New York; but am informed by Prof. Rogers that it is common in Pennsylvania and Virginia. Where the limestone comes in contact with mica and talcose slates, they are often highly impregnated with carbon, for several feet or rods from the line of junction. There can hardly be a doubt that the carbonic acid, which has penetrated the slates, has been decomposed to produce this result. Farther examination, in other localities, will probably throw additional light on the subject.

The phenomena of dykes and veins, especially along the eastern margin of the primary ranges of New England, are of a highly interesting character. Some of the dykes of greenstone appear to be of great size and extent. Dr. Percival has, with great labor, traced two of these, through gneiss and mica slate, nearly across the state of Connecticut; and I think I have found their continuation across the whole of Massachusetts; nor do I doubt that they extend far into New Hampshire. Already these two dykes have been followed nearly ninety miles in length; and they are usually several rods wide. Their direction almost coincides with the strike of the strata. In Maine, they have been found in great number and extent, by Dr. Jackson, in the primary strata, and they have more distinctly the character of genuine dykes than in Connecticut and Massachusetts.

In some of our sienitic rocks, we find a perfect plexus of dykes and veins. I have examined one spot in the city of Salem with a good deal of attention, and I cannot see why it does not afford us evidence of the protrusion of unstratified rocks at eleven different epochs; admitting that the intersection of one vein by another proves the posteriority of the latter. The dykes at this spot are varieties of greenstone, and the veins chiefly feldspar.

Among the multitude of substances in our primary rocks that deserve further attention, I can here mention only the oxide of tin. Three localities of this mineral, affording however only small disseminated crystals, have long been known in Massachusetts. But within the last year, Prof. Shepard has discovered a more promising locality in Connecticut, and Dr. Jackson another in New Hampshire. The probability is, therefore, strong, that this interesting metal will ere long be obtained from our own mountains.

You will perceive that under the term primary rocks I have included none that are fossiliferous. The latter, especially those usually denominated transition, have, as is well known, an immense developement in our country. A single vast basin, extending from the Apalachian chain nearly to the Rocky Mountains, and from the centre of Alabama, in a northern direction, perhaps even to the Arctic sea, not less than two thousand miles long and twelve hundred broad, and consequently covering about two and a half millions of square miles;—this wide region forms almost one uninterrupted deposit of older secondary or transition rocks; the largest undoubtedly on the globe. Until recently, these rocks could be described only under the vague designation of graywacke. But light is beginning to shine in upon the chaos. The upper member, that which embraces the bituminous and anasphaltic coals of Pennsylvania, Ohio, Indiana, Illinois, Michigan, and Missouri, seems now to be well identified with the coal measures of Europe. This forms a convenient starting point, and all that remains is to compare the groups below the coal, with those similarly situated in other parts of the world. Professors Henry D. and William B. Rogers have divided this vast series into twelve formations; and these, including the coal measures, which make the thirteenth formation, they find to be not less than forty thousand feet thick. Whatever may be their views as to the identity of these groups with rocks described in other parts of the world, they have refrained from expressing an opinion, in their annual geological reports. But other gentlemen suppose they have discovered marks of identity, in respect to several of the groups, too strong to be resisted. It is difficult to read the reports of the Ohio geologists, especially that of Dr. Locke, and those of Dr. Houghton, Mr. Featherstonhaugh, Prof. Troost, and that of Dr. Owen on the mineral lands of Wisconsin and Iowa, and that of Mr. Conrad on the New York survey for 1841, without being convinced that the carboniferous or mountain limestone is extensively developed from Pennsylvania westward at least fifteen hundred miles; while here, as in England, it forms the repository of an immense accumulation of lead ore. Mr. Taylor, I believe, first pointed out the probable existence of the old red sandstone, or Devonian system, in the western part of Pennsylvania and New York; and this opinion becomes more probable, since the discovery by Mr. Conrad, of remains of the *Holoptychus nobilissimus*, a fish very characteristic of the old red sandstone in Great Britain. Mr. Hall makes this group four hundred feet thick, lying immediately beneath the coal measures.

Strong reasons have been presented by Messrs. Conrad and Vanuxem, founded upon a comparison of organic remains, for supposing that a large part of the rocks below the old red sandstone, in the vast area under consideration, and especially in New York, are identical with the Silurian rocks of Great Britain. The former gentleman recognizes all the important subdivisions of this group described by Mr. Murchison, except perhaps the Llandeilo rocks, which are the lowest. The Caradoc sandstone, the Wenlock shale and limestone, and the Ludlow rocks, are distinctly marked.* And in speaking of organic remains, as a means of identifying strata, he remarks, that "an instance never occurs in this country, where the species of one formation are continued into an upper one in such numbers as to cause the least perplexity or dispute regarding its geological age. All the various eras are admirably recorded, each by its peculiar group of animal or vegetable remains; and to him who has carefully studied them, they are quite as intelligible as if the hand of nature had arranged them in a cabinet for his use."—*Am. Journal of Science, Vol.* 35, *p.* 237.

These suggestions open a wide field for investigation. It is one of the most important problems in American geology;—and from the immense extent occupied by these rocks, I can hardly doubt that here will be found the most complete type of the transition formations that has yet been described. Accordingly, in his report for 1841, Mr. Conrad says, that "nature has probably enabled the geologist to apply this classification (of Murchison) in a more clear and satisfactory manner to the rocks of this country than to those of Europe, since the series is certainly more complete, and the organic remains more abundant in species." I

* He says also, that "the inhabitants of the seas (in which these rocks were deposited) have been destroyed, and new creatures succeeded, at five different epochs; and one of these groups is no more to be compared with another, than is the oolite with the green sand formation."—*Am. Journal of Science, Vol.* 35, *p.* 246.

rejoice that the work is in such able hands, and that so many observers are busy at so many points, and on different sides of the vast field.

Besides the principal basin of the transition rocks just described, detached deposits are sometimes met with in our country; as for example, in the eastern part of Massachusetts and Rhode Island;—and I mention this, just to say, that I have recently come to the conclusion, that even that limited district probably contains, in a descending order, coal measures, the old red sandstone, and beneath these, older transition strata.

Are we to infer that the coal-bearing strata once extended over the immense basin of the Mississippi, and that they have been worn away, except in particular districts? I shall not discuss this question: but if the negative be true, we may still lay claim probably to the largest coal-fields in the world. It is a fact of great interest, also, that the coal along the eastern part of the great valley, or in the vicinity of primary rocks, as has been abundantly shown by Professors Rogers and Johnson, is almost destitute of bitumen; and that as we go west, it becomes more and more bituminous. It is an interesting inquiry, whether the coal in the vicinity of the Rocky Mountains exhibits a similar change, as we recede from the chain. It is also a curious fact, that gypsum and salt springs should usually be found below the coal measures in this country, and not above them, as in Europe.

In extensive troughs of the primary rocks, along the Atlantic slope of the United States, there occurs a formation of fine and coarse sandstones and shales, with a predominant red color, associated with beds of limestone and calcareous breccia. The most extensive deposit of this group commences on the Hudson river, in New Jersey, and thence pursues a southwesterly course through that state, Pennsylvania, Maryland, and Virginia, into North Carolina, and perhaps beyond that state. A smaller deposit occupies the valley of Connecticut river, and extends across the states of Connecticut and Massachusetts. A third deposit, according to Dr. Jackson, is found in the eastern part of Maine; and a fourth has been described in Nova Scotia; where, according to Jackson and Alger, it contains gypsum and salt springs, and overlies bituminous coal.

The lithological characters of the rocks in all these deposits, are so similar, that the observer is at once satisfied of their identity. Besides, in all of them we find limestones of a similar character, extensive ridges and dykes of greenstone, and ores of copper, associated with the sandstones and shales;—so that there can hardly remain a doubt as to the identity in age of all these deposits. But can we determine their true place on the geological scale?

The Professors Rogers, who have extensively examined this formation in the middle states, have ascertained that it is more recent than the coal measures; and with commendable caution, they have called it the middle secondary. With less of prudence I long since ventured to denominate it the new red sandstone;—and I hope it is not prejudice which makes the argument in favor of this opinion appear to be now almost complete. A careful comparison of numerous specimens of this formation with a series from the new red sandstone of continental Europe, and Great Britain, shows a striking resemblance in lithological characters. But the argument from the organic remains is the most decisive. In the shales of this formation, especially in New England and New Jersey, have been found numerous specimens of fossil fishes of the genera Palæoniscus and Catopterus, all of which have heterocercal tails.* Now in Europe, Prof. Agassiz finds that such fish rarely, if ever, occur in any rock above the new red sandstone. But in that formation he finds not less than a dozen species of the genera just mentioned. They occur, however, in the coal formations, beneath the red sandstone. But it seems to be admitted on all hands, that the group in our country under consideration, is more recent than the coal measures. And since the heterocercal fishes found in it show that it must be older than the lias, I see no escape from the conclusion that it is the new red sandstone. There are other arguments to the same point; but they are less decisive. Whether we shall find all the subdivisions of this formation in our country that exist in Europe, remains to be seen. I have little doubt, however, that several of them may be easily recognized; as, for example, the variegated marls and sandstone, the new red conglomerate, (*Rothe todte liegende,*) and the *zechstein.*

---

* I am informed by Messrs. Redfield, the father and the son, who have so successfully devoted themselves to an examination of our fossil fishes, that they have found not less than nine species of these genera in our rocks.

Thus far, as we have ascended on the scale of rocks, we have found, if I mistake not, so full a developement of the European formations on this side of the Atlantic, that it would not be strange, if at no distant period, this country should become classic ground for their study. But we now reach a wide hiatus of the extensive groups of the lias, oolite and wealden, which have as yet been scarcely identified on this continent. Humboldt did, indeed, express the opinion, that he had met with the oolite in the equinoctial zone of South America; and Mr. Lea has described some fossils from New Grenadà, in the seventh volume, second series, of the Transactions of the American Philosophical Society, which he refers to the same formation. But Von Buch, in his recent splendid work on some of the fossils of South America, regards them as belonging to the cretaceous group. Mr. Conrad, however, has just announced the existence "of well characterized and undoubted oolite in the state of Ohio."—*Report on the New York Survey for* 1841.

When we rise still higher on the geological scale, we meet with a remarkable group of rocks, occupying a wide belt from New Jersey to Alabama, and much surface also in Mississippi, Louisiana, Tennessee, Arkansas, and, as I am informed by Mr. Nicollet, extending from Council Bluffs on the Missouri, several hundred miles westward, nearly to the Rocky Mountains, all of which was identified, I believe, first by Prof. Vanuxem, with the cretaceous formation of Europe, although it contains no chalk. The subsequent extensive and accurate researches of Dr. Morton, Mr. Conrad, and others, have completely confirmed this opinion;—and it furnishes an interesting example of the value of organic remains in identifying groups of rocks very much unlike in lithological characters. It is another instance, moreover, of the enormous scale on which geological operations have taken place in this country. From the recent memoir of the veteran geologist, Von Buch, just referred to, it appears that this same formation extends through a considerable portion of South America, and decidedly predominates among the secondary rocks of the Andes.

Equally successful, as in the case of the cretaceous rocks, have been the labors of Conrad, Vanuxem, Morton, Lea, the brothers Rogers and others, in developing the tertiary deposits of this country. The most northerly point along our coast where these are found, is the island of Martha's Vineyard, or perhaps Nantucket. Thence in passing southerly, we find them occupying Long Island and the eastern part of the Atlantic states, from New Jersey to Florida, and the southern part of the Mississippi valley. These too correspond to the other features of our geology, in being of vast extent and of decided characters. Three principal groups of these strata, as described by Conrad and Morton, viz. the lower or eocene, the medial, and the upper or newer pliocene, seem to be well made out on this side of the Atlantic. The group named post-tertiary by Mr. Lyell, is found also in the northern part of New York and in Canada, containing shells of a more arctic character than those now living in the same latitudes.

Excepting the remarkable insulated labors of Mr. Hayden, the drift, or diluvium of this country, has, until recently, received less attention than almost any other formation. The same has been true in Europe. This results in part from the fact, that it cannot be successfully studied until the character and limits of all the subjacent formations are well understood. The state surveys, however, have brought to light enough of our diluvial phenomena to show us, that though a difficult subject, it is one of the most interesting in the whole history of our rocks.

It is an important inquiry, whether the phenomena of drift in this country, correspond with those of the eastern continent. Until recently, I confess, I have doubted whether some of the most striking of these phenomena were not much more fully developed here than in most countries of Europe. I refer particularly to the smoothing, polishing, scratching and furrowing of the rocks in place, and to those accumulations of gravel, bowlders, and sand, which form conical and oblong tumuli, with tortuous ridges of the same, and which abound in the northern part of the country, from Nova Scotia to the Rocky Mountains. But the recent investigations and accurate descriptions by Agassiz, Buckland, Lyell, Sefstroom, and others, have satisfied me of the almost exact identity of the facts in relation to drift on the two continents. The resemblance, however, seems to be most complete in this respect between Scandinavia and this country. Except in Sweden, I have not yet seen evidence that the scarification of the rocks is as common in Europe as in New England, where if they were denuded of soil it seems to me, one third of the surface would be found smoothed and furrowed. But it is now found to be very common in Scotland, England, and especially in Switzerland. It appears too, that those countries abound in those peculiar accumulations of gravel and bowlders to which I have referred, and which are now regarded as ancient *moraines*. Bowlders, also, appear to have been dispersed in a similar manner on both continents.

If I do not greatly mistake, the drift of this country exhibits usually the following lithological characters and superposition. The principal mass of the drift consists of coarse sand, pebbles, and bowlders, often several feet in diameter, usually mixed together confusedly, but sometimes exhibiting, at least for small distances, more or less of a stratified arrangement. This mass of detritus, not unfrequently one hundred feet thick, occupies the lowest position; that is, it rests immediately on the smoothed and striated rocks in place. Sometimes there is mixed with it fine sand or mud; and occasionally a limited mass of clay, appearing as if out of its original position. Above this deposit, in most of the larger valleys, as those of the Hudson, Connecticut, and Penobscot, and in many smaller ones, we find horizontal layers of fine blue clay, rarely as much as one hundred feet thick. Above the clay, and of less thickness, we have a bed of sand, becoming coarser towards the top, and exhibiting sometimes at its surface, marks of a stronger movement in the waters by which it was deposited, than could have taken place while the clay was in a course of formation. Scattered over the whole surface, but confined chiefly to the region abounding in gravel, we find insulated blocks, sometimes rounded and sometimes angular.

Now if I have not mistaken the recent descriptions of European drift, its composition and arrangement correspond with those of the drift of this country; and scarcely any thing seems wanting to make out a complete identity.

It is well known that the theory of drift has for some years been the most unsettled part of geology. The mass of geologists have, indeed, admitted that in some way or other, currents of water have been the principal agency employed, because they witness somewhat analogous effects from aqueous action; and, until recently, no other power of adequate energy and extent has been known to exist. Hence they have been willing to retain the term *diluvial*, as a generic expression, implying simply aqueous agency in general. Yet so many difficulties attend any theory of mere currents, that many geologists have become sceptical in regard to every particular theory that has been proposed. I confess myself to have been long of that number. Yet it has seemed to me of useful tendency to make isolated inferences from the facts developed; and although they may seem to favor rival hypotheses, and will need modification, as new light falls on the subject, yet they will form the elements out of which a legitimate theory will ultimately spring. Allow me to present for your consideration, a summary of the most important of these inferences, as they have been developed to my own mind in examining the diluvial phenomena of this country.

In the first place, these phenomena must have been the result of some very general force, or forces, operating in the same general direction; that is, southerly or southeasterly. For in a southerly direction has the drift been so uniformly carried, and the furrows and scratches on the rocks so generally point southerly, that the force which produced these effects must have tended thither. Our valleys have, indeed, considerably modified the course of the drift; but not enough to contradict the general statement. It would be strange if careful examination should not discover here, as in the Alps and in Great Britain, that the moving force had sometimes been exerted outwardly from the axes of high mountains. But I am not aware that as yet any facts of importance in favor of such an opinion, have been brought to light. At any rate, the evidence of a force urging detritus and bowlders in a southerly, or more strictly in a southeasterly direction, is too marked, and has been noticed by too many independent observers, over a breadth of nearly two thousand miles, to be doubted; even though local exceptions should be discovered;—and such a uniformity of direction over so vast an area, indicates a very general agency.

Secondly, this agency has operated at all altitudes, from the present sea level, and probably beneath it, to the height of three thousand or four thousand feet. In New England, most of our hills and mountains, not excepting insulated peaks, not higher than three thousand feet, are distinctly smoothed and furrowed on their tops and northern slopes, and upon their east and west flanks, to the bottom of the lowest valleys. Dr. Jackson supposes he has found transported detritus on Mount Katahdin, four thousand feet high. But he could discover no marks of this action at the summit of the White mountains of New Hampshire, which are six thousand two hundred and thirty four feet high; although the nature of the rock there, is most unfavorable for preserving furrows and markings.

Thirdly, the smoothing and furrowing of the rocks exhibits almost equal freshness at all altitudes, which indicates an approach to synchronism in the producing cause.

Fourthly, the almost perfect parallelism preserved by the grooves and scratches over wide regions, shows that they were made by the projecting angles of very large and heavy masses of great extent, moving over the surface with almost irresistible force, by water or some other mighty agent. There is sometimes more than one set of scratches, which intersect one another at a small angle, as has been shown by Prof. Locke to occur in Ohio, but each set preserves its parallelism most perfectly. Even where they pass over high and precipitous ridges, they are rarely turned out of their course.

Fifthly, this agency appears to have been less and less powerful as we go southerly. We have as yet, indeed, had but few trusty reports on this subject from the southern portions of North America; but had the phenomena of drift been as striking there as in New England, New York and Canada, they would certainly, ere this, have been described. It ought not to be forgotten, however, that De la Beche has described the drift of Jamaica as very similar to that of New England.

Sixthly, the relative levels of the surface have not been essentially changed by vertical movements, since the epoch in which this agency was exerted. They could not have been much changed without disturbing the detritus, often fancifully arranged in the valleys and on the flanks of the hills; nor without sometimes breaking up the smoothed and furrowed surfaces of the rocks along their joints or planes of stratification. But such a disturbance I have never witnessed.

Seventhly, the North American continent must have attained essentially, its present height above the ocean, previous to the exertion of this agency. For all our formations, as high at least as the eocene tertiary, are covered with drift; and I know of no evidence of any important uplift subsequent to that which has tilted up our tertiary strata. This work, therefore, could not have been accomplished while the continent was beneath the ocean. Other evidence of this position might be adduced, did time permit.

Eighthly, water must have been one of the forces employed in this agency. The regular deposits of clay and sand which form the upper part of the diluvial deposit, must surely have been accumulated at the bottom of bodies of water, which have subsequently been drained off. Much, also, of the finer part of our drift is more or less stratified, and exhibits that oblique lamination which is peculiar to aqueous deposits. Nor can I conceive of any other mode in which detritus has been transported hundreds of miles, as ours has been, but by the aid of water; although this alone could not do it. In New England, we have been able to trace erratic blocks not more than one hundred or two hundred miles, because we then reach the ocean. But in the central parts of the country, I am informed by Prof. Mather, that the primary bowlders from Canada and the western part of Michigan, are found as far south as the river Ohio; which would make their maximum transit from four hundred to five hundred miles; about the same distance as the bowlders from Scandinavia have been carried into Germany. What agency but water could have effected such a transportation?

It is very natural, also, to ascribe the smoothness and furrowing of the rocks to the action of water. But I have in vain examined the beds of our mountain torrents and the shores of the Atlantic, where the rocks have been exposed to the unshielded and everlasting concussion of the breakers, and can find no attrition that will compare at all with that connected with drift; and I am satisfied that to explain it we must resort to some other agency.

Ninthly, ice must have been another agent employed to produce the phenomena of drift. What else could have transported large blocks and gravel over such a wide space as has been mentioned, and have lodged them too, upon the crests of narrow and precipitous ridges; and especially, what other agent could have produced those singular mounds and peculiar ridges of gravel and bowlders that meet us in so many places?

Tenthly, this agency must have been exerted previously to the existence of man upon this continent, and have been of such a nature as to destroy organic life almost entirely. For the remains of man and other existing animals have not been found in drift; but those occurring there belong chiefly to extinct species, while the deposits of clay and sand made during the same period, scarcely contain a species of animal or plant.

Yet eleventhly, this agency must have been comparatively recent. For the disintegration of the surface of the smoothed and furrowed rocks to the depth of half an inch, would usually obliterate all traces of their erosion. Yet in how many places does this effect appear as distinct as if produced during the present century!

Finally, this agency must have been far more powerful than any now operating upon the globe. In the language of Prof. John Phillips, which he applies to the phenomena of drift in general, "such effects are not at this day in progress, nor can we conceive the possibility of their being produced by the operation of existing agencies operating with their present intensities and in their present directions."—*Treatise on Geology, Vol. 1, p.* 296.

Beyond such independent inferences as these, I confess I have been of late years unwilling to go; and have regarded the numerous theories of diluvial action, which have recently appeared, only as ingenious hypotheses. But it is well known that the *Glacier Theory*, originally suggested by M. Venetz, and subsequently adopted by M. Charpentier, and more fully developed of late by Agassiz, is now exciting great interest in Europe. To say nothing of geologists in this country who have expressed themselves favorably towards it;* it is surely enough to recommend it to a careful examination, to learn that such men as Agassiz, Buckland, Lyell and Murchison, after long examination, have more or less fully adopted it; although on the other hand, it ought to be mentioned, that such geologists as Beaumont, Sedgwick, Whewell, Mantell, and others, still hesitate to receive it.

In a country like ours, where no glaciers exist except in very high latitudes, and with the very defective accounts which have hitherto been given of those in the Alps, it is not strange that the attempt to explain the vast phenomena of diluvial action by such an agency, should appear at first view, fanciful, and even puerile. But the recent work of Agassiz, entitled *Etudes sur les Glaciers*, gives a new aspect to the subject. It is the result of observations made during five summers in the Alps, especially upon the glaciers; about which so much has been said, but con-

* See Mr. Conrad's Notes on American Geology, in Am. Journal of Science, Vol. xxxv, p. 237.

cerning which so little of geological importance has been known. Henceforth, however, glacial action must form an important chapter in geology. While reading this work and the abstracts of some papers by Agassiz, Buckland and Lyell, on the evidence of ancient glaciers in Scotland and England, I seemed to be acquiring *a new geological sense;* and I look upon our smoothed and striated rocks, our accumulations of gravel, and the *tout ensemble* of diluvial phenomena, with new eyes.* The fact is, that *the history of glaciers is the history of diluvial agency in miniature.* The object of Agassiz is, first to describe the miniature, and then to enlarge the picture till it reaches around the globe.

The glaciers are vast masses of ice, often leagues in extent, formed of melting and freezing snow, which are sent out from the summits of the Alps by the force of expansion into the valleys below, often to the distance of twelve or fifteen miles. Those elevated and wide *plateaux*, called in Switzerland *Mers de Glace*, exhibiting only one thick sheet of ice, through which the crests and summits of the mountains sometimes rise like volcanoes, are the grand source or birthplace, of the glaciers. In their descent they plough their way through the soil, pile up pebbles and sand along their sides and at their extremities, and even upon their backs; which, upon the retreat, or melting of the glacier, constitute *moraines*, and correspond exactly in composition and shape to those accumulations of gravel and bowlders that have been ascribed to diluvial action. The stones and sand frozen into their lower surface, also, like so many fixed diamonds, smooth and furrow the surface of the rocks in precisely the same manner as they

* I trust that the members of the Association will pardon me for having made some alterations in the form, though not in the leading thoughts, of this part of my Address, since it was delivered. They will recollect, that while I expressed a very favorable opinion of the Glacial Theory, so far as I understood it, I stated that I had not seen the work of Agassiz named in the text. Through the kindness of Prof. Silliman, I have since been favored with the perusal of the copy of this work which, with its splendid alpine illustrations, he received from the author. I am indebted, also, to Dr. J. Pye Smith, of London, for an abstract of the papers of Agassiz, Buckland and Lyell, read before the London Geological Society last autumn, on the ancient glaciers of Scotland and England. A flood of light having thus been unexpectedly thrown in upon my mind, I am free to acknowledge that many of my difficulties in respect to this theory have been removed, and that the great mass of evidence in its favor, thus brought before me, has led me to express a warmer admiration of its leading features and a greater readiness to adopt its leading principles, although satisfied that it will need important modifications.

are abraded over all northern countries. Vast blocks of stone are likewise conveyed without abrasion, by the advance of the glaciers, and lodged in peculiar situations.

From year to year, the evidence has been increasing, of the prevalence of intense cold in northern regions in the period immediately preceding the historic. The elephants and rhinoceros found, undecayed, in the frozen mud of Siberia, the arctic character of the few organic remains found in the post-tertiary strata of Scotland and Canada, and described by Lyell and Bowman, and of the borders of Lake Champlain, as described by Emmons and Conrad; and the great extension of the ancient moraines in the Alps, are the evidence from which Agassiz infers that in that period, all northern countries were covered with a vast sheet of ice, filling the valleys and extending southerly as far as diluvial phenomena have been observed. Glaciers would then be formed on mountains of moderate altitude; and, indeed, he supposes that all the northern parts of the globe might have constituted one vast Mer de Glace, which sent out its enormous glaciers to the south; thus giving the same direction to the drift and the striæ on the rocks. As these vast masses of ice melted away, when the temperature was raised, immense currents of water were the result, which would lift up and bear away huge icebergs, whereby extensive erosions would be produced, and blocks of stone be transported to great distances. Subsequently, lakes would be formed where moraines had produced barriers, clay and sand would there be quietly deposited, and the waters be ultimately drained by the wearing down of the barriers of detritus.

It is doing injustice to this theory to attempt so brief a description of it. A detailed account of existing glaciers, which cannot here be given, forms the best preparation for a just appreciation of the theory. Admitting its truth in the main, let us see how it applies to the phenomena of drift in this country.

In the first place, it explains satisfactorily, the origin of those singular accumulations of gravel and bowlders, which we meet with, almost every where, in the northern parts of our country. I cannot doubt that these are ancient moraines; just such as exist in Scotland and England. Were this the proper place, I could point out a multitude of localities of these, most of which have been a good deal modified by subsequent aqueous agency; but some of them retain the very contour which they had as the ice melted away.* The lateral moraines are perhaps most common, especially if, with Dr. Buckland, we regard our terraced valleys as modifications of these; but I am confident that in our mountain valleys, the terminal and the medial moraine are not infrequent. I have long been convinced that the agency of ice was essential to explain these accumulations; but I was not aware that their antitypes existed in the moraines of the Alps.

In the second place, this theory explains in a most satisfactory manner, the smoothing, polishing and furrowing of the rocks at different altitudes. All these effects are perfectly produced beneath the glaciers in the Alps; nor can I conceive of any other agent by which the work could be executed. It certainly was not done by currents of water alone. One has only to cast his eye upon the splendid plates by Agassiz, of the polish and striæ produced by the glaciers, to be satisfied that the multitudes of examples of analogous phenomena in New England, and in New York and Ohio, as described by Profs. Dewey, Emmons, and Locke, and Dr. Hayes, are precisely identical with those in the Alps.

In the third place, it explains the transportation of bowlders, and their lodgment upon the crests and narrow summits of mountains, and that often without having their angles rounded.

In the fourth place, it accounts for the occurrence of deposits of clay and sand above the drift. For it furnishes the requisite quantity of water to fill the valleys, and the means of damming up their outlets for a season.

In the fifth place, it shows us why these deposits of clay and sand are almost completely destitute of organic remains, either of animals or plants, although probably centuries must have been consumed in their formation.

In the sixth place, it accounts for some rare and peculiar phenomena connected with diluvial action, which seem to me inexplicable on any other known principle. I shall name only two. The first is, that the northern slopes of some of the mountains of New England, although quite steep, and their summits rounded, exhibit striæ and furrows which commence several

---

* Descriptions of some of these with sketches, will be found in my Report on the Geology of Massachusetts, published in 1833: but more numerous descriptions and drawings are given in the Final Report just published. See especially Figs. 15, 19, 73 and 74, of the wood cuts, and plate 3 of the lithographs.

hundred feet below their tops, and pass over them without losing their parallelism; and yet the situation of the drift shows that these markings were made by an ascending and not a descending body. Such might be the effect, if the whole surface of the country were covered by a thick sheet of ice expanding in a southerly direction.

Of the other case, I have met with two examples in New England, and know not that they have been noticed elsewhere. In these cases, the perpendicular layers of argillaceous and hornblende slate, covered in one place, by fifteen or twenty feet of drift, have been fractured to the depth of ten to fifteen feet, so as to be more or less separated, producing horizontal fissures, which are filled by mud, while the laminæ are inclined, at various angles. In short, it seems as if an almost incredible force had been exerted upon the surface in an oblique direction. Such a force might be exerted by an immense mass of ice in the process of expansion; but I know of no other source from which it could have been derived.*

On the other hand, there are features in the phenomena of diluvial action in this country, which are explained by this theory in a much less satisfactory manner. One is the southerly direction which our drift has taken, and the great distance to which it has been carried. It cannot be conceived that any single glacier should have expanded several hundred miles in a southerly direction, especially over a surface which could have had scarcely any southerly slope. Even if we admit a Mer de Glace in the northern regions so lofty as, in the beginning of the work, to send glaciers a vast distance, yet the force seems to have continued to operate in the same austral direction, even to the bottom of our valleys. It is, however, probably true, that the great mass of our drift will be found within fifteen or twenty miles from its original place; and that which occurs at greater distances, may perhaps, have been transported by powerful currents of water. It is almost certain that the sheet of ice which covered the surface, according to this theory, must have been at least three thousand or four thousand feet thick, because our mountains have been to that height, swept over. Now if, as Agassiz and others suppose, the fall of temperature, at the beginning of the glacial period, was

---

* Descriptions and sketches of these cases are given in the Final Report on the Geology of Massachusetts, Vol. ii, p. 396 and 559.

very sudden, why may not the return of the heat have been equally sudden? If so, the most powerful debacles must have been the result;* and as the ice would disappear most rapidly along its southern border, perhaps in this way a current in that direction may have been produced. And yet, I confess that I regard this theory more defective in not furnishing an adequate cause for the southerly course of our drift, than in any other point.

I find another difficulty in explaining satisfactorily by this theory, how drift could have been often carried from lower to much higher levels; as it has been sometimes if I am not greatly mistaken. Thus, the Silurian rocks of New York, and the quartz rock in the valleys of western Massachusetts, have been carried over, and left upon, Hoosac and Taconic mountains and the Highlands of New York. It is easy to conceive how an immense sheet of ice, by its expansive power, should force portions of its mass to ascend moderate declivities, of a few hundred feet, but not so easy to imagine them thus forced upwards one thousand or two thousand feet, as they undoubtedly have been in New England.†

Another difficulty results from the fact that some of the most remarkable of our moraines are found, not in valleys, but on the sea coast, some of them fifty, and others one hundred miles distant from any mountains much higher than themselves. I refer to those remarkable conical and oblong tumuli of drift, sometimes

---

* A curious example illustrative of this point has just been communicated to me by Rev. Justin Perkins, American missionary in Persia, not far from Mt. Ararat, in a letter dated at Ooromiah, Nov. 6th, 1840. In giving an account of two very powerful earthquakes experienced on and around that mountain in the summer of last year, he says, "The vast accumulation of snow which had been increasing on and about the top of the mountain for so many centuries, was broken into pieces, and parts of it shaken down on the sides of the mountain in such immense quantities, that (it being midsummer, and the snow descending down as far as a warm climate, and suddenly melting,) torrents of water came rolling down the remainder of the mountain, and flooded the plain for some distance around its base."

† In the north of Europe, also, the drift has been carried "from lower to higher levels," according to Mr. Murchison; and he imputes the striæ to "icefloes and detritus, set in motion by the elevation of continental masses, and grating upon the bottom of a sea." *On the Geological Structure of Northern and Central Russia, &c., by Murchison and Verneuil, p.* 13. *London*, 1841, pp. 16. Very likely the Glacier Theory may need some analogous modification to adapt it to this country; and yet it seems to me that expanding ice is a far more powerful agent to force detritus up an inclined plane, than currents of water.

more than two hundred feet high, which occur in Plymouth and Barnstable counties in Massachusetts. I see nothing in this theory that will explain such astonishing accumulations in such circumstances; and yet their existence may not militate against its truth. For even the present mighty glaciers of the Alps, may give us but a faint idea of the effects of the advance and retreat of a sheet of ice thousands of feet thick. We have no evidence in this country, that any of our mountains have been elevated since the glacial epoch; as seems to be proved to have been the case with the Alps, and this circumstance may have produced a considerable modification of glacial action on this continent.

I do not mention these difficulties (to which I might add more,) as any strong evidence against this theory. For so remarkably does it solve most of the phenomena of diluvial action, that I am constrained to believe its fundamental principles to be founded in truth. Modifications it may require: for it would be strange enough if it had already attained perfection, even in the skillful hands that have thus far framed and fashioned it. But I can hardly doubt that *glacio-aqueous* action* has been the controlling power in producing the phenomena of drift. Having hovered so long over the shoreless and troubled ocean of uncertainty and doubt, I may be too ready to alight on what looks like *terra firma.* But should it prove a Delos, I have only to plume my wings again, when it sinks beneath the waves.

I have dwelt long on this subject; its great importance, its interesting aspect at this time, and its wide developement in our country, must plead my apology.

In referring to our alluvial formations I shall call your attention only to a single subject, and that is, *microscopic palæontology.* The splendid discoveries of Ehrenberg in this department, were yet fresh among us, when Prof. Bailey demonstrated that similar relics abound in this country. They form extensive deposits, covering many acres, and sometimes several feet thick, beneath our peat-bogs. The substance appears to be the *Bergmehl*, or *mountain meal*, or *fossil farina*, of the Germans, and is mostly composed of the *Shields* or *Carapaces* of the family *Baccillariæ.* Some do, indeed, yet doubt the animal origin of

* By which I mean the joint action of ice and water, without deciding which has exerted the greatest influence.

this family; but the weight of opinion seems to be on the other side. Hitherto I believe, in this country, these relics have been found only in primitive regions; but as it is not always the case in Europe, (Am. Journal of Science, Vol. xl, p. 177,) we may believe it is not so here. Over the primary regions they have been found from Maine to Wisconsin, and south to Virginia. So numerous are the localities, that in New England at least, I am confident they may be found in nearly every town based on primary rocks. The report on the geology of Massachusetts is enriched with a valuable paper on the fossil infusoria of that state, by Prof. Bailey; and a memoir on the same subject, embracing the whole of the United States, may soon be expected from that gentleman. It will give some idea of the wide field which the microscope has opened to palæontology in this country, to state, that in a single specimen of fossil farina from West Point, Ehrenberg has detected fourteen species of siliceous infusoria. Besides, it can hardly be doubted that our iron ores and other deposits in addition to the Bergmehl, will afford these remains.

These remarks receive strong confirmation from the interesting discovery by Prof. W. B. Rogers, in the tertiary strata of Virginia, as announced in his Geological Report of 1841, of a deposit of these infusoria. It is composed almost entirely of their siliceous shields, occupies areas of considerable extent, sometimes attains the enormous thickness of twenty five feet, and is rarely less than twelve feet thick. If such is the beginning, what, gentlemen, will be the end of this *infinitesimal geology!* We seem fast advancing towards a realization of the proverb, *omnis calx e vermibus, omnis silex e vermibus, omne ferrum e vermibus.*

Having thus ascended to the top of the scale of American rocks, and briefly shown how far their characters have been fixed, and their equivalence to European strata demonstrated, a few miscellaneous topics only remain for examination.

One of these subjects is that of concretions. And it seems to me that it needs to have light thrown upon it as much as any in the whole range of mineralogy and geology. It needs a second Haüy to develop the fundamental principles of concretionary structure. Brongniart, De la Beche, and Fitton, have, indeed, thrown out many valuable hints on the subject, and rendered it probable that concretions result from segregation by means of elective affinity. But why the particles should arrange themselves in curved rather than straight laminæ, and why the curves should differ from one another, does not appear. The siliceous limestone of Fontainbleau contains more sand than the calcareous concretions of this country, called claystones; and yet the former assumes a polyhedral and the latter a spheroidal form.

These claystone concretions, which abound in our diluvial clays, seems to me to afford a better opportunity than any other for studying this subject. They appear to consist of the clay containing them, cemented by carbonate of lime, which usually forms about fifty per cent. of the mass; although I doubt whether it exists in definite proportion. I have found in them, also, both in those of New England and in specimens from the diluvial clay of Sweden, a small amount of organic matter, very probably resulting from the crenic acid, which existed in the water when the clay was deposited. I am informed, however, by Dr. Tamnau, of Prussia, that "the Swedish scientific men believe these claystones to be something of organic remains:—some sort of mollusca, which were more or less wrapped in a mantle." But even if we admit that some soft animal formed the nucleus, it is impossible to doubt that the claystones have assumed their present forms as the result of a concretionary agency. Those forms are often so very regular, and furnish such mimic representations of numerous artificial objects, that we need not wonder they should be regarded, both in this country and in England, as the work of art; that among us, they should be imputed to the ingenuity of the aborigines; and in England, be supposed to have been turned in a lathe, as a substitute for metallic coin, and have taken the name of *Kimmeridge coal money.*

An examination of numerous specimens from New England, has led me to the conclusion that certain predominant forms may be discovered, which they affect; although between them are numerous intermediate varieties; and sometimes there would seem to have been a struggle between two of these forms for the mastery. These predominant forms are the sphere, the oblate spheroid, the prolate spheroid, the annulated, the lenticular, and the cylindrical. The first is the most important, though least common; and perhaps all the others may be conceived to result from it. I think, also, that if we may suppose the clay to have been in a plastic and not a fluid state, so as to admit of the permeation of carbonate of lime in a state of solution, and that different centres of attraction existed in the clay, it is not difficult to conceive how all the varieties of form assumed by the concretions, may have been produced by a modification of circumstances. I find, that as in crystals of minerals, certain forms predominate at particular localities, so it is with the claystones. And finally, I am led by all the facts to the conclusion, that these concretions are produced by laws as fixed and definite as those of crystallography. To discover and develop these laws, therefore, must be an object of great interest.

There is another interesting concretion in the same diluvial clay, in all parts of our country, consisting generally of concentric alternating layers of clay or loam, and the same material more or less colored and consolidated by the hydrate of iron. The axis consists usually of the root of a vegetable, or some other organic body. Portions of the same clay are sometimes crossed by parallel divisional planes, so as to produce rhomboidal prisms, precisely like those in the older consolidated rocks, which have usually been referred to the agency of heat. But this clay can, probably, never have been even sun-dried; and, therefore, we must resort to some other explanation of this jointed structure. And since the experiment of Mr. Robert Weare Fox upon the influence of galvanism upon clay, I can hardly doubt but this agency might have produced it, and also the ferruginous concretions that have been described, and perhaps have aided in forming the claystones. But to settle these points will require numerous observations and experiments; and my chief object in these remarks is to show that this is a promising, though long neglected field of research.

It is expected in many of the state surveys, that particular attention will be given to the connection between geology and agriculture. To do this, the geologist is obliged to call in the aid of organic and analytical chemistry; obviously the most difficult branches of that most useful science. Hence the analysis of soils, of the plants which they produce, and of the various fertilizers which are applied by the farmer, as well as of the rocks whose disintegration produces the soil, ought to form the objects of a commission distinct from that of ordinary geology: and I hope the time is not far distant, when such an office will exist in all the states of the Union. For although we ought not to look for striking benefits from such a work so soon as from a geological survey, yet, with sufficient time given to a geological chemist, there can be no doubt but most valuable ultimate results would follow his labors.

Although the science of agricultural chemistry had a vigorous commencement in the labors of Davy and Chaptal, yet its subsequent progress has not been correspondently rapid; and it must yet be regarded as in its infancy. Hence too much has been expected from the analyses of soils in our country, and a consequent disappointment has been felt. Distinguished chemists are not yet agreed in respect to some of the fundamental principles of the science. The recent able work by Prof. Liebig on organic chemistry, affords evidence of the truth of this statement in the numerous new views which it presents, and which he declares to be different from those usually maintained. Many of these views will be adopted at once, as original discoveries; but in regard to others, it is not to be expected that chemists will receive them without discussion. With Raspail he maintains, that plants are nourished solely by the absorption of carbonic acid from the atmosphere and from soil; whereas, the prevailing opinion is, that they derive their support partly from carbonic acid and partly by the direct imbibition of organic matter in some of the forms of humus, by their roots. He supposes that the humus acts no other part than to furnish carbonic acid by its decomposition. Others maintain that some of it is taken up in a state of solution, by capillary attraction, or by galvanic action. And as all chemists were not convinced by the arguments of Raspail, they may not be satisfied with the more able reasoning of Liebig, on the same subject. His views of the agency of nitrogen in vegetable nutrition,—his discovery of ammonia in the atmosphere, and his many new views respecting the agency of salts upon vegetation, and on other subjects, will render this work a most valuable addition to agricultural chemistry.

The earlier agricultural chemists laid by far too much stress upon the mineral constitution of soils, and disposed of the organic matter by one simple act of combustion. But more recent experimenters have found, that the composition and condition of the organic matter are of the highest importance in relation to vegetation, and they have made great efforts to ascertain the true character of mould or humus. It is agreed on all hands, that it is composed of several distinct compounds. But, in determining their nature we find, as we might expect, that distinguished chemists differ somewhat in their conclusions. Berzelius includes all the organic matter of soils under the term *humus.* In this he supposes that he finds crenic, apocrenic, and humic acids, with extract of humus and humin. This was his view of the subject, if I understood it, in 1840; and it does not differ from his views seven years before, except in substituting recently the term humic acid for geine, and humin for carbonaceous mould. In this country, Dr. S. L. Dana employs the term geine in two senses. When he speaks *agriculturally*, he means by it "all the decomposed organic matter of the soil," which he divides into the soluble and insoluble; and in this sense he regards crenic and apocrenic acids, humin and extract of humin, as *forms of geine.* When he speaks *chemically*, he regards geine as a distinct compound, the same in composition as the substance denominated geine by Berzelius in 1833, and humic acid in 1840; although he supposes the humin and carbonaceous mould of the same chemist to be identical with humic acid, and of course only varieties of geine. Dr. Jackson, however, contends that geine is not a distinct compound, and that it is essentially composed of crenic and apocrenic acids. I mention these facts, not with a view to enter at all in this place, into the discussion of these points, but merely to call the attention of gentlemen to them as matters of great interest. I am too well acquainted with the gentlemen who have adopted different views on these subjects, not to believe that they will thankfully accept of light from any quarter, and consider it an honor, rather than a disgrace, to give up opinions which experiment or sound argument shows to be untenable. For they well know, that in a progressive science, like agricultural chemistry, the honor of original discovery belongs to him who makes an advance upon his predecessors; nor can it pluck the laurel from his brow, although others aided by his labors, should subsequently go beyond him. In the present case it may be thought, that rules for the analysis of soils, founded upon different views of the character of their organic matter, must be useless. But I must express the opinion, that the agricultural value of analyses, conducted according to these conflicting views, cannot be very different; and in a scientific respect, in the present state of agricultural chemistry, analyses performed in different modes must be an important means of arriving ultimately at the truth.

The remarkable fertilizing power of green sand, first discovered in this country, has raised another question in agricultural chemistry, of great practical interest, concerning which, there is not so much of an opposition, as of an unsettled state of opinion. The question is, which of the ingredients of this substance produce the fertilization. All will agree, probably, that the potassa found in some green sand, acts an important part. But if this is the only ingredient, then the green sands of New England and old England, will be of no agricultural value, as they are destitute of potassa. But others suppose that the iron exerts a favorable influence; and others, that the minutely divided state of the silica is important; as it seems to be in the Bergmehl, which is also useful in agriculture. But I have time only to express the confident expectation, that some of the gentlemen who hear me, will, ere many years, clear up this subject.

Had not these subjects been so intimately connected with several of the state surveys, they might seem irrelevant on this occasion. I return, therefore, to one more appropriately geological.

But little has yet been published respecting the anticlinal and synclinal axes and their correspondent systems of strata in our country; although I doubt not that numerous facts on the subject are in the note-books of our geologists, in respect to the particular sections of country which they have examined. But this is one of those subjects upon which, as upon diluvial action, general results, applicable to the whole country, can be made out only after long examination; it is one, therefore, peculiarly proper for such an association as I now address;—and I predict, that when the facts from different parts of this continent are collated and compared, it will be found that we have some of the most remarkable and magnificent systems of elevation and depression on the globe. There is no small reason to believe, indeed, that on the western side of this continent, from Cape Horn to the northern Arctic Ocean, one vast anticlinal axis exists, along the crest of the Andes and the Rocky Mountains. Subordinate and perhaps intersecting systems of strata will undoubtedly be found along this extended line; but this appears to be the great controlling and probably the most recent uplift on the continent. The occurrence of volcanic vents along the whole line, while they do not exist in the eastern part of the continent, renders it probable that the former has been upheaved at a later epoch than the latter. But there is another fact that makes this almost certain, or it shows at least, that the western, and particularly the southwestern part of the continent, has been raised to a much greater height than the eastern side. It is well known that the cretaceous formation of North America passes under the Atlantic Ocean near New York, with its superincumbent tertiary strata. The latter reappear on Long Island, and in great distinctness on Martha's Vineyard, near the coast of Massachusetts, beyond which they are no more seen south of Greenland. But as we go southwesterly from New York, the chalk formation gradually rises, and between Council Bluff and the Rocky Mountains, as I am informed by Mr. Nicollet, it sometimes reaches the height of two thousand feet, which is much higher than on the Atlantic coast. It appears, also, from the recent memoir of Von Buch, on the petrifactions of South America, that the same formation exists extensively developed in the Andes, from 10° north to 15° south latitude. It there attains the astonishing height of thirteen thousand feet above the ocean. Subsequent to the cretaceous period, therefore, the Andes must have risen to that height; while the coast of New England and the middle states has been elevated only a few hundred feet. In the southern states the uplift appears to have been still less.

The Appalachian range of mountains forms another anticlinal ridge, extending northeasterly through New England, and not improbably to Labrador. The rise of this chain elevated the cretaceous and tertiary rocks on the Atlantic slope, as well as the new red sandstone, and tilted up the southeastern margin of the transition rocks in the valley of the Mississippi. The uplift of the Rocky Mountains raised the western side of the same rocks, and produced the easterly slope of the strata extending to the Mississippi. That river, therefore, flows through a synclinal valley, and it was the existence of that valley which determined its course. The same is true of the river Ohio, which, according to Dr. Hildreth, flows through a synclinal valley. The sections given by Prof. Emmons, show that the same is true of the St. Lawrence. From the last report of Mr. Houghton, it appears that Lake Superior occupies a synclinal valley, and not improbably a valley of elevation. East of Little Falls, according to Mr. Conrad, the Mohawk flows many miles through a valley of depression. In New England, the primary strata dip towards the Connecticut river on both sides, at least through a considerable part of its course; and there is evidence, also, that an extensive fault in the primary rocks runs through that valley. Prof. Mather describes "a line of fracture and anticlinal axis" as passing a little west of the Hudson river, as well as numerous joints and fractures in the valley of the Hudson, which not improbably may be a valley of dislocation. Indeed, I doubt not that in most of those cases where rivers have found their way through gorges of lofty and precipitous ridges, it will be discovered that a break previously existed in the strata. To give an example:—the great western railroad, leading from Boston to Buffalo, and destined, ere long, to reach St. Louis, and ultimately perhaps the Pacific Ocean, is carried across the Hoosac range of mountains through a deep cut made across the ridges by Westfield river; and in no other place, probably, could it have been carried through. But I have recently satisfied myself that the course of that river was determined for a considerable distance, at least, by the existence of a wide fissure in the primary strata, which was subsequently filled in part by an enormous vein of granite. Is not this a beautiful example of prospective benevolence on the part of the Deity, thus, by means of a violent fracture of primary mountains, to provide for easy intercommunication through alpine regions, countless ages afterwards!

These slight sketches are sufficient to show that the great and striking features of our country are dependant upon a few extended axes of elevation and depression; and that probably subordinate anticlinal and synclinal lines will be found connected with most of the minor features of our surface. To trace them all out will be a great, yet most interesting work; as it will be to ascertain the systems of strata connected with them. Of the latter, we have in New England no less than five or six distinctly marked. They are all of them of ancient date, and most of them very ancient. The oldest, which may be called *the oldest meridional system*, because it runs not far from north and south, is composed chiefly of gneiss and mica slate; and crosses Massachusetts near its centre, including, although this is not certain, probably the most elevated land in New England. The second, which I call the *northeast and southwest system*, because it runs in that direction, is more distinctly marked than any other; having a high and uniform northwesterly dip. It corresponds in direction with that of the Alleghany mountains, and probably forms a part of their most easterly ranges. It extends, also, through almost the whole of Maine. It is composed chiefly of primary rocks. The third system, I call the *east and west system.* It is composed of primary and the oldest fossiliferous rocks; having a northerly dip. It occupies no great space in New England. But perhaps the east and west ridges of mountains described by Dr. Houghton, on the south side of Lake Superior, may belong to the same system; although I think we ought to be very cautious in referring the rocks of widely separated regions to the same system; especially if their strike is merely parallel, and not upon the same continuous line. The fourth system embraces the rocks from gneiss upwards, so as to include most of the clay slate and Silurian groups. The strata have a perpendicular or inverted dip. I call it the *Hoosac or Green mountain system*, because it embraces most of those mountains: but if I mistake not, it extends through the whole of the Appalachian chain of mountains, and possesses some remarkable peculiarities, to which I shall shortly call your attention. The fifth system embraces only the new red sandstone and its associated trap; and hence it may be called the *new red sandstone system.* But I am in doubt whether it ought not to be embraced in the fourth system. The sixth system I call the *northwest and southeast system*, because such is its strike, with a small northeasterly dip. It occurs in Rhode Island, the southeast part of Massachusetts, and southwest part of Maine: but it is very limited, unless it should be found that the four ranges of mountains, described by Dr. Richardson, in the extreme northwesterly part of this continent, belong to it. I regard it as the most recent system in New England; because, although composed of gneiss and the older slates, it corresponds, in strike and dip, with the eocene tertiary on Martha's Vineyard; and probably both were elevated at the same time.

The whole number of systems of strata, corresponding in their general strike and dip in this country, will undoubtedly be found to be much greater than these now described. Nor should I have mentioned these, which have been observed in a limited district, had I not great confidence in the uniformity and great extent of the geological features of this country; so that if we find a particular arrangement in one district, we may safely presume upon the existence of its counterpart in other parts of the land. These hints, therefore, may afford some feeble aid in the great work of tracing out the systems of elevation that exist on this continent.

I have alluded to some peculiarities in the Green Mountain system of strata; and if I may venture a little longer upon your patience, I will ask a few moments' attention to what I must regard as one of the most remarkable features in American geology. Still, I have so imperfect a knowledge of the subject, as to be conscious of venturing forward with few landmarks to guide me, into an almost unknown region. I am aware, also, that there are gentlemen before me, who have given the subject more attention than I have, and who are perhaps prepared for its full developement.

We have all read of the enormous dislocations and inversions of the strata of the Alps; and similar phenomena are said to exist in the Andes. Will it be believed, that we have an example in the United States on a still more magnificent scale than any yet described? I have mentioned in another connection, a series of strata, consisting of gneiss, mica, talcose, and argillaceous slates, with limestones and Silurian rocks, extending from Canada, along the western side of New England and the eastern side of New York, to the Highlands on Hudson river, and thence southwesterly through the Appalachian mountains as far as Alabama; a distance of at least twelve hundred miles. Along a large part of this distance, a remarkable apparent inversion of the dip exhibits itself; so that the newer rocks appear to pass beneath the older ones; and that too over a great width of surface. Certainly this is the case from Canada to New Jersey, and thence through Pennsylvania and Virginia, I infer from the reports of the Professors Rogers, that similar phenomena occur, which these gentlemen have been studying with great care and success; and the results, I learn, will soon be given to the public. The effects of the extraordinary agency under consideration, has not been simply to toss over the strata, so as to give them an inverted dip, but in general to produce a succession of folded axes, with a gentle slope and dip on their eastern sides, and a high dip, or more frequently an inverted one, on their western side.

Such a disturbance as this would be far less remarkable, were it not so extensive. I cannot describe the width of the belt that has been thus plicated, except in that portion of it which has fallen under my notice. It appears to me, that in the latitude of

Massachusetts, at least all the strata between the Hudson and Connecticut rivers, and probably a little west of the Hudson, about fifty miles in breadth, were affected by this disturbance. The first ridge, in going westerly from the Connecticut to Hudson river, is Hoosac mountain, and its eastern slope is gentle, while its western side is very steep, and the strata are nearly perpendicular, or radiate from the axis of the mountain. This appears to have been the principal axis of elevation. Next succeeds a deep valley and then the Taconic range of mountains, which also slopes gently on its east side, while its west side is very steep, and its crest very narrow. The dip of the strata is also small on the east side, and high on the other. Between this ridge and the Hudson, are no ranges of mountains very well marked, but the same large inverted dip continues, and probably more than one folded axis may be found in this space. Whether the belt of strata that have been subjected to this singular disturbance, is as broad, north or south of Massachusetts, I have no certain knowledge; but presume it to be as wide and probably wider.

I am aware that some able geologists,* whose opinions I highly respect, and who have carefully observed these phenomena, endeavor to explain them by supposing that we have mistaken the secondary divisional planes of the rocks for true planes of stratification; or that the character of the slaty and calcareous rocks of Taconic and Hoosac mountains has been misunderstood; and that they are in fact more recent than the fossiliferous rocks near the Hudson; in other words, that they are metamorphic. But for reasons that cannot now be given, for want of time, I have been forced to relinquish all these modes of explanation; and although I will not say that I fully adopt, yet I cannot but look with a favorable bias upon the only remaining solution of the problem already hinted at, that the strata have actually been tossed over from their original position.

Let us suppose the strata between Hudson and Connecticut rivers, while yet in a plastic state, (and the supposition may be extended to any other section across this belt of country from Canada to Alabama,) and while only slightly elevated, were acted upon by a force at the two rivers, exerted in opposite directions. If powerful enough, it might cause them to fold up into several

---

* See Prof. Emmons and Mather's views in the reports of the New York survey for 1837, p. 232, and for 1841, p. 92.

ridges; and if more powerful along the western than the eastern side, they might fall over so as to take an inverted dip, without producing any remarkable dislocations, while subsequent denudation would give to the surface its present outline.

Now in support of such a supposition, it may be said, first, that it would satisfactorily explain the present position of the strata. For if they could now be lifted up and made to dip in an opposite direction, every thing, for the most part, would be brought right; that is, the natural order of superposition would be restored. Secondly, this supposition explains the moderate dip of the rocks in the valleys, and the gentle slope of the mountains on their eastern sides, and the abrupt escarpment of their western sides. Thirdly, the occurrence of thermal springs along many of these folded axes, as is the case in New England and Virginia, and the extensive dolomitization of the limestone in the valleys, afford presumptive evidence of long lines of fracture, just where, by this hypothesis, they ought to exist. Fourthly, we should readily admit that such a plication and inversion of the strata might take place on a small scale. If for instance, we were to press against the extremities of a series of plastic layers two feet long, they could easily be made to assume the position into which the rocks under consideration are thrown. Why then should we not be equally ready to admit that this might as easily be done, over a breadth of fifty miles, and a length of twelve hundred, provided we can find in nature, forces sufficiently powerful? Finally, such forces do exist in nature, and have often been in operation. After we have admitted, as every geologist does admit, that the existing continents and mountains of the globe have been elevated from the ocean's bed, there is scarcely any effect, short of an impossibility, which we may not impute to the same agency. Merely for illustration, without maintaining its truth, let us suppose with Beaumont, that the vertical movements of our continents result from the shrinking of the internal parts of the earth, which causes a plication of its crust, simply by the force of gravity. And suppose the present crest of the Appalachian and Green mountains to have formed the line of least resistance on this continent. Is it difficult to conceive, that by such a power, a broad belt of the earth's crust, more than a thousand miles long, might have been ridged and overturned, with just as much facility, as a section two feet long, with the force which a man could exert? I apprehend that the chief difficulty is to bring the mind up to a realization of so mighty an agency. In other words, the difficulty lies in the narrowness of our views, rather than in the inadequacy of nature. I confess, that as I have sometimes stood upon some of our loftiest mountains, that seem to have been overturned, and looked into the valleys, from one thousand to two thousand feet deep, and abroad upon the vast ridges that stretched away far as the eye could follow them on every side, and then tried to conceive of them extending from Canada to Alabama, and to have been ridged up and thrown over, my mind has staggered under the mighty thought, and I have involuntarily exclaimed, that such a work could have been performed only by the immediate agency of Him, who *meted out heaven with a span, and comprehended the dust of the earth in a measure, and weighed the mountains in scales, and the hills in a balance.*

But I must not dwell longer upon this fascinating theme:—and I must desist also, not for want of materials but for want of time, from pointing out other objects of interest in American geology that deserve the special attention of this Association.

And now, on looking back upon the ground which I have gone over, I am astonished and delighted at the progress of American geology, and it seems to me more like a dream than the reality. Only twenty five years ago, when first my attention was turned to the subject, excepting the grand but rough outline sketched by Maclure, and a few insulated efforts by Professors Silliman, Cleaveland and Eaton, and Dr. Hayden, all was darkness and perplexity. A geologist was as rare as an oasis amid the sands of Africa; and to be seen accoutred geologically, with hammer and knapsack, would subject one to ridicule, if not to a suspicion of insanity. But how changed the scene! From the top to the bottom of the series, the principal groups of our rocks seem now to be nearly settled and identified. And as the rapid rise and developement of this great nation is a spectacle of deep interest and sublimity, so our geologists find a correspondent grandeur in our rock formations. Now too, nearly all the state governments of this country extend their patronage to geological researches; lectures upon geology are demanded and given in all our larger towns; and the wonders of this science form the theme of discussion in the drawing-rooms of taste and fashion.

You perceive, therefore, gentlemen, that in the work which we have undertaken, we are urged forward by powerful motives;—and although much has been done, still more remains to be accomplished. Indeed, the enterprise is as yet only just begun. Even when the state surveys are completed, there will be abundant opportunity to gather fresh laurels in the same field. For, then the way will be prepared to go into particular districts, unincumbered by horses and carriages, and spending time enough there on foot, fully to explore and understand their structure; a work, which can hardly be done, except in a few instances, during the limited time devoted to the state surveys. A multitude of points in our geology, also, are yet only dimly seen, or imperfectly settled. In fixing these, and developing new discoveries, there will arise differences of opinion, and we may expect to fall into frequent perplexities and mistakes. But let not such differences generate distrust and alienation, among those who have an important common cause to sustain, and an interest as well as fellow-feeling in sustaining one another. Let discussion be as free as air; and let every man keep his mind open to conviction;—but American geologists, above all other scientific men, have no time for personal altercation. They have too great a work before them; they are scattered over so vast a field, that it will be difficult to interfere with one another; and all of them, I doubt not, would welcome other laborers, to aid in gathering the abundant harvest.

I make these remarks, not because I have observed among our geologists any peculiar tendency to alienation and controversy, but because I have witnessed the reverse; and, therefore, such remarks may have some influence in preserving them from those jealousies and personal altercations that have too often broken up the harmony of scientific associations.

As motives to continued exertion I have mentioned the favor of government, and the just estimation in which the community are beginning to hold our favorite science. But there are considerations of a much more elevated character, to urge onward the genuine student of nature. The cultivation of this science carries with it its own reward. It is continually disclosing to its votaries, facts and inferences of most thrilling interest. How eagerly does the antiquary unroll the newly discovered papyrus, that reveals an earlier chapter in a nation's history, or the existence of some hitherto unknown race! The farther back the new record carries him, the deeper is his interest and enthusiasm. Such developements of lost races and lost ages in the world's history, are continually rewarding the labors of the geologist;—and in point of antiquity, I had almost said, that the most ancient event in chronology, the six days' work of creation, is the most recent in geology. From that beginning of *registered* time, we wander back through cycles of duration, which we can measure only by a succession of events, and not by chronological dates, except to be assured that they are inconceivably long;—and yet, the relics of those early periods are as fresh as if entombed yesterday. The fossil reptile, or fish, or shell,—nay, even their most delicate parts, are as perfect as when alive; although tens, and perhaps hundreds of thousands of years have rolled away since they died. We see their footmarks following one another in regular succession, as distinct as those of living animals upon the snow and the mud; and even the pattering of a shower, that fell thousands of ages ago, is as fresh before us, as if every drop had been instantly petrified. In short, there passes before us a series of distinct creations of organic beings, adapted to the varying condition of our planet; each successive group becoming more and more perfect, until every thing in nature was prepared for the existing races, with man as the crown of all.

Such developements as these are no longer to be regarded as the dreams of disordered fancy, but as the sober and legitimate deductions of science. And what large and refreshing views do they present of the plans and the benevolence of the Deity! They open back a vista as far and as wide into the arcana of time, as astronomy discloses into the arcana of space. They show us that the brief space of man's existence on the globe, is but one of the units of a vast series of cycles that have passed already away;—and the time is at hand, when geology, equally with astronomy, will be celebrated for its power of liberalizing the mind and filling it with noble conceptions of the universe and its Infinite Author. Surely, in such ennobling thoughts, the geologist finds a rich reward for all his toils.

I know indeed, that our science has been regarded as coming into collision with that sacred volume, to which, as Christians, we are bound to bow as the invariable standard of religious truth. Geologists, too, have been represented, and I must say without any proof from their writings, as exulting in the supposed collision;—but I am happy to believe, that such apprehensions are rapidly passing away. Theologians of enlarged and impartial minds are beginning to study geology; and instead of finding its truths hostile to revelation, they find, that it furnishes them with new and interesting matter, such as no other science can, for illustrating the perfections and government of Jehovah;—and such men as Drs. Chalmers and Smith,* have already reaped from it a rich harvest. I trust that the day is not distant, when the supposed geological objection to revelation will be as little remembered, as is now the analogous objection derived from the Copernican system of astronomy; and which, two or three hundred years ago, was supposed to be fraught with so much danger.

Another mode in which practical geology carries with it its own reward, is by bringing us into constant communion with unsophisticated nature, in her most sublime and interesting aspects. It is hardly possible to place the geologist in any spot on the globe, where he does not witness around him the marks of mighty agencies and revolutions, that are unheeded by the common mind, but which furnish him with a rich fund for reflection. But his most appropriate place is among the wildest scenery of nature; now, plunging into the deep cavern, studded with glittering spars, and perhaps the charnel-house of the antediluvian world; now, tracing his way through the dark gorge, with jutting rocks rising around him, as if they formed the battlements of heaven; now, mounting the lofty ridge and drinking in the glories of the vast landscape; and now, standing upon the edge of the yawning precipice, to witness the roaring cataract, as the waters thunder down their steep and rocky bed, until, escaping from their narrow passage, they flow out quietly, as the calm and majestic river, to fertilize and beautify the extended plain. In all these scenes, he sees the arm of Omnipotence laid bare, and is initiated into the sublimest mysteries of nature. There, while his body and his mind are invigorated, he acquires a permanent relish for all in creation that is sublimely great and elegantly little. Henceforth, he possesses a source of gratification of which

---

* See Chalmers's Natural Theology, and Smith's Lectures on the Relation between the Holy Scriptures and some parts of Geological Science. An able view of this subject is also given in the sermons of Rev. Mr. Melville, of London, Vol. ii, p. 297, Am. edition.

all the fluctuations and calamities of life cannot deprive him. Other sources of happiness, as circumstances change and age advances, will pass away. But, a genuine attachment to nature clinging to the heart will buoy it up, when the powers begin to fail, and the floods of affliction to roll over us; and like the volcano surrounded by polar snows, the flame will seem more bright and beautiful amid the frosts of age. *Hæc studia adolescentiam alunt, senectutem oblectant, secundas res ornant, adversis perfugium ac solatium præbent; delectant domi, non impediunt foris; pernoctant nobiscum, peregrinantur, rusticantur.* (Cicero, Orat. pro Archia.)

Gentlemen, in these remarks I am confident that I am describing your own experience. For this love of nature, and not governmental or individual patronage, has been your chief stimulant in geological research. Should that patronage, which is now extended to your efforts, be withdrawn,—of which I have little fear,—and should the tide of popular favor turn against you, I know that you will not, therefore, be diverted from your favorite pursuit. No: let us rather pledge ourselves to more vigorous efforts in this noble enterprise, which has already done so much, and is destined to do much more, to develop the resources of our beloved country; so much to awaken youthful genius; so much to promote our personal happiness; so much to enlarge the boundaries of science; and, above all, so much to unfold the glories and illustrate the perfections of the INFINITE DEITY.

ART. III.—*Notice of a Flora of North America;* by JOHN TORREY, M. D., F. L. S. &c., and ASA GRAY, M. D. &c. Vol. II, Part 1. May, 1841. New York: Wiley & Putnam.

THREE years have now elapsed since the commencement of this truly national work, of which the first volume was completed in the past summer. The fifth number, being the first of the second volume, is now before us, and in presenting our readers with a brief analysis of its contents, we do not pretend to do justice to the work itself—not even to the portion of it more immediately under consideration, the value of which will only be adequately appreciated by the scientific botanist. It may seem extraordinary, that this undertaking, which has attracted so much attention amongst European naturalists, should have excited so little comparative interest amongst those of our own country, for whom it is more especially designed. We are not inclined to ascribe this indifference to any contempt for the branch of natural science of which it treats, since botany is very extensively taught amongst us, and forms, indeed, part of the regular course of instruction in most of our high schools and colleges; but we are persuaded, that the *low standard* adopted by our professors, has induced the prevalent opinion that it is of very subordinate importance. Indeed, if we were to judge from the manner in which it is commonly taught, it might be doubted whether our actual knowledge of this eminently progressive science has materially increased within the last ten or twenty years. A few general principles, with an exemplification of the classes and orders of the artificial system of Linnæus, by means of which the *Latin names* of plants may be obtained without difficulty, is the usual amount of botany taught in our seminaries, and it is not surprising that the community should lightly estimate the value of the acquisition. *Vegetable physiology*, connected with an actual, practical knowledge of the natural affinities of plants, with their respective qualities and value to mankind, is scarcely, as yet, beginning to be taught to the youth of our country, nor can we hope to see botany elevated to its due rank, amongst us, until its professors direct the attention of their classes to the *philosophy of the science*, rather than to technical rules, and long catalogues of unmeaning names. For this purpose, many of the manuals, local floras, and popular treatises, in common use, are utterly insufficient; and we require the introduction of simple, yet philosophical, text-books, adapted for the American student.

Botany is the only science in which, as taught with us, little information beyond *the mere names of things*, is sought to be conveyed, and hence it arises, that a sound physiological work, such as that before us, being in advance of the general standard of our country, is, in some measure, a sealed book, even to those who have studied botany after the manner in which it is usually taught. The few who can estimate the labors of Drs. Torrey and Gray, are, doubtless, in possession of their valuable Flora, so far as published; and we feel confident, that it will gradually exert a beneficial influence on the botany of our country. We shall notice, very cursorily, the contents of the recent number, which commences the *Monopetalous Exogenous plants*, and comprises the orders *Caprifoliaceæ*, *Rubiaceæ*, *Valerianaceæ*, *Dipsaceæ*, and a part of *Compositæ;* viz. the tribes *Vernoniaceæ*, *Eupatoriaceæ*, and the first subdivision of the tribe *Asteroideæ*. In *Caprifoliaceæ*, we have two new species of *Symphoricarpos*, collected at California, by Mr. Nutttall, and described from his MSS., a new species of *Lonicera*, from Arkansas, and a species of *Sambucus* from Oregon. The order *Rubiaceæ*, consists of three sub-orders, being, 1. *Stellateæ*, R. Br.; 2. *Cinchoneæ*, (ord. *Cinchonaceæ*, Lindl. ;) and 3. *Loganieæ*, R. Br.

In the first sub-order, two new species of *Galium* are described; and in the second, a species of *Spermacoce*, also a species of *Borreria*, and a new *Hedyotis*, (?) from Louisiana, which, on account of the pentamerous flowers, our authors are inclined to consider as the type of a new genus. With reference to the third sub-order, it is remarked, that

"In thus appending *Logania*, and its nearest allies, to *Rubiaceæ*, (which seems inevitable when we compare *Ophiorhiza* with *Mitreola*, a portion of *Hedyotis* with *Cœlostylis*, &c.,) we trust we are following the indications thrown out by the most profound botanists who proposed the order or tribe, (*Appendix to Flinders*, 2, p. 564, and *Tuckey's Congo*, p. 448;) although it is still no less true than when Mr. Brown first made the remark, that there are no satisfactory characters known to distinguish *Rubiaceæ* from *Apocynaceæ*."

Now, if it be difficult to assign any arbitrary characters by which *Rubiaceæ* may be decidedly distinguished from *Apocynaceæ*, it certainly does not seem desirable to keep up a distinct order, intermediate between the two, and, in this view, we think the arrangement of Drs. Torrey and Gray very judicious. It may be said, that, upon this principle, *Spigeliaceæ* must be reunited with *Gentianaceæ*, and that even the latter are very nearly related to *Apocynaceæ*, though possessing sufficiently obvious distinctions. We do not pretend to anticipate what course may be taken by the authors, when these orders are under their consideration; but we incline to the opinion that there is, upon the whole, less practical inconvenience in regarding all very close alliances, such, at least, as they have merged, in the instance before us, as sections of one large family, than in endeavoring to circumscribe them as separate orders, where no sufficient *natural* characters appear to exist. We feel convinced, indeed, that the onward progress of the science will have a tendency rather to diminish than increase the number of orders, which have been so largely multiplied since the days of Jussieu. Nor is it surprising that such should have been the case, since the introduction of new plants irreconcilable with the characters of any of the families indicated by that great physiologist, required, from time to time, the establishment of such additional orders as appeared to limit and define their respective properties and peculiarities. But with our increased, and daily increasing, knowledge of vegetable forms, we become acquainted with genera so exactly intermediate in character between what have been regarded as separate groups, or orders, existing in nature, that we are compelled to view the dividing lines as of secondary, or subordinate value ; and hence, we conceive, will arise the necessity of reuniting some, perhaps ultimately many, orders now considered as distinct. It is in this way only, that families of plants can be preserved as *natural* alliances ; and any approach towards *artificial* arrangement will be deprecated by every true botanist, who feels that his province is limited to the observation of kindred tribes, and the elucidation of such characteristics as they are severally found to present. No doubt the types of new and very distinct orders are yet to be discovered, but we repeat our conviction, that for the present, at least, the number will, upon the whole, be rather reduced than augmented.

Under the sub-order *Loganieæ*, is described a second species of the very interesting genus *Cœlostylis*, T. and Gr., collected in Texas by the late Mr. Drummond. In *Valerianaceæ*, we have a new species of *Valeriana* from the West, and two new species of *Fedia*, collected on the plains of Arkansas, by Mr. Nuttall. These last are particularly interesting, forming a separate section of the genus, (*Siphonella*,) which "is almost exactly intermediate between *Fedia* and *Valerianella* of Mœnch and De Candolle, having the corolla of the former, with the fruit, stamens and stigmas of the latter." The genus *Valerianella*, of Mœnch, is accordingly reduced to a section of *Fedia*. Passing over the order *Dipsaceæ*, in which we have only a single (naturalized) species, we come to the immense family of *Com-*

positæ, which is justly termed by De Candolle, "Ordo omnium naturalissimus et vastissimus et idèo difficillimus," and which, Drs. Torrey and Gray, following in that respect the arrangement of Lessing, divide into three sub-orders, comprising, together, eight tribes, which are of very unequal importance in respect to the number of genera which they embrace. The first tribe, *Vernoniaceæ*, is very small, being limited to six genera, of which *Vernonia* is the most important; and we notice a new species from Florida. The second tribe, *Eupatoriaceæ*, comprises twenty-one genera, none of much importance, (within the geographical limits of this work,) except *Liatris* and *Eupatorium*. Of the former genus, a new species is introduced from Georgia, and three species, as heretofore characterized by botanists, are now removed to the genus *Carphephorus*, established by Cassini on the *Liatris squamosa*, Nutt., our authors observing, that "on examining the allied species of *Liatris*, we find that all those with many-flowered heads disposed in corymbose cymes also belong to the genus, which is well marked in habit." In this tribe, we have also a new species of *Cœlestina*, from Key West and Florida, and a well known tropical species of *Ageratum* is recorded, for the first time, as a native of the United States. The authors remark, that these two genera are not sufficiently distinct. We further notice, in this tribe, a new species of *Clavigera*, DC., from the interior of Texas. We now come to the important tribe *Asteroideæ*, of which the first subdivision (*Euastereæ*) occupies nearly one half of the present number. This section is confined to the several genera, and sub-genera, comprehended under the popular name of *Asters*, which have so long been the terror and stumbling-block of American botanists. It appears, from an introductory note, appended to the genus *Aster*, that Drs. Torrey and Gray have enjoyed great advantages in the study of this most difficult tribe, being "greatly indebted to several botanists and public institutions in this country, for the use of their entire collections of American *Asters*; and we would especially render our acknowledgments to Sir Wm. Hooker, who, by most liberally entrusting to our care his vast materials in this and other allied genera, has afforded the most important assistance." * * *

The original genus *Aster*, of Tournefort and Linnæus, has been largely subdivided by modern botanists, and, according to their arrangement, is made to consist of some eight or ten genera.

These are distinguished by characters of greater or less importance, and, in so extremely natural a group, it follows, almost of course, that in some instances they are incapable of any very precise definition. Following the order of our authors, we have one, or possibly two, species of *Galatella*, Cass., though apparently very rare, and even doubtful natives of the United States. It is remarked, that " *Galatella* scarcely differs from *Linosyris*, except by the presence of (white or blue) rays; and these, according to Ledebour, are sometimes wanting in *G. dracunculoides:* the two genera have also nearly the same geographical range. The sterile rays chiefly distinguish it from *Aster* § ? *Orthomeris*, (species of *Calimeris* of authors;) to which *Aster nemoralis*, Ait., belongs." The genus *Sericocarpus*, of Nees, consisting of five species, is retained by Drs. Torrey and Gray, and it forms, no doubt, a natural group, distinguished by habit, the densely silky achenia, &c., but, as both they and Mr. Nuttall remark, so nearly connected with *Aster*, through *A. gracilis*, that we should, upon the whole, have preferred seeing these plants reunited to that genus, though, perhaps, the distinctive characters may be a shade more important than those of *Biotia* and *Tripolium*, which are both described under different sections of *Aster*, and from which, we think, they cannot conveniently be separated. After all, it is a matter of small moment, practically considered, whether the subdivisions recognizable in large and very natural genera, be ranked as sections merely, or as independent generic forms; the sole difference being the *value* and *importance* of the discriminative characters which they respectively present; and upon these points, which vary materially in different families, the most profound botanists are not unfrequently at issue. But, in such genera as *Aster*, *Saxifraga*, *Œnothera*, and some others, in which we cannot but notice the continual recurrence of intermediate forms, connecting the subdivisions with each other by characters possessed by them in common, we prefer a combination of the whole, by the larger and more comprehensive generic type, under which, as it appears to us, the sub-genera or sections may be studied with equal, if not greater facility. Under the several subdivisions of the genus *Aster*, Drs. Torrey and Gray enumerate, in all, one hundred and thirty-one species, of which number eight are new, and described for the first time, whilst nine species, said to have been founded on native specimens by European botanists, are unknown to our authors; and no less than twenty-two species, professedly described originally from *garden specimens,* (some of them of doubtful origin,) have not been identified with native plants. On this subject, Drs. Torrey and Gray remark:

"It is well known that many of the enumerated species, both of earlier and later authors, have been described from plants long cultivated in European gardens, where they have doubtless undergone great alterations in appearance; to say nothing of the strong probability of occasional hybridization. A large and indeed increasing number of these are only known as garden plants; and it is probable that many will never be identified with their original types; even supposing them to have been derived in all cases from this country, which is by no means certain. As we have chiefly directed our attention to the indigenous plants, and have drawn our descriptions from these alone, we have thought it advisable to bring together, at the close of our account of the proper *Asters* known to us, those species of garden origin which we have not identified with native specimens. A fuller comparison than we have been able to institute, will doubtless considerably reduce their number. Those botanists who are most familiar with our *Asters*, in their native situations, and with the changes produced by difference of soil, exposure, season, &c., will not be greatly surprised at numerous reductions of species, which others may think unwarranted. We have only to say, that we have seldom ventured upon such reductions, except on the authority of a full sùite of specimens which appeared to present absolute transitions. An obvious difference between two or three specimens is often entirely inappreciable in a fuller series, and thus loses its value as a means of distinction; but the claims of a genuine species are generally confirmed by a large number of specimens. It must, however, be admitted, that in this as in all large and natural genera, several species which we cannot but consider as distinct, (such for instance as *A. cordifolius* and *A sagittifolius*,) do frequently present very puzzling intermediate forms; and that an apparent transition is not always real. Yet it is better, perhaps, to hazard the occasional reduction of even true species to varieties, than to multiply species which we are confessedly unable to define. We may remark in conclusion, that we are the more inclined to act upon our own convictions, on account of the very frequent and wide disagreement even of the highest authorities upon this genus."

Now, these observations are very just, and will be at once confirmed by the personal experience of the very few of our botanists who have had the courage to attempt for themselves the study of this vast and most intricate genus, and its congener *Solidago*. We entirely approve of the reduction of many doubtful species to mere varieties of a recognized type ; and it appears that in many cases the approximation of forms was too close to admit even of this distinction : thus, under *A. lævis*, Linn., we have nine synonyms! whilst only two marked varieties of the plant occurred to the authors, who say, "The large suite of specimens before us have been named for the most part by several distinguished botanists, whose determinations so seldom accord, that we feel the greater confidence in our own opinion." * * * Now, we are very far from feeling any surprise that these distinguished botanists should not have understood each other in such cases, as indeed their laborious volumes sufficiently prove, and we should have esteemed it very marvellous had it been otherwise. A variety (perhaps in the first instance accidental) is introduced into some one of the botanical gardens of Europe, where it is carefully propagated *by division of the root*, and in time distributed generally amongst them. It is not difficult to perceive, that such characters as it originally presented are thus perpetuated throughout the series, and the peculiarity of form becomes known, whilst the slight modification of the original type, from which it was derived, is not susceptible of precise definition in terms. Indeed, it may be said, that the characters relied upon for the discrimination of even acknowledged and understood species, are but modifications, more or less marked, of the same general structure, and hence the many difficulties which have arisen, and the perplexed and entangled synonymy with which we have now to contend. We give our authors all credit for what they have done towards removing these difficulties, but we feel convinced that some of the species of Willdenow, Aiton, and Linnæus, will ever remain matter of more or less probable conjecture. The section *Aster* proper is subdivided with great care, and such characters as the several groups afford, have been clearly and well defined. We confess, however, that we do not esteem these as of very great value or importance. Helps to the student they undoubtedly are ; but they pass into each other too imperceptibly to be relied upon as certain distinctions, and what we have just observed in respect of the discrimination of individual species, is almost equally applicable to the arbitrary arrangement of them in separate series. In some sort, these do no doubt exist, but the differences are scarcely more than matters of *degree*, and hence, where extensive means of comparison are not within reach of the student, the appreciation of the terms necessarily employed to describe these variations, becomes mere matter of opinion, and therefore open to misapprehension. These difficulties, inseparable from the nature of the case, are not within the control of any systematic writers, and we believe that the present arrangement will not disappoint the hopes of the authors, who "trust that most of our indigenous *Asters* may be satisfactorily identified by the student." * * *

We shall only further notice the kindred and very difficult genus *Erigeron*, which we shall do in the succinct and philosophical observations of Drs. Torrey and Gray:

"It appears to us impossible to effect generic divisions among these plants, by characters derived from the single or double series of rays, or from the simple or double pappus, or by any combination of these. The characters of our sections, or sub-genera, exhibit the diversities which the North American species present in these respects. As a whole, the genus is distinguished from *Diplopappus* by the very short and obtuse appendages of the style, the nearly simple involucre, and the naked receptacle: the same characters also distinguish it from *Aster;* but the *alpigenous Asters* almost connect the two genera."

The genus has been entirely remodelled, and bears ample testimony, no less to the science than to the unwearied patience which have been devoted to the subject. We feel justified, indeed, in extending this encomium, not merely to the last number, but also to the entire work of Drs. Torrey and Gray, so far as it is before the public; and we trust, for the sake of natural science, more especially that branch of it so much below its just appreciation in our own country, that this most valuable contribution towards its elevation, may be carried to completion by the laborious and talented authors. J. C.

Art. IV.—*A Sketch of the Infusoria, of the family Bacillaria, with some account of the most interesting species which have been found in a recent or fossil state in the United States ;* by J. W. Bailey, Professor of Chemistry, Mineralogy, and Geology, in the United States Military Academy.

Those organized beings which Ehrenberg has placed among the Infusoria, in the family Bacillaria, present almost equal claims on the attention of the zoologist, botanist, and geologist. Containing, as this family does, those obscure organic bodies which form, as it were, the connecting links between the animal and vegetable kingdoms, and which appear to possess characters belonging to both, the student of either zoology or botany must examine them, and in fact the very simplicity of their structure renders them peculiarly proper for the observation of many phenomena of great physiological interest.

The geologist must attend to them, for the discoveries of Kutzing, Fischer, and especially of Ehrenberg, have shown that many of these minute bodies possess siliceous coverings, which occur in vast abundance in the fossil state, and which form the minutest, and yet not the least important nor least interesting, of the series of "nature's medallions."

Believing that some account of the structure, classification, &c., of this family would be acceptable to many in this country, and that figures of our most remarkable species would be of interest to the students of this family, both in this country and in Europe, I have devoted for some time past the very few leisure hours at my command, to the preparation of the following sketch. As an apology for the very imperfect state in which I now present it, I must state that my knowlege of the labors of others is necessarily very slight, as it is almost impossible to procure in this country any works relating to this branch of natural history. Of the many European works which contain figures of these obscure beings, scarcely one has been at my command, and as no one in this country has previously studied this subject, I have had to trust almost entirely to my own observations.

To avoid the risk of adding to the already burdensome synonymy, I have not attached any names to the species which I believed new, or which I could not determine satisfactorily, and

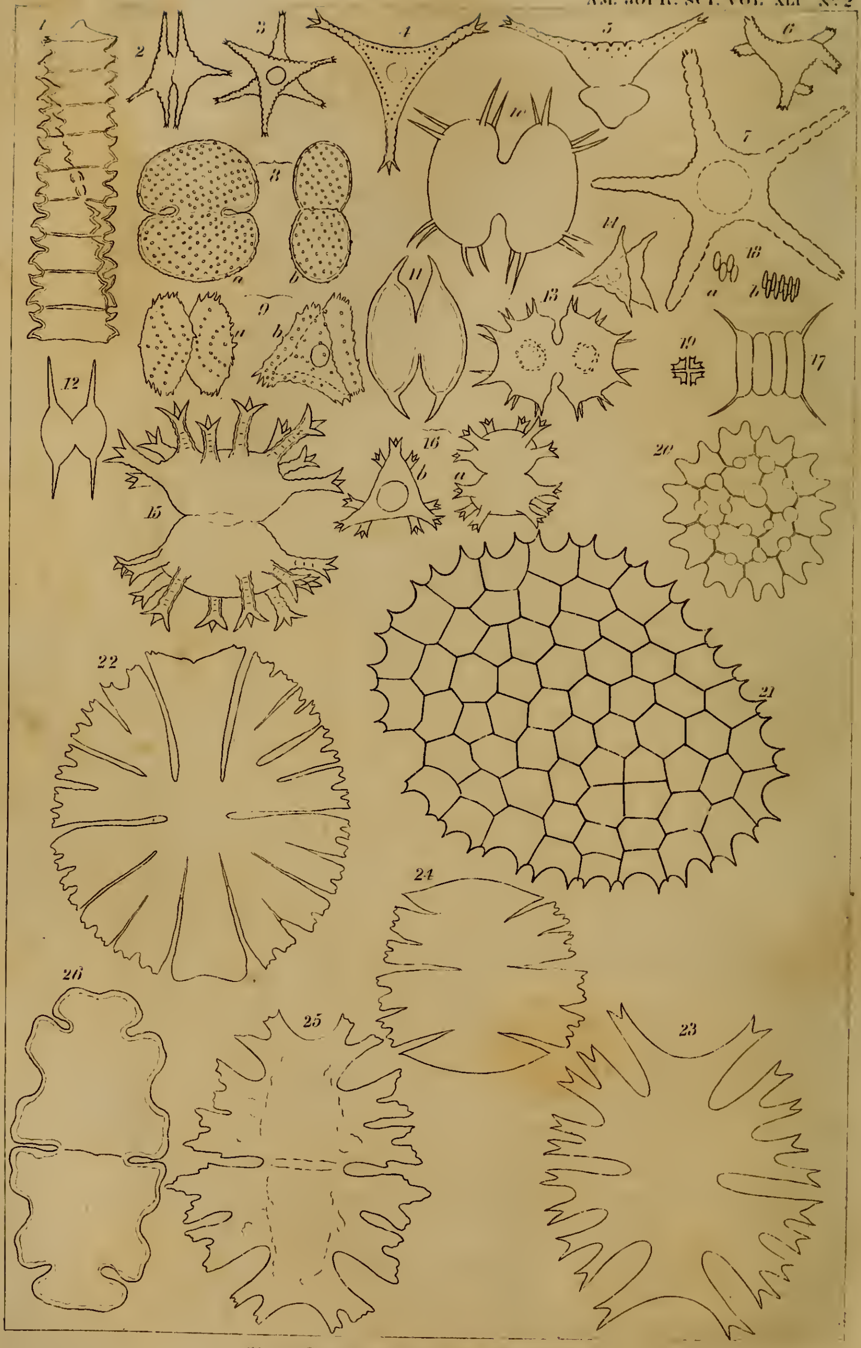


Plate first Part first the Desmidaceae .

ILLUSTRATIONS TO PROF. J.W. BAILE

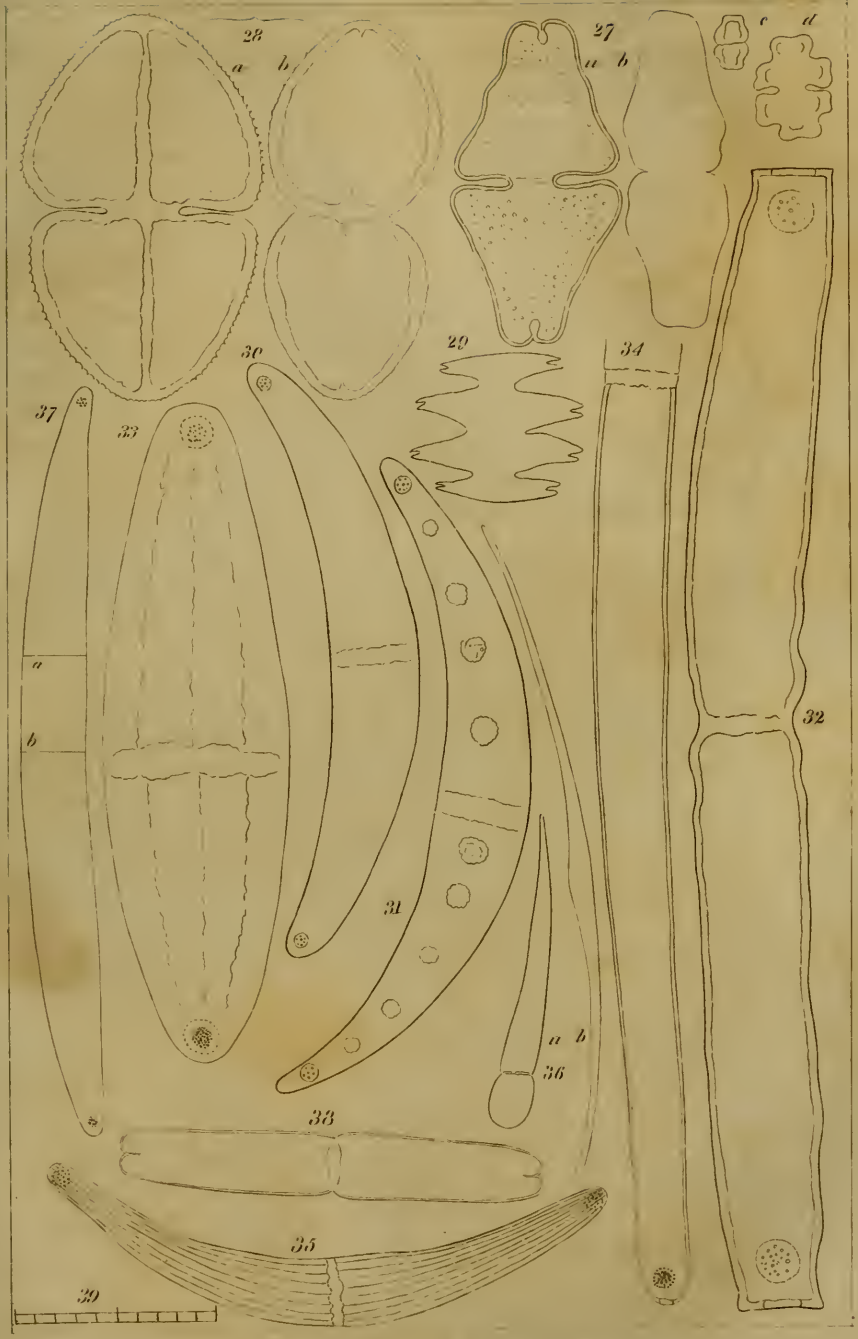


Plate first. Part first. the Desmidaceae.

APER ON AMERICAN BACILLARIA

shall merely refer to them by the numbers given to the figures representing them. I hope that these figures will enable some of the learned students of this family in Europe to decide which species are new, and perhaps if this paper should meet the eyes of Ehrenberg, he may oblige us by furnishing for this Journal the authentic names of the species I have represented in the accompanying plates.

For what relates to the classification and synonymy of this family I am chiefly indebted to an abstract of Ehrenberg's work on Infuriosa, which is appended to Mandl's *Traité pratique du Microscope. The generic and specific characters which I give are in most cases literal translations from this work.* I have also studied with much profit Kutzing's *Synopsis Diatomearum* in the *Linnæa* for 1833. The plates accompanying Kutzing's memoir have decided many doubts for me. Ehrenberg's great work on Infusoria I have not yet seen.

Without further preface, I shall now present a translation of Ehrenberg's characters for the *family*

BACILLARIA.

"Polygastric (distinctly or probably*) without intestinal canal; appendices (distinctly or probably) variable, undivided, body multiform; carapace often prismatic and siliceous, with one or several openings, often having the form of articulated polypidoms, in consequence of imperfect, spontaneous (longitudinal) division." Ehrenberg divides this family into the following groups, viz.

Desmidiacea, having the carapace simple, free, and univalve, (not usually siliceous.)

Naviculacea, with the carapace simple, free, with two or more valves, (siliceous.)

Echinellea, with the carapace simple, fixed, (siliceous.)

Lacernata, with the carapace double, (siliceous, and enveloped in tubes or gelatine.)

He separates the Closteria as a distinct family, but this genus is so closely allied to Euastrum, that I cannot hesitate to follow the example of most writers upon the subject, and to class them with the Desmidiaceæ.

* The question as to the correctness of Ehrenberg's views with regard to the internal structure of his Polygastrica, appears still undecided.

The following analytical table of the genera of Bacillaria, is translated from Ehrenberg.

A. CARAPACE SIMPLE.	
A. Free.	
a. Univalve, ... *Desmidiacea.*	
a. Prismatic.	
1. Trilateral, ...	Desmidium.*
2. Quadrilateral, ...	Staurastrum.*
3. Pentagonal, ...	Pentasterias.*
b. Round.	
1. Smooth.	
aa. Polypidoms moniliform, . . .	Tessararthra.
bb. " bacciform, . . .	Sphærastrum.
2. With projections, ...	Xanthidium.*
c. Flattened.	
1. In forms of bands.	
aa. Serrated, ...	Arthrodesmus.*
bb. Not serrated, ...	Odontella.*
2. In form of discs or plates.	
aa. Several in each disc, ...	Micrasterias.*
bb. Two in each disc, ...	Euastrum.*
cc. Isolated in plates, ...	Microtheca.
b. Two or more valves, . . *Naviculacea.*	
a. Round.	
1. Globular, ...	Pyxidicula.*
2. Forming polypidoms.	
aa. One cell, chain filiform, . . .	Gallionella.*
bb. Several concentric cells, . . .	Actinocyclus.*
b. Prismatic.	
1. Division perfect, never forming bands.	
aa. With six (?) openings, . . .	Navicula.*
bb. With four openings, ...	Eunotia.*
cc. With one opening, ...	Cocconeis.
2. Division imperfect, forming bands.	
aa. Articulated.	
aa. Prisms ("baguettes"), . . .	Bacillaria.*
bb. Plates, ...	Tessella.*
bb. Without articulations.	
aa. Strait ribbands, ...	Fragillaria.*
bb. Spiral ribbands, ...	Meridion.*
B. Fixed, ... Echinella.	
a. Wider than long, ...	Isthmia.
b. Longer than wide.	
a. Without a pedicel.	
1. Prismatic, ...	Synedra.*
2. Wedgeform, ...	Podosphenia.*
b. Supported by a pedicel.	
1. Wedgeform.	
aa. Dichotomous, ...	Gomphonema.*
bb. Verticillate, ...	Echinella.*
2. Fixed perpendicularly, ...	Cocconema.*

3. Fixed obliquely.	
aa. Opening in the middle, ...	Achnanthes.*
bb. Without opening in the middle, .	Striatella.*
B. Carapace double, ... *Lacernata.*	
A. Surrounded by an amorphous gelatinous mass.	
a. Scattered, ...	Frustulia.
b. Joined in rings, ...	Syncyclia.
B. Surrounded by membranous tubes.	
a. Tubes separate.	
a. Frustules strait, ...	Naunema.*
b. " curved, ...	Gloenema.
b. Tubes joined.	
a. Fasciculate, ...	Schizonema.*
b. Branching, ...	Micromega.

The genera marked thus (*) have been detected in the United States.

Ehrenberg remarks of this family, that "the organization is difficult to recognize, in consequence of the hardness and refraction of the carapace. None have yet been found with calcareous coverings, but they are either hard and siliceous, (sometimes containing a little iron,) or membranaceous without silica. The differences observed in the forms of the carapace have been made use of in classifying them, (see preceding table.) In several genera, are found internal hyaline vesicles of variable form ; these are colorless, and resemble the stomachs of the Polygastrica, and in recent experiments they have been colored by means of indigo. The female organs are colored or colorless granules, forming two or four groups, which are placed near the middle of the body, as in Navicula, Cocconema, Naunema, &c. These eggs are frequently divided into several globular groups which finally unite in a cruciform manner (Achnanthes) or which become confounded together, before being emitted, (Gallionella, Pyxidicula, Isthmia, &c. ;) at other times they appear under the form of a tube enveloping the stomachs and other organs, (Xanthidium, Euastrum, Micrasterias.) The genera Micrasterias, Arthrodesmus, Tessararthra and Xanthidium, have organs which may be compared to seminal vesicles. Spontaneous division produces much variety in the forms of the polypidoms. It sometimes takes place longitudinally, sometimes transversely."

The living species of this family may be found in almost every situation where water occurs upon the surface of the earth. Some genera are exclusively marine, others are exclusively fluviatile, while some genera, as Navicula, Gomphonema, &c., include both salt and fresh water species. The marine species may often be found in great quantities among the filiform Algæ, which they often invest completely with their crystalline carapaces.

The fluviatile species may be found in every pond, stream, rivulet, bog, or pool, either nestling among Confervæ, parasitic on aquatic plants, or living in the sedimentary matter at the bottom. They often occur in such vast quantities as to cover hundreds of square yards, to which they give a peculiar color—green, yellowish or ferruginous, according to the peculiar internal coloring matter of the individuals. Most of the species are exceedingly minute, many are entirely invisible to the naked eye; others, however, are quite perceptible without the aid of the microscope. Notwithstanding their extreme minuteness, it is evident, from their vast abundance, that they have some important offices to perform in the economy of nature; and like the coral insect, although the individuals are minute, the result of their united labors is on a scale by no means insignificant.

Few organic bodies exceed in beauty the symmetrical, elegantly sculptured forms of many of the species. Their beauty, the singular phenomena they present, and the interest they have lately received from being detected in a fossil state in Europe and America, will be sufficient inducement for all lovers of microscopic research to study this family.

I shall now proceed to describe the most interesting American species, commencing with the

DESMIDIACEA.

Desmidium.

Free, carapace simple, urceolate, trilateral, often catenate.—Mandl. and Ehrenberg, l. c. p. 244.

1. *Desmidium Schwartzii. Ag.* (Pl. 1, fig. 1.) "Corpuscles smooth, quadrangular on three sides, slightly emarginate, triangular on the other two sides, end obtuse, ovarium green, $\frac{1}{92}$ to $\frac{1}{96}$ of Paris line."

The corpuscles of this species are united together by their triangular faces, so as to produce long triangular (often twisted) filaments, which are of a beautiful green color, and exceedingly lubricous. Each filament is enveloped in a very transparent gelatinous matter, which is not visible on the dead specimens. The filaments often occur together in great quantities, and form a stratum in the water which is not distinguishable from a mass of some species of Zygnema, with which remarkable genus of Algæ, this presents many points of resemblance. Allusion is made by several writers* to a "curious pinnatifid appearance" which this species presents "before the ultimate separation of the joints." I have not seen the filaments in this state, nor have I met with any detailed account of this change in the few works which I have been able to consult.

This species is extensively diffused in Europe, and appears to be equally so in this country. I have met with it from Rhode Island to Ouisconsin, and south to Virginia; it occurs at West Point in great abundance in ditches and peat bogs, where I have found it most abundant in early spring. I believe Desmidium cylindricum of Greville to be merely a *state* of this species.

2. *Desmidium hexaceros*, *Ehr.* (Compare figs. 2 and 3, Pl. 1.) Corpuscles binary, trilateral, the points drawn out to three horns and truncate at the extremity, $\frac{1}{48}$ of line.

3. *Desmidium aculeatum*, *Ehr.* (Compare figs. 4, 5, and 6, Pl. 1.) Corpuscles spiny trilateral, the points drawn out to three truncate horns, often terminating in three spines.

I copy Ehrenberg's description of the two last species, that they may be compared with the figures referred to above, which represent various binary triangular bodies, some of which agree pretty well with the above characters. They however are so unlike D. Schwartzii, and present so many points of resemblance to Euastrum that I shall describe them as species of that genus.

Staurastrum.

Free, a simple univalve quadrangular carapace.

1. *Staurastrum paradoxum.* Corpuscles rough, single or binary, four setaceous horns in form of a cross.

Micrasterias Staurastrum, Micrasterias tetracera, didicera, tricera. Kutzing, Linnea, Vol. viii, p. 599, Pl. 20, figs. 83, 84, and 85.

St. paradoxum, Meyen, Nov. Act. Nat. Cur., xiv, p. 777, Pl. 43.

"Formed principally of two cells united end to end, and each terminated by cross-shaped prolongations, on which are perceived vestiges of articulation." See Ferussac's Bulletin, June, 1830.

I am not sure that I have yet met with this species in America; I have, however, often seen the binary bodies represented by figs. 3 and 4, Pl. 1, having *four* arms instead of the three represented in our drawing. In the four-armed state they agree closely with the above characters of *S. paradoxum*, as well as with Kutzing's figure 83.

* See *Agardh.* Systema Algarum, p. 15, and *Greville* in British Flora, Vol. v, p. 402.

Pentasterias.

Free, a simple univalve pentagonal carapace.

1. *Pentasterias margaritifera.* Surface granulated, rays thick and obtuse. Mandl. and Ehrenberg, l. c. Pl. 8, fig. 46.

I am unacquainted with this genus, unless it is founded on five-rayed bodies resembling figure 7, which are only varieties of figures 3 and 4, the number of arms being, as I have repeatedly seen, liable to much variation.

Tessararthra.

Free, a simple carapace, univalve, globular, smooth, forming chains of four or more individuals by spontaneous division.

1. *Tessararthra moniliformis.* Corpuscles green, two or four united in a right line. M. and E. l. c. Pl. 8, fig. 47.

I have not noticed this genus in America.

Spherastrum.

Free, a simple carapace, univalve smooth, inflated, forming groups of various forms by imperfect spontaneous division.

I have not yet detected any specimens of this genus.

Xanthidium.

Free, carapace simple, univalve, globular, bristling with points or setæ, isolated, binary or quaternary, (catenate?)

This genus is very interesting, from the fact that bodies, almost identical in form with the living species, occur abundantly, preserved in the fossil state in *flint.* Drawings of several fossil species may be seen in the Annals of Natural History, Pl. 9, accompanying an interesting paper on the Organic Remains in the Flint of Chalk, by the Rev. J. B. Reade. The directions given for finding these bodies in flint, are "to chip off thin fragments which may be attached by means of Canada balsam to slips of glass, and then coated on the outer surface with hard spirit varnish. A hundred specimens may be thus cut, polished, and mounted for the microscope without trouble and expense, and in less time than an expert lapidary could prepare a single slice with the diamond mill and polishing tool." Many of the common gun flints contain these bodies. I am indebted to E. J. Quekett, Esq. of London, for very fine specimens of fossil Xanthidia; among them is a slice of flint prepared by a lapidary, which contains in a space of less than a square inch, as many as eight or ten very perfect Xanthidia, a scale of a fish, and other organic bodies.

There has been much discussion as to the real nature of the bodies in flint which so closely resemble the recent Xanthidia; Turpin mistook them for eggs of Cristatella, to which however they have but little resemblance. Ehrenberg gives figures in his small treatise, "die Fossilen Infusorien und die lebendige Dammerde," Pl. 1, figs. 2, 12 to 17, which show the closest resemblance between the recent and the fossil species. It is remarkable however that the recent species are inhabitants of fresh water, while flint is undoubtedly of marine origin. I have not seen distinct motion in any of our species.

1. *Xanthidium ? ramosum.* Corpuscles globular, separate or binary, spines scattered, terminated in three or more points $\frac{1}{96}$ to $\frac{1}{24}$ line. Fossil in gun flints.

2. *Xanthidium* ———. (Fig. 15, Pl. 1.) Binary, each portion having numerous rather long arms terminating in three diverging points.

This very beautiful American species is, when living, of a fine green color. Its carapace is hard and apparently siliceous, as it retains its form in spite of the action of fire and acids. Every living specimen of this species which I have seen, has been composed of two symmetrical portions, as shown in the figure. These bodies when in the position shown in the figure, have much resemblance to Ehrenberg's drawings illustrating the spontaneous division of some of the species. When thrown into other positions, so that the line of union of the two portions is not seen, it may easily be mistaken for a *simple* spherical body bristling with arms. The two portions often separate after death, and may then be mistaken for individuals resulting from spontaneous division, but by throwing them into various positions by means of a compressor, the orifice corresponding to the line of separation of the two original parts may always be seen. The same remark will apply to the next species also.

It occurs not unfrequently in a subalpine pond a few miles from West Point; it is also occasionally found in ditches, in peat bogs, &c.

3. *Xanthidium* ———. (Fig. 16, *a*, *b*, Pl. 1.) Binary, each portion somewhat triangular and terminating at each angle in three short, diverging arms, each having three small diverging points.

The smaller size and triangular form of these bodies, make me think them of a different species from the preceding. I have not seen any intermediate forms, although they occur together in about equal abundance at the above mentioned locality.

I must here remark that a transition from Xanthidium to Euastrum appears quite evident, through the binary triangular bodies, often having projecting arms, represented in figures 2, 3, 4, 9 and 8, Pl. 1.

ARTHRODESMUS.

Free, carapace simple, univalve, compressed in the form of a plate, or a compressed band, articulated by spontaneous division.

1. *Arthrodesmus quadricaudatus.* (Fig. 17, Pl. 1.) Corpuscles oblong, straight; chain or polypidom of four to eight individuals formed by imperfect spontaneous division, four horns, (the middle corpuscles rounded at the ends, the others with a horn at each end.) Size of corpuscles $\frac{1}{182}$ to $\frac{1}{96}$ line; chains $\frac{1}{48}$ line. *Scenedesmus magnus*, *Meyen.*

This species, as it occurs in this country, is composed of from four to eight green elliptical corpuscles arranged in a thread or chain, usually having horns only on the two extreme corpuscles, but not unfrequently similar projections may be seen on the middle ones also. Meyen, who considers this as a plant, states that in spring it contains starch globules.* It is extensively diffused, both in Europe and in this country; I have noticed it as far west as Ouisconsin, and south to Virginia. It is abundant in ponds near West Point, N. Y.

2. *Arthrodesmus cutus.* (Fig. 18, *a*, *b*, Pl. 1.) Corpuscles green, oblong, alternating in a right line, by spontaneous division.

The small green bodies represented in our fig. 18, occur in the well at Fort Putnam, West Point. They appear to belong to this species.

ODONTELLA.

Free, carapace simple, univalve, compressed, form of flattened articulated ribbands, often pierced, produced by imperfect spontaneous division, articulations united by small projections.

I have not *satisfactorily* determined any American species of this genus, although I have occasionally seen small filaments which agreed tolerably well with the generic character.

MICRASTERIAS.

Free, carapace simple, univalve, compressed, groups of a number of individals in form of a flattened star, produced by imperfect spontaneous division.

---

* Meyen, Pflanzen Physiologie, Vol. 3, p.437.

This is a very beautiful genus, its species presenting elegant star-like arrangements of green corpuscles, some of which closely resemble in form the stars and badges of honor worn in Europe.

There appears to me to be much confusion in the specific characters, arising from the circumstance, that the *number* of corpuscles in the different rows has been made a character of specific importance. From what I have seen of the species, I am satisfied that the number of corpuscles in a star is liable to great variation in the same species. Perhaps the *form* of the corpuscles would prove a more certain character.

1. *Micrasterias Tetras.* (Fig. 19, Pl. 1.) Four corpuscles united in form of star, the edge slightly notched.

This is a very minute species, which occurs in ponds near West Point. I have also noticed it in Virginia.

2. *Micrasterias Boryana.* (Fig. 20, Pl. 1.) Ten corpuscles in the exterior circle, five in the interior, and one in the centre, edge acutely dentate. Corpuscles $\frac{1}{80}$ to $\frac{1}{100}$ of line.

It happens *accidentally* that our figure represents an individual having just the number of corpuscles above described. I am satisfied, however, from frequent observation, that the *same species* sometimes has a much greater number of corpuscles, certainly *another row of fifteen* is often developed. It then agrees with M. tricyclia of Ehrenberg. It is a very beautiful object for the microscope. I have found it in New York, Virginia, and in Onisconsin.

3. *Micrasterias* ———. (Fig. 21, Pl. 1.) Corpuscles very numerous, forming large *imperforate* plates of a circular or elliptical form. Exterior corpuscles deeply emarginate, each having two projecting points.

This large and very beautiful species is not uncommon in ponds near West Point.

Euastrum.

Free, carapace simple, univalve, compressed, binary, sometimes quaternary, having the form of a two-lobed disc or lamina, often dentate.

The elegant forms and emerald green color of the species of this genus render them exceedingly fine objects for the microscope. The forms, as usually seen, appear tabular, but when thrown on their sides by means of a compressor, they show considerable thickness.

I have noticed in several species groups of molecules moving actively, precisely like those seen in Closterium. Indeed this genus is most closely allied to Closterium, and some forms occur which show a complete transition from one genus to the other. (See remarks under the head *Closterium.*)

Capt. Carmichael, with his usual acuteness, detected their animal nature. He remarks of two of the species, "these are animals instead of plants, if the faculty of locomotion will entitle them to that rank." (See Hooker's British Flora, V, p. 398.) I have frequently noticed the motion of several species; it is quite as distinct as in Closterium.

1. *Euastrum rota*, *Ehr.* (Fig. 22, Pl. 1.) Body binary, lenticular, discoid, smooth, the edges dentate or spiny, $\frac{1}{24}$ to $\frac{1}{10}$ line. *Echinella rotata*? Greville.

The species represented in our figure appears to be the *E. rota* of Ehrenberg, and agrees pretty well with the account given by Greville of his *Echinella rotata*, which he describes as having the "frond plane, circular, divided by a line passing through the centre, each portion composed of radiating segments cleft nearly to the central line." (See Brit. Flora, V, p. 398.) Having seen no figures of the European species, I cannot be sure of their identity with ours.

The species represented in fig. 22, is quite common in the United States. I have seen it in Rhode Island, New York, Virginia, and Ouisconsin; I have generally found it scattered among Confervæ, but I once in early spring found *many hundreds* of them collected together on the bottom of a very small pool of water in a sphagnous bog. Some variety occurs in the outline; thus the two large central portions of each half are *often* perfectly symmetrical, and not unfrequently dentate near the ends. I have seen specimens twice the size of the one represented.

2. *Euastrum crux melitensis.* (Fig. 23? Pl. 1.) Body binary, lenticular, discoid, smooth, the edges deeply divided into six dentate and spiny rays.

I copy this description, that it may be compared with fig. 23, Pl. 1, which represents a very beautiful form which I have found in various situations near West Point, and also at Staten Island. I have seen it move distinctly.

3. *Euastrum* ———. (Fig. 24, Pl. 1.) This is possibly only a younger state of *E. rota*, (fig. 22,) with which it occurs.

4. *Euastrum* ———. (Fig. 25, Pl. 1.) This very elegant form is somewhat rare. It occurs at West Point with the preceding.

5. *Euastrum* ———. (Fig. 26 and fig. 27, *a*, *b*, *c*, and *d*, Pl. 1.) I suspect that the species represented in figs. 26 and 27, is the same as the Echinella oblonga of Greville, which he describes as being "compressed, oblong crenato-pinnatifid, and lobed, divided transversely almost to the centre." (Brit. Flora, Vol. V, p. 398.)

Fig. 27, *a* and *b*, show two positions of the same individual; *c* and *d* show small individuals, which are probably the young of this species. Fig. 26, although much more deeply lobed than fig. 27, is *probably* only an older state of the same. They occur at West Point, also near Detroit, Michigan, and in Ouisconsin.

6. *Euastrum* ———. (Fig. 28, *a*, *b*.) This species is neither lobed nor undulate; but while the general outline is convex, a minutely serrated edge may be seen. When thrown on its side, (fig. 27, *b*,) it presents an unusual thickness.

If this species is compared with fig. 38, Pl. 1, the close relation existing between the genera *Closterium* and *Euastrum* will be manifest.

7. *Euastrum* ———. (Fig. 29, Pl. 1.) I suspect that the bodies represented in fig. 29, belong to this genus. They consist usually of four somewhat elliptical green bodies placed parallel to each other, and united laterally, as seen in the figure. Each elliptical portion is bidentate at the ends. I have met with specimens in which eight such corpuscles were united, producing such an appearance as would be given if a figure like that shown in fig. 29, had a similar one added immediately below it.

If this is a species of Euastrum, perhaps it may be thus characterized; Euastrum ———. Fig. 29. Binary (or sometimes quaternary,) each corpuscle divided by deep lateral sinuses into two transverse somewhat elliptical bidentate portions, the middle portions longest. Occurs in ponds near West Point.

8. *Euastrum margaritiferum, Ehr.* (Fig. 8, *a*, *b*, Pl. 1.) Body binary, elliptical, each part semi-orbicular with the margin entire, $\frac{1}{120}$ to $\frac{1}{24}$ line. *Heterocarpella tetropthalma, Ktz.* Linn. 1833, Pl. 19, fig. 87.

Our species (fig. 8) agrees pretty well with the above description and with Kutzing's figure. A figure given by Meyen in his Pflanzen Physiologie, Vol. III, Pl. 10, fig. 31, apparently belongs to this species also.

The surface has a great number of minute hemispherical projections, disposed in a quincuncial order. It is possible that the pearl-like appearance of these projections, when seen on the empty carapace, may have suggested the specific name. I have seen this species move quite distinctly, and have also seen in it (as well as other species of Euastrum) groups of moving molecules as in Closterium, sometimes indeed the whole cavity is filled with such particles. Meyen states that the species he represents (l. c. fig. 31, Pl. 10) contains globules of starch.

This species is very common in the neighborhood of West Point. I once found, in the spring of the year, in a small pool caused by the melting of snow in a peat meadow, a large cloud-like mass in the water, which when touched, broke to pieces and became diffused through the water. On examination, it proved to be wholly made up of this species. I have found it in Rhode Island, New York, Ouisconsin, and Virginia.

9. *Euastrum* ———. (Fig. 9, Pl. 1.) Binary, triangular, angles rounded, each corpuscle having several rows of minute points. Hab. West Point.

10. *Euastrum* ———. (Fig. 10, Pl. 1.) Binary, elliptical, each corpuscle having three pairs of long subulate spines. Hab. West Point. I have met with individuals having the spines developed on only one side.

11. *Euastrum* ———. (Fig. 13, Pl. 1.) Binary, corpuscles cordate at base, each having six pairs of short spines.

This is a very pretty species which occurs at West Point. The figure shows the position of two groups of active molecules.

12. *Euastrum* ———. (Figs. 11 and 12, Pl. 1.) Binary, each corpuscle elliptical and terminating at each extremity in a single spine. Hab. West Point.

13. *Euastrum* ———. (Figs. 2, 3, 4, 5, 6, and 7, Pl. 1.) Binary, (sometimes quaternary,) generally triangular, and terminating in three long arms, each of which ends in three minute spines. Hab. West Point.

The number of arms is *usually* three, but I have met with specimens in which one corpuscle had *three* and the other *four* arms, others in which *both* had *four*, and others again in which *both* had *five* arms.

It appears to me that the five-armed variety may have given rise to the genus Pentasterias, (page 290,) and the four-armed ones are possibly the same as Staurastrum, (page 289.) This however is only a conjecture, hazarded without having seen authentic specimens or good figures of those genera. The structure of the arms is exactly as in the Xanthidium, (fig. 15, Pl. 1,) and there is indeed an evident relation between the genera.

The reader is requested to compare some of the figures last referred to with the descriptions of *Desmidium hexaceros* and *D. aculeatum*, page 289.

14. *Euastrum* ———. (Fig. 14, Pl. 1.) Binary, corpuscles triangular, each angle terminating in a sharp spine. Hab. West Point.

I have seen several other species of Euastrum, but the number figured is sufficient to give an idea of the variety and beauty of the forms in this interesting genus.

Closterium. (See Figs. 30 to 38, Pl. 1.)

Ehrenberg makes of this genus a distinct family of Infusoria, which he calls the Closterina, and characterizes thus:

"*Polygastric* (*distinctly or probably*) *without alimentary canal, without appendices, polypidoms having the form of a wand,* ("*baguette,*") *thread or spindle, by spontaneous division, papillæ fixed and movable in the opening of the carapace.*"

I have before stated that I consider the genus Closterium most closely related to Euastrum, and therefore with the Desmidiacea generally. This relation to Euastrum is manifest in their apparent identity in internal structure, the chief difference between them is only in the *external* forms, and even in them, we find there is a perfect transition from the highly lobed and tabular forms of some species of Euastrum, to the entire, elongated and fusiform species of Closterium. It is therefore without hesitation that I place Closterium (as indeed most writers do) among the Desmidiacea.

There has been much discussion of the question, whether the Closteria are plants or animals, and as this inquiry is one of general interest, the decision of which will affect the position of all the family Bacillaria, I may be excused for giving at some length, an account of the present state of the question. *Ehrenberg* gives the following reasons for believing the Closteria to be *animals*. 1. Their voluntary motion. 2. Their terminal openings. 3. The incessantly moving organs placed against the openings and sometimes projecting. 4. Their spontaneous lateral division.

Morren, in his celebrated memoir "Sur les Closteriées," (some notice of which may be found in this Journal, Vol. xxxv, p. 122,) supports the view that the Closteria are *plants*.

Meyen, in his Report on Vegetable Physiology, for 1837, (p. 54 of Francis's translation,) and also in his Pflanzen Physiologie, Vol. III, p. 437, has brought forward the fact of the presence of *starch*, in the Closteria, as conclusive evidence of their being *plants*. He states that the large and small globules in these bodies "at certain times, and particularly in spring, are almost wholly composed of starch." He adds that in the month of May he had observed "many specimens of Closterium in which the whole interior substance was granulated, and all the grains gave with iodine a beautiful blue color, as is the case with starch, which is not an animal product."

In the Annals of Natural History for August, 1840, (No. 33, p. 415,) is given a notice of a paper read by *Mr. Dalrymple* before the Microscopical Society of London. As this paper gives a good idea of the present state of the discussion concerning the nature of the Closteria, I believe that no apology is necessary for taking from it the following extract, especially as my own observations enable me to confirm some of the statements and to correct others.

"The author, after detailing the history of Closterium, from its discovery by Corte in 1774, down to the present time, entered into a detail of its appearance and general structure; he described it as consisting of a green gelatinous and granular body, invested by a highly elastic and contractile membrane, which is attached by variable points to a hard siliceous shell, which was afterwards stated by Mr. C. Varley to resist even the action of boiling nitric acid. The form of Closterium is spindle-shaped or crescentic—the shell consisting of two horns, tapering off more or less to the extremities, and united at the central transverse line, constituting a perfectly symmetrical exterior. At the extremity of each horn is an opening in the shell, which, however, is closed within by the membranous envelope, wanting however in some specimens. Within the shell and at the extremity of the green body, is a transparent chamber containing a variable number of active molecules, measuring from the 20,000th to the 40,000th of an inch; these molecules or transparent spheroids, occasionally escape from this chamber, and circulate vaguely and irregularly between the periphery of the gelatinous body and the shell; further, the parietes of this chamber have a contractile power. The author denied the existence of any papillæ or proboscides at this part, as well as the supposition of Ehrenberg that these moving molecules constitute the basis of such papillæ. He also denied the statement of the same distinguished observer, that if coloring matter was mixed with the water in which the *Closterium* resides, any motion was communicated to the particles of such coloring matter by the supposed papillæ, or by the active molecules within the terminal cells. A circulation of the fluids within the shell was observed, independent of the vague movements of the active molecules; this was regular, passing in two opposite currents, one along the side of the shell, and the other along the periphery of the gelatinous body. When the shell and body of the Closterium was broken by pressure, the green gelatinous matter was forcibly ejected by the contraction of the membranous envelope.

"The action of iodine upon the specimens was very remarkable; 1st, it did not, as reported by Meyen, stain the green body violet or purple, but orange-brown; 2d, it produced violent contraction of the investing membrane of the body, whereby the green matter was often forcibly expelled from the shell at the transverse division; it instantly annihilated the motion of the molecules in the terminal sacs, and the sacs themselves became so distended with fluid as to burst and allow the molecules to escape.

"The mode of reproduction was stated to take place, 1st, by spontaneous division; 2d, by ova; 3d, by interbudding or the conjugation of two *Closteria.*

"The author, after balancing the arguments of the two theories respecting the classification of this body, gave as his reasons for retaining them on the side of the animal kingdom, the following summary:—

"1st. That while Closterium has a circulation of molecules greatly resembling that of plants, it has also a definite organ, unknown in the vegetable world, in which the active molecules appear to enjoy an independent motion, and the parietes of which appear capable of contracting upon its contents.

"2d. That the green gelatinous body is contained in a membranous envelope, which, while it is elastic, contracts also upon the action of certain reagents whose effects cannot be considered purely chemical.

"3d. The comparison of the supposed ova with cytoblasts and cells of plants, precludes the possibility of our considering them as the latter, while the appearance of a vitelline nucleus, transparent but molecular fluid, a chorion or shell, determines them as animal ova. It was shown to be impossible that these eggs had been deposited in the empty shell by other infusoria, or that they were the produce of some entozoon.

"4th. That while it was impossible to determine whether the vague motions of *Closterium* were voluntary or not, yet the idea the author had formed of a suctorial apparatus, forbade his classing them with plants.

"Lastly, in no instance had the action of iodine produced its ordinary effects upon starch or vegetable matter, by coloring it violet or blue, although Meyen asserts it did in his trials.

"The author therefore concluded that *Closterium* must still be retained as an infusory animal, although it is more than doubtful whether it ought to rank with the polygastric families."

Upon the above statements of *Mr. Dalrymple*, I venture to offer the following remarks.

1st. *As to the siliceous nature of the carapace:* Ehrenberg expressly states, (l. c. p. 446,) that "the carapace can be burned and completely volatilized." This statement of Ehrenberg, together with the undoubted *flexibility* of the covering of many of the Closteria, which I have often noticed as wholly unlike the *brittle* siliceous coverings of the Naviculæ, and the fact that I have never found their coverings among the fossil Naviculæ, although the living species of each genus occur abundantly together, all induce me to think the carapace of Closterium can scarcely be siliceous.

2d. *Motions apparently voluntary.*—These are easily seen; I have often been unable to sketch the form of a specimen by means of the camera lucida, as the body was constantly changing its position, and this too when *certainly* undisturbed by the motion of other animalcules or any extraneous cause. Their power of locomotion may also be rendered apparent thus: if a portion of mud covered with Closteria is placed in a glass of water, exposed to light, and the *Closteria* are then buried in the mud, they will soon work their way to the surface, covering it again with a green stratum which may be buried over and over again, with the same results.

3d. *Presence of moving molecules in distinct cavities.*—These are easily seen; generally there is *one* such cavity in each extremity, as indicated in most of our figures of Closterium, (see figs. 30 to 38, Pl. 1,) but sometimes there are *many* such cavities; at other times almost the whole interior appears filled with active molecules, as has already been stated (p. 296) is sometimes the case with Euastrum. In specimens where the cavities at the ends were very distinct, and which also showed very distinctly the circulation referred to by Mr. Dalrymple, I noticed that the form of the cavity containing the active molecules was constantly changing, being sometimes globular, then elongating to the left or right, and then becoming globular again, in a rapid but very irregular manner.

4th. *Presence of distinct circulation.*—This was noticed many years ago by *Gruthuisen*. The account by Mr. Dalrymple, given in the above extract, agrees exactly with what I have seen in several species. The currents are very distinct, so much so, in fact, that they attracted my attention before I was aware that they had been noticed by others.

5th. *Action of Iodine.*—I cannot otherwise account for *Mr. Dalrymple's* statement that iodine "in no instance produced in the Closteria, the violet or blue color indicating starch," than by supposing that the specimens he examined were not in the proper state to exhibit it. *Meyen* expressly states, that it is "at certain times, particularly in spring," that the starch may be detected.

I am able by conclusive experiments to *confirm* Meyen's statements as to the presence of starch in these bodies. In specimens gathered in November, many of which I have still by me in a living state, I find no difficulty in producing the blue or purple color with tincture of iodine. Sometimes, however, the specimen becomes so opaque by the action of this reagent, that the purple color of the granules can only be detected after crushing the specimen by means of the compressor. *The characteristic color of iodide of starch is then shown most distinctly.* I have repeatedly treated in this way the large species, C. trabecula (fig. 32, Pl. 1) as well as others, and have uniformly found that a portion of the interior takes the blue or purple color.

I cannot however consider the presence of starch in these bodies as conclusive evidence that they are plants. Is it not possible that they are animals which feed, wholly or in part, on amylaceous matter extracted from the aquatic plants among which they live? If so, the detection of starch in their stomachs is not surprising.

6th. *Organs of motion and moving papillæ.*—These I have not yet seen, but do not feel authorized to deny their existence, as I am well aware that my microscope,* although a very good one, is probably inferior to the one used by Ehrenberg. It shows the lines on the scales of Podura as well as I have been able to see them by any instruments in this country, yet I have not sufficient confidence in its power, or my skill in using it, to contradict the statements of results obtained by so distinguished an observer as Ehrenberg, in using the best instruments of Europe. I can vouch very positively for what I have seen, but will not pretend that more may not be seen by others.

* Made by Charles Chevalier, 130 Palais Royal, Paris.

I will now proceed to describe briefly some of the most interesting American species of Closterium, giving in connection with each the characters of the European species which *appears* to correspond to our own, as far as I can determine by the brief accounts, usually unaccompanied by figures, in the works to which I have access.

1. *Closterium lunula.* (Fig. 30, Pl. 1.) Semilunar or straight, diminishing gradually towards the rounded extremities, internal glands scattered, green granules arranged in several (10) threads, $\frac{1}{4}$ to $\frac{1}{12}$ line.

2. *Closterium moniliferum.* (Fig. 31, Pl. 1.) Semilunar, never straight, smooth, acute, and rounded at the ends, internal glands in the middle of the body arranged in a moniliform manner, green granules in several rows, of which the three middle ones are most distinct.

Specimens agreeing with the characters of each of the above species are common in this country. They do not however appear to be specifically distinct. They may be easily recognized by their smooth, green, crescent-like forms.

3. *Closterium trabecula.* (Fig. 32? Pl. 1.) Straight, cylindrical, contracted in the middle, smooth, the ends truncate, glands scattered or in several series, numerous obscure bands, $\frac{1}{12}$ to $\frac{1}{5}$ line.

I have seen no figure of this European species, but I nevertheless venture to refer to it, the fine species represented in figure 32. This is the *largest* Closterium which I have seen in the United States. It occurs at West Point, at Staten Island, and in Virginia.

Its motions are quite distinct, the cavities containing moving particles very apparent, and what appear to be terminal openings may be easily seen. By application of tincture of iodine, and then crushing the specimen under the compressor, starch globules may easily be detected. In crushing, the globules are often forced out at the terminal openings, and on relieving the pressure are drawn back again. No rupture of a membrane at these points was perceived.

4. *Closterium digitus.* (Fig. 33? Pl. 1.) Straight, oval, cylindrical, four or five times longer than broad, smooth, the ends very much rounded, sometimes showing traces of a spontaneous triple division, longitudinal bands often denticulate, $\frac{1}{20}$ to $\frac{1}{10}$ line.

With this account may be compared fig. 33, Pl. 1, which represents a species not uncommon at West Point, and which I have also seen in Rhode Island, Virginia, and Ouisconsin.

Its endochrome usually presents a central mass, from which several (10?) undulating ridges radiate to the carapace. It is a very elegant species.

5. *Closterium lineatum.* (Fig. 34? Pl. 1.) Very long, acute, slightly arcuate, cylindrical, filiform in the middle, the ends truncate, and very acute, ("*très amincis*,") ridges distinct, in form of smooth lines. Often thirty times longer than broad, $\frac{1}{18}$ to $\frac{1}{3}$ of a line.

Compare with this fig. 34, Pl. 1, which represents a species quite common in ponds near West Point, and which also occurs in Virginia. The figure represents only one half of the excessively elongated body. In the living specimens, the endochrome shows distinct ridges.

6. *Closterium striolatum.* (Fig. 35? Pl. 1.) Fusiform and arcuate, ends acute and truncate, ridges smooth, not deep, ten or twelve times longer than broad, $\frac{1}{10}$ line.

Compare fig. 35, Pl. 1, which represents a common species.

7. *Closterium rostratum*, *Ehr.* Fusiform, slender, ends acute, setaceous horns about as long as the body, sometimes shorter. *C. acus*, *Ktz.* Linn., 1833, fig. 81.

I suspect the species shown in fig. 36, Pl. 1, is a young state of this species. I found it among Lemna minor, on Staten Island, New York. Fig. 36, *a*, shows an individual produced by spontaneous division, one portion of which is still imperfectly developed.

8. *Closterium tenue*, *Ktz.* "Corpusculis minutis lineari-lanceolatis, viridi hyalinis, transverse fasciatis acutis." See Linnæa, 1833, Pl. 8, fig. 78.

I find no notice of this species in the extract from Ehrenberg's work, appended to Mandl's work on the microscope, but as Kuetzing's fig. 78 resembles our species, (fig. 37, Pl. 1,) I quote his description, that they may be compared.

Our species occurs in vast abundance on the muddy bottom of a brook which crosses the Canterbury road, a few miles from West Point. It forms a mass of such extent, and of so bright a green color, that I at first mistook it for a layer of Oscillatoria.

There are very fine transverse lines, often visible on the carapace, and it often appears as if a portion of the shell between these lines (as at *a*, *b*, fig. 37, Pl. 1) had been removed. Its motions are distinct and lively.

9. *Closterium* ———. (Fig. 38, Pl. 1.) Nearly cylindrical, contracted in the middle, ends obtuse, and in one position showing a re-

entering fold of the carapace. Hab. ponds near West Point. A similar fold in the carapace is visible in some species of Euastrum. (See fig. 27, Pl. 1.)

MICROTHECA.

Free, carapace simple, univalve, compressed, separate, lamelliform.

M. octoceros. Carapace quadrangular, hyaline, four spines at each end, internal body golden yellow.

I am unacquainted with this genus.

End of the Desmidiacea.

Note.—Having, in what precedes, completed a sketch of the Desmidiacea, I propose to offer in the next part of this memoir (now nearly ready for the press) some account of the other divisions of the Bacillaria, viz. the *Naviculacea*, the *Echinellea* and the *Lacernata*. Many of the species of these families have *siliceous* coverings and are found in a *fossil* state. An account of our American species will therefore, I trust, be found of some interest to American geologists, especially as the recent discovery by Prof. W. B. Rogers, of the vast infusorial stratum in the tertiary of Virginia,* cannot fail to attract new attention to these interesting bodies.

Explanation of the Figures, on Plate I.—The sketches which accompany this memoir, were all made by means of a camera lucida eyepiece attached to Chevalier's horizontal and vertical microscope. In fig. 39 is shown a sketch of $\frac{10}{100}$ths of a *millimetre*, magnified and drawn with the *same* combination of lenses, camera lucida, distance, &c. This being *equally magnified* with the drawings, will serve as a scale to determine the *absolute dimensions* of any of the objects. The highest power of the instrument was not used in getting the outlines, as it would have given figures inconveniently large. The details were, however, occasionally corrected or confirmed, by the use of a higher power.

PLATE I.

Fig. 1. Desmidium Schwartzii, page 288, from a twisted portion of a filament.

Fig. 2 and 3. Euastrum? page 296, two positions of same individual.

Fig. 4 and 5. Euastrum? a larger variety of the same species as fig. 3.

Fig. 5. Shows the same in side view, the lower half being in a state of imperfect development.

Fig. 6. Variety of fig. 3?

Fig. 7. Five-armed variety of fig. 4, page 296.

---

* See a notice of this discovery in the present volume of this Journal, p. 214.

Fig. 8. a. b. Euastrum margaritiferum, page 295, a. b. different position of same individual.

Fig. 9. a. b. Euastrum ——, page 296, a. b. two positions.

Fig. 10. Euastrum ——, page 296.

Fig. 11 and 12. Euastrum ——, page 296.

Fig. 13. Euastrum ——, page 296.

Fig. 14. Euastrum ——, page 297.

Fig. 15. Xanthidium ——, page 291, a recent species from West Point.

Fig. 16. a. b. Xanthidium ——, page 291, another species from West Point.

Fig. 17. Arthrodesmus quadricaudatus, page 292.

Fig. 18. a. b. Arthrodesmus cutus? page 292.

Fig. 19. Micrasterias Tetras, page 293.

Fig. 20. Micrasterias Boryana, page 293.

Fig. 21. Micrasterias ——, page 293.

Fig. 22. Euastrum rota? page 294.

Fig. 23. Euastrum Crux-Melitensis? page 294.

Fig. 24. Euastrum ——, page 294, possibly a young state of E. rota.

Fig. 25. Euastrum ——, page 295.

Fig. 26 and 27. 27 a. b. c. d. *Euastrum* ——, page 295, (Echinella oblonga of Greville?) a. b. two positions of same individual, c. d. smaller individuals.

Fig. 28. a. b. Euastrum ——, page 295, two portions.

Fig. 29. Euastrum ——, page 295.

Fig. 30. Closterium lunula, page 302.

Fig. 31. Closterium moniliferum, page 302.

Fig. 32. Closterium trabecula? page 302.

Fig. 33. Closterium digitus? page 303.

Fig. 34. Closterium lineatum? page 303.

Fig. 35. Closterium striolatum, page 303.

Fig. 36. Closterium rostratum, page 303.

Fig. 37. Closterium tenue? page 303.

Fig. 38. Closterium ——, page 303.

Fig. 39. Scale, each division of which represents $\frac{1}{100}$th of a millimetre magnified equally with the sketches.

Note.—Since the preceding sketch was sent to press, I have seen in Schlechtendal's Linnæa for 1840, p. 201, a valuable memoir, by *J. Meneghini*, entitled *Synopsis Desmidiearum hucusque cognitarum.* It gives a very good account of the genera and species, with copious lists of synonyms. I find my views as to the relations of several genera confirmed on perusal of this Synopsis, which I recommend to the attention of students of this curious class of bodies. J. W. B.

Art. V.—*Observations on Bills of Mortality, with a proposal for their improvement;* by James Mease, M. D., &c.

I have had for many years the subject of the longevity of the people of the United States under consideration, and have made large collections of the deaths of our citizens throughout the Union who reached the age of eighty years, with the view of showing, as I hope to show, that the chances for long life are as great in the United States as in any European country, whatever may be its reputation for salubrity. I confine my idea of longevity to the age of eighty years, and reject in my list of the names of deceased persons, all those who did not reach that number of years; but I might with more propriety have taken seventy as my maximum, because I think that any climate which permits a human being to attain that age, may justly be termed healthy, and we know that where one person is capable of rendering him or herself useful, after that age, fifty at least, do little more than vegetate.*

Being otherwise engaged at present, I cannot enter fully on this subject, and shall therefore merely state the principle upon which I intend to form my calculations, and suggest an improvement in the bills of mortality.

The true object of publishing the bills of mortality is, first, to enable any inquirer to ascertain the comparative healthiness of some city or locality with others in different parts of the same or in a foreign country, (the population being the same,) and secondly, to inform us of the diseases peculiar to either, or of their general nature, that they may be compared with those in other places at home or abroad. Now a just estimate of these objects of inquiry cannot be formed in the United States at least, by reason of the practice of including in the bills, deaths from *every* cause, whereas I have long thought, that no death ought to be

---

* "The days of our years are threescore and ten, and if by reason of strength, they be fourscore years, yet is their strength labor and sorrow." Psalms, xc. Dr. Watts thus beautifully versifies this passage:

"Our age to seventy years is set,
How short the time, how frail the state!
And if to eighty we arrive,
We rather sigh and groan than live."

noticed in the general result of an annual bill of mortality, except those which are incident to mortality in all countries, from internal or constitutional causes, or which may be fairly ascribed to or are influenced by climate; all others being merely adventitious ought to be excluded. If it should be thought necessary to record the precise number of persons who finish their mortal career in any place, a column might be specially appropriated to these extraneous causes of death, and such an improvement I suggested several years since for the bills of mortality in Philadelphia, but without effect.*

The items which I think ought to be omitted in the regular columns of all bills of mortality, are burns, childbed, contusions, casualties, drowned, drunkenness, fractures of all parts of the body, hemorrhage, intemperance, inanition, injury of the head, of the brain, of the spine, of the hip; duels, laudanum, *mania a potu*, (madness from rum,) found dead, murdered, syphilis, still-born, small-pox, varioloid, suicide, unknown, neglect. These items are taken from the Philadelphia bills. I add hanged, Asiatic cholera, (which was included in the Philadelphia bills for the year 1832,) any other epidemic, deaths from the almshouse and prisons, which are included in the Philadelphia bills, and "died abroad."

The inutility of specifying particular fractures and injuries in separate lines, and of giving one line to each special fracture causing death, and separate lines to drunkenness, intemperance, and *mania a potu*, must be obvious upon a moment's reflection. The two first ought to be included in the line or column for "casualties," and the three last placed in one line after the head "alcohol." "Laudanum," "morphine to excess," and "poisoning," should be in one line after the last head. Malignant cholera "comes we know not how, it goes we know not where;"† and therefore cannot be claimed exclusively as an indigene by

* Since writing the foregoing, I find by reference to the British Annual Register, that in the Edinburgh bills, there is a column with the head "casualties," and comprises all those deaths which may be properly arranged under it.

† We know that it first appeared in the army of the Marquis of Hastings, in the year 1817, when encamped during a military expedition in the sandy soil, and under the burning sun of Bengal, and that it visited Europe and North America, unaffected by the cold of Russia, the moisture of England, the dry climate of France, or the variable climates of Canada and the United States. In 1836, it prevailed in Italy, and since that year I have not heard of it.

any country, but being merely accidental ought not to swell the regular bills, although it may be proper to note in an extra column the number cut off by it. The same may be said of influenza, or any other epidemic. In Philadelphia, nine hundred and forty-eight persons died of cholera in the year 1832. I found the singular item, "died abroad," only in the bill of mortality for Marblehead, in Massachusetts, for 1823, between which year and that of 1801, the number amounted to one hundred and fifty-four. The London bills, inserted in the Annual Register until within five or six years, record among the causes of deaths, "headmold-shot" and "horseshoe-head."* The deaths in the alms-houses ought also to be excluded. I am authorized to say, that nineteen of twenty of the diseases causing these deaths in the Philadelphia alms-house, originated from the intemperate use of ardent spirits, and besides that very many of the victims consist of vagabonds, who had no legal residence in the city or county of Philadelphia, but may have recently come from Europe, from other states, or the interior of this state, and having been found destitute or sick, are sent to the great depot on the principle of humanity. The deaths in the county prison and in the eastern penitentiary are inserted in the annual bills, and since the year 1837 the interments from the county of Philadelphia are made to increase the annual sum total. Now as the solitary cells in which the prisoners are confined are all above ground, and are dry and warm, and the prisoners perform just so much light labor as promotes health, and have wholesome food, the conclusion is, that their diseases, with very few exceptions, are the result of previous irregular lives; at least they are not the effects of solitary confinement.

To show how greatly the items objected to swell the sum total of the bills of mortality, I will state that in the year

1820	they	amounted	to	317,	total	mortality	being 3,374
1824	"	"	"	603,	"	"	" 4,399
1828	"	"	"	618,	"	"	" 4,292
1832	"	"	"	1,577,	"	"	" 6,699

---

* *Headmold-shot* is a disease in children, in which the sutures of the skull, usually the coronal, ride, that is, when their edges shoot over one another, and are so close locked as to compress the brain, often occasioning convulsions and death.

Horseshoe-head is also an infantile disease, *opposite to headmold-shot;* the sutures of the skull being too open.—*Webster's Dictionary*, 1841.

1835	they	amounted	to	857,	total	mortality	being	5,666
1836	"	"	"	762,	"	"	"	5,357
1837	"	"	"	582,	"	"	"	5,202
1838	"	"	"	623,	"	"	"	5,462
1839	"	"	"	555,	"	"	"	5,113
1840	"	"	"	657,	"	"	"	4,949

Deaths in the alms-house in

1824,	590,	1833,	406,			
1825,	346,	1834,	356,			
1826,	337,	1835,	398,			
1827,	443,	1836,	445,			
1828,	356,	1837,	365,	from the	country,	6
1829,	424,	1838,	295,	"	"	36
1830,	390,	1839,	269,	"	"	39
1831,	414,	1840,	355,	"	"	54
1832,	590,					

The useful and practical bearing of the improvement I suggest, if adopted, would be the enabling any one to form a just estimate of the health of a country or city, and secondly to establish a basis for premiums on life insurance. Persons desirous to do either would naturally first ascertain the population of the place, then the number of deaths annually, and from these data would calculate the proportion of one to the other, and draw his conclusions therefrom. A series of years, say twenty, would of course be taken. Now any one may easily imagine the magnitude of the influence in favor of or against the character of a country or city for salubrity, which the retention or rejection of the proscribed causes and places of death must have upon the result. In the one case a numerous list of these causes, altogether accidental and adventitious, will lead to unfair conclusions on the point in question, while in the other, none but such as may be termed legitimate would be taken into consideration. Upon the first mode of procedure, Philadelphia, for instance, would show an alarming but unjust disproportion of deaths to its population, and if the result were to determine the premium of life insurance in an European office upon a resident in it, a high one (if any risk were taken)* must necessarily be

* It is probable that owing to a conclusion or calculation of the increased risk of life in the United States, and founded on the great proportion of deaths to the

asked, while upon the second mode she would stand as she deserves, high in the scale of health, and lead to the demand of a small premium. As the question is one of figures, and I have stated the data, the problem may be easily solved, and the result I am satisfied will be conclusive as to the justness of the principle I lay down for our rule of conduct on the two occasions I have mentioned above. It may be said in favor of continuing the old form in the bills of mortality, that as the deaths from all causes are enumerated in them in every country where they are published, the influence of the accidental sources of death I object to would operate equally in one place as in another; such an argument has in fact been urged when I have introduced the subject in conversation, but its weakness must be apparent when we reflect upon the very great difference in the proportionate number of mortal accidents in different countries; in cities for instance they are numerous, and in some much greater than in others.

Art. VI.—*Meteorological Observations made at Hudson, Ohio, lat.* 41° 14′ 40″ *N.; long.* 5^h 25^m $47^s.5$ *W.; during the years* 1838, 1839 *and* 1840; by Elias Loomis, Prof. of Mathematics and Natural Philosophy in Western Reserve College.

In the autumn of 1837 I arrived at Hudson with a set of meteorological and other instruments, recently purchased in Europe. It was some time before a suitable place could be provided for their reception, and they were not observed with much regularity until March, 1838. During the first three months the register was frequently interrupted, but since August, 1838, only one observation has been lost. In vol. xxxvi, pp. 165–173 of this Journal, is given a specimen of the register and a description of the instruments. It will be sufficient to add, that the position of the instruments has throughout remained unchanged, and the same hours of observation adhered to. It is proposed now to

---

population, which the retention of their causes in the bills I object to made apparent, was the reason why an insurance company in England refused to continue a policy on the life of Mr. Dunglison when about to embark for the United States, unless the premium were doubled.—*Dunglison's Hygiène, Sect.* 4, *p.* 132.

give the monthly averages of the observations, with such particulars as are thought most worthy of notice.

BAROMETER.

MONTHS.		1839. Barometer uncorrected.	1839. Attached Thermom.	1839. Barometer corrected.	1839. Diurnal oscillation.	1838. Barometer uncorrected.	1838. Attached Thermom.	1838. Barometer corrected.	1838. Diurnal oscillation.	1840. Barometer uncorrected.	1840. Attached Thermom.	1840. Barometer corrected.	1840. Diurnal oscillation.	Mean. Barometer corrected.	Mean. Diurnal oscillation.
March,	9 A. M.	28.863	47°.5	28.820	.054	28.872	40°.1	28.848	.044	28.712	44°.0	28.678	.038	28.782	.045
	3 P. M.	.811	48.3	.766		.829	40.6	.804		.673	43.9	.640		.737	
April,	9 A. M.	.774	44.9	.738	040	.893	56.4	.827	.057	.914	53.3	.856	.034	.807	.044
	3 P. M.	.736	45.6	.698		.838	57.2	.770		.880	53.3	.822		.763	
May,	9 A. M.	.772	52.0	.718	.038	.803	62.4	.722	.052	.838	61.8	.758	.046	.733	.046
	3 P. M.	.737	53.2	.680		.754	63.5	.670		.793	62.2	.712		.687	
June,	9 A. M.	.866	68.3	.770	.031	.832	64.0	.747	.031	.903	68.1	.805	.040	.774	.034
	3 P. M.	.840	70.0	.739		.804	65.2	.716		.866	69.4	.765		.740	
July,	9 A. M.	.944	73.5	.834	.037	.889	71.0	.786	.034	29.017	69.6	.916	.033	.845	.034
	3 P. M	.912	75.6	.797		.859	72.5	.752		28.987	71.1	.883		.811	
Aug.,	9 A. M	29.004	70.9	.898	.039	.964	65.7	.874	.039	.923	68.9	.825	.041	.866	.040
	3 P. M.	28.969	73.4	.859		.930	67.6	.835		.885	69.8	.784		.826	
Sept.,	9 A. M.	29.021	61.1	.942	.054	.905	59.5	.831	.047	.980	58.4	.909	.039	.894	.047
	3 P. M.	28.973	63.8	.888		.859	60.0	.784		.943	59.2	.870		.847	
Oct.,	9 A. M.	.844	49.4	.796	.037	29.036	56.9	.968	.069	.907	52.3	.852	.047	.872	.051
	3 P. M.	.807	49.6	.759		28.969	57.8	.899		.862	52.8	.805		.821	
Nov.,	9 A. M	.954	38.2	.935	.062	.890	39.3	.868	.041	.827	39.9	.804	.048	.869	.050
	3 P. M.	.891	38.0	.873		.848	39.0	.827		.780	40.1	.756		.819	
Dec.,	9 A. M.	.785	28.8	.790	.040	.748	33.6	.741	.031	.853	30.0	.855	.034	.795	.035
	3 P. M.	.745	28.7	.750		.717	33.5	.710		.819	30.1	.821		.760	
Jan.,	9 A. M.	.902	33.2	.896	.060	.840	24.3	.857	.042	.813	29.8	.816	.030	.856	.044
	3 P. M.	.842	33.3	.836		.800	24.9	.815		.784	30.1	.786		.812	
Feb.,	9 A. M.	.912	35.1	.901	.048	.916	39.4	.894	.065	.723	27.3	.732	.041	.842	.051
	3 P. M.	.865	35.5	.853		.847	37.9	.829		.682	27.5	.691		.791	

The mean diurnal oscillation is for the spring and summer months 0.0405; autumn and winter months, 0.0463. Difference 0.0058. It is least of all in summer, and greatest in the autumn. Average for the year, 0.0434. Prof. Forbes's formula, $z = .1193 \cos.^{\frac{5}{2}} \theta - .0150$, z being the oscillation in latitude θ, gives for Hudson $z = 0.0435$. The accordance is certainly remarkable.

The mean of all the above observations gives for the spring months 28.751; summer 28.810; autumn 28.853; winter 28.809; the mean pressure being .102 greater in autumn than spring. So large a difference derived from three years' observations might be regarded as indicating a law of nature. According to observations of fourteen years at London, the mean pressure was least in spring and greatest in winter, the difference amounting to .038. I have sought for information on this subject from other barometric observations made in this country, but they are almost without exception uncorrected for effect of temperature, nor are any data furnished for computing it; and even if the temperature of the mercury were given, the barometers are seldom to be depended upon, even for *differential* observations.

According to observations for five years at Montreal, by Mr. M'Cord, the mean pressure was greatest in autumn and least in summer, the difference being .090. This coincides in part with my own result. The fluctuations of the barometer are however so great and seemingly anomalous, that observations need to be continued for a long period to establish any general law. The mean pressure at Hudson for the three years is 28.806 inches. In order to appreciate the value of this result, we must compare it with observations made at other stations. The following table exhibits the mean pressure of the atmosphere at the places named, according to M. Schouw, the heights being reduced to the level of the sea at the temperature of 32° F., and corrected for the change of gravity in different latitudes. The table is found in the Comptes Rendus des Séances de l'Académie, 1836, i, 573.

Place.	Latitude.		Barom.	Place.	Latitude.		Barom.
Cape G. Hope,	33°	56′s.	30.008	London,	51°	28′n.	29.977
Rio Janeiro,	22	56′s.	.026	Altona,	53	32	.961
Christianborg,	5	24′n.	29.849	Dantzic,	54	21	.952
La Guayra,	10	48	.856	Königsberg,	54	43	.967
St. Thomas,	19	0	.881	Apenrade,	55	3	.950
Macao,	22	10	.985	Edinburgh,	55	57	.882
Teneriffe,	28	18	30.044	Christiania,	59	54	.907
Madeira,	32	37	.093	Hardanger,	60	00	.841
Tripoli,	32	55	.182	Bergen,	60	10	.843
Palermo,	38	7	.019	Reikiavig,	64	00	.654
Naples,	40	52	.003	Godthaab,	64	00	.651
Florence,	43	47	29.993	Eyafiord,	65	50	.721
Avignon,	43	56	.999	Godhavn,	68	00	.731
Bologna,	44	30	30.006	Upenavik,	73	00	.796
Padua,	45	24	.007	Melville Isl.	74	30	.872
Paris,	48	50	29.988	Spitzbergen,	75	30	.862

The depression of the barometer near the equator, as well as in high northern latitudes, is strikingly exhibited by this table. With this standard let us compare some observations made in this country. The data for New York are furnished by Mr. Redfield, Am. Journal, vol. xxxviii, p. 326. Those for Montreal, by Mr. M'Cord, being the mean of five years' observations, and those for Quebec by Mr. Watts, N. Y. Regents' Report for 1837, p. 223. The reduction to the level of the sea was computed by the formula of Laplace, Mec. Cel. vol. iv, p. 572, and the correction for change of gravity from the formula $H=H'(1-0.0025935 \cos. 2\,\Phi,)$ where H represents the height of the barometer in latitude Φ, and H′ the height corresponding to latitude 45°.

Hudson, - -	28.806	New York, -	30.086
Zero error, - -	+.007	Cor'n for temperature,	−.108
Reduc. for 1131 feet,	+1.231	Reduc. for 10 feet,	+.011
Correction for gravity,	−.010	Correction for gravity,	−.012
	30.034		29.977
Montreal, - -	29.886	Quebec, - -	29.543
Reduc. for 91 feet,	+.102	Reduc. for 330 feet,	+.371
Correction for gravity,	+.001	Correction for gravity,	+.005
	29.989		29.919

In the transactions of the Albany Institute, vol. ii, p. 156, the zero error of the Quebec barometer is stated at +0.110 compared with that at Montreal. It is presumed that this correction has been applied to the above mean result. The elevation of the Montreal barometer is referred to the tide-waters of the St. Lawrence; that of Quebec to the level of the St. Lawrence. It is not known how far this exceeds the mean level of the sea, but probably not much, as the tide is said to rise at Quebec. As it is now clearly proved that the mean pressure of the atmosphere at the level of the sea is *not* every where the same, a most extensive field of inquiry is presented to determine experimentally the law of its variation. In particular, it is desirable to know, whether over an elevated continent the pressure is the same as at the same height *over* the sea in the same latitude. In order, however, that observations may be of any value for this purpose, they should embrace a period of several years, the zero error of the instrument should be accurately determined, and also the elevation above the level of the sea.

The following table exhibits the instances, corrected for temperature, in which the barometer has risen above 29.25. Although the barometer may have remained at this height during the period of several successive observations, it is regarded as but one instance.

1838, March 25, 9 A. M.	29.252	1839, Feb. 18, 3 P. M.	29.265
Oct. 17, 9 A. M.	.305	March 31, 9 A. M.	.397
Nov. 10, 11 A. M.	.466	Oct. 21, 9 A. M.	.301
Nov. 26, 9 A. M.	.251	Nov. 22, 9 A. M.	.560
Nov. 29, 9 A. M.	.295	1840, Jan. 26, 9 A. M.	.358
Dec. 31, midn't.	.515	Dec. 4, 9 A. M.	.272
1839, Feb. 6, 3 P. M.	.298	1841, Jan. 19, 9 A. M.	.479

The following table exhibits the instances in which the barometer has sunk below 28.25.

1838, Feb. 16,	1 P. M.	28.122	1840, Mar. 24,	11½ A. M.	27.953
Dec. 22,	4 A. M.	.079	May 3,	6½ P. M.	.964
1839, Feb. 28,	3 P. M.	.164	Nov. 22,	3 P. M.	28.234
May 27,	3 P. M.	.240	Dec. 15,	3 P. M.	.207
Nov. 5,	3 P. M.	.161	Dec. 26,	8 A. M.	.035
1840, Jan. 30,	7 A. M.	.009			

The greatest range of the barometer for the three years is 1.607; and it appears that, with a few extraordinary exceptions, the oscillations are confined to one inch, the exceptions being but twenty-five in three years. The greatest range at Montreal for five years is 2.138; at New York in seven years, 2.25. If the range of the barometer were proportioned to its absolute height, this would give for Hudson 2.163. At Boston, during a period of twelve years, the range of the barometer is stated at 2.50. It may then be naturally inferred, that for considerable elevations, the range of the barometer diminishes more rapidly than its absolute height.

Fluctuations of the barometer, not corrected for temperature, exceeding .6 inch in twenty-four hours.

Date.	Barom	Oscillation.
1838. Feb. 16, 1 P. M.	28.086	.680 in 21 hrs.
17, 10 A. M.	28.766	
March 4, 10 A. M.	29.064	.686 in 20 hrs.
5, 6 A. M.	28.378	
Nov. 5, 3 P. M.	28.327	.594 in 18 hrs.
6, 9 A. M.	28.921	.608 in 24 hrs.
6, 3 P. M	28.935	
Dec. 11, 3 P. M.	28.449	.601 in 24 hrs.
12, 3 P. M.	29.050	
28, 9 A. M.	29.008	.694 in 21 hrs.
29, 6 A. M.	28.314	
1839. Jan. 7, 8 P. M.	28.437	.609 in 13 hrs.
8, 9 A. M.	29.046	
21, 3 P. M.	28.808	.503 in 24 hrs.
22, 3 P. M.	28.305	
midnight	28.288	.512 in 7 hrs.
23, 7 A. M.	28.800	.643 in 15 hrs.
23, 3 P. M.	28.931	
Feb. 28, 3 P. M.	28.188	.614 in 18 hrs.
March 1, 9 A. M.	28.802	
29, 9 A. M.	28.535	.708 in 24 hrs.
30, 9 A. M.	29.243	
1840. Jan. 23, 2 A. M.	28.301	.633 in 31 hrs.
24, 9 A. M.	28.939	

Date.	Barom	Oscillation.
1840. Jan. 29, 9 A. M.	28.634	.596 in 22 hrs.
30, 7 A. M.	28.038	
30, 9 A. M.	28.043	.874 in 24 hrs.
31, 9 A. M.	28.917	
Feb. 12, 3 P. M.	28.596	.527 in 18 hrs.
13, 9 A. M.	29.123	
13, 3 P. M.	29.092	.711 in 24 hrs.
14, 3 P. M.	28.331	.783 in 18 hrs.
15, 9 A. M.	29.164	
March 23, 9 A. M.	28.850	.813 in 24 hrs.
23, 3 P. M.	28.736	.699 in 18 hrs.
24, 9 A. M.	28.037	
24, 11½ A. M.	27.953	
24, 3 P. M.	28.073	.614 in 21 hrs.
25, 9 A. M.	28.567	
April 12, 9 A. M.	28.532	.665 in 24 hrs.
13, 9 A. M.	29.197	
Dec. 25, 9 A. M.	28.816	.781 in 23 hrs.
26, 8 A. M.	28.035	.681 in 25 hrs.
27, 9 A. M	28.716	
1841. Jan. 17, 9 A. M.	28.594	.712 in 24 hrs.
18, 9 A. M.	29.306	

The greatest range in twenty-four hours was .874, Jan. 30, 1840; but the most remarkable motion of all was that on the 23d of January, 1839, .512 in seven hours. It was accompanied by a most violent wind from the north, and a heavy snow. It will be observed that these extraordinary fluctuations occur chiefly in winter; not one occurred in the summer months. The same remark applies to the table of maxima and minima given before.

THERMOMETER AND HYGROMETER.

		1838.		1839.		1840.		Mean.		
		Therm.	Hygro.	Therm.	Hygro.	Therm.	Hygro.	Therm.	Hygro.	Diff.
March,	9 A. M.	45°.3	36°4	35°.1	29°.1	39°.4	33°.6	39°.9	33°.0	6°.9
	3 P. M.	55.5	41.4	43.5	29.5	46.8	34 1	48.6	35.0	13 6
April,	9 A. M.	43.2	35.7	56 2	43.0	52.6	44.7	50.7	41.1	9 6
	3 P. M.	50 3	36.5	67.4	46.2	59.3	46 2	59.0	43.0	16.0
May,	9 A. M.	49.8	42.9	61.0	53.9	62.0	54 6	57.6	50.5	7.1
	3 P. M.	53.6	45.7	68.8	55.6	68.4	53.3	63.6	51.5	12.1
June,	9 A. M.	70.2	63.8	64.1	58.2	69.2	61.8	67.8	61.3	6.5
	3 P. M.	75.3	66.1	69.7	61.2	75.4	64.1	73.5	63.8	9.7
July,	9 A. M.	75.9	70.8	71.8	66.0	71.9	65.1	73.2	67.3	5.9
	3 P. M.	82.1	73.1	78.3	67.5	76.7	66.3	79.0	69.0	10.0
Aug.,	9 A. M.	72.9	67.2	67 5	60.1	69.9	65.1	70.1	64.1	6.0
	3 P. M.	80.4	70.5	73.9	61 8	75.8	66.7	76.7	66.3	10.4
Sept.,	9 A. M.	61.3	55.2	57.7	54.8	57.3	54.5	58.8	54.8	4.0
	3 P. M.	72.1	58.3	64.3	55.7	65.2	55.8	67.2	56.6	10.6
Oct.,	9 A. M.	45.4	41.0	54.5	50.1	49.9	45.9	49.9	45.7	4.2
	3 P. M.	51.8	40.8	64.3	53.0	56 9	47.9	57.7	47.2	10.5
Nov.,	9 A. M.	32.0	28.1	33 8	28.8	36.9	33.1	34.2	30.0	4.2
	3 P. M.	38 8	30.6	39.3	30.4	44.2	34.0	40.8	31 7	9.1
Dec.,	9 A. M.	22.2	18.4	28.6	26.3	26.8	23.6	25.9	22.8	3.1
	3 P. M.	27.7	19 9	33.2	27.3	30.9	24 1	30.6	23.8	6.8
Jan.,	9 A. M.	28.0	23.9	20 1	16.3	26 6	23.2	24.9	21.1	3.8
	3 P. M.	34.3	26 2	27 3	18.3	32.3	25.6	31.3	23.4	7.9
Feb.,	9 A. M.	28.8	23.4	35.4	31.7	25.1	20.3	29.8	25.1	4.7
	3 P. M.	37.3	26.8	44.5	31.9	33.4	20.9	38.4	26.5	11.9

Average for the seasons.

		Thermom.	Hygrom.	Difference.
Spring, . . .	9 A. M.	49°.4	41°.5	7°.9
	3 P. M.	57.1	43.2	13.9
Summer, . . .	9 A. M.	70.4	64.2	6.2
	3 P. M.	76.4	66.4	10.0
Autumn, . . .	9 A. M.	47.6	43.5	4.1
	3 P. M.	55.2	45.2	10.0
Winter, . . .	9 A. M.	26.9	23.0	3.9
	3 P. M.	33.4	24.6	8.8
Year, ...	9 A. M.	48.6	43.1	5.5
	3 P. M.	55.5	44.8	10.7

It appears, then, that spring is the driest season, and April the driest month of the year; winter is the most humid season, and December the most humid month. The amount of vapor in the air varies but little with the hour of the day, the mean difference in the dew point at 9 A. M. and 3 P. M. being less than two degrees. The preceding observations do not furnish us directly the mean temperature of the place, yet with the assistance of the hourly observations made at Philadelphia by Capt. Mordecai, we may be able to determine it pretty satisfactorily. The diurnal oscillation from 9 A. M. to 3 P. M. is nearly the same at both places; we may then without much fear of error, assume that the excess of the 9 A. M. observations above the mean temperature is also the same, viz. one degree. Subtract then one degree from 48°.6, and also 0°.2 for the zero error of the thermometer, we have 47°.4 for the mean temperature of Hudson.

The same problem I have attempted to solve by means of the temperature of wells. There are two deep wells in this vicinity whose temperature I have frequently tried. The following are the observations.

Date.	A.		B.	
	Depth to surf. feet.	Temperature, degrees.	Depth to surf. feet.	Temperature, degrees.
1838. March 27,	54	49.8	46	48.4
June 6,	53	50.1	46	48.9
August 4,	54	50.4	46	49.9
Sept. 20,	54	50.4	47	49.7
Nov. 3,	53	50.3	46	49.1
1839. Jan. 5,	54	49.4	47	47.5
Feb. 13,	54	49.8	47	46.4
Mean of 1st yrs. obs.	53.7	50.0	46.4	48.6

It will be perceived that the temperature varies very little with the season. Observations were therefore subsequently made only twice a year, about the time of maximum and minimum.

Date.	A.		B.	
	Depth to surf. feet.	Temperature, degrees.	Depth to surf. feet.	Temperature, degrees.
1839. Aug. 16,	54	51.0	47	49.8
1840. Feb. 19,	54	50.1	48	48.4
Mean of 2d yrs. obs.	54	50.5	47.5	49.1
1840. Sept. 7,	53.5	51.0	47.5	50.0
1841. March 27,	55	50.4	46.5	48.7
Mean of 3d yrs. obs.	54.2	50.7	47.0	49.3
Mean of the 3 years.	54.0	50.4	47.0	49.0

These observations present several remarkable circumstances. One is, that the mean temperature of the well A should be for each year exactly 1°.4 higher than B; yet they are only twenty-four rods distant from each other, are similarly exposed, are nearly of the same depth, the surface of the earth at A being fourteen higher than at B. The entire range of A is 1°.6, of B 3°.6. The water of the two wells is very unlike. I hence infer that they are fed by different springs; and as the annual change in the temperature of A is less than that of B, I conclude that the springs of A are the deepest, and hence have a higher temperature. It would seem, then, that wells do not necessarily indicate the mean temperature of the earth at the same depth. Their temperature depends partly upon the depth of the springs by which they are supplied. Which well then represents most nearly the mean temperature of the earth at the same depth? In my opinion it is the well B; for its depth of water is almost quite invariable, and the autumn of 1838 was a period of nearly unprecedented drought. I think then the depth of its springs cannot be less than forty or fifty feet. The water of the well A, I infer, comes from about twice that depth. Assume that the mean temperature increases 1° F. for every forty feet below the surface, and we have 1°.2 correction for a depth of forty-seven feet. Subtract this from 49°.0 and we have 47°.8, the mean temperature of the earth's surface by the well B. The mean between this and the former result is 47°.6, which is probably a near approximation to the mean temperature of the place. It will be observed that both of the wells indicate an increase of temperature for each of the last two years, and by precisely the same quantity. The same is also seen in the daily observations on page 315. The mean temperature of the three years at 9 A. M. was 47°.9; 48°.8; 49°.0.

The following table exhibits all the days in which the thermometer has fallen to zero.

1838, Feb.	20,	10 P. M.	−1°.0	1840, Jan.	17,	7½ A. M.	−6°.2
	21,	0¼ A. M.	−6.0		18,	2 A. M.	0.0
	22,	7 A. M.	−1.8		24,	7½ A. M.	−1.9
	25,	6¾ A. M.	−8.1		25,	7 A. M.	0.0
	26,	6½ A. M.	−0.2	1841, Jan.	3,	2 A. M.	−9.1
1839, Jan.	23,	8 P. M.	−2.5		18,	7½ A. M.	−10.1
Mar.	4,	6 A. M.	−5.9		19,	6 A. M.	−3.7
1840, Jan.	16,	7 A. M.	−9.1				

The following table exhibits all the days in which the thermometer has risen to 90°, all being in 1838.

July 18, 91°.2	July 28, 91°.5
19, 91 .2	29, 91 .7
26, 90 .7	Aug. 23, 90 .8
27, 92 .0	

The greatest heat of 1839, was July 30, 88°.2; and of 1840, July 16, 86°.7. The entire range of the thermometer has been 102°.1.

The following table exhibits all the cases in which the thermometer has been 25° above the dew point, all occurring at 3 P. M. I have also given the interval between each of these dates and the next subsequent rain.

Date.	Thermom.	Hygrom.	Diff'rence.	Rain followed.	Date.	Thermom.	Hygrom.	Diff'rence.	Rain followed.
1838. March 31,	61°.8	31°.0	30°.8	in 8 hs.	1839. April 15,	62°0	26°.0	36°.0	in 19 hs.
April 4,	48.7	18.0	30.7	in 44 "	19,	68.4	39.5	28.9	in 3½ ds
5,	61.3	34.0	27.3	in 20 "	22,	76.7	49.4	27.3	in 15 hs
23,	57.6	29.0	28.6	in 9 "	26,	75.2	45.2	30.0	in 36 "
30,	49.5	22.7	26 8	in 24 "	May 6,	65.9	40.3	25.6	in 15 "
Sept. 17,	77.4	52.4	25.0	in 5 ds.	1840. March 12,	45.3	20 0	25.3	in 9 "
Oct. 5,	72.4	47.4	25.0	in 24 hs.	22,	42.0	12.2	29.8	in 28 "
1839. March 6,	49.3	21.1	28.2	in 40 "	May 6,	53.5	18.0	35.5	in 45 "
11,	38.2	10.8	27.4	in 48 "	12,	61.4	30.8	30.6	in 47 "
April 3,	73.7	39.8	33.9	in 8 ds.	13,	67.5	41.9	25.6	in 23 "
5,	81.5	55.7	25 8	in 6 "	19,	84.9	58 0	26.9	in 18 "

The greatest difference is 36°.0. It will be observed that all of the above cases, with two exceptions, occurred in the spring of the year, and half of them in the month of April. This affords the explanation of the remark before made, that April was the driest month of the year. So far as these observations afford ground of predicting for the future, they lead to the following conclusions.

1. When the thermometer rises 25° above the dew point, rain will *certainly* follow within eight days.
2. The probability that rain will follow within forty-eight hours is $\frac{9}{11}$.
3. The probability that it will rain in thirty-six hours is $\frac{13}{22}$.
4. It is an even chance that it will rain in twenty-four hours.
5. It *certainly* will not rain in less than eight hours.

Do these facts indicate any necessary connection between extreme dryness of the air and subsequent rain? From the average of all the observations I have made here, it rains once in sixty-six hours. Assume that each rain last six hours; it is then upon an average sixty hours between two successive rains. But if the thermometer be 25° above the dew point, there must have been a considerable interval since the preceding rain; call it twelve hours. Rain then falls upon an average in forty-eight hours more; that is, there is an even chance that it will rain in twenty-four hours. If then it does not appear that extreme dryness of the air constitutes any ground to expect rain, it is at least worthy of notice that it furnishes no ground for anticipating that it will *not* rain.

The following are the only instances in which the dew point has risen to 80°, all in 1838, and observed at 3 P. M.

July 10,	82°.1	July 28,	80°.2
20,	80 .0	29,	80 .7
26,	80 .0	Aug. 6,	81 .8
27,	80 .5		

In four of these cases a heavy thunder shower followed the same afternoon. The following are the only cases in which the dew point has sunk to zero. It should be borne in mind that these observations are made only at 9 A. M. and 3 P. M. If they had been made at sunrise the list would have been larger.

1839.		1840.		1841.	
Jan. 23, 9 A. M.	—4°.0	Jan. 16, 9 A. M.	—4°.5	Jan. 3, 9 A. M.	—3°.0
3 P. M.	—2 .0	3 P. M.	—0 .4	18, 9 A. M.	—5 .0
Feb. 5, 3 P. M.	—4 .0	17, 9 A. M.	—5 .0	19, 9 A. M.	—2 .0
Nov. 25, 9 A. M.	—5 .0	25, 9 A. M.	—0 .7		

WINDS.

The chief instruction to be derived from mean observations of the winds is a decision of the question, In what direction does the wind *progress*? This does not mean, towards what quarter does the wind blow most *frequently* at a particular hour, or at all hours, though some seem to have so regarded it. The wind's force is an element in the inquiry no less important than its direction. It is obvious that we cannot give a very exact and satisfactory answer to this question without the aid of self-registering anemometers. Instruments of this kind have long been known, but for some reason appear not to have attracted much attention until recently. It is hoped that those which are now in operation will soon set the question at rest. Meanwhile my own observations, made with a vane attached to a revolving shaft, and in which the force of the wind is estimated by an arbitrary scale, explained in this Journal, vol. xxxvi, p. 173, may possess some interest. The mean of the extreme excursions of the vane during an interval of five minutes, I call its true direction; and the force I regard as the distance. This is resolved in the direction of the four cardinal points by the usual method. Each observation is resolved separately, and the sum of the northings and southings, eastings and westings, for a month, furnishes its mean direction. The following table exhibits the sum of these quantities for the corresponding months of the three years.

	9 A. M.					3 P. M.				
	N.	S.	E.	W.	Course.	N.	S.	E.	W.	Course.
March,	54.43	31.75	18.74	85.45	N. 71°13′ W.	68.81	24.55	18.57	102.47	N. 62°11′ W.
April,	61 51	46.69	39.47	84.78	" 71 53 "	91.86	44.98	25.45	96.48	" 56 35 "
May,	38.89	37.78	36 14	76.24	" 88 25 "	70.27	38.02	32.98	80.87	" 56 3 "
June,	51.55	49.12	23.58	90.79	" 87 56 "	81.71	45.01	17.93	100.61	" 66 4 "
July,	40.56	37.67	22.89	99.19	" 87 50 "	79.55	42.35	16.68	111.86	" 68 39 "
August,	43.83	37.20	37.57	79.87	" 81 6 "	72.17	32 90	24.56	87.10	" 57 52 "
Sept.,	36.15	48.70	20.81	83.46	S. 78 40 "	62.10	36.39	25.40	105.57	" 72 13 "
Oct.,	35.65	55.74	35.09	97.96	" 72 17 "	57.13	52.32	25.18	116.45	" 86 59 "
Nov.,	25.94	48.93	32.38	102.64	" 71 53 "	38.00	43.26	27.24	103.78	S. 86 4 "
Dec.,	44.58	55.31	32.75	123.34	" 83 15 "	58.07	50.83	25.33	131.53	N. 86 6 "
Jan'ry,	44.63	59.42	38.97	95.89	" 75 26 "	42.57	57.00	29.28	108.88	S. 79 43 "
Feb'ry,	42.23	53.88	24.33	99.22	" 81 9 "	55.31	58.27	11.16	123.39	" 88 29 "
Total,	519.95	562.19	362.72	1118.83	S. 86 48 W.	777.55	525.88	279.81	1269.04	N. 75 44 W.

	N.	S.	E.	W.	Course.
Sum of 9 A. M. and 3 P. M.	1297.50	1088.07	642.53	2387.87	N. 83° 9′ W.

It appears then that during a period of three years, the progress of the wind in spring and summer has been at 9 A. M. from north to south, in autumn and winter from south to north, the latter motion being slightly in excess so as to produce a resulting motion from the south. At 3 P. M. the wind's progress has been from the north during nine months of the year, and this excess of northerly winds has been such, that taking the sum of observations at both hours, the absolute progress has been from a point seven degrees north of west. Dividing this absolute progress from north to south by the whole number of observations, we obtain .0991 for the mean motion in this direction corresponding to each observation. The mean force of the wind has been 2.04, making the actual progress of the wind from north to south about five per cent. of the total motion; and if we assume with Mr. Redfield (Am. Jour. vol. xxv, p. 125) the average velocity of the wind at eighteen miles an hour, we arrive at the following important conclusion, that at this place *the absolute progress of the wind, in the direction of the meridian, is from north to south at the rate of seven eighths of a mile per hour.*

I have said that this question of the wind's progress does not mean towards what point does the wind blow most *frequently.* If we arrange the observations under the heads north, south, east and west, so that when an observation falls, for example, between north and west, it shall be counted in *each* of those columns, we obtain the following result of the number of observations for the three years.

9 A. M.				3 P. M.				Sum of both hours.			
N.	S.	E.	W.	N.	S.	E.	W.	N.	S.	E.	W.
402	634	315	726	526	515	278	766	928	1149	593	1492

It appears then that at 9 A. M. the wind has blown most frequently from a southern quarter, and at 3 P. M. from a northern, but that on the whole the wind has blown at 221 observations more from the south than the north; and if we take these numbers to represent the relative progress of the wind in the direction of the four cardinal points, we shall have the wind's mean direction S. 76° 11′ W.

The preceding results will enable us to appreciate various statements and conclusions which have recently been published. Thus in the New York Regents' Report for 1840, Mr. Coffin has deduced the mean direction of the wind for the state of New York from twelve years' observations at the different academies, S. 76° 54′ W., a result almost identical with my own given above; and instead of being surprised at this, we ought rather to have been surprised if the case had been otherwise: for Hudson is situated near the highest point of an immense territory, which is nearly a plane surface. For more than a thousand miles to the west of us, there is not an eminence materially higher than this station. No spot then could perhaps be selected more entirely free from local influence than this, while the academies of New York being scattered over a surface exceedingly diversified, are subjected many of them to strong local influences, which, however, are mostly neutralized in taking the mean of observations at fifty different stations. The New York academies are nearly in the same latitude with Hudson, and the winds are observed twice a day, A. M. and P. M. at hours not stated, but probably not differing much from my own. The above direction, however, (S. 76° 54′ W.) denotes merely the point from which the wind *most frequently* blows, and the Hudson observations afford reason to believe that throughout the entire State of New York the absolute progress of the wind is from north to south. The same remarks are applicable to Mr. Redfield's observations at New York city, his results being based solely upon the *frequency* of the different winds, without regard to their intensity.

The following table exhibits the mean force and variableness of the wind for the different months, the variableness being measured by the arc of vibration of the vane at the several observations.

MONTHS.	FORCE.		VARIABLENESS.	
	9 A. M.	3 P. M.	9 A. M.	3 P. M.
March, . . .	1.79	2.14	37°.8	48°.8
April, . . .	2.00	2.22	46.4	57.9
May, . . .	2.25	2.57	51.5	53.2
June, . . .	1.91	2.09	38.5	43.9
July, . . .	1.74	2.17	34.1	37.0
August, . . .	1.74	1.90	42.2	40.0
September, . .	1.69	2.00	42.1	47.5
October, . .	1.93	2.19	38.9	43.1
November, . .	1.87	1.92	33.7	36.7
December, . .	2.17	2.25	35.4	38.7
January, . .	2.02	2.02	35.9	38.9
February, . .	2.04	2.31	39.9	48.4
Year, . . .	1.93	2.15	39.7	44.5

The mean force of the wind is greatest in winter and spring, and greater at 3 P. M. than 9 A. M. As this last is a most important conclusion, it is desirable to know whether it may be safely generalized. In the London Athenæum for Oct. 3, 1840, is given a summary of Mr. Osler's observations at Birmingham, made with his anemometer during the years 1837, 1838, and 1839. The following is his table, exhibiting the relative force of the wind for each hour of the day, distinguishing the seasons.

	1 A.M.	2	3	4	5	6	7	8	9	10	11	12
Winter,	55	54	49	47	47	48	48	51	50	67	73	82
Spring,	26	28	28	27	29	29	32	41	56	70	80	82
Summer,	19	19	19	19	22	20	18	21	25	40	47	55
Autumn,	19	19	19	19	22	20	18	21	26	40	47	55
Total,	119	120	115	112	120	117	116	134	157	217	247	274
	1 P.M.	2	3	4	5	6	7	8	9	10	11	12
Winter,	89	89	85	79	75	65	63	63	63	59	61	57
Spring,	90	89	89	80	81	72	52	45	46	38	33	29
Summer,	58	54	53	44	34	28	27	24	22	19	21	20
Autumn,	58	54	53	44	34	28	27	24	22	20	21	20
Total,	295	286	280	247	224	193	169	156	153	136	136	126

The force of the wind is least at 4 A. M. and greatest at 1 P. M.; the latter being 2.63 times the former. The curve representing these observations is found to be almost identical with that of the thermometer, not only for the whole year but for each season. This result develops a most important principle, and demonstrates the error of those who ascribe to heat an unimportant influence in the production and modification of winds. It is here demonstrated by direct observation, that at certain localities, and there is good reason to believe that in this respect they are not peculiar, the wind is subject to a controlling influence, *which has a period of twenty four hours;* an influence not feeble or of doubtful existence; but powerful, at one hour retarding it by nearly one half its mean velocity, and at another accelerating it by the same quantity; changing not merely its intensity but its *direction*, for from the table on page 320, it will be seen that the wind is more northerly at 3 P. M. than at 9 A. M. for every month of the year. Taking these facts in connection with the well known effects of heat, we are warranted in the conclusion that the heating power of the sun's rays is to be regarded as the physical cause of this diurnal change in the direction and intensity of the wind.

CLOUDS.

The following table exhibits the progress of the clouds, each observation being resolved in the direction of the cardinal points in the same manner as for the winds. The table contains the sum of the observations for three years.

MONTHS.	9 A. M.					3 P. M.				
	N.	S.	E.	W.	Course.	N.	S.	E.	W.	Course.
March,	15.66	12.42	1.79	29.30	N. 83° 17′ W.	12.10	8.78	3.41	35.35	N. 84° 4′ W.
April,	11.46	15.64	2.50	32.14	S. 81 58 "	14.58	12.02	.71	36.15	" 85 52 "
May,	10.16	15.62	6.71	31.04	" 77 21 "	10.42	18.04	6.01	36.49	S. 75 28 "
June,	23.46	21.87	2.72	46.44	N. 87 55 "	20.98	19.26	4.31	54.84	N. 88 3 "
July,	22.82	17.34	3.42	55.65	" 84 1 "	22.73	26.36	3.50	57.47	S. 86 9 "
August,	30.39	18.77	8.16	50.30	" 74 35 "	29.20	18.34	8.46	55.22	N. 76 55 "
September,	20.44	14.84	6.68	38.51	" 80 1 "	17.60	17.70	6.12	41.90	S. 89 50 "
October,	22.08	24.86	3 83	50.83	S. 86 37 "	20.85	17.81	2.57	52.35	N. 86 30 "
November,	18.50	24.06	8.99	45.82	" 81 25 "	19.23	27.01	4.83	50.65	S. 80 22 "
December,	17.44	21.34	11.16	46.24	" 83 39 "	18.00	22.80	7.02	50.04	" 83 38 "
January,	18.00	26.86	6.62	58.66	" 80 20 "	16.68	29.78	6.37	59.49	" 76 9 "
February,	14.23	22.16	4.12	54.55	" 81 4 "	14.61	19.12	1.92	51.60	" 84 49 "
Total,	224.64	235.78	66.70	539.48	S. 88 39 W.	216.98	237.02	55.23	581.55	S. 87 49 W.

It appears then that the mean direction of the clouds for each month is very nearly from the west, fifteen degrees being the greatest deviation. The clouds are all of necessity supposed to move with the same velocity. If we knew their absolute or relative velocity for each observation, it is uncertain what resultant would be obtained; but as the diurnal change in the *direction* of the clouds appears almost or quite insensible, it may be presumed that the diurnal change in their *velocity* must be small; so that it appears probable that the mean of the above observations, S. 88° 13′ W. represents very nearly the absolute *progress* of the atmosphere in the region of the clouds.

There is a very simple and obvious corollary arising from the constancy of the mean pressure of the atmosphere, which I do not recollect ever to have seen noticed, but which deserves to be kept constantly in view, viz., that over any parallel of latitude the mean northerly and southerly motions of the entire atmosphere must be equal to each other. One half of the atmosphere is below the height of 3.44 miles. According to observations given in Dalton's Meteorological Essays, about two fifths of all the clouds observed in England in five years were above 1050 yards high. The mean height of the clouds is probably greater in the United States, yet there would seem no room for doubt that the clouds here generally float in the lower half of the atmosphere. The observations then on the preceding page exhibit mainly the progress of the lower half of the atmosphere. It would be desirable to separate the two classes of observations, yet it is difficult to accomplish this satisfactorily. Mr. Espy has proposed to determine the height of the bases of cumulus clouds by the rule, that this height in feet is equal to the complement of the dew point multiplied by 300. In order that the base of the cloud may reach to the height of 3.44 miles, the complement of the dew point must be 60°, which is much higher than has ever been observed in Hudson. If we assume with Prof. Forbes the diminution of temperature to be 1° F. for 352 feet elevation, a complement of the dew point equal to 50° will be required, while the highest observation here has been only 36°. Perhaps the *bases* of cumulus clouds never attain so great a height; yet clouds are sometimes seen at a much greater elevation. Thus Gay Lussac, at the height of 23,000 feet, saw clouds above him, apparently at a great elevation; and clouds are sometimes seen above the summit of Chimborazo. All accounts, so far as I have seen, agree in representing the winds on the summits of the loftiest mountains, in all latitudes, as blowing nearly from the west, and in the northern hemisphere generally from a point a little south of west. This seems to indicate that the mean progress of the upper half of the atmosphere is from south to north, which being admitted, it follows of necessity that the mean progress of the lower half is from north to south, and by the same quantity. If it should be found that in particular longitudes the mean progress of the entire atmosphere is from south to north, there must be other longitudes where there is a compensation.

The following table exhibits the proportion of the different varieties of clouds, the number under each month being the sum of the observations for the three years.

MONTHS.	9 A. M.						3 P. M.					
	Cirrus.	Cumulus.	Stratus.	Cirro-cumulus.	Cirro-stratus.	Cumulo-stratus.	Cirrus.	Cumulus.	Stratus.	Cirro-cumulus.	Cirro-stratus	Cumulo-stratus.
March,	10	3	26	5	7	11	8	11	21	8	5	11
April,	6	12	32	2	8	6	5	14	25	6	6	10
May,	5	7	17	3	8	19	4	14	11	5	6	23
June,	11	15	21	7	9	10	10	32	15	7	6	10
July,	13	28	16	13	3	4	6	47	11	10	4	8
August,	12	31	15	11	5	8	7	51	11	8	3	7
September,	7	16	14	5	8	14	3	21	7	7	5	25
October,	4	13	28	6	11	18	7	18	29	2	7	12
November,	4	7	36	3	7	20	5	10	31	7	10	18
December,		3	66	1	5	9	2	3	56	1	9	12
January,	3	2	57	2	10	13	3	3	51	5	10	16
February,	9	2	37	5	12	13	7	2	33	7	12	12

In my classification of the clouds, I follow the nomenclature of Howard, rejecting one of his divisions, the nimbus. I do not perceive from Howard's description, that the nimbus differs from some of his other varieties, except in the fall of rain, a circumstance, as appears to me, not sufficiently characteristic to form the basis of a separate variety. Clouds sometimes undergo a decided change as rain begins to fall, but the contrary is also of frequent occurrence. During a large part of the winter season, the sky at this place is overcast with a sheet of cloud so uniform and unbroken, that it is with the utmost difficulty the least motion in it can be detected. From such a bed, flakes of snow frequently fall day after day in such slow succession as not sensibly to increase upon the ground; nor do the clouds sensibly change their appearance whether snow is, or is not, falling at the time. This variety I call stratus, under which term I include all clouds which cover the heavens with a nearly uniform and unbroken sheet. It appears, then, that the stratus is the most common cloud at all seasons of the year except summer, when the cumulus prevails. This is more strikingly exhibited in the following table, which is arranged by seasons.

	9 A. M.						3 P. M.						Sum of both hours.					
	Cirrus.	Cumulus.	Stratus.	Cirro-cum.	Cirro-stratus.	Cum. stratus.	Cirrus.	Cumulus.	Stratus.	Cirro-cum.	Cirro-stratus.	Cum. stratus.	Cirrus.	Cumulus.	Stratus.	Cirro-cum.	Cirro-stratus.	Cum. stratus.
Spring,	21	22	75	10	23	36	17	39	57	19	17	44	38	61	132	29	40	80
Summer,	36	74	52	31	17	22	23	130	37	25	13	25	59	204	89	56	30	47
Autumn,	15	36	78	14	26	52	15	49	67	16	22	55	30	85	145	30	48	107
Winter,	12	7	160	8	27	35	12	8	140	13	31	40	24	15	300	21	58	75
Total,	84	139	365	63	93	145	67	226	301	73	83	164	151	365	666	136	176	309

More than half the clouds of winter are stratus, and nearly half those of summer are cumulus. The former, then, is with propriety called the winter cloud, and the latter the summer cloud. The cumulus is also more common at 3 P. M. than at 9 A. M., a natural effect of the sun's heat.

The following table exhibits the average cloudiness of the different months according to three years' observations, 0 representing a sky perfectly clear, 10 entirely overcast. For comparison I have added the observations made at Dartmouth College by Professor Young.

MONTHS.	HUDSON.		HANOVER.		
	9 A. M.	3 P. M.	Sunrise.	1½ P. M.	9½ P. M.
March,	6.24	5.64	4.17	3.70	3.47
April,	4.63	5.20	3.27	3.97	3.80
May,	6.23	6.88	4.35	4.39	3.39
June,	5.68	5.83	4.63	3.43	3.00
July,	5.00	5 36	5.65	3.12	2.64
August,	4.99	5.51	6.00	3.68	3.11
September,	4.75	5.14	6.87	2.73	1.97
October,	6.01	6.32	5.26	5.21	4.06
November,	7.13	7.15	4.85	5.08	4.60
December,	8.62	8.19	5.42	5.05	4.42
January,	8.13	8.38	4.30	4.17	4.00
February,	7.46	7.04	4.23	3.68	4.01

	HUDSON.		HANOVER.		
	9 A. M.	3 P. M.	Sunrise.	1½ P. M.	9½ P. M.
Spring,	5.70	5.91	3.93	4.02	3.55
Summer,	5.22	5.57	5.43	3.41	2.92
Autumn,	5.96	6.20	5.66	4.34	3.54
Winter,	8.07	7.87	4.65	4.30	4.14
Year,	6.24	6.39	4.92	4.02	3.54

The Hanover observations embrace but nineteen months, being all which are in my possession. In this register the column headed 'cloudiness' is sometimes entered 'fog.' In taking the average I have called such an entry 10, which probably is sometimes too great. This is the reason why the cloudiness at sunrise in the warm months appears so high. Such entries, however, are not made for either of the other hours. The two sets of observations being made at different hours, are not very well suited for comparison. Those, however, at 1½ and 3 P. M. may be regarded as made about simultaneously. The cloudiness then at Hudson is somewhat more than half greater than at Hanover. Nearly the same ratio exists throughout the entire year, but is somewhat greater in winter, when it is nearly as two to one. This is a very striking result, and exhibits an important feature of this climate. The Lake country in this respect resembles the western coast of Europe, and probably for a like reason.

The following table exhibits the average number of days for each month in which the sky was perfectly clear at the hours named. When the clouds cover less than one twentieth of the visible heavens, the cloudiness is called 0.

MONTHS.	HUDSON.		HANOVER.		
	9 A. M.	3 P. M.	Sunrise.	1½ P. M.	9½ P. M.
March,	5.5	6.0	7.5	8.0	11.0
April,	9.0	9.0	12.0	8.0	11.0
May,	6.5	3.5	8.0	5.0	12.0
June,	5.7	3.0	7.0	3.0	14.0
July,	4.7	1.7	5.0	8.0	13.0
August,	3.3	0.7	6.5	4.0	12.5
September,	8.7	7.3	3.0	9.0	17.0
October,	4.3	5.7	6.5	4.5	12.0
November,	4.0	3.0	7.0	3.0	6.0
December,	3.0	3.3	6.5	8.0	8.0
January,	2.0	1.7	9.0	8.5	9.5
February,	2.3	4.0	7.5	6.5	7.5

	HUDSON.		HANOVER.		
	9 A. M.	3 P. M.	Sunrise.	1½ P. M.	9½ P. M.
Spring,	21.0	18.5	27.5	21.0	34.0
Summer,	13.7	5.4	18.5	15.0	39.5
Autumn,	17.0	16.0	16.5	16.5	35.0
Winter,	7.3	9.0	23.0	23.0	25.0
Year,	59.0	48.9	85.5	75.5	133.5

The number of clear days for the entire year at the two places is about inversely as the degree of cloudiness; yet comparing the observations at 1½ and 3 P. M. we find the number of clear days for spring and autumn nearly the same at the two places; while for summer and winter the number at Hanover is from two to three times that at Hudson. It must, I think, be admitted, that observations of this kind possess considerable interest; and considering the ease with which they are taken, it is a matter of surprise that they should be almost universally neglected. To the astronomer they are perhaps the most important of all meteorological observations, as they indicate the most suitable site for a public observatory. At Hudson, for example, during four months of the year, astronomical observations are almost entirely impracticable.

RAIN.

The following table exhibits the amount of rain for each month since the gauge was erected. The numbers for 9 A. M. show the amount fallen since the preceding 3 P. M.

Months.	1838.			1839.			1840.			Average.		
	9 A. M.	3 P. M.	Total.	9 A. M.	3 P. M.	Total.	9 A. M.	3 P. M.	Total.	9 A. M.	3 P. M.	Total.
March,				1.412	1.361	2.773	5.075	.937	6.012	3.243	1.149	4.392
April,				1.162	.226	1.388	1.641	1.506	3.147	1.401	.866	2.267
May,				3.229	.695	3.924	1.845	.905	2.750	2.537	.800	3.337
June,				3.205	2.003	5.208	3.419	1.041	4.460	3.312	1.522	4.834
July,				1.925	.444	2.369	1.899	1.573	3.472	1.912	1.008	2.920
August,				1.717	.080	1.797	2.094	1.987	4.081	1.905	1.033	2.938
Sept.,		.105	.105	4.727	1.469	6.196	2.049	.697	2.746	2.259	.757	3.016
October,	1.721	.703	2.424	.653	.157	.810	2.209	.955	3.164	1.528	.605	2.133
Nov.,	1.267	.666	1.933	3.263	.674	3.937	1.640	.672	2.312	2.057	.671	2.728
Dec.,	.353	.191	.544	1.514	.017	1.531	2.134	.231	2.365	1.334	.146	1.480
January,	1.011	.499	1.510	1.092	.434	1.526	2.109	.250	2.359	1.404	.394	1.798
Feb'ry,	1.226	.343	1.569	4.005	2.214	6.219	.369	.219	.588	1.867	.925	2.792
Year,	5.578	2.507	8.085	27.904	9.774	37.678	26.843	10.973	37.456	24.759	9.876	34.635

	9 A. M.	3 P. M.	Sum.
Spring, ...	7.181	2.815	9.996
Summer, . . .	7.129	3.563	10.692
Autumn, . . .	5.844	2.033	7.877
Winter, ...	4.605	1.465	6.070

The average annual amount of rain is by these observations 34.635 inches; yet this period includes an extraordinary drought in the autumn of 1838. It is presumed that the mean of a long series of years would be about 36 inches. This is the average for the State of New York according to the Regents' Reports. The average amount at Hanover for four years is 38.05 inches; so that with a much less degree of cloudiness, it has a greater amount of rain than Hudson. The amount of rain here is greatest in summer and least in winter, being in the inverse ratio of the degree of cloudiness; and what is still more remarkable, in December, which is the most cloudy as well as most humid month of the year, the fall of rain is the least. The stratus cloud then which prevails in winter throughout all the Lake country, must evidently have a peculiar origin. I ascribe it to the evaporation from the Lakes. During the winter season, and especially the month of December, these vast collections of water are warmer than the land; the water which evaporates from them is immediately condensed, forming a hazy cloud which spreads all over the neighboring territory. This explains an anomaly to which I have already alluded (p. 325), that it sometimes snows here uninterruptedly for a week, with the thermometer about 30°, and during the entire period there will not be sufficient snow to whiten the ground. It appears also that more rain falls in the day than in the night. The observation at 9 A. M. includes the rain of eighteen hours, and that at 3 P. M. of six. The former then should be three times the latter, which is not the case except in winter, which period I exclude from the comparison, because the precipitation being mostly snow, which cannot be collected in a gauge, I am accustomed at the conclusion of each snow storm to collect what appears to me to be the average depth of snow and melt it. This operation is most frequently performed in the morning, and hence the excess of rain at 9 A. M. in the winter. During the rest of the year, however, and especially in summer, there is a considerable excess in favor of 3 P.M. If the observations had been made at 6 A. M. and P. M. they would have better exhibited the ratio for the two periods. The inequality however is sufficiently palpable, as during summer, the amount of rain from 9 A. M. to 3 P. M. is one half of that from 3 P. M. to 9 A. M.

The following are all the instances in which an inch of rain has fallen in twenty four hours.

Date.	Amount.	Time.	Date.	Amount	Time.
1839. June 12,	1.094	12 hs.	1840. Feb. 20–23,	4.017	4ds.*
13,	1.001	1 "	March 24,	2.109	24 hs.
21,	1.255	10 "	29,	1.636	24 "
Sept. 15,	1.493	24 "	June 22,	1.168	9 "
Nov. 14–15,	2.615	36 "	July 23,	1.012	12 "
1840. Feb. 9,	1.058	12 "	Nov. 22,	1.263	24 "

Throughout the entire preceding discussion, I have carefully distinguished the observations of different hours. The propriety of this course has I trust been made sufficiently apparent. Almost every meteorological phenomenon has a diurnal inequality. Thus the heights of the barometer, thermometer and hygrometer, direction and force of the wind, character and amount of the clouds, as well as the amount of rain, change sensibly with the hour of the day. I trust that heat will no longer be considered as an unimportant agent in the modification of meteorological phenomena; and that hereafter, in publishing meteorological means, the distinction of hours will not be disregarded. The observer who publishes the mean of all his thermometrical observations without regard to hours, not only cannot be sure of obtaining the mean temperature of his locality, but deprives others of the data necessary for determining it.

Art. VII.—*Meteorological Summary of the Weather at Montreal, Province of Canada, in Lat.* 45° 30′ *N., Lon.* 73° 22′ *W.,* (*for five years, from* 1836 *to* 1840, *inclusive;*) from registers kept by J. S. M'Cord, Associate Mem. Lond. Met. Soc., Mem. Nat. Hist. Soc. Montreal, Cor. Mem. Lit. and Hist. Soc. Quebec, and Albany Institute, New York.

ANNUAL TEMPERATURE.

Years.	Mean Temp.	Maximum Temp.	Minimum Temp.	Range.
1836, . .	40.43	90.00	–19.00	109 00
1837, . .	41.22	90.00	–18.00	108.00
1838, . .	41.58	90.00	–13 00	103 00
1839, . .	44.07	89.00	–18.00	109.00
1840, . .	44.29	91.00	–14.50	105.50
Mean, . .	42.31	90.00	–16.50	106.90

* Of this, 2.838 fell in thirty six hours.

MEAN MONTHLY TEMPERATURE.

Years.	Jan.	Feb.	Mar.	April.	May.	June.	July.	Aug.	Sept.	Oct.	Nov.	Dec.	
1836	17.05	11.32	20.90	35.24	53.32	65.25	71.90	63.05	57.46	39 37	32 24	18 09	
1837	8 70	15.78	25.15	40.40	52.15	65.55	65.60	65.15	58.30	43.20	33 70	20.95	
1838	21.00	9 00	31 60	34.50	52.80	69.90	68.90	65.50	57.90	46.60	28 80	12 40	
1839	12.70	20 80	29.30	45.50	52.90	62 20	70.00	66.50	59.60	50.00	34.40	25.00	
1840	10.45	23.50	30.55	44.46	55.73	66.99	71.96	70.87	59.26	46 35	34.36	17 02	
Mean,	13 98	16.08	27.50	40 02	53.38	65.97	69 67	66.21	58.50	45.10	32.70	18.69	42.316

WEATHER.

Years.	Clear.	Cloudy.	Rain.	Snow.	Show'rs.	Fog.	Days observ'd.	Averaging per cent.
1836	151 50	134 79	34.00	25.75	10.00	1.00	357 04	Clear days, about 44
1837	135.25	132.75	34.25	13.00	9.50	2.25	327.00	Cloudy, - - - 35
1838	146 75	112.50	32.50	16.50	13.25	0.50	322.00	Rain, - - - 11
1839	167.00	118.50	43.00	13.75	10.00	6 75	359.00	Snow, - - - 5
1840	141.25	138.50	38.50	21.00	11.25	5.50	356.00	Showery, - - 3
Mean.	148.35	127 40	36.45	18 00	10.80	3.20		Fog, hail, &c., 2

PRESSURE. Corrected and reduced to 32° F. Years.	Mean annual pressure.	Annual range.	RAIN. Inches.	WINDS. Westerly. N.W. W. S.W	Easterly. N.E. E. S.E.	North.	South.	No. days observed.
1836	29.920	1.550	19 20	189.20	46.25	65.85	55.66	357.00
1837	29.823	1.758	16.90	200.00	32.00	56 50	38.50	327.00
1838	29.884	1.498	18.60	163 00	32.25	66.75	49 25	311.25
1839	29.909	2.128	15.90	179.00	83.00	63.50	33.50	359.00
1840	29.895	1.569	27.55	188.50	71.50	47.50	45.50	353.00
Mean,	29.886	1.700	19.63	183.94	53 00	60.02	44.48	
				Or about 54 per cent.	15 per ct.	18 per ct.	13 per ct.	

SNOW GAGE.

Winter of 1830, 31	73.90 inches.
31, 32	107 60 "
32, 33	60 60 "
33, 34	51.00 "
34, 35	84.95 "
35, 36	86 45 "
36, 37	65.50 "
37, 38	49 85 "
38, 39	47.70 "
39, 40	49.40 "
Mean,	67 695

These observations are all made with instruments by first (British) artists, duly compared with standards, and every precaution taken in their position to insure the most correct indications. The city of Montreal is about forty six feet above the tide waters of the St. Lawrence; the barometer about forty five feet above the mean level of the St. Lawrence. Total elevation of barometer above the tide waters of the St. Lawrence, ninety one feet.

Note.—As the above tables cover the years embraced in the observations of Prof. Loomis, and are referred to by him, (p. 313,) they have been inserted in this place for the purpose of more ready comparison. No doubt their comparative and intrinsic value will be thus enhanced.—B. S., Jr.

Art. VIII.—*Observations on the Secondary and Tertiary Formations of the southern Atlantic States;* by James T. Hodge, Member of the Association of American Geologists.* *With an Appendix, by* T. A. Conrad.

Messrs. Editors,—Having spent a few months of the last year in making a tour on horseback through parts of the southern Atlantic states, I have put some of my observations into the present form, and if you deem them worthy of a place in the American Journal of Science, they are at your service. My object was to obtain from my own notice a general idea of a large portion of our country not familiarly known, but so rapidly did I pass through it, that my notes are crude and imperfect. From the investigations of Mr. Conrad, to whose instructions I am much indebted, the accompanying lists of fossil shells, which I there collected, were made out, and a few new species are now added to our catalogue of the tertiary fossils.

Until I arrived in the southern part of Virginia I was unable to see much of the "marl beds" of the tertiary, owing to the snow and ice that covered the ground. I found, however, that the marl extended all along the eastern section of the state, and that it was extensively used as a manure by the planters. On the Rappahannock, seven miles below Fredericksburg, the bed is at least ten feet thick, and so conveniently exposed that vessels might come alongside and load with it, if it were important enough to transport. It here abounds in shells, and also contains teeth of sharks, and fossil bones. Nearly all belong to extinct species, and the genera besides are those most common to the lower tertiary. The fertilizing properties of the marl appear to depend almost entirely upon the carbonate of lime afforded by the slow and long continued decomposition of the shells; it contains more or less of the green sand, but in too small proportion to add much to its richness; and the quartzose sand and mica that constitute the remainder of it can be of little service. The effect of this marl upon the cotton and clover is superior to that of any other manure, but then it is put on at the rate of 700 bushels to the

* The substance of this paper was communicated to the meeting of the Association of American Geologists, as referred to in the report of their proceedings, published in the last. No.—*Eds.*

acre, so that the surface of the fields is covered. Weeds are said to be killed the first year, but to come up luxuriantly afterwards, and clover appears spontaneously where none would grow before. In Caroline and Hanover counties, on land that would yield only three barrels of corn (fifteen bushels) to the acre, from ten to fifteen barrels have been obtained after one generous application of the marl. On the Pamunkey river is a beautiful plantation, regenerated from the old and exhausted tobacco lands; an example of what the Virginia soil once was, and what it may be again, if treated with the skill and enterprise which characterize Mr. William Wickham, the proprietor of this place. He has used the marl for eighteen years, applying it once profusely, 800 bushels to the acre. The bed here varies from ten or twelve feet to not more than two or three. It is often cut through, as if by a stream, and the space afterwards filled up by the ferruginous sand which overlies the marl bed.

In those tracts of country not cut through by large streams, as Prince George county, the marl is obtained with great difficulty, pits being dug that constantly fill up with water, which cannot be kept out even by expensive pumping. Still it is in great demand, the planters digging it in the winter months, when their hands are least occupied, and then carting it over the fields, where it lies in heaps ready to be spread in the spring. The lands in this county are generally stiff and clayey, and would be benefitted by a free application of common sand, as well as of the marl. From these heaps I collected a great variety of fossil shells, enough to show that the deposit belongs to the same formation with that which I afterwards traced through North Carolina, but the specimens never have arrived from Murfreesboro', N. C., where I left them. This is the case also with many I collected at Murfreesboro', on the south side of the Meherrin river. Some brooks have cut deep into the sand hills on which the town stands, and have distinctly exposed the marl; while from the bed they have washed out a profusion of fossil shells, chiefly Pectens and Ostreæ. Not far above the marl is a stratum of stiff, red clay, alternating with layers of sand. A bed of this character I have noticed in a similar position throughout a great part of the southern states; at Richmond, Va., it is very conspicuous near the summits of the hills, as well as in the southern part of Sumpter district, S. C.; and in many places in Georgia, near the Savannah river, it is well exposed. It is accompanied by white clay, and sometimes by layers of gravel. From its position only on high hills, and want of fossils, I consider it to belong to the diluvium spread over the country after its elevation above the sea, most of which has been subsequently washed and worn away by long continued degradation.

Near the Roanoke, some miles above Williamston, I observed the marl by the road-side, and although it was hard frozen and nearly covered with snow, I recognized in it many individuals of our common recent shell, the Venus mercenaria. This is a very poor part of North Carolina; the land is quite flat and sandy, and during the winter season one frequently rides for miles through water several inches deep, by which the roads are flooded. The principal growth is the pitch pine, from which a great deal of tar and turpentine are made. The woods along the roads present a singular appearance, every tree being half stripped of its bark to the height of seven or eight feet, and the exposed surface bleached by the white turpentine oozing out. A little cavity is hollowed out at the base of the trunk, into which most of the turpentine runs, and the remainder is scraped off and put into barrels. The tree dies and becomes fat pine. This is burned in pits, as wood is for charcoal, and the tar runs out upon the carefully prepared clean floor. North Carolina tar is inferior to that made in Norway, owing probably to less pains being taken in the preparation. It is sent principally to the north, where it sells for about a dollar and a half per barrel, but the makers receive only ninety cents, out of which they pay twenty-five for the barrel.

In the very western part of Jones county, I first met with the limestone of the secondary formation. It is on the plantation of Mr. Humphreys, at the heads of New river and Trent river. The rock lies in a heavy ledge on the borders of a swamp; its surface is much worn and ragged. It is of a straw color, and apparently of good quality for making lime;—on submitting a piece to chemical examination, I find it as good as the limestone of this formation usually is. It yielded in 100 parts,

Water, - - - - - -	1.00
Silica, - - - - - -	5.60
Iron and al., - - - - -	5.60
Carb. lime, - - - - -	87.00
Carb. mag. - - - - -	trace
	99.20

Several springs come out at its edge, which bring up small fossil shells and pieces of coral. Among the shells, which are generally very imperfect, may be recognized the Pecten membranosus, a Cardium, and others common to the same formation in New Jersey. The water seems well adapted to the recent Planorbis trivolvis, Physa heterostropha, and Paludina integra, which inhabit it in profusion, and also to the luxuriant water-cresses, which equally abound in it. Around the limestone is a deposit of calcareous marl of a light yellow color, affording a very convenient and rich natural manure; but it has been entirely neglected as well as the limestone, the little lime required for the country being brought from Thomaston, Maine, although they have enough of the rock, wood at the expense of cutting it only, and a suitable sandstone for kilns scattered through this region. This stone belongs to the same formation, and has been used sometimes for making millstones. Having fortunately some plans of kilns with me, I was happy to leave them, with the advice that the people make the attempt at least to supply themselves with their own lime.

Not far from this place, is that interesting locality in Duplin county, called the "*natural well.*" It is two miles west of the rail-road, (forty-seven miles from Wilmington,) on the road from Kenansville to Elizabeth, Baden county. Before reaching it, one may notice by the side of the road a large sink-hole, fifteen feet deep, overgrown by trees and bushes; a little beyond this, a path turns off to the left to the cabin of a Mr. John Smith, within two hundred yards of which, in the woods, is the well. It is a large circular basin, about twenty yards across and sixteen feet deep to the surface of the water; its banks are nearly vertical, although the strata are entirely obscured by the loose sand, trees and bushes that have covered them, excepting in one narrow spot, where a correct section may be obtained and specimens collected. The soil, which is sand and yellow loam, a little clayey at bottom, is from three to four feet thick. It rests on the shell marl, which is about four feet thick, and under this is a tough blue clay from six to eight feet thick, overlying a sandstone like the clay in color, the lowest visible rock. The marl consists entirely of shells, and fragments of shells, with a very small quantity only of fine white siliceous sand. The shells are of a great variety of species belonging to this formation, and they lie promiscuously together in great confusion; single valves of the bivalves are more frequently found than the two together, and even the stronger univalves are most often seen in fragments. So abundant are they, that in cleaning out some of the larger shells a great number of small and more perfect specimens were found in their interior, and added to my collection. A Pectunculus quinque-rugatus, in particular, enclosed between its two valves a multitude of shells and fragments closely imbedded in a fine clear quartz sand. The contents when picked out occupied a space full twice that in which they were so closely packed. Although the diameter of the Pectunculus was only two inches and one fourth, there were in it a Cytherea reporta, beautifully preserved with its natural polish, one and one third of an inch long, and itself filled with other smaller shells and a purer sand than that which surrounded it—several small Ostreæ, Corbulæ, and duplicates of twenty or thirty other species. There are, as seen by the accompanying catalogue, about 80 species found at this locality. Of these 12 are recent, and 20 at least heretofore undescribed. Some yet undetermined remain in the hands of Mr. Conrad, and of these only the genera are given. The Oliva idonea has been previously described by Mr. Conrad; it is one of the most beautiful shells found here, being finely preserved, and most of the specimens not having lost their natural polish. The people living in the neighborhood know them by the name of "key shells," from their procuring them to attach to a bunch of keys. The cones are of the species *adversarius*, so named from their being nearly all reversed; one of mine is the only exception known. The bivalves are only occasionally met with entire.

It is remarked that the water in the well never varies in freshets nor droughts, and tales are told of ineffectual attempts having been made to sound it, and of a strong current setting through it, sucking down whatever is thrown in; that there is a current I doubt not, it being nothing unusual for a stream of water to sink suddenly under ledges of limestone, as at the Eutaw springs in South Carolina, and the limestone of the secondary formation I believe to be not far below the surface of the water, perhaps directly under the blue sandstone at the surface, which is very likely the upper rock of that formation, and the blue clay the lowest of the middle tertiary, as it often is, (the lowest tertiary is wanting in this part of the country.) The limestone of Jones county is not far off, and such sinks as these are frequent over limestone beds; in Georgia they are called "limestone sinks."

With my valise and saddle-bags well stored with specimens, and a keg full strapped behind the saddle, I proceeded to South Washington, on the way to Wilmington. In this neighborhood I discovered again the secondary formation on the N. E. banks of Cape Fear river. The rock is a blue sandstone, containing the characteristic Exogyra costata, Belemnites, Plagiostoma palagicum, Anomia ephippium, &c. The existence of this rock between the tertiary deposits on the east and the west, seems to indicate an anticlinal axis here, which extending north, accounts for the elevation of the secondary throughout Jones county, and the broad extent of country on each side occupied by the tertiary marls. The axis must be low, and the dips very gentle, causing at most a slight undulation of the strata.

To the east of South Washington are large tracts, called bays and swamps, one of which, called on the map Angola Bay, was described to me by one who had crossed it. After traversing its margin, which was a broad swamp covered with thick trees and bushes, my informant and his companion came out upon an open heath, spreading as far as they could see, and destitute of all vegetation, save a thick covering of moss and a few scattered bushes. Through this moss they travelled on with great difficulty, plunging in at every step nearly knee deep. Procuring a pole from the trees on the border, he sunk it down eight feet into the mud without finding bottom. The moss and mud of these swamps would thus seem to be those always due to *peat* swamps. At night they reached an island in the swamp with a few trees upon it. Here they remained till morning, and then continued their course across, and reached the other side about noon, greatly fatigued. He thinks they are called "bays" from the quantity of bay trees that grow around them. Holly Shelter swamp is similar to this. These swamps remind one of those in the southern parts of Georgia, the islands of which were once inhabited, according to an old Creek tradition, by a superior race of beings, whose beautiful women, called "daughters of the sun," occasionally condescended to help out the poor bewildered traveller, lost in their intricacies, but who, with their houses on the islands, were never to be found by man, though eagerly sought after.

At Wilmington is another interesting locality, where not only the tertiary marl corresponding to that at the natural well is found, but the secondary sandstones and conglomerates below it are well exposed on the banks of Cape Fear river. The lower tertiary is wanting, and this marl bed, which is from two to three feet thick, is seen resting upon the sandstones, containing shells belonging altogether to the upper secondary formation. Above the marl, and resting on it, the upper stratum observed is of sand and loam, containing a few layers of small white pebbles. The marl is nearly as prolific in shells as the same formation at the natural well. The species, however, are not the same, as will be seen from the appended catalogue, (p. 344.) More than fifty species were collected, of which eight are recent, and those in italics new. Comminuted shells and fine sand form the marl stratum, and these are either converted into a solid mass not easily broken, or remain unconsolidated. The sandstone beneath is in one place a compact siliceous mass, but a little way off it changes to a coarse pebbly rock, consisting of small, rounded quartz pebbles of a dark color; and then again it becomes so calcareous that with proper care it might be burned to tolerable lime. The pebbly rock or conglomerate contains shells, and corals, and small sharks' teeth, in abundance. Large teeth are rare, as are bones, which are said to have been found it. The shells themselves have generally disappeared and left only the casts; these, though very abundant and perfect, render it extremely difficult to determine the species. The genera are principally Cyprea, Cirrus, Gryphea, Ostrea, Anthophyllum, Nautilus, &c.

The proximity of this locality to the wharves and the town of Wilmington, would render it easy for strangers to obtain specimens thence. The marl is not here esteemed as a manure, probably because the soil is so sandy that the shells must decompose very slowly in it. By first applying large quantities of clay to it, and then the marl, no doubt very beneficial effects would result.

To the south of Wilmington I noticed the marl again on Little river, near its mouth, just over the boundary line in South Carolina. The shells closely resemble those at Wilmington, the most abundant being large Arcas; but they were all tightly cemented together by a ferruginous cement, forming a solid ledge on the edges of the water. This is the farthest point to the south where I observed this deposit; but I was told it occurs again on the Waccamaw river.

In treating of the tertiary formations of our country, I have preferred using the conventional names adopted by Mr. Conrad, of upper, medial, and lower tertiary, to those of pliocene, miocene, and eocene, applied to similar formations in Europe. The former merely indicate relative position, and are therefore sufficient at present; the latter imply a relative proportion, of course not very exact, of extinct and recent shells; and though our own formations may now each give about the same proportion with the English formations, still when two hours' work may discover more than twenty undescribed species in one locality, besides some recent species not before noticed in the formation; and when our knowledge of the living shells of our coast is so imperfect, it certainly proves that the adoption of these new names would now be hasty. It would be at once taking it for granted, (as it is certainly not proved,) that our strata will be readily divisible into the same three formations as those of Europe; and all difficulties, if any are hereafter encountered, will be too apt to be made to bend, or to be neglected, for the sake of keeping to this favorite but unproved system of classification. This subject, I hope, will be more fully discussed by Mr. Conrad, and sustained by more complete details than I am able to furnish.

At the locality last mentioned, near the boundary line, are found through the fields singular deposits of oyster shells, each extending over several feet square and about two feet deep. No account is preserved of the time or cause of these collections. They were made centuries ago, probably by the Indians. Corn does not grow well by these heaps, at which I was rather surprised.

There is a well known locality of the secondary limestone at the Eutaw Springs, near Nelson's ferry over the Santee, in the western part of Charleston district. The striking similarity of this rock to that in Jones county, N. C. first attracted my attention. Like it the limestone rose above the surface in heavy ragged ledges, here at least fifteen feet high; it was of the same light yellow color, and contained similar fossil shells. Similar springs too rise among the ledges of the rock, and they contain the same recent shells, and water-cresses in greater abundance. But the water, I noticed, was lukewarm, and one of the largest of the streams, after running only about fifty yards, suddenly disappeared under the limestone, and was no more seen. The rock does not contain a great variety of fossil shells; the most abundant are some large Ostreæ, of what species I know not, the specimens being lost. On this account, too, I cannot speak so decidedly as I wished to have done of the quality of the limestone, which ought certainly to be of some practical importance, being on a river navigable by steamboats, and in a region where lime bears a high price and wood is very cheap. A little enterprise and skill only are requisite to create an extensive business here in the manufacture of lime. But though its good effects as a manure force itself upon the notice of those who use the adjacent fields, still no attempts have been made to extend its use farther than nature has seen fit to spread the rock, and the calcareous deposits formed from it.

So on the Edisto, in Colleton district, this rock is equally available, and equally neglected; and though lime enough might be made on these two streams to supply the whole of the eastern parts of South Carolina and Georgia, at an expense not exceeding fifteen cents a bushel, yet the inhabitants prefer to import their lime from Thomaston, Maine, and pay at Charleston $2,00 per cask, or when brought up to the neighborhood of these vast quarries it sells for $3,00 per cask! In Chester county, Penn., while on the geological survey of that state, I have seen lime made and sold for *ten cents* a bushel, where the natural facilities are no greater than here. And when on the same business in Maine, and employed at Thomaston in obtaining the statistics of the lime business, I came to the conclusion that the southern states must be remarkably deficient in limestone; that notwithstanding the difficulties the Thomaston people had to contend with, in the high price of fuel, a bad harbor, that frozen up one third of the year, and their remoteness, still they managed to monopolize the lime business of the Atlantic coast, of the Gulf of Mexico, and up the Mississippi to Natchez. The average cost of a cask of lime at the wharves at Thomaston, was, as near as we could estimate it, about seventy cents, and this included twenty cents for the cask. But unfortunately the term "cask" represents no definite measure. By law it should hold "forty gallons," five bushels, but every mason of whom I have made inquiries, and who has measured them, says their capacity is continually changing; that most of them contain *less* than a common flour barrel, and that *three bushels and a half* is probably near the average measure of their contents. Their large size is made up by enormously thick staves, and heads about an inch thick, and frequently large empty spaces remain in the cask. Yet for this meagre amount of lime, the people of South Carolina are willing to pay a sum for which they themselves might make full *eight times* as much; and by thus rendering it cheap, the labor lost to their favorite crop would not be missed, when thereby a bale of cotton to the acre would not be considered a maximum product, nor *two* ears of corn to each of the widely separated hills a subject worthy of remark.

The prejudice of workmen—their not liking to use a different material from that they have been accustomed to—is one reason why the Thomaston lime has successfully competed with all other lime made on the Atlantic coast. The name of that is favorably known and deservedly so, and it will sell when another equal to it from another locality will not bring even a very inferior price. There was a remarkable instance of this a short time since in New York, some excellent lime from Rhode Island hardly finding a market at any price. Most of the Pennsylvania lime contains magnesia, and yet celebrated as is the Philadelphia mortar for whiteness and durability, and as are the fine farms of Chester and Lancaster counties, which are enriched almost entirely by lime, there is a universal prejudice against magnesian limestones. But this cannot last; and now that the Tide-water canal is opened, the Susquehannah river lime must soon rival that from Thomaston in our southern ports; that the home-made lime must here come into extensive use, though prejudice and a want of enterprise may long keep it unused and unknown. This rock belongs to the same formation, and precisely resembles much that which I have seen in the western part of New Jersey. Its composition is no doubt the same, and this is seen in Prof. Rogers's Geological Report of that state, to vary as to the proportion of carbonate of lime from seventy-five to eighty-eight per cent., the residue being chiefly silica, with a very small amount of carb. magnesia, iron and alumina. This too corresponds with the analysis given above of the limestone from Jones county, N. C. The silica is the least injurious ingredient, its principal effect being, unless the rock is burned at too intense a heat, merely to render less sand necessary in tempering it for mortar, while the magnesia is not only of small quantity but doubtful tendency, and the oxide of iron is generally in too minute proportion to seriously injure the lime by giving it a very dark color.

Besides the lime that will hereafter add to the mineral wealth of this region, there is near Pocataligo, on navigable tide water, a deposit of the purest quality of quartz sand, suitable for the manufacture of glass. The pebbles of which it consists are small, sharp, angular fragments of perfectly pure quartz, without any foreign mixture. It may be obtained in any quantity, and would probably pay for transportation as ballast under the cotton loads, either to the northern glass-houses or across the ocean. The locality is just half way between Charleston and Savannah, by the turnpike, on Mr. Spike's plantation.

There is one more locality of the secondary limestone I visited, which is near the Savannah river, in Georgia, at a place called Jacksonboro'. The perfect similarity of the rock with that in Jones county, North Carolina, and on the Santee and Edisto in South Carolina, admits of no doubt that it is the same with them, and continuous through this wide extent of country. Even to the straw color of the rock, the brooks flowing out at its base, the lukewarm water, and the little shells that inhabit it, they are all nearly alike. This has been partially quarried, and the limestone burned. On opening the bed it is found to be about twelve feet thick, and within from the surface, the stone is of a much whiter color, closely resembling chalk, and appearing as if it had already been burned. The heap of rock in a hot southern sun presents so dazzling an appearance that one's eyes and head seriously suffer from closely examining the pieces. They contain a great variety of fossil shells, affording most beautiful specimens; but I have not succeeded in obtaining those I collected. A rude kiln has been constructed of the coarse sandstone belonging to the same formation, and a considerable quantity of lime made. It is very white and good lime, such probably as the other localities described would afford, if worked as extensively. It is packed in boxes, which hold about three bushels, and these are sold along the river at the same price as a cask of Thomaston lime, *three dollars!* The locality is probably injudiciously selected, as it is eight miles from the river, up a little stream called Brier's creek, which is navigable for boats and rafts but a short time in the year, while the same bed might no doubt be found and opened on the river.

The inferior limestone at Shell Bluff belongs to a higher formation, and cannot be so important, though here too a great deal of cheap lime might be made from the rock and the fossil Ostrea. The specimens I collected at Shell Bluff, and on which I depended to describe the locality, having been lost, I will attempt no account of it, as it could now be only a repetition of what others have said. In one of the lowest fossiliferous bands at the bluff, I discovered a small jaw bone of some land animal, which has not before been noticed there.

Catalogue of Medial Tertiary Fossils found at the Natural Well, Duplin Co., N. C. Those in italics are new; of those undetermined, only the generic name is given.

Amphidesma constricta.
Amphidesma.
Anomia Ephippium.
Arca incile.
" transversa.
Astarte lyrata.
" lunulata.
Astarte.
Balanus ovularis.
Buccinum lunatum.
" *interruptum.*
" *multirugatum.*
" *obsoletum.*
Calyptrea costata.
Calyptrea.
Calyptrea.
Cancillaria lunata.
" perspectiva.
Cardita tridentata.
" *perplana.*
Carditamera arata.
Cassis Hodgii.
Cerithium Carolinensis.
" dislocatum.
" *unilineatum.*
Chama congregata.
Conus adversarius.
Corbula cuneata.
Crassatella undulata.
Crepidula fornicata.
Cypræa Carolinensis.
Cytherea Carolinensis.
" reporta.
Diplodonta Americana.
Dispotæa multilineata.
" *dumosa.*
Fasciolaria mutabilis.
" rhomboidea.
Fissurella.
Fulgur canaliculatus.
" excavatus.
" perversus.
Fusus.
Gnathodon minor.
Infundibulum centralis.
Lucina Jamaicensis.
" *radians.*
" *trisulcata.*
Lunulites denticulata.
" *depressa.*
Mactra crassidens.
" congesta.
" *subparilis.*
Marginella.

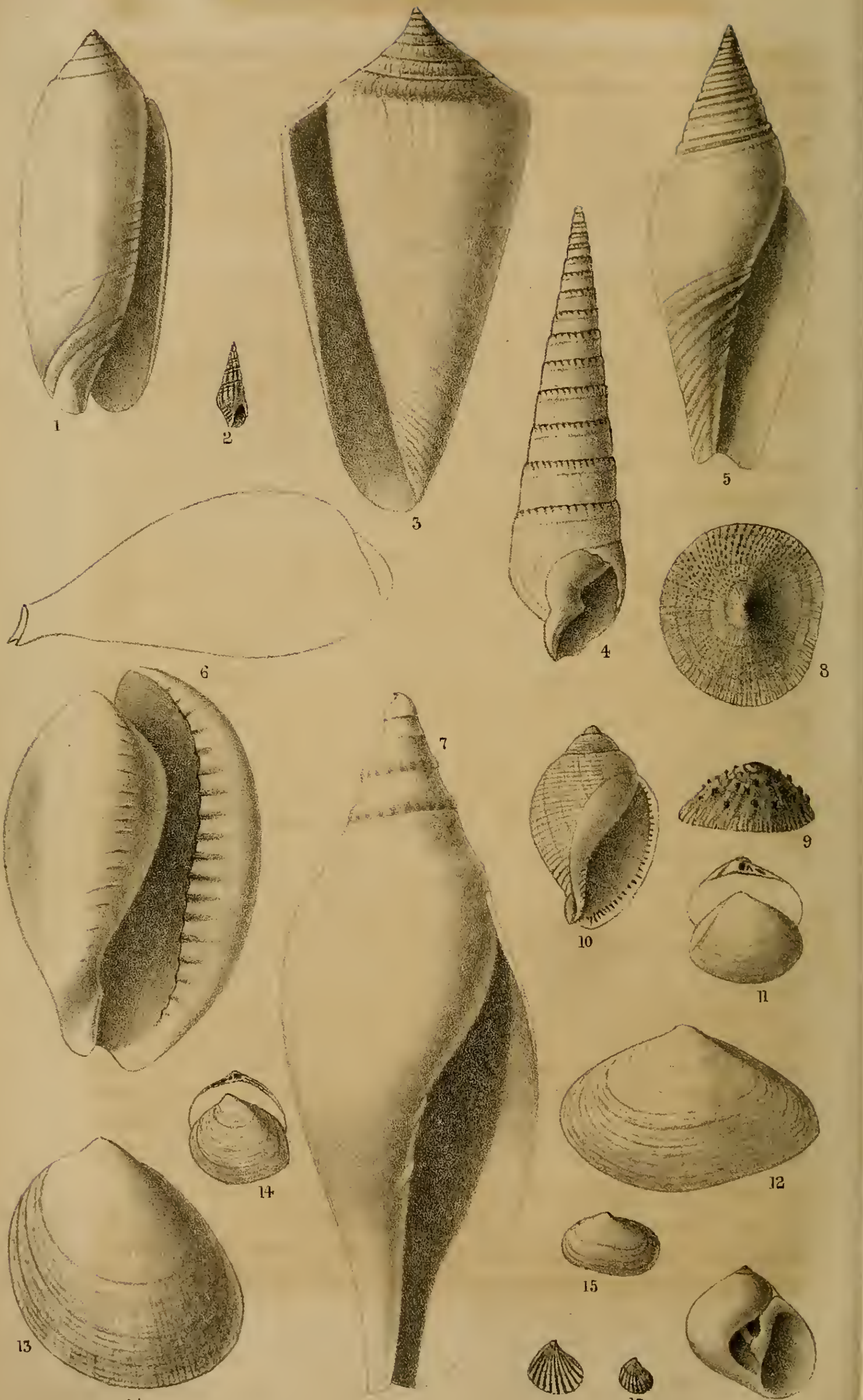
Mitra Carolinensis.
Monodonta.
Mytilus incrassatus.
Natica canrena.
" duplicata.
" heros.
" *Caroliniana.*
" *percallosa.*
Oliva idonea.
" litterata.
" mutica.
Ostrea disparilis.
" sculpturata.
Ostrea.
Panopœa reflexa.
Pecten eboreus.
Pectunculus Carolinensis.
" *quinquc-rugatus.*
" subovatus.
Pleurotoma Virginiana.
Plicatula marginata.
Serpula granifera.
Solen.
Tellina.
Turritella.
Venericardia granulata.
Venus cortinaria.
" Rileyi.

Catalogue of Medial Tertiary Fossils found at Wilmington, North Carolina.

Amphidesma æquale.
" subobliqua.
" *nuculoides.*
" *protexta.*
Arca lienosa.
" limula.
" transversa.
Astarte lunulata.
" lyrata.
Balanus.
Cardium sublineatum.
Cardita abbreviata.
Chama congregata.
Corbula cuneata.
Crassatella undulata.
Crepidula fornicata.
Cytherea reporta.
Cytherea.
Diplodonta Americana.
Fulgur canaliculatus.
" Carica.
" perversus.
Infundibulum.
Lucina anodonta.
" crenulata.
Lunulite.
Mactra congesta.
" lateralis.
Madrepora palmata.
Marginella.
Mytilus.
Nucula acuta.
" obliqua.
Oliva zonalis.
Orbicula lugubris.
Ostrea sculpturata.
" subfalcata.
Ostrea.
Panopœa reflexa.
Pecten eboreus.
Pecten.
Pectunculus Carolincnsis.
" aratus.
Scutella.
Solecurtus cariboeus.
Solen.
Tellina.
Venericardia granulata.
Venus alveata.
" cortinaria.
" mercinaria.
" Rileyi.

Appendix to Mr. Hodge's paper, describing the New Shells, &c.; by T. A. Conrad.

Mr. Hodge having requested me to name the fossils collected in his late excursion through a portion of the tertiary region, and to describe such as are new, I send you the following descriptions, and references



T. A. Conrad del. on stone.

Lith. of T. Sinclair, Phil^{a}.

to species formerly described; the localities visited by Mr. Hodge belong chiefly to the medial tertiary deposits; the other formations are the cretaceous and lower tertiary. One of the tertiary localities I noticed in Volume xxxix, p. 387, and described some of the shells which occur there, from specimens in the collection of my friend D. B. Smith. The following list of species is made out from his collection. The fossils are imbedded in quartzose sand, with a large admixture of comminuted shells:

Natica canrena, Conus adversarius, Mitra Carolinensis, Fulgur excavatus, F. contrarius, F. maximus, Cyprea Carolinensis, Crepidula fornicata, Turritella Mitchelli, Cerithium Carolinensis, Buccinum multirugatum, Fasciolaria rhomboidea, Lucina Jamaicensis, Arca transversa, Mactra crassidens, Pectunculus quinquerugatus.

References and descriptions of new species.

Oliva.

Oliva litterata, Lam. Plate II, fig. 1.

Buccinum.

Buccinum interruptum. Plate II, fig. 2. Elevated, subfusiform, with longitudinal ribs and transverse impressed lines, two below the suture; middle of the whirls entire, sides flattened, lower half of body whirl with equal prominent lines.

B. multirugatum. Ovato-conical, with numerous wrinkled spiral lines, coarser and more distant near the suture and at base of the body whirl; base bicarinated and subumbicated; columella with a thick fold at base. Length, 2 inches; width, 1⅛ inches: from the collection of D. B. Smith. Locality, Natural well, Duplin Co., N. C.

Conus.

Conus adversarius. Plate II, fig. 3. For description, see Vol. xxxix, p. 388. Mr. Hodge has one dextral shell, the only one I have seen among eight or ten specimens of the species.

Cerithium.

Cerithium unilineatum. Plate II, fig. 4. Slightly turrited; volutions with each a spiral impressed line above the middle; space between this line and suture with oblique plicæ.

C. Carolinensis. Subulate; whirls with impressed spiral lines and numerous acute longitudinal ribs which are dislocated by a sulcus below the suture. Length, 2¼ inches. Resembles *C. dislocatum*, but is far larger, and has much more numerous and less prominent ribs.

Mitra.

Mitra Carolinensis. Plate II, fig. 5. For description, see Vol. xxxix, p. 387, where it is inadvertently described as a *Voluta.*

CYPRÆA.

Cypræa Carolinensis. Plate II, fig. 6. Ovate, ventricose, superior margin of the labrum prominent at the apex; base plano-convex.

FASCIOLARIA.

Fasciolaria mutabilis. Plate II, fig. 7. For description, see Journ. Acad. Nat. Sci., Vol. VII, p. 135.

DISPOTŒA.

Dispotœa multilineata. Plate II, fig. 8. Subovate, depressed; apex prominent; one side with squamose lines, the opposite with finer ramose lines destitute of scales; diaphragm contracted.

D. dumosa. Plate II, fig. 9. Elevated, with ramose radiating lines and obsolete ribs, and with erect tubular spines; apex minutely spiral.

CASSIS.

Cassis Hodgii. Plate II, fig. 10. Elliptical, with numerous spiral lines most prominent towards the base; spire conical, volutions convex.

PECTUNCULUS.

Pectunculus quinquerugatus. Lentiform, with very fine crowded longitudinal striæ, and convex slightly raised ribs; behind the umbo are five or six recurved plicæ or undulations. Length, 3⅝ inches.

P. Carolinensis. Lentiform, thick, posteriorly subcuneiform; ribs obsolete, radiating striæ strongly marked, minutely granulated; cardinal plate broad and thick; teeth obsolete; inner margin with numerous crenulations. Length, 1⅝ inches.

P. aratus. Obovate, with about twenty eight ribs, about as wide as the intervening spaces crossed by wrinkled lines; posterior margin obliquely truncated above the extremity, which is angulated; margin with about fifteen wide crenulations. Closely allied to *P. pectinatus.*

MACTRA.

Mactra crassidens. Plate II, fig. 11. Triangular, thick, convex-depressed; umboidal slope submarginal, angulated; beaks central; cardinal plate thick, lateral teeth robust. This species is from Mr. Smith's collection. It belongs to the genus *Mulinia* of Gray.

M. subparilis. Plate II, fig. 12. Triangular, elongated, moderately thick, convex-depressed; posterior side cuneiform; apex hardly oblique, subcentral; fosset wide; lateral teeth transversely striated. Belongs to the genus *Spisula*, Gray.

LUCINA.

Lucina trisulcata. Obovate, convex; with concentric lines, and two or three distant concentric furrows; lunule profound. Differs from *L. alveata* of the lower tertiary in being less ventricose, and in the much more profoundly impressed lunule; the cardinal teeth are also very different.

L. radians. Orbicular, convex, with minutely waved or rugose concentric striæ, and radiating striæ obsolete on the middle of the valves, and most distinct on the anterior side; posterior submargin destitute of radiating lines; beaks central, prominent; margin crenulated. This species is recent at Mobile Point, Ala., and occurs near Newbern, N. C.

MYTILUS.

Mytilus incrassatus. Thick, much inflated; anterior margin slightly incurved near the middle; basal margin not obtusely rounded; hinge thick, with slightly prominent robust teeth. Length, about 3 inches.

CARDIUM.

Cardium sublineatum. Plate II, fig. 13. Obliquely obovate, thin, slightly ventricose, with obsolete radiating lines, most distinct near the ends; submargins of anterior and posterior sides destitute of radiating lines; within striated; margin crenulated.

GNATHODON.

Gnathodon minor. Plate II, fig. 14. Trigonal, convex-depressed, posterior margin truncated and nearly direct. This makes the fourth species of this interesting genus, two recent and two fossil.

AMPHIDESMA.

Amphidesma constricta. Plate II, fig. 15. Oblong oval, ventricose; basal margin opposite the apex slightly contracted; end margins rounded; beaks nearest the posterior extremity; fosset profound; cardinal teeth prominent, lateral teeth none.

A. protexta. Oblongo-elliptical, compressed, with minute raised punctæ; basal margin straight; apex prominent, about one fourth the length of the valve from the posterior extremity; posterior side slightly reflected; lateral teeth none. Length, ⅞ inch.

A. nuculoides. Ovate, convex, with very regular minute concentric lines; anterior extremity acutely rounded; beaks near the posterior extremity; basal margin arcuate; lateral teeth obsolete.

CARDITA.

Cardita perplana. Plate II, fig. 16. Trigonal, nearly flat; ribs about eleven, angular, minutely granulated.

C. abbreviata. Plate II, fig. 17. Trigonal, elevated, convex-depressed, ribs about eleven, convex, minutely granulated; posterior extremity angulated. This and the preceding species belong to the genus *Venericardia* of Lam.

NATICA.

Natica Caroliniana. Plate II, fig. 18. Obliquely suboval, with obsolete spiral lines; umbilicus large, with a central rounded prominent thick carina; apex prominent.

N. percallosa. Spire convex-depressed; surface with fine obsolete spiral striæ, umbilicus closed by a profound callus. Differs from *N. duplicata* in having the umbilicus perfectly concealed, and in the depressed spire. I think this species occurs recent on the southern coast.

INFUNDIBULUM.

Infundibulum centralis. Obtusely ovate, with fine concentric irregular lines; apex central.

POLYPARIA.

LUNULITES.

Lunulites denticulata. Prominently convex; apex slightly prominent; pores elliptical, arranged in quincunx order, and having slender projecting spines on the inner submargin; base with ramose minutely granulated lines; margin denticulated.

L. depressa. Suboval, convex-depressed; pores unequal in size and irregular in form; many of the larger pores filled with a minutely porous plate or diaphragm, solid in the centre; others denticulated on the inner submargin; base granulated, and with very numerous minute ramose striæ, slightly impressed; margin denticulated.

ART. IX.—*Analysis of various Ores of Lead, Silver, Copper, Zinc, Iron, &c., from King's Mine, Davidson County, North Carolina;* by Prof. JAMES C. BOOTH.*

THE Washington Mining Company of Davidson county, North Carolina, was commenced under the enterprise of Roswell King, Esq. several years since, but in consequence of the monetary difficulties of our country, the progress of the active mining operations was retarded until it received an impulse during the past year, which promises liberally to reward the enterprise of those engaged in it. Having received specimens of the vein only, and none from the adjacent formations, I am unable to give its geological position. The main body of the working vein appears to

* To B. SILLIMAN, JR.

Dear Sir—Agreeably to the opinion expressed to me when you were lately in this city, that the results of the analyses performed in my laboratory on the silver and lead ores from North Carolina, would be interesting to the public, and having obtained permission of the proprietors of the mines to give publicity to the results, I herewith transmit you an outline of the same. Respectfully yours,

Philadelphia, June, 1841. J. C. BOOTH.

consist of carbonate of lead, as far as it concerns the valuable portion of the formation. Besides this we have also zinc, copper, silver, iron, manganese, silica, alumina, and magnesia; the two former, and sometimes the lead, being in the state of sulphurets as well as carbonates, the silver being metallic, rarely sulphuretted, and the iron and manganese as oxides, constituting with silica, alumina, and magnesia, the gangue.

The object being to ascertain the relative amounts of the metals and the constituents of the gangue, the analysis was conducted by simple solution in nitric acid, of all soluble materials, and then by igniting the insoluble with carbonate of soda and treating it as a silicate. As we did not endeavor to ascertain the exact manner of combination in each specimen, the sum of the substances will necessarily vary from one hundred, which, however, has no influence on the general results.

No. 1, analyzed by E. Mayer, a selected specimen from the best portion of the vein, containing masses of solid silver disseminated through it, independently of small spangles barely visible to the eye, and that contained in the carbonate of lead.

			Per cent. of metals.
	Metallic silver,	12.51	12.51
	Carbonate of lead,	55.15	46.145
	Sulphuret of zinc,	3.32	2.23
Chiefly gangue.	Peroxide of iron,	14.25	9.88
	" manganese,	a trace	
	Silica,	10.92	70.765
	Alumina,	2.47	
	Magnesia,	2.83	
		101.35	

No. 2. By J. V. Z. Blaney. Bluish grey, very talcose, containing a few masses of silver; the greater part of the metal being in the form of minute spangles, particularly in the dark bluish portions of the ore, and becoming visible only when rubbed by a hard and smooth substance on a white surface.

			Metals.
	Silver,	11.14	11.14
	Carbonate of lead,	trace	
	" copper,	8.88	4.55
Gangue.	Oxide of iron,	7.50	5.20
	Silica,	32.29	
	Alumina,	30.40	20.89
	Magnesia,	9.07	
		99.28	

No. 3. Copper and zinc ore, by William M. Uhler. Black, containing both sulphuret and oxide of copper.

			Metals.
	Sulphuret of lead,	0.81	0.70
	" copper,	54.27	36.08
	" zinc,	23.86	15.92
Gangue.	Oxide of iron, (partly sulphuret,)	8.32	5.768
	Silica,	10.20	
	Alumina,	1.96	58.468
	Magnesia,	trace	
		99.42	

No. 4. Yellowish (ferruginous) carbonate of lead, by J. V. Z. Blaney.

			Metals.
	Silver,	0.54	0.54
	Carbonate of lead,	64.70	49.86
	" copper,	trace	
	Sulphuret of zinc, (carbonate ?)	2.60	1.78
Gangue.	Oxide of iron,	14.40	10.05
	" manganese,	1.40	0.977
	Silica,	12.40	
	Alumina,	3.70	63.207
	Magnesia,	1.00	
		100.74	

No. 5. Carbonate of lead with ferruginous gangue. By J. V. Z. Blaney.

			Metals.
	Carbonate of lead,	43.60	36.39
	" copper,	3.30	2.10
Gangue.	Oxide of iron,	15.00	10.40
	" manganese,	5.60	3.91
	Silica,	14.49	
	Alumina,	9.27	52.80
	Magnesia,	6.63	
		97.89	

No. 6. Zinc and copper ore. The zinc and lead are chiefly regarded as sulphurets, the copper chiefly carbonate, but it also exists as sulphuret and oxide. J. V. Z. Blaney.

			Metals.
	Silver,	0.05	0.05
	Sulphuret of lead,	5.30	4.59
	Carbonate of copper,	25.50	16.60
	Sulphuret of zinc,	41.30	27.56
Gangue.	Oxide of iron,	7.15	4.96
	Silica,	20.00	
	Alumina,	0.60	53.76
	Magnesia,	1.60	
		101.50	

No. 7. By J. V. Z. Blaney. Black, copper chiefly as carbonate, with oxide and sulphuret.

			Metals.
	Silver,	0.5	0.5
	Carbonate of lead,	1.8	1.5
	Do. of copp'r (oxide and sulphuret),	48.2	29.8
Gangue.	Oxide of iron, (trace of mangan.)	10.4	7.21
	Silica,	20.0	
	Alumina,	5.2	39.01
	Magnesia,	13.7	
		99.8	

No. 8. Carbonate of lead with ferruginous gangue. By J. V. Z. Blaney.

			Metals.
	Carbonate of lead (trace of copp'r),	81.80	62.82
Gangue.	Oxide of iron,	12.00	8.32
	Silica,	3.12	
	Alumina,	1.10	71.14
	Magnesia,	0.40	
		98.42	

No. 9. Carbonate of lead with highly ferruginous gangue. By Wm. M. Uhler.

			Metals.
	Carbonate of lead,	56.40	43.75
	Oxide of copper,	2.63	2.50
	" zinc,	0.17	0.137
Gangue.	" iron, (trace of mangan.)	18.60	12.897
	Silica,	10.58	
	Alumina,	3.66	59.284
	Magnesia,	6.17	
		98.21	

No. 10. Brownish yellow, and highly ferruginous. By H. C. Lea.

			Metals.
	Carbonate of lead,	62.21	48.21
	Sulphuret of zinc,	6.00	4.00
Gangue.	Oxide of iron, (trace of mangan.)	23.70	16.432
	Silica,	4.00	
	Alumina,	0.002	68.642
	Magnesia,	3.103	
		99.015	

No. 11. Yellowish white or grey lead ore. By H. C. Lea.

			Metals.
	Carbonate of lead,	59.83	46.366
	Sulphuret of zinc,	3.78	2.53
Gangue.	Oxide of iron,	5.20	3.606
	Silica,	12.50	
	Alumina,	9.70	52.502
	Magnesia,	9.423	
		100.433	

Note to No. 2.—William M. Uhler tried approximately the process of amalgamation with No. 2, suffering the mercury to remain in contact with the pulverized ore for several days, and found that 1 lb. avoir. took up 419 grs. troy, which gave 145 lb. 5 oz. 16 dwt. 16 grs. to the nett ton avoirdupois of 2000 lbs.

Art. X.—*Improvement in the Daguerreotype process of Photography;* by F. A. P. Barnard, Prof. of Math. and Nat. Phil. in the Univ. of Alabama.

Messrs. Editors,—I commenced, about a year since, in connection with Dr. Wm. H. Harrington of this place, a series of experiments in photography, according to the methods of Mr. Fox Talbot and M. Daguerre. Our attention was directed principally to the Daguerreotype process. From the analogies known to exist between iodine and chlorine, we were strongly impressed with the belief that the latter substance might in some manner be employed to render the surface of silver more sensitive to the action of light than it had yet been made. To determine the correctness of this opinion we instituted a variety of experiments, which, as they proved for the most part unsuccessful, it is unnecessary to detail. The coating formed by the direct action of chlorine gas upon polished silver, was not found to possess the desired photogenic properties. We were led, therefore, to seek whether by the decomposition of some compound of the metal, a sensitive chloride could not be produced. Mr. Talbot had already done this in the preparation of his photogenic paper; but as it was our desire to avail ourselves of the beautiful lights formed by the vapor of mercury, and as the prepared paper, at least so far as our experiments go, is not susceptible of receiving them, we endeavored to produce the decomposition upon the surface of the solid metal. It occurred to us as a possibility that the iodide formed in the usual manner, by exposing a plate over the vapor of iodine, might perhaps give up its silver to chlorine, and thus produce the desired coating. This impression was not verified in our first experiments, owing to a cause which will presently be noticed. Perseverance, however, at length brought its reward. By varying in every possible manner, the circumstances of the experiment, we succeeded in producing a surface so exquisitely sensitive to the action of light, that the image of an illuminated object was formed upon it in the camera in a space of time almost inappreciable.

The following is the process by which this result is obtained. Let the plate be prepared in every respect as if an impression were to be taken according to the method of M. Daguerre. Let it be then exposed for the space of half a minute to the action of chlorine gas, diluted with common air to such a degree that it may be inhaled without any particularly unpleasant sensation. It will then be found so extremely sensitive, that on being placed in a camera, with an aperture such as is commonly employed in taking miniature portraits, an impression will be produced upon it in the smallest time in which it is possible to remove and replace the screen. The completion of the picture over mercury is effected in the usual way.

A plate thus chlorized, on exposure to light almost immediately assumes a very deep violet color, nearly approaching black. The mercury is not directly tarnished, and in this state the picture is even more beautiful than after being washed with the hyposulphite of soda. But without this washing it cannot be preserved.

M. Daguerre has announced that he is able to take images of objects in an instant of time. I have not seen any statement of his method. Some of the artists in the Atlantic cities have been equally successful. Their process is not that which I have here described. I suppose that I am acquainted with the mode of preparation which they employ; but as it was communicated to me under an injunction of secrecy, before I had discovered it myself, although I had actually employed it unskilfully, and therefore without complete success before, I can say nothing of it here. It will, without doubt, soon be made public, if it is not already known. I believe, at any rate, that the chloride coating is more sensitive than any other which has yet been used.

This discovery has opened a new field of experiment, in which we are now actively engaged. The results may be communicated hereafter.

It appears to us that the lights produced by this process of preparation are much finer and smoother than those of the original process of M. Daguerre. Some idea of the quickness of the camera operation may be formed from the statement of the fact, that a man walking may be represented with his foot lifted as about to take a step.

The quantity of chlorine necessary to produce the effect is exceedingly minute. In our early experiments we employed a quart bottle of the gas, opening it in a deep box, and leaving out the stopper while deliberately counting twenty. Replacing then the stopper, the plate was laid for half a minute over an opening in the top. After fifty experiments the gas in the bottle seemed not to have lost any of its original intensity of color. We have better arrangements at present in preparation.

Much care is necessary to avoid an excess of chlorine. The principal cause of our early failures arose from an error of this kind. One may easily determine, with any apparatus, the time and quantity necessary, by laying a plate over the aperture and drawing it partially off at intervals. The action of the gas will then be greatest, of course, upon the part longest exposed. Too much care cannot be taken to exclude the light during the process of preparation.

Tuscaloosa, July 1, 1841.

Art. XI.—*On two decomposed varieties of Iolite;* by Charles Upham Shepard, Professor of Chemistry in the Medical College of the State of South Carolina.

1. *Pinite of Haddam.*—This mineral is mentioned in Cleaveland's Mineralogy on the authority of Prof. Silliman, as occurring at Haddam, (Conn.) where it was probably noticed as early as the discovery of the chrysoberyl, along with which it was first found. It presents itself most commonly, in small foliated masses of a dull bluish green color, disseminated through the

same albitic granite in which the chrysoberyl, garnet, columbite, zircon, automalite and bismuthic ores occur. A few specimens have been observed, in which the pinite assumed a regular crystalline form; the figure of the crystal being either an hexagonal prism, or this form altered by the bevelment of its lateral edges.

The discovery at a more recent date by Prof. MATHER, of the beautiful blue iolite in the same town, has led to the development of a much greater supply of the pinite; and its occurrence is under such conditions as to leave no doubt of its being the first mentioned mineral, in a new state of chemical arrangement as regards its constituent particles. Both varieties here occur together in a large grained, highly crystalline, albitic granite, which also abounds in small black tourmalines. The pinite is by far the most abundant variety; good plates of iolite occurring only now and then, in limited areas of a foot or two in diameter, while the pinite is often so abundant, as mechanically to take the place of mica, in the formation of the granite.

The iolite has frequently been procured here in tabular plates, several inches across; and is remarkable for the facility with which it admits of cleavage into still thinner layers. This separation is undoubtedly promoted by the presence of exceedingly thin plates of what seems to be mica. The crystals are but seldom possessed of well defined lateral planes, in consequence of the implantation upon them of mica, albite, tourmaline, and more rarely of tabular spar. When perfect, however, they are either regular hexagonal prisms, or else this form, modified by the replacement of its lateral edges. Their color is a rich dark blue, with an occasional inclination to green; the depth of the color, as is usual in this species, is enhanced by the inspection of the plates in a direction perpendicularly to their cleavage. The specific gravity of a clear specimen, according to Dr. THOMSON, (Outlines of Mineralogy and Geology, Vol. I, p. 278,) is 2·651 . . . 2·6643.

From the same author, we learn that the mineral has the following composition:

Silica,	49.620
Alumina,	28.720
Magnesia,	8.640
Lime,	0.228
Protoxide of iron,	11.580
" manganese,	1.508
	100.296

The pinite variety, though generally occurring in indeterminate shaped pieces, yet nevertheless is occasionally seen in forms of the same shape and regularity as the iolite, from which, however, it differs essentially in color and hardness. The peculiar tint affected by the pinite is a pale, bluish, chloritic green. Its lustre is pearly, and not particularly shining, except in a few specimens, where the color approaches silver-white. Hardness 2.5. Laminæ neither flexible nor elastic. Common mica frequently pervades the mineral. Specific gravity=2.8, which, however, is a little too high, from the impossibility of disengaging the air from the mineral, when weighed in water.

The alteration which has taken place in the iolite does not appear to be attributable in all instances to the weathering of the rock; for we notice perfect specimens of iolite upon the surface of the ledge: and on the other hand, the pinite variety occurs at considerable depths from the top of the rock, where it has been laid open by gunpowder. Many individuals are, in part, hard and transparent, while the remaining portions present the aspect of true pinite. It must be mentioned, however, that a new locality of the pinite has of late been discovered, distant about half a mile from that of the iolite, where, judging from the specimens, the engaging rock is less sound, and where no examples of iolite have been noticed. The pinite here is in gigantic crystals, (five or six inches in diameter,) and possessed of unusual regularity. Some of them resemble large hexagonal plates of mica.

So obvious did it seem that the Haddam pinite is merely a variety of iolite, that I deemed it a superfluous labor to strengthen the opinion by a resort to chemical analysis; for we have in the present instance, identity of crystalline form as well as of internal structure, similarity in specific gravity, and still farther, both varieties entering into one and the same individual. Analogous changes, moreover, are frequent among species whose chemical formulæ are not very diverse from iolite; for example, in the tremolitic hornblende of Amity, New York, and the sahlitic pyroxene of Canada and St. Lawrence county, N. Y., which often present themselves in a soft serpentine-like state, and this without having suffered any apparent interchange of elements with contiguous minerals, and wholly unaltered in chemical composition, with the exception of their hydrous content. The Haddam pinite loses 3.07 per cent. of water on ignition.

2. *Chlorophyllite.*—The following description of this substance is abstracted from the "First annual report on the geology of the State of New Hampshire," by Dr. Charles T. Jackson.

"*Chlorophyllite.*—I have given this name to a new mineral found near J. Neal's mine in Unity. The name is derived from the Greek words signifying *green folia*, a name which is eminently characteristic of the species. It occurs in tabular, or short six sided prisms, arranged in folia or in columnar masses, resulting from the openness of its natural joints. The extremities of the prisms or tables are often covered with thin layers of mica, which circumstance has caused some mineralogists to mistake the true nature of the mineral. It cleaves readily into regular six sided prisms, with resplendent surfaces. On trying its hardness it was found to yield to the knife readily, but it scratches glass, and is harder than phosphate of lime. Its powder is of a very pale greenish white. Alone before the blowpipe it glazes slightly on the surface, but does not fuse entirely. It is fusible with carbonate of soda, with slow effervescence, and forms an opaque greenish enamel, which becomes of a darker green in the reducing flame. Its specific gravity is 2.705.

"Mr. J. D. Whitney analyzed a specimen of it in my laboratory, and obtained the following results. One hundred grains of the mineral consist of

Water, - - - -	3.600
Silica, - - - -	45.200
Phosphate of alumina, - -	27.600
Magnesia, - - - -	9.600
Protoxide of iron, - - -	8.256
Manganese, - - - -	4.100
Traces of potash, and loss, - -	1.644
	100.000"

I am indebted to Mr. B. Silliman, Jr., one of the editors of this Journal, for a specimen of this mineral. In handing it to me, he remarked that it had a strong resemblance to the Haddam pinite. Its color is more bluish, however, than that of the Haddam mineral. Its general lustre also is higher, but this is owing to scales of what appears to be silvery mica traversing the chlorophyllite, both in the direction of its horizontal and vertical cleavages. Its hexagonal cleavage is remarkably distinct. It still farther differs from the Haddam pinite in presenting points where the iolite remains unchanged, possessing not only its usual blue color, but its characteristic hardness and vitreous lustre.

As the analysis quoted gives phosphate of alumina, I applied the customary test for phosphoric acid, (of fusion with carbonate of soda, neutralization by acetic acid and the addition of nitrate of silver,) without being able to detect any traces of this acid. I conclude, therefore, that some error has been committed in stating the results of Mr. Whitney's analysis. If we set down the phosphate of alumina as alumina, the constitution of the mineral would not be inconsistent with that of the Haddam iolite, as determined by Dr. Thomson. I verified the aqueous content of the chlorophyllite, and in so doing observed, that the powdered mineral, both before and after ignition, corresponded very exactly with that of the Haddam pinite.

By the reader of my treatise on Mineralogy, it will be observed that I referred the Haddam mineral, as well as that from Lancaster, Massachusetts, and the French pinite, to the species mica. I now relinquish that opinion, although I am not equally satisfied that the minerals from the two last localities belong to iolite, as in the case of those which form the subject of the present notice.

New Haven, July 15, 1841.

Art. XII.—*Steam Navigation to the Pacific by the Isthmus of Panama and along the Western Coast of South America.*

Some interesting pamphlets on the subject named in the title were placed in our hands early in 1840 in Boston, by a brother of Mr. William Wheelwright, to whom mainly the world is indebted for an undertaking which may be with propriety ranked the first among the enterprises by steam. Mr. Wheelwright has labored several years at this undertaking and is now on the eve of success. From himself we have just received a communication, which, although not intended for the public eye, contains many facts in which the world is interested, and we therefore venture to annex certain portions of his letter or abstracts from it.

Talcahuano, March 8th, 1841.

TO PROFESSOR SILLIMAN.

Dear Sir—I had the honor of receiving your valued favor only a day or two since, having left the United States about the time it was written, to take up the superintendence of the Pacific

Steam Navigation Company, which I had previously formed in England. Two of our steam ships, of about seven hundred tons each, the Peru and Chile, arrived in this port in fifty-five days from England, passing through the Straits of Magellan, from sea to sea, in thirty hours; sails were employed when the winds were fair, otherwise steam, and the voyage may be said to have been one of the most brilliant ever undertaken. The field for steam navigation in these seas is so ample that our first voyages came off most successfully, proving and fulfilling every statement made: unfortunately, however, the directors in England, neglecting to send a supply of coal, as previously arranged, the operations of the company have ceased, for the present, and I am now engaged in this place in mining for coal, an operation never before undertaken in this country, and which of course presents a thousand difficulties. My first object when I arrived here was to make a practical examination, to ascertain the strength of the coal, and see its influence upon our boilers and fire bars; for this purpose I proceeded south, with the double object of proving the coal and exploring Valdivia and the island of Chiloe. After some unsatisfactory experiments, we finally came to such an arrangement of our fire bars as to produce a result decidedly favorable; the excess of expenditure over the best Welsh coal was twenty-seven per cent., which is nearly as good as Newcastle coal. The formation of clinker is great, but it is not of an adhesive character, and the fires are easily cleared; the coal seems to possess no sulphur, and there is nothing disagreeable in the smoke; the ashes are white and the coal free from smut. The coal lies in horizontal strata, rising or falling not more than ten or eleven degrees; is about three to four feet wide, and is found, most generally, cropping out on the precipitous sides of hills: the upper stratum is generally soft; the next stratum, which is what I now send you, is found from twenty to forty feet beneath; and I am now engaged in sinking a perpendicular shaft for the purpose of finding a third stratum and still better coal. Some two or three cargoes of this coal have been shipped, and spontaneous combustion has been produced, which set fire to the vessels; it must be considered that the coal first used was never mined, and was taken merely from the surface. I have ascertained that in two instances the vessels which have been set on fire had vegetable matter on board—the first was a cargo of wheat stowed over a deep bed of coal: the next, the coal was shipped in what are called here *chequas*, made of grass. What influence they may have had in producing spontaneous combustion it is not in my power to say, and I should be much obliged if you could account to me for its spontaneous ignition. I cannot at present make any large deposit of this coal until I make some experiments, and for this object I shall load one or two small vessels with the coal, and watch it carefully, keeping it free from any vegetable matter, and from water, and giving it all the ventilation in my power; it is a great drawback upon my operations at present. On board the steamers we have iron bunkers for about ten or eleven days' fuel, and it causes me no anxiety in putting it on board. I had this arrangement of our bunkers made with a view of using this coal.

On my voyage south, I found at Valdivia and Chiloe the same strata of coal, and in a line of coast of more than four hundred miles there does not appear to exist the slightest difference in quality. It is perhaps worthy of remark, that the coal found at Boca del Toro, on the Atlantic side of the isthmus of Panama, and near Cherokee on the Pacific side of the isthmus, is the same to all appearance as that found in this district.

I am at present mining about fifty tons a week, but hope in the course of a few days to open some more mouths, and mine in much farther than I am doing at present; my only fear is that in sinking a shaft I shall be obliged to contend with a large quantity of water. As it is a new thing and a work in which I have no knowledge, I am obliged to adopt a common sense view of it, and work on as well as I can, until miners can be sent me from England. The cost at the pit's mouth will not exceed two dollars per ton; should I get it lower down, it will be necessary to clear it of water by a steam engine, which will render it somewhat dearer. Notwithstanding our operations are paralyzed at present, I feel persuaded that by the end of this year our line of intercourse to Panama will be completed, and our communication with North America and Europe greatly facilitated.

I have no doubt that the coal beds here will bring about sooner the steam intercourse westward from Europe to Australasia: this has been a favorite plan of mine for several years, and I hope that the arrangements which I made before I left England, patronized by Sir Edward Parry, Captain Fitzroy, Mr. Montague and others, will soon go into effect. Perhaps the greatest change ever effected will be produced by opening an intercourse westward from Europe to Asia, and making America the stepping stone between them. The isthmus of Panama is destined to become one of the most interesting spots in the world: a ship canal will be formed, and it will become the highway between the Pacific and Atlantic oceans. I have been frequently on the isthmus, have passed often between the two seas, have examined with much attention the facilities and obstacles which it offers for the object proposed, and have satisfied myself of the perfect feasibility of establishing a communication between the two oceans. On leaving England I was requested to report upon my journey over, and to examine the isthmus with care, as well as the river Chagres. As it may, perhaps, be acceptable, I extract from the report such parts as I conceive may prove interesting to you.

"Having prepared myself with the necessary apparatus, I commenced by sounding the Chagres bar, where I found at low tide fourteen feet of water; the river being then swollen eighteen inches, left twelve and a half feet of water, from thence upwards to the junction of the rivers Chagres and Trinidad, (which you will find in the map in my pamphlet,) where there are four and three fathoms close to bank, which vessels might use as a pier to discharge goods. A little above the junction the water shoals to seven or eight feet—the channel below is never less than three hundred to four hundred feet, and often one thousand to twelve hundred feet; a steamer of five hundred tons, properly built, might navigate as high up as the Trinidad, with perfect safety and ease; at this point it is also perfectly healthy; from this junction the distance is twenty-eight miles to the Rio Grande, which empties into the Pacific about three quarters of a mile from the city of Panama. Vessels of any size may enter this river, as the tide rises in spring twenty-two feet; the space between the two points has but a very slight rise. I should say that it could not exceed forty feet, for in passing over to Panama from Gorgona, I found there was not a hill to ascend, and that a good carriage road could be formed without making a single cut. While the land to the left towards Cruces was mountainous and broken, that to the right seemed to decline to an unbroken plane; hence, it appeared to me, that Lloyd's statement respecting that line was strictly true.

"My impression is, that the first object, before thinking of a canal, should be to make a good road from the junction of the rivers Trinidad and Chagres to the Rio Grande or Panama; by this means an intercourse between the steamers on the Atlantic and the steamers on the Pacific could be effected in three or four hours with perfect ease, and a cargo even transported in that time."

As it regards steam navigation in the Pacific, I feel convinced that it will gratify you to know, that the great work is going on. Even the few voyages made between Chile and Peru have shown, so palpably, its advantages, that the stopping of the steamers has produced a great sensation throughout the land; it is impossible to form an estimate of what it will do for these countries—the governments of Chile, Peru and Bolivia, have granted every protection and continue to give me every support; and I am under the firm conviction that when once perfected, its advantages will be found vastly beyond what I have described them. I am very much indebted for the insertion in the American Journal of Science, of my paper on iron steamboats. I have made considerable efforts to bring forward that subject in England; I have gone into its detail and examined with all minuteness the whole subject, and I am perfectly convinced that not only all our western waters will be navigated by steam vessels built of iron, but that transatlantic steamers will and must be of iron. Mr. Brunel, the celebrated engineer of England, wrote me a letter of thanks for the paper, and promised to lay it before the board of directors of the Great Western Company, and I have reason to believe that it was mainly instrumental in bringing about the building of the great iron steamer, which will shortly ply across the Atlantic, and show herself as vastly superior to the Great Western, as the Great Western was superior to others, when she commenced transatlantic navigation.

New Haven, July 20th, 1841.

TO MR. WHEELWRIGHT.

My Dear Sir—I am much gratified by your very interesting letter of March 8th, received yesterday, with two specimens of coal, for which I thank you. You rightly judge that I feel a deep interest in your project, which I consider to be one of the most interesting that has ever been undertaken. Your present mining operations are of the utmost importance, and their success must, I should suppose, be decisive of that of your enterprise; it must be too expensive, one would think, to bring coal from England, and it is most happy that Providence has supplied it in such immense quantities in the very regions where it is wanted, not only for navigation coastwise, along your immense ocean barrier from Panama to Patagonia; but for the supply of those points in the Pacific—Gallipagos Islands, Sandwich, Otaheite, &c., where depots will anon be established for the navigation of the Pacific, and eventually around the world. Your South American coal is a treasure of inappreciable value, and with the aid of trained English miners and engineers, I cannot doubt you will succeed. I dare say, however, that your New England "common sense," will suggest expedients that do not always occur to those who have been trained to move in a beaten track. Can you not drain your water out at a lower level, by carrying in galleries connected by shafts? You do not say which way your strata incline—if towards the declivity of the hill or mountain in whose sides the coal crops out, then your drainage will be easy. You will of course look out for vallies and gorges, and all those positions to which you can make a communication so as to have the water go off by gravity—for even a long tunnel may be a less expense in the result than a steam engine, and it is vastly more simple and easy in the management. I have made some little blow-pipe experiments upon the coal you have sent me; that from the upper layer appears more like lignite, which you know, is merely wood of trees, altered by time, pressure, and fermentation. The lower stratum is good bituminous coal, and from the abundant flame with which it burns, it must be well adapted to produce steam. It is very probable, that your next stratum below will be still better, as having undergone a more perfect assimilation, for you are aware that the true coal, (as distinguished from lignite,) is also a product of vegetable decomposition, but the plants were of a much earlier date, and in general not composed of firm woody fibre, but more soft and succulent. It would require extensive and skillful geological observations, on the spot, to decide whether you have the true bituminous coal formation of Europe and of North America, or a coal of a more recent date and less perfect—for such coals there are, as that at Brora in Sutherland, Scotland. The lignite belongs to the tertiary formation, a much more recent deposit than even the newest coal; but you may have a tertiary reposing directly upon the true coal formation. If your coal beds are of the more recent formation—which is very possible, (although I would not hazard an opinion from seeing merely hand specimens,) then it will never be as good as the true coals of an earlier geological date; still however, you must mine it at all events, as it is your only resource, (wood being I suppose out of the question;) it is certainly well worth mining, and judiciously managed, will no doubt yield you a good result.

As to the spontaneous combustion, it is probably occasioned by the fermentation of iron pyrites, (sulphuret of iron;) which, in the present case is abundantly visible to the eye, and where invisible, may be disseminated in minute and thin flakes and points through the body of the coal. It is very prone to absorb oxygen from the air and from water, and thus to heat and inflame. Your security, as I conceive, will be to lay out your blocks of coal in the dry, warm air, so as to have them thoroughly dry before they are shipped; and if it ever rains where your mines are, the coal after being above ground should be housed. In the ship, the coal should not be in contact with wood; if your bunkers are not all of iron, those that are of wood can be lined with stout sheet iron, and the coal should be covered from the air, especially the damp air of the sea, and the spray; if protected by wooden covers they should be lined with black tin, (thin sheet iron as prepared for tinning.) I mention this, because it is light, and covers ought not to be heavy; but no combustible thing should lie in contact with the coal—certainly not wheat or grass as you mention, or any other vegetable. You will, of course, reject any large visible masses of pyrites from the coal; any masses that are *visibly sprinkled* with it, you will also throw away; the English miners call the pyrites *mundie.* With these precautions, I do not believe your coal will spontaneously ignite, and should it do so, it will burn so slowly that it can be kept under till you make a port. I should remark that the small coals should never be taken on board, as being much more liable to ferment; they may be consumed in the engines on shore.

Art. XIII.—*Bibliographical Notices.*

1. *A Discourse on the character, properties, and importance to man, of the natural family of plants called Gramineæ or True Grasses; delivered as a lecture before the Chester County Cabinet of Natural Science, Feb. 19th,* 1841; *by* William Darlington, M. D.—The Chester County Cabinet of Natural Science, an institution of which the town of West Chester may well be proud, has, in addition to a regular course of public lectures on chemistry, astronomy, natural philosophy, &c., for the last year or two provided a series of *extra discourses* on miscellaneous topics of science and literature, several members of the society delivering each one or more lectures upon any subject they may deem sufficiently interesting or instructive. On a previous occasion, viz. in the course for the winter of 1839, Dr. Darlington chose for his theme, the *theory of the development and transformation of the external organs of plants*, as propounded by Wolf and Gœthe, and now constituting as integral a part of the science of botany, as the *atomic theory* does of a sister science. This interesting discourse upon a somewhat novel subject for a popular audience, as well as the recent lecture now under notice, has been printed in the pamphlet form, for private distribution. Premising that the subject may be found to possess a degree of general interest, in a district so distinguished for its agricultural advancement, Dr. Darlington first defines what a grass is; remarking, that "the term *grass*, in our vernacular tongue, is frequently used in a vague sense, to designate every kind of herbage found in our meadows and pastures; hence, we often hear people speak of *clover*, *lucerne*, and other plants—which have no botanical affinity whatever with true grasses—as though they really belonged to that remarkable tribe of vegetables. But such is not the language of naturalists, and ought not to be of any well-informed person. An accurate knowledge of objects can neither be acquired nor communicated without precision in the use of terms." .... "Having thus hastily glanced at some of the more striking features of the extensive tribe technically denominated grasses, and the characters by which they are distinguished from other plants, I flatter myself we shall have no difficulty in recognizing any member of that family which may hereafter come in our way. It will be no news, indeed, to any of us, to be told that *red-top*, *Timothy*, and *fox-tail*, are grasses; and we all, perhaps, may be aware that our cultivated oats, barley, wheat and rye, and even rice, belong to the same category. But the fact may not be equally familiar to every one, that our *Indian corn*, and *broom corn*, the *sugar cane*, and the *bamboo*, are also true and genuine grasses. Much as

these last mentioned plants may seem to differ from the multitude of common grasses, the disciplined eye of the botanist perceives at a glance that they all belong to the same family; and indeed, so eminently *natural* is the whole tribe, that is, so strong is the general resemblance in the characters and habits of its members, that superficial observers, finding it so much easier to adopt, than to verify [correct?] the crude notions of the vulgar, have actually supposed several species to be continually and reciprocally changing into each other! It is a curious circumstance in the history of this vulgar error, that in former times, when the occult sciences flourished, the peasantry of Europe imagined all our cultivated small grains to be subject to this kind of transmutation; that wheat was often changed, first into rye, then into barley, from barley into ray-grass or Lolium, from Lolium to Bromus or cheat, and finally from Bromus to oats. They supposed, moreover, that by the agency of a fertile soil, the degenerate grass could be gradually restored to its original form; or at least, that it could be brought back as far as rye!—'Veteres credebant frumentum per gradus degenerare in macriori terra, atque Triticum in SECALE, Secale in HORDEUM, Hordeum in BROMUM, Bromum in AVENAM et sic per gradus descendere, immo credebant et jam semina Bromi vel Hordei in fertiliori terra producere Secale.'* CAROLI A LINNE, AMŒNITATES ACADEMICÆ, Tom. V.—Even in our own enlightened age and country, as we are wont to phrase it, there are yet many persons strongly tinctured with the notion, that wheat is frequently transmuted into Bromus, or cheat; though I have not met with any so full in the faith as to believe that they can bring the degenerate offspring back again to its pristine state. It is remarkable, also, that this obsolete notion, so entirely exploded among scientific naturalists, has lately found an advocate in a gentleman of some pretensions as a geologist, and who has, more recently, acquired considerable notoriety by his researches concerning *territorial limits*.....As that gentleman has been so astute in detecting the muta-

* To continue the quotation:—"Duravit hæc opinio, quamdiu plantæ earumque flores conspiciebantur e longinquo et fugitivis occulis; postquam vero Malpighius, Tournefortius, et alii armatis occulis inspiciebant, describebant, et delineabant florum partes, etiam minutissimas, eum earum differentiis, et ex his plantarum genera constituebant diversissima, ex fructificationibus æque diversis prognata, obmutuit hæc opinio. Differunt enim inter se hæc dicta Cerealia seu Gramina ut Ovis, Cervus, et Camelus, nec unica pars figura et proportione convenit, sed æque diversa sunt generum structura hæc gramina, ac in sua specie constanter uniformia. Qui potest concipere Hœdum progenerari a Lepore, Cervum a Camelo, ille etiam solus capiat Secale ex Avena aut Hordeo prodire. Sic cæcutimus luce meridiana, ut talpæ in suo domo, dum nolumus exire et apertis occulis intueri naturam."—*Transmutatio Frumentum; Amœn. Acad.* 5, *p.* 116.

bility of the *laws of nature*, we ought not, perhaps, to be surprised at his discovery of the extraordinary mutation in our northeastern boundary, since it was established by the fathers of our republic. It is quite as likely that landmarks should change their locality, as that objects of natural history should lose the distinctive characters impressed on them by the hand of the Creator." The proportion which grasses bear to other families of plants is next considered; and we are informed that the grasses of Chester County, native, naturalized, and cultivated, amount to about one hundred species, or one-tenth of the whole number of flowering plants inhabiting the same district; but, owing to the immense number of individuals of many species, their proportion to the actual amount of vegetation is much higher. Their peculiar places of growth, their general distribution throughout the world, and the particular distribution of individuals, the limits and modifications of agriculture as produced by climate, or in some degree by national peculiarities and customs, are next considered; and to this follows a brief notice of the general properties and uses of this tribe of plants, which, comparatively humble as it is, "probably contributes directly and indirectly, more largely to the sustenance and comfort of the human family, than any, if not all, of the other groups of the vegetable creation." We can cite only a portion of this account. "In an *agricultural* point of view, the superior value of the grasses as materials for pasture and hay, is owing to the large quantity of saccharine matter with which they abound about the time of flowering; and which is the source of that rich sweet odor observable in well preserved hay. This saccharine matter, which pervades the whole plant before flowering, and is most perfectly elaborated at that epoch, is designed to be ultimately concentrated and deposited in the seeds, chiefly in the form of *farina;* and hence we find the herbage of comparatively little value after the fruit is fully matured. The skillful agriculturist, therefore, when he wishes to have good *hay*, cuts his grass at the moment when the nutritious juices are most perfect, and while they are diffused throughout the whole plant. But when his main object is the *seed*, as in our cultivated grains, he of course postpones his harvest until the career of vegetation is finished. It is needless to enlarge upon the importance of the herbage of the grasses, in supplying the food of our domestic animals, and, indirectly, the animal portion of our own food. I will, however, mention those species which are deemed of chief value in our meadows and pastures,—naming them in what I consider the order of their excellence. 1. The meadow, or green grass, erroneously called 'blue grass' in Kentucky, (*Poa pratensis*, L.)—2. Timothy, or the 'herd's grass' of the northern states, (*Phleum pratense*, L.)—3. Orchard grass, (*Dactylis glomerata*, L.)—4. Meadow Fescue, (*Fes-*

tuca pratensis, L.)—5. Blue grass, (*Poa compressa*, L.)—6. Ray grass, (*Lolium perenne*, L.)—7. Herd's grass of Pennsylvania, often called 'red top,' the 'bent grass' of the English, (*Agrostis vulgaris*, L.) and 8. Sweet scented vernal grass, (*Anthoxanthum odoratum*, L.) There are a few other grasses—native or partially naturalized—to be found on our farms, and which are more or less eaten by cattle when the better ones are wanting. But they are comparatively of little value, and good farmers are always desirous to supersede them by some of those above named. It is remarkable that all the grasses here enumerated are believed to have been introduced into our country. Those generally cultivated here, are the Timothy and orchard grass; and occasionally we see the ray, and herd's grass, or red top; though these last are not so much esteemed. Now and then we hear of attempts to introduce *new* grasses to the notice of our agriculturists—accompanied by exaggerated statements of their value—such as the taller oat-grass, (*Avena elatior*, L.) sometimes called 'grass of the Andes:' and a few years since one of our coarse indigenous grasses, called 'sesame' or 'gama grass' (*Tripsacum dactyloides*, L.) was so extravagantly lauded in the journals, that many lovers of novelties were induced to try the experiment of cultivating it, in place of the old approved plants; but, like some other experiments that we wot of in our day, it resulted in a total failure. It is indeed exceedingly doubtful whether any other grasses are so well adapted to our climate, and our wants, as those old and long-tried acquaintances of our farmers which I have already enumerated."—"But it is from the *seeds* of the grass tribe, with one exception, that we derive the most eminent and immediate advantages. To them we are indebted for what has been emphatically called the staff of life. The chief bulk of these seeds being made up of farinaceous matter, which is always innocent and nutritious, they are consequently well adapted to the sustenance of man. They not only supply us with *bread*, but with all the countless variety of dishes which ingenuity has prepared, both from the flour and the unground grain; and if but few species are commonly employed for that purpose, it is because the large size of their seeds, compared with those of other grasses, renders them more eligible as objects of culture. There is but a solitary instance alledged of the unwholesomeness of the seeds in the entire family of the grasses, viz. those of the darnel, (*Lolium temulentum*, L.)—a common weed in many parts of Europe, but scarcely known in the United States; and even in this case, the deleterious effects are probably much exaggerated. It is only when the seeds are damaged or diseased, that they become injurious to health; as when putrefaction has commenced, or when that peculiar disease and enlargement of the grain occurs, which is known by the name of *ergot*.

Accordingly we find the poisonous plants furnishing the multifarious ingredients of the apothecary's shop; while the simple grasses, in their sound and unsophisticated condition, yield nothing but the wholesome materials for food and nourishment. It is true that human ingenuity has extracted a potent medical agent, in the form of *alcohol*, from the fermented seeds and juices of the Gramineæ; and it is equally true, that man has wickedly converted that *extreme medicine* into a daily beverage. But this is only a signal instance of his depravity, in perverting the blessings bestowed upon him, and argues nothing against the intrinsic value of the materials thus abused. It merely illustrates the ancient truth—*corruptio optimi pessima*, that the prostitution of the best things produces vilest results."

The remainder of this interesting popular lecture is chiefly occupied with an account of the chief grasses which are cultivated for grain, commencing with the least esteemed, such as oats and barley, and ascending in the scale of value to rye, Indian corn, to wheat, which in point of intrinsic value may justly claim the highest place, and to rice, which is believed to afford sustenance to a larger portion of the human family than any other grain. Lastly, the sugar cane is noticed, the value of which does not consist in its seeds, but in the saccharine juice contained in its pithy stem. We have only space for the concluding remarks:—" Thus are we furnished, by this magnificent grass, with the purest, most nutritious, and universally palatable of all the ingredients that enter into the composition of our food. The large portion of our globe adapted to the growth of the plant, and the copious product of its juices, render it probable that the cane will ever be our principal resource for the supply of sugar. The *maple* may furnish a tolerable substitute to foresters, who live remote from the channels of commerce; and systems of policy, or other considerations, may induce a partial resort to the *beet*, to obtain this delicious and indispensable commodity; but it may be doubted whether any, or even all the other species of the vegetable kingdom can rival this single grass in the production of sugar, either in the quality, the quantity, or the cheapness of the supply. That the history of the plant and its products, is closely interwoven with a melancholy tale of oppression and human misery, is unhappily as true as it is reproachful to our race; and it is no less true that the choice product of the cane, like that of its grain-bearing kindred, is often prostituted to the vilest and most mischievous uses; yet we must recollect, that these evils are the results of man's own folly and wickedness, and are no more chargeable upon the blessings thus perverted, than they are imputable to the designs of a bounteous Providence."

2. *Address delivered at the annual meeting of the Boston Natural History Society, May 5th, 1841; by* J. E. Teschemacher. Boston, pp. 46, 8vo.—This well-written discourse is chiefly occupied with a sketch of the recent progress of the different branches of natural history, beginning with zoology, and ending with mineralogy and geology. In such a wide range, it is obviously impossible, within the limits of an ordinary address, to give more than a glance at some of the prominent discoveries and most interesting facts in each department; indeed, nothing beyond a mere outline could be expected from a single person, without great expense of time and labor. The author offers the following pertinent remarks upon the prevalent idea; that these sciences become essentially more difficult as they attain a higher degree of perfection, so that it is almost hopeless for a man of common leisure and intellect successfully to prosecute their study, or even to keep pace with the march of discovery.

"That the accumulation of facts and objects of interest in every science is great, cannot be denied; but it is also certain that every discovery, every approach to truth, dissolves the clouds of error in which a science may be enveloped, and instead of making it more intricate, simplifies and renders it more amenable to the commonest understanding. The process is clearly this; the great accumulation of facts in any science, causes an absolute necessity for arrangement into divisions and subdivisions; the more extensive the knowledge and the number of the facts, the more natural, the more clearly defined and simple are these divisions, and each becomes the object of a separate study; hence, the subject is more easily mastered, more easily grasped by the mind, while the man of comparatively little leisure can undertake a single division and not only keep pace with discovery, but even add something to what is already known.

"In botany, for instance, how few students are acquainted with the cryptogamous plants, and of those who are, how few know much of the *Algæ* or sea-weeds. In each of these divisions of the vegetable kingdom, the facts have accumulated so much as render it sufficient for a separate study. So in chemistry, how little was formerly known of the chemistry of organic bodies: there is now sufficient to make it a division of great importance and separate study. And in geology, how few geologists are well versed in that most interesting portion, fossil vegetation. The person who would attempt to embrace at once any extensive branch of natural history as a whole, might just as well endeavor to learn the dictionary of a language by heart; but taken in detail, division by division, there is much less difficulty in obtaining insight into science than formerly, while separate fields are thereby offered to the increased number of laborers, where each may expect to reap a reward. Indeed, the concentration of the whole attention on one division has been the evident cause of many brilliant discoveries. Let no one, therefore, be discouraged by the idea that science is more difficult of attainment now than formerly; it is in fact less so, as can be readily testified by those who have had to unlearn the errors they had previously counted on as truths, and noticed the simplicity which the removal of these always introduces."

We would observe, if we may hazard a passing observation upon the question, that these sciences, like most other branches of knowledge, were formerly simple because they were meagre; and they are now more difficult of attainment on account of their extent and copiousness. Geology was certainly a much less formidable science in the time of Werner, chemistry in the days of Lavoisier, and botany even as left by Linnæus, than in their present condition. But if the amount of labor demanded in both cases be compared with the amount and importance of the knowledge thereby acquired, which is the proper method of stating the question, we shall doubtless arrive at the same conclusion with our author.

3. *Leçons de Botanique, comprenant principalement la Morphologie Végétale, la Terminologie, la Botanique Comparée, l'Examen de la valeur des Caractères dans les diverses familles naturelles, etc.:* par Auguste de St. Hilaire. Paris, 1 vol. 8vo. (2 parts,) pp. 930, with 24 plates. 1840–41.—This treatise is written in a remarkably lucid and elegant, we may say brilliant style; and, although it contains much original matter, and such profound views as might be expected from the pen of so distinguished and philosophical a botanist as M. Saint-Hilaire, yet it is sufficiently elementary for the use of the tyro. It does not treat of vegetable anatomy or physiology, but is confined, as the title denotes, to morphology, &c., or to botany in the restricted sense; upon which subject it is not only the latest, but doubtless the best text-book extant. The *discours préliminaire* commences, in the usual manner, with an attempt to distinguish by a definition organized from inorganized bodies, and plants from animals. "Parmi les corps qui nous environnent, les uns, bruts et inertes, sonts privés de mouvement et de vie; ils ne naissent point, ils se forment; ils ne se nourrissent, ils s'agglomérent; ils ne meurent point, ils se décomposent. Les autres, au contraire, naissent pourvus d'organes destinés à des fonctions diverses; ils vivent, se nourrissent, se développent, et, avant de se décomposer, ils meurent. Les premieres sont les corps inorganiques, les seconds les corps organisés. Ceux-ci, cependant, n'atteignent tous le même degré de perfection; il en est qui, doués de sensibilité, le sont en même temps de diverses qualités qui semblent être la conséquence de la faculté de sentir; appelés à éviter la douleur à rechercher le plaisir, ils peuvent à volonté se transporter d'un lieu dans un autre; toute espèce de nourriture ne leur convient point, ils savent choisir celle qui leur est propre, et risquant de ne pas toujours rencontrer des alimens capables de s'assimiler à leur substance, ils les déposent dans un cavité interieure qui leur sert en quelque sort de magasin; enfin, ayant un centre de nutrition et de vie, ils peuvent rarement être séparés en plusieurs parties, favorisées chacune d'une vie individuelle; ce sont les animaux. Les plantes, au contraire, paraissent ne point

avoir le sentiment de leur existence, et sont étrangères à la souffrance et au plaisir ; elles restent fixèes au sol qui les a vues naitre ; elles absorbent, sans aucun acte de volonté, les matières inorganiques qui les entourent, ne les déposent point dans une cavité particulière, et, dépourvues d'individualité proprement dite, elles peuvent être multipliées par la division des parties qui les composent." In giving a brief history of the received doctrine of vegetable metamorphosis, (on which the present work is essentially based, and of which it is a complete developement,) the author very properly commences with the name of Linnæus. "A la fin du *Philosophia Botanica*, est une chapitre de quelques lignes intitulé *Metamorphosis plantarum*, où l'on trouve cette phrase : *Principium florum et foliorum idem est ;* phrase développée d'une manière admirable dans un autre écrit qui fait partie des *Amœnitates Academicæ*, et porte pour titre *Prolepsis plantarum.* Mais les disciples de Linné, qui se découvraient en prononçant le nom de leur maître, le comprenaient à peine ; ils admiraient en lui ce qui était peut-être le moins digne d'admiration, et l'aphòrisme si remarquable qui je viens de vous citer passa inaperçu. Longtemps après, un écrivain dont l'Allemagne s'honore en offrit le commentaire le plus élégant et le plus ingénieux. Son livre eut, le même sort que la phrase de Linné, il fut dédaigné comme elle ; les savans ne le lurent point, et s'imaginèrent que, sorti de la plume d'un poëte, il ne pouvait offrir qu'une rèverie écrite du style faussement poétique du *Connubia Florum*, ou des *Amours des plantes.* C'était bien mal connaître le génie de Goëthe, ce flexible génie qui prenait toutes les formes, et choissait toujours celle qui convenait le mieux au sujet qu'il avait à traiter ; qui, dans une œuvre merveilleuse qu'on voudrait brûler et relire, sait nous faire entendre tour à tour les célestes harmonies du chœur des anges, le grincement sardonique de l'auteur du mal, le bruit confus de la populace qui se presse, et les cris déchirants que le remords arrache à une infortunée coupable. Lorsque Goëthe voulut écrire sur la science, il fut grave comme la science elle-même ; il avait offert des modéles pour plusieurs genres de composition littéraires ; il en offrit un pour les compositions scientifiques. Si la *Metamorphose des plantes* ne fut point goûtée d'abord, c'est qu'elle avait paru trop tôt, c'est que l'auteur avait devancé son siècle." The chapters of the work treat successively of the different organs of plants, their diversities, and of the laws which regulate their development and disposition ; of their symmetry, and the manner in which this is often interfered with or disguised, (a very interesting chapter ;) of classification and its principles ; of vegetable anomalies ; and, finally, a short chapter is devoted to the peculiarities of cryptogamic plants. It was a happy idea, also, of the learned author, to combine with the explanation of the plates a concise, but very complete recapitulation of the facts or principles which they illustrate; thus rendering these pages a most interesting and readable portion of the book.

4. *Genera Plantarum secundum Ordines Naturales disposita, auctore* STEPHANO ENDLICHER. Vienna, 1836—1840. (18 fasc.) pp. 1483, imp. 8vo.—We have previously directed the attention of our botanical readers to this truly classical work. We have now to announce the completion of this laborious undertaking, which forms perhaps the most important contribution to systematic botany for the last twenty years. The number of genera described in the body of the work is six thousand eight hundred and ninety-five; and the additions in the supplement (which occupies a part of the seventeenth and the eighteenth fasciculus) extend this number at least to 7000. The sixth edition of the Genera Plantarum, by Linnæus, (Stockholm, 1764,) comprises only one thousand two hundred and thirty-nine genera. The work is published, if we mistake not, at a Saxon thaler for each fasciculus, and can be obtained in New York. It may be useful to state, that the learned author proposes to publish supplements to this work, either annually or at longer intervals. The tenth and final fasciculus of the *Iconographia Generum Plantarum*, by the same author, has also appeared.

Apropos to the above, we may remark that Dr. Walpers of Berlin proposes to publish, in occasional numbers, a *Repertorium Botanices Specialis*, a digest of the new species, &c. which appear in separate articles or pamphlets, or are scattered throughout the scientific periodicals of the day. The work is announced in the *Linnæa*, part 1, for the year 1841.

5. *Nomenclator Botanicus, seu synonymia plantarum universalis, enumerans ordine alphabetico nomina atque synonyma, tum generica tum specifica, et a Linnæano et a recentioribus de re botanica scriptoribus plantis phanerogamis imposita;* auctore E. T. STEUDEL. (Nomina si nescio, periit cognitio rerum.) *Ed. 2, ex nova elaborata et aucta.* Stuttgard and Tubingen, (Cotta,) two vols. imp. 8vo. 1840–1841.—The plan of this elaborate index to phanerogamic botany is nearly the same with the first edition; but the work is of course very much enlarged. The first volume, (of eight hundred and fifty-two pages, published in seven fasciculi,) extends from A to K inclusive. We have received the second volume down to the letter R, and ere this the remainder has probably been published. This is a work of immense labor, and great utility; but the library of the author appears not to be well supplied with the recent works of English and American authors.

6. *Kunze, Supplemente der Riedgräser* (*Carices*) *zu Schkuhr's Monographie, &c.*—The first fasciculus (the only portion that has yet appeared) of Prof. Kunze's supplement, or continuation of Schkuhr's well known work on *Carex*, comprises detailed descriptions and beautiful figures of fifteen species of that large and difficult genus. The specific characters are given in Latin; the remainder of the text in German. The figures are executed in the same style as those of Schkuhr, but are not so much crowded. Four of our North American species are here illustrated, viz. *C. leiorhynca*, Meyer; *C. Steudelii*, Kunth; *C. gracillima*, Schwein.; and *C. Frankii*, Kunth. The last is identical with the prior *C. stenolepis*, Torr.

7. *Hooker and Arnott's Botany of Capt. Beechey's Voyage;* part 10. 1841. (tab. 90–99.)—The tenth and last fasciculus of this work concludes the account of a collection on the Pacific coast of Mexico, and is terminated by a complete index. The ten plates it comprises are nearly all devoted to Californian plants described in prior fasciculi; among which *Pterostegia*, a curious Polygonaceous genus, *Anemopsis Californica* of Nuttall, and *Lophochlæna* of Nees, a singular grass, are the most remarkable.

8. *Elémens de Tératologie Végétale, ou Histoire abrégée des anomalies de l'Organisation dans les Végétaux;* par A. Moquin-Tandon. Paris, 1841, 1 vol. 8vo. pp. 403. (Monstra in animantibus horremus, amamus in pomis. *Ferrar., Hesperid.* lib. iv. cap. xi.)—This interesting treatise on vegetable monstrosities is very properly prefaced by a statement of what is meant by the normal structure of plants, by vegetable individuality, and vegetable symmetry. The author proceeds to consider, first, those slighter deviations which are called *varieties;* and secondly, those more grave and mostly congenital anomalies which bear the name of *monsters*. As to the latter the author remarks, that nearly every monstrous or abnormal condition that has been observed is to be met with as the normal state of other vegetables; and that between a monstrous and a normal flower, the only difference often is, that the former is the occasional, the latter the habitual state. "*La monstruosité est donc, en général, l'application insolite, à un individu ou à un appareil, de la structure normale d'un autre appareil ou d'un autre individu. C'est un organisation transposée, c'est une loi changée de place. On l'a dit avec raison, la monstruosité ne se trouve pas en dehors de la nature, mais seulement en dehors de la coutume.*" It is clear, therefore, that while abnormal states may always be explained by the laws which regulate the normal structure, *monsters* themselves, as the etymology of the name indicates, often

show us the true structure when it could not be certainly inferred from the habitual condition. The author arranges monstrosities under four primary classes; those of volume, of form, of disposition, and of number. These are divided, the first class into monsters by diminution of volume, (*Atrophy*,) and by augmentation, (*Hypertrophy*;) the second class into monsters by alteration of form, whether irregular (*Difformation*) or regular (*Pelorias*,) and monsters by the transformation of one organ into another, (*Metamorphosis*;) the third class into monsters by the abnormal connection of parts, or by the disunion of parts habitually united, and into those caused by change of situation, or displacement; the fourth class into monsters by diminution of number, or abortion, and those by augmentation of number. Under these heads the monstrosities of the different organs of plants are considered in detail, and in a philosophical and very interesting manner. This brief notice of the plan of Moquin-Tandon's work, we are confident, will suffice to commend it to the attention of the botanists of this country.

9. *A Manual of Botany, adapted to the productions of the Southern States; in two parts:* Part I. *Vegetable Anatomy and Physiology:* Part II. *Descriptive Botany, arranged on the Natural System, preceded by an Analysis;* by John Darby, A. M., Prof. of Chem. and Nat. Phil. in the Georgia Female College. Macon, (Geo.,) 1841. 1 vol. 12mo.—The publication of a new local Flora, on the natural system, preceded by a treatise on vegetable anatomy and physiology, embracing many of the most recent views and discoveries, furnishes an unequivocal indication of the advancement of botanical science among us. The first part of this volume comprises, within the compass of about 150 pages, a good account of vegetable organography and physiology in its present state.

The phenomenon of cyclosis, the laticiferous tissue, the recent views of Endlicher, Schleiden, &c. respecting the origin of the embryo are all noticed, although the author's limits have often prevented him from entering into sufficient details. To write a flora of any extensive district, or even to prepare a creditable compilation, involves an amount of labor of which few who have not made the attempt can have any conception; and there are, moreover, several circumstances which just at present render the production of a Southern Flora a difficult undertaking. It will not be surprising, therefore, that we should more highly estimate the first part of this work than the second. We are in doubt, also, as to the geographical limits which the latter is designed to embrace; since a few plants peculiar to Texas and Western Louisiana are included, while perhaps a greater number of published species indigenous to Carolina and Georgia are omitted. Such omissions, however, will not materially interfere with its usefulness as a class-book, and they may be readily supplied in a second edition.

10. *Report on the Manufacture of Iron*, addressed to the governor of Maryland, by J. W. Alexander, Topographical Engineer of the state. Printed by order of the senate, 1840; 269 pp. 8vo, with plates and tables.—We have already noticed the labors of Mr. Alexander in the survey of the state of Maryland, (Vol. XXVII,) and we shall now briefly mention the heads of the present important report.

It is contained in four chapters.

Chap. I. Historical researches into the manufacture of iron. Ninety two pages.

Sect. 1. *Ancient testimonies.*

Sect. 2. *Modern history of the manufacture.*

Sect. 3. *Researches into the manufacture in Maryland.*

Chap. II. Metallurgic and geographic distribution of the ores of iron. Twenty one pages.

1. *Native iron.* 2. *Meteoric iron.* 3. *Magnetic iron ore.* 4. *Specular iron.* 5. *Fibrous brown hæmatite.* 6. *Carbonate of iron.* 7. *Silicated iron.* 8. *Titaniated iron ore.*

Chap. III. Means, machinery and materials employed in the manufacture of iron. Sixty eight pages.

Sect. 1. *Blast furnaces generally—their location, construction, &c.*

Sect. 2. *Cost of construction and permanence of blast furnaces and their accessories.*

Sect. 3. *Of the materials used in blast furnaces—their method of extraction and preparation, and their cost.*

Chap. IV. Principal chemical phenomena of the manufacture of iron. Eighty four pages.

Sect. 1. *Exemplification of the general chemical theory of blast furnaces.*

Sect. 2. *Furnace cinder—its constitution and phenomena.*

Sect. 3. *Characteristics and constitution of the metal produced under various circumstances.*

Sect. 4. *Gaseous materials and products accompanying the formation of crude iron.*

It is obvious that this is an outline of a great work, and it is very ably executed by Mr. Alexander. As he justly observes in another communication, this effort appears to be of general interest as marking an epoch, when, in consequence of the conjoined exertions of those who are devoted to the cause of science, the systematic protection and diffusion of knowledge has come to be recognized as one of the important functions of a representative government. With this view the author of this report undertook the *gratuitous* labor of making it, in hopes that the state of Maryland, already signalized by having caused an early geological, and the first trigonometrical survey of a state territory, might also by her example, induce her sister states to enter upon this career—only another branch of the same general enquiry—by carefully collecting statistical facts in relation to different domestic resources and manufactures, exhibiting their actual condition and providing for further periodical statements as to their progress.

So far as this Journal has influence, we should be happy to employ a portion of its pages in disseminating impressions and arguments having the object indicated above, and we have to regret that our engagements have prevented an earlier notice of the labors of Mr. Alexander on the subject of iron. It is magnanimous in this gentleman to perform this great and responsible duty gratuitously—*pro bono publico*—and it is happy that in this respect, science as well as liberty can occasionally boast a *Washington;* but we feel bound to protest against such a course as a general one. This country is abundantly able to pay for all the scientific investigation which its interests demand, and the public sentiment should be formed to the full admission of this duty.

The report now before us has evidently been prepared with great care and labor. It exhibits abundant proof of a careful investigation of authorities—in the history, in the science, and in the arts relating to iron—with a wide range of personal observations carefully made both at home and in foreign countries, in all the most important practical operations, from the mining of the ores of iron to the conversion of this most important metal into the numerous forms in which it is demanded by the necessities of civilization. It is, in an important degree, a monograph of its subject, which, if carried into full detail in all its bearings, would demand volumes for its elucidation. Mr. Alexander's report is a full half of a full octavo, and he still finds his limits too narrow for all that he desires to communicate. Without wishing to confine his active and well furnished mind within more narrow bounds, we would venture to suggest the expediency of giving, in a separate pamphlet, a condensed abstract of the most important practical facts, for the use of iron masters and manufacturers, omitting many historical details and some of those scientific views which are most interesting to those who cultivate science for its own sake.

Did our limits permit, we would gladly recapitulate many interesting and important facts which are brought out in the present report. Among them are the results of the microscopic examination of the different varieties of cast iron, and also of bar iron. It appears that their minute particles have different crystalline forms, varying with the qualities of the iron. The crystals of the gray cast iron belong to the octahedral system, and present themselves under the forms of several of its classes; their minuteness is extreme; when the form is cubic, the linear dimensions are not over $\frac{1}{15000}$ of an inch, and the weight of a crystal $\frac{1}{2000000000}$ of a grain.

The crystals of the white cast iron are more generally six-sided prisms, &c. A work involving innumerable details must be carefully perused, in order duly to appreciate its value, and this course we, in the present instance, strongly recommend to all those, who from a scientific or practical interest, are disposed to study the history of the most important of the metals—for to iron is man indebted for the highest attainments in civilization. Its natural history, and its chemical and physical properties, combine a series of attractive results hardly equalled by any other substance, and in proportion to the importance of this metal to the human family, it is by the bounty of the Creator, diffused over the world with a profuse liberality, which places it almost every where within the reach of man.

Mr. Alexander has set an example worthy of imitation, by a full and able exhibition of both science and practice in relation to iron, and we trust that this is only a prelude to other labors of the kind, that will contribute to the honor and advantage of the country.

11. *Report on the Invertebrate Animals of Massachusetts, comprising the Mollusca, Crustacea, Annelida, and Radiata. Published by order of the Legislature; by* A. A. Gould, M. D. 8vo. pp. 373. Cambridge, Mass. 1841.

Reports on the Herbaceous Plants and Quadrupeds of Massachusetts, the first by Rev. Chester Dewey—*the last by* Ebenezer Emmons. 8vo. pp. 277 and 86. Cambridge, 1840.

We have already been presented with the reports of Dr. Storer and Rev. Mr. Peabody, on the *fishes*, *reptiles* and *birds*, of the state of Massachusetts,* and which we were given to understand, would be followed up by those whose titles stand at the head of this notice. The publication of these reports on the various departments of nature, taken in connection with the extended labors of Prof. Hitchcock on the geology, and of Dr. Harris (not yet published) on the insects, has thrown into the hands of the scientific world, and all lovers of nature, a body of accurate and useful information hitherto unequalled in the annals of any of our states; creditable alike to the liberal foster-care of the renowned commonwealth, whose patronage has called them forth; and

* See notice of these reports in Vol. xxxviii, pp. 379 and 393.

to the enlightened zeal and scientific accuracy of the native-born band of naturalists, whose united labors have been concentrated in the production of so brilliant a result. There is probably no country in the world where the familiar knowledge of nature promises with more certainty to become the privilege and enjoyment of the people than in our own, where alone researches of a truly scientific character, are conducted under the eye and with the approbation and support of the people themselves.

Those who would have a full view of this class of labors, as actually performed in the United States, will find it in Prof. Hitchcock's address before the associated geologists at Philadelphia, and published in the former part of this number.

Dr. Gould in the report under notice, has selected the mollusca as the principal field of his labors, both because it is the most extensive and therefore most important, and the one to which his previous researches and labors had been more particularly directed, and *we* may say, the one which his own discoveries had greatly augmented.

The reader may judge of the extent of Dr. Gould's labors in this department, when he learns that the present history of the testaceous mollusca of Massachusetts, embraces nearly double the number of species enumerated in the list appended to Prof. Hitchcock's first report, and which was the result of the united contributions of several conchologists.

Full descriptions are given in the present report, of two hundred and seventy four species. They are divided into families and groups, according to the recent views and classifications of the most celebrated malacologists, and each class is introduced by appropriate descriptive remarks.

The two hundred and seventy four species enumerated, are divided as follows among the various classes :—Annelida, six ; Cirripedes, twelve ; Conchifera, ninety seven ; Brachiopoda, two ; Gasteropoda, one hundred and fifty seven. Of these, twenty nine species are terrestrial, forty two are inhabitants of the fresh waters, and two hundred and three are marine, beside which there are in addition, seven marine and two terrestrial naked molluscs, or slugs, as they are vulgarly called.

The value of this work is much enhanced by the accurate although uncolored figures which Dr. G. has drawn, evidently with much labor, of two hundred and thirteen of the species described. Every one who has experienced the difficulty of determining shells from unillustrated descriptions, will fully appreciate the value of accurate figures.

But there is one point to which Dr. Gould has devoted much attention, and which has claims of interest to all naturalists, especially to geologists, namely, the geographical extent and distribution of the species. Peculiar pains were taken to give accuracy to this most difficult branch, and a correspondence and exchanges were opened with all the principal conchologists in this country, and several in Europe, for the purpose of ascertaining what species were peculiar to the territory of Massachusetts, and what were common to both shores of the Atlantic. By these means it has been proved, that at least twenty species of shells which had been described as new by our naturalists, were well known before in Europe, while seventy species, according to the present state of our knowledge, are peculiarly American. And what is a little singular, just about the same number are believed to be common to both countries. That narrow tongue of land called Cape Cod, which forms a long curved beach, extending for forty or fifty miles to the south and east of Boston, and is scarcely twenty miles over in the widest part,—usually much less, presents a great natural boundary between two seas, and by its peculiar form and extent gives fine opportunities for observing the limits of migration for many species of marine shells. "Many whole genera do not pass from one side to the other of this limit. Thus no species of Panopæa, Glycymeris, Cyprina, Terebratula, Cemoria, Cancellaria, Rostellaria, or Trichotropis, has yet been found to the south of the extreme point of Cape Cod; while Corbula, Conchlodesma, Cumingia, Montacuta, Tornatella, Cerithium, Ranella, and Pyrula, do not pass to the north of it. Of the two hundred and three marine species, eighty one do not pass to the south, and thirty have not been found to the north of the cape, though many of them approach within a very few miles of each other. The remaining ninety two species take a wider range, and are found on both sides."*

The entire extinction of certain species and even genera at particular localities where they have abounded within the human era, is a fact claiming particularly the attention of geologists, in estimating the value of shells as a means of determining the age and extent of geological formations. It will be remembered by geological readers, that some facts of this character in Scotland, early excited the notice of the distinguished author of the "Principles of Geology," whose calm and philosophic views of the phenomena of nature have done so much to recall geologists from the fairy land of wild speculation, and tame their exuberant fancy to the sober views of inductive philosophy.

The future student of American molluscs will owe an unending obligation to Dr. Gould, for the labor and science he has brought to the completion of this task. He has accumulated within the compass of a convenient volume, all that was valuable in the published memoirs of his predecessors, scattered through the pages of scientific journals

---

* Boston Journal of Natural History, Vol. III, No. 4.

and the transactions of societies, or existing in the form of separate monographs, all difficult of access—certainly beyond the means of most students, and nowhere perhaps to be found in any one collection. He has simplified the synonymy, which in certain departments was peculiarly complex, and given to the nomenclature an accuracy of orthography and accent, which is the more creditable, inasmuch as it is so rarely found in American scientific works. The accomplished conductor of the Cambridge press has won for himself an enviable reputation for classic accuracy, while in the mechanical details of typographic execution that press has long stood unsurpassed.

Prof. Dewey, in his report on the herbaceous flowering plants of Massachusetts, has endeavored, as far as was practicable without too great a sacrifice of scientific accuracy, to adapt his labors to the comprehension and use of those who were not particularly scientific. With this view, the economical value, the properties and uses of all plants interesting to the agriculturist, have very properly been expounded and made plain. The arrangement is mostly in accordance with the orders of Lindley, with a blending of the artificial classification of Linnæus. Prof. Dewey is well known to all readers of this Journal, more particularly in the earlier volumes, by his numerous and valuable papers on the Carices of North America. The report is such as might have been expected, from the well known reputation of the excellent author.

The report of Prof. Emmons on the quadrupeds is of course short, since the small number of genera which fall within so narrow a territory as Massachusetts, leaves but little room for expansion. This report, although not equal to the purposes of a thorough naturalist, is yet good of its kind, and fully sufficient for the use intended. Forty three species of quadrupeds are given as inhabiting Massachusetts; these fall into the orders, Carnivora, Rodentia, and Ruminantia, and are grouped into twelve natural families, as follows:—Bats, three; Shrew mole, one; Common mole, three; Ursidæ, two; Canidæ, three; Felidæ, three; Mustelidæ, (weasel family—including the skunk,) seven; Castoridæ, two; Leporidæ, two; Muscidæ, fourteen; Hystricidæ, one; Cervidæ, three. The reindeer is also given, on the rather improbable supposition of its having once appeared as a winter visitant; moose-deer have not been seen in Massachusetts within the past forty years—they are still hunted in the northern parts of New England, and in Pennsylvania, between the branches of the Susquehanna. We cannot but regret that this report is unaccompanied by figures of the animals described; the species are so few that it might have been done with comparative ease, and would have greatly enhanced the value of the work in the estimation of students.

12. *Boston Journal of Natural History, containing papers and communications read before the Boston Society of Natural History.* Vol. III, No. 4. Boston: Little & Brown. 1840–41.—Our readers have been well apprised of the sound progress of the society, whose last published transactions stand at the head of this notice. One of the best evidences of useful activity in a scientific society, is the regular publication of valuable memoirs on those departments of the great field in which its labors are prosecuted. The stated meetings of such bodies are very interesting and important to those who have the good fortune to live within the sphere of their influence. But to naturalists, occupying distant and isolated positions, publications of the above character are the only symptom of vitality, and are welcomed as the evidence of good deeds done, and the earnest of better things to come.

The articles contained in the present number, are a continuation of Dr. Amos Binney's monograph of the Helices of the United States, with plates: Further notices of some New England lichens, by Edward Tuckerman, Jr., LL. B: Attempts to ascertain some of the hepatic mosses of Massachusetts, with remarks, by Rev. John Lewis Russell: Descriptions of the fishes of the Ohio river and its tributaries, by Jared P. Kirtland—continued, (with plates:) Results of an examination of the shells of Massachusetts, and their geographical distribution, by Augustus A. Gould, M. D.—(vid. notice of this able memoir in our present number.) In addition to the above, the number contains the constitution and by-laws of the society, with a list of members and officers of the society, additions to the library, and an index of the contents of the third volume of the Journal, of which this is the concluding number.

13. *Transactions of the Royal Society of Edinburgh.* Vol. XV, Part I. Edinburgh, 1841, 4to, pp. 263.—This part of the Royal Society of Edinburgh's Transactions contains that remarkable paper by Dr. Samuel Brown, of Edinburgh, on the decomposition of substances heretofore considered elementary, or the transmuting of one substance into another by the aid of heat and pressure. It may be remembered, that we noticed this paper in our last, (see this Vol. p. 208,) having received an early copy in proof, and then expressed the intention of republishing the article entire, but our engagements to home correspondents have prevented its appearance in the present number.

Dr. J. K. Mitchell has since informed us orally that he has, in connection with Mr. Clarke Hare, of Philadelphia, repeated all the experiments of Dr. Brown, step by step, with an apparatus made exactly according to his description; and although the repeated experiments were conducted in the most careful manner, the results stated by Dr. Brown *were not obtained.* We are at a loss how to reconcile these contradictory statements, and still more to understand how so astute a chemist as Dr. Christison is believed to be, could give his sanction to any thing of so extraordinary a character, unless the facts were as stated by Dr. Brown. It is to be hoped so singular a subject should not be dropped until the truth is ascertained.

The other articles contained in this volume, are Prof. Forbes' fourth (and last) series of researches on heat; also, an account of some additional experiments in terrestrial magnetism made in different parts of Europe in 1837. Rev P. Kelland on the plane and angle of polarization of light reflected at the surface of a crystal. Dr. W. P. Alison, on certain physiological inferences which may be drawn from the study of the nerves of the eyeball. Dr. T. S. Trail, on the fossil fishes of the old red-sandstone of Orkney, particularly of an undescribed species, Diplopterus Agassis. John Goodsir, Esq. on the mode in which musket balls and other foreign bodies are inclosed in the ivory of the tusks of the elephant, (with a plate.) Rev. P. Kelland, on the theory of waves. Dr. T. S. Trail, examination and analysis of berg-meal or mineral flour, from West Bothnia, confines of Swedish Lapland. A. Connell, Esq., further researches on the voltaic decomposition of aqueous and alcoholic solutions. Dr. Samuel M. Brown, on the preparation of paracyanogen in large quantities, and on the isomerism of cyanogen and paracyanogen. John Stark, on the supposed progress of human society from savage to civilized life, as connected with the domestication of animals and the cultivation of Cerealia. Johanne Michelotti de Solariis in Supracretacis Italiœ Stratis repertis, (Tab. II.) Prof. Forbes on the theory and construction of a seismometer, or instrument for measuring earthquake shocks and other concussions, (plate 3). Dr. Samuel M. Brown, experimental researches on the production of silicon from paracyanogen. John Goodsir, on the anatomy of Amphioxus lanceolatus.

14. *First Annual Report on the Geology of the state of New Hampshire; by Dr.* CHARLES T. JACKSON, *state geologist.* 8vo. pamphlet, 164 pp. Concord, N. H. 1841.—The labors of Dr. Jackson in Maine and Rhode Island have, we trust, been familiar to our readers, by our notices in former numbers of this Journal, and we now congratulate him and the state under whose authority the present report is made, upon his appearance in a new field of labor. As it is understood that this report is only prefatory to a more full and elaborate one to be published on the completion of the commission, we shall not attempt its analysis, but content ourselves with mentioning one or two of the most interesting results. Dr. Jackson took especial pains to ascertain,

by repeated simultaneous observations of the best barometers, the true elevation of all the important mountain elevations, which constitute such an important feature in New Hampshire scenery, and also, by proper astronomical observations, to fix their geographical position. Mount La Fayette, the most elevated point in the group, commonly known as the Franconia Notch, was found to be five thousand and sixty seven feet high, and its latitude, N. 44° 8′ 59″.4. Mount Washington, the highest point in New England, was measured with every precaution to insure accuracy, and an interesting account is given in the report of the modes adopted to ensure this end. The result was, that the summit of Mount Washington is N. 44° 16′ 34″.48 ; and the height, as ascertained by a series of barometrical and thermometrical observations, made under the most favorable circumstances, during twelve hours, was six thousand two hundred and twenty six feet above the high-water mark in Portsmouth harbor ; the same, calculated by a series of observations, was six thousand two hundred and twenty eight feet, making but two feet difference in the elevations, and in single observations the difference is but six feet.

The most interesting mineralogical discovery made during this exploration, was that in the town of Jackson, of a vein of OXIDE OF TIN, in a powerful lode of arsenical pyrites. This is the first instance of the occurrence of this metal in America in any tangible quantity, and although the amount of the tin hitherto observed is not great, yet the discovery, in its indications, may be looked on as one of the most valuable metallurgic observations yet made among us.

Dr. Jackson also mentions the occurrence of a mineral at Unity, to which he gives the name of chlorophyllite, and is inclined to consider as new ; it will be seen by reference to page 357 of the present number, that this opinion is there discussed.

The chemical department of the report evinces, as usual, much activity and patience in the laboratory, and when the final report appears, we hope to transfer some of the valuable results to our pages.

15. *Final Report on the Geology of Massachusetts; by* EDWARD HITCHCOCK, LL. D. 2 vols. 4to. pp. 831, 54 plates. Amherst, J. S. & C. Adams; Northampton, S. Butler. 1841.—It is with great pleasure we announce at last the appearance of this work, the most elaborate and laborious treatise on the subject of geology which has yet appeared in America, and the first (except Prof. H. D. Rogers' report on New Jersey) of that series of final reports which are to form the grand summing up of the twenty state surveys now in progress or just completed.

If we reflect that the vast mass of facts and information of various descriptions, and the reasonings and inferences contained in these volumes, are all the result of the untiring—nay, almost Herculean efforts of an individual mind, continued amid the harassments of constant professional duty, during a period of ten years ; we are encouraged to hope that we may yet see the day, when the united efforts of our small army of working geologists now laboring in the common cause, shall reduce the whole of our wide-spread territory to an intelligible and perfect system.

These volumes will come again under our notice, when time and space shall give us an opportunity to review their contents in a way that will accord better with their sterling value, than is now possible.

16. *Fifth Annual Report on the Geology of Pennsylvania ; by* Henry D. Rogers, *state geologist.* Harrisburg, 1841. 8vo. pp. 179.

Report of the progress of the Geological Survey of the state of Virginia for the year 1840 ; *by* William B. Rogers, Professor of Natural Philosophy in the University of Virginia. Richmond, Va. 1841. 8vo. pp. 132.

Reports of Messrs. Beck, Conrad, Mather, Emmons, Vanuxem *and* Hall, *on the progress made in the geological examination of the state of New York, during the year* 1840. (Assembly document, No. 150.) Albany, Feb. 17th, 1841. 8vo. pp. 184.

Annual Report of the Geologist of Maryland, for 1840 ; *by* J. T. Ducatel, *state geologist.* Baltimore, Jan. 1, 1841. 8vo. pp. 59.

Fifth Geological Report made to the Assembly of Tennessee, Nov. 1839 ; *by* G. Troost, M. D. Nashville, Tenn. 1840. 8vo, pp. 75.

The great number of geological laborers now in the field, in this country, acting under the legislative patronage of the various states, causes so great an accumulation of facts, and a publication of results so frequent and voluminous, that it requires no small amount of industry and activity to keep up with the progress of things. Fully to elucidate the views and inferences of the various able men engaged in these researches—to understand all their reasonings and reconcile their discrepancies—would demand a separate journal devoted exclusively to these objects. It is generally understood that the annual reports, of several of the States at least, are intended principally to show the gradual progress of the work, and give evidence to the people under whose authority the commission was instituted, that their public servants are not remiss in the fulfillment of their duty.

With this view, peculiar prominence is given to the economical geology—the strictly utilitarian part, the discovery of valuable deposits of metallic ores, and limestones, and quarries of useful stones, and their chemical constitution, as well as that of the soils and peats. It is to the *final reports* that we are to look for the comprehensive generalizations of theoretical geology, whose sure basis is the firm foundation of facts which so many industrious laborers are garnering up for future use. Of the grand results which time will bring forth from this great storehouse of material, we have even now occasional glimpses and dim shadowings.

For these reasons it is, that less care perhaps has been taken to present to the readers of this Journal the particular details of all the valuable annual reports which have from time to time come into our hands. Of those whose titles stand at the head of these remarks, each has its peculiar interest and is richly worthy of a separate notice. A full review of one of the most important was promised for insertion in the present number, from the hands of a gentleman peculiarly able to do justice to his subject; but it has not as yet reached us. The ingenious and philosophic views of the Messrs. Rogers are well known to their friends, although thus far they have confined themselves almost entirely to the detail of their annual reports, and have reserved till the last the expansion of their final results. The regions of country over which their researches extend, are believed to embrace some of the grandest phenomena of dynamic geology which have ever been brought to light, and on a scale of magnificent extent quite startling to those accustomed to circumscribe their views. In the State of New York, the Silurian system of Murchison has found a counterpart more full in its details than the original, and in geographical extent making the narrow bounds of the Welch system seem almost insignificant.*

17. *A sketch of the Geology of Surrey*—(written for and extracted from Brayley's Topographical History of Surrey,) by Gideon Algernon Mantell, LL. D., F. R. S., &c. &c., author of the Wonders of Geology, Geology of Sussex, &c.—This thin quarto of 50 pages, is illustrated by a colored geological map of the county of Surrey, with five colored sections—also by two beautiful plates on India paper, containing fifty six figures of the fossils so well known to geologists as common to the tertiary and upper secondary of the S. and S. E. of England.

From the ample table of contents, it is evident that the author has examined his subject in great detail, and this work, like all those that have emanated from the same superior mind, is clear, exact, elegant and instructive.

Nihil tetigit quod non ornavit.

We are happy to observe that Dr. Mantell, in his new position near London, (despite of the cares of a laborious and responsible profession,)

---

* See our remarks on geological surveys in Vol. XXXIV, p. 185.

is again embarked in geology, and that his popular lectures to local audiences in Clapham, are rendered subservient both to purposes of science and benevolence.

18. *Popular Lectures on Geology, treated in a very comprehensive manner;* by K. C. Von Leonhard. Translated by Rev. J. G. Morris, D. D., and edited by Prof. F. Hall, LL. D. Baltimore, 1841. Nos. 1, 2, 3, 4. pp. 400, 12mo.—Our opinion of the French translation of this work was fully given in Vol. xxxix, p. 393, of this Journal. The present English translation by the Rev. Dr. Morris, is done up in a very readable and attractive style; while the editorial care of Prof. Hall, has ensured accuracy in technical details, and added numerous notes illustrative of American geological facts, which render the work more acceptable to readers in this country. The beautiful steel engravings of the original are represented in the present edition by wood cuts, which, as well as the typography, are not favorable examples of American art in these matters. The translation of Prof. Leonhard's popular lecture will prove an acceptable present to all who take an interest in the subject, and may safely be recommended as one of the most interesting and instructive works extant.

19. *Notice of the Relation between the Holy Scriptures and some parts of Geological Science;* by John Pye Smith, D. D., F. R. S. and F. G. S., Divinity Tutor in the Protestant Dissenting Chapel at Homerton. Second edition, with many additions. London, Jackson & Walford, 1841. pp. 528, large 12mo.

In the hope of giving a full account of this excellent volume, it has lain on our table, waiting "a more convenient season;" but as that season seems not likely to arrive, we prefer giving a brief notice now to incurring a longer delay. This work is contained in eight lectures, delivered in the Congregational Library, Bloomfield street, Finsbury, as a part of a series of lectures by different gentlemen.

From the skeleton of the subject given in the table of contents, it will be obvious to geologists that Dr. Smith has touched upon the most interesting topics of the science, and it is evident that he has bestowed upon it a diligent, persevering, and intelligent study, until he has made himself master both of its facts and its doctrines. He meets them with the spirit of a man, a philosopher, and a Christian, while he finds nothing in them to impugn his faith as a believer in the Scriptures. From his high character as a religious man, a learned theologian, and a distinguished teacher, he was naturally most anxious to reconcile the facts of geology with the Mosaic history.

Dr. Smith has not, like some theologians, denied, neglected, avoided, or slurred over the facts; he has met them in their full force; he gives entire credit to the researches of geology, both as regards the accuracy and ability of the investigations and the fidelity of the reports. He admits the obligation also which geologists have laid upon the world by their arduous labors and important discoveries.

Did our space admit, it would give us great pleasure to establish the truth of these statements by numerous citations from the work; and we are happy in finding ourselves in almost perfect accordance with the scientific views of the author. He is decidedly convinced not only of the high antiquity of the planet, but also of its fossilized races of plants and animals, and of the perfect consistency of this view with the brief statement contained in the first verse of the first chapter of Genesis, in which it is declared that God created the heavens and the earth *in the beginning*,—as the author believes and we believe, long anterior to the creation of man, whose appearance on the planet is *geologically* a very modern event. While perfectly agreeing with him in this view, it is with pain that we differ from so distinguished and excellent an author with respect to the six days. The scheme presented in Lecture VII, Part II, however it may be thought to be philologically convenient, does not appear to us to meet the geological difficulties, or to be consistent with geological laws. As the writer of this notice has explained his views in the appendix to Bakewell's Geology, (third American from the fifth English edition,) we feel it to be the less necessary to repeat the statements here. We have indeed preferred to avoid a polemical discussion in this Journal, which is mainly devoted to facts.

To us it appears that no explanation of geological phenomena in relation to the days of the creation can be satisfactory, unless it allows the requisite time for the events, as occurring in their proper order, consistently with the laws of mineral dynamics and of organic life; no imagined new arrangement, either local or general, in a period of a few common days, disposing anew of ancient deposits, the products of long gone by ages, can answer the purpose.

In every other view we entirely approve of this most excellent work, and we earnestly recommend it to the faithful study of those religious persons who are anxious to know the truth.

Dr. Smith has rendered a signal service to science and to religion by meeting this great subject in the fulness and richness of its evidence—in the splendor and deep interest of its discoveries. With admirable candor and perfect comprehension of his materials, he has disposed of them with masterly skill.

This work will, if we mistake not, do more than any other has done, to disabuse the religious world of their unreasonable fears of geology—

to place it side by side with astronomy, the only physical science which excels it in grandeur—and to prove that only dark and limited views can ever make us fear the development of truth in every department of the works of the infinite Creator.

20. *Astronomy for Schools; upon the basis of M. Arago's Lectures at the Royal Observatory of Paris, and in which the leading truths of that science are clearly illustrated without Mathematical Demonstrations; with numerous engravings and an appendix;* by R. W. Haskins, A. M. New York, 1841. pp. 324, 12mo.

Mr. Haskins has been for some time favorably known to the readers of this Journal by various papers which have appeared under his signature on his favorite science. The object of the present work is fully explained in the above copious title, and we may say that the mode in which the subject is treated is well calculated to secure the end in view. The well known patient accuracy of the author is a sufficient warrant for the fidelity with which the task has been performed. How far it is possible to obtain a full understanding of astronomical truths without the aid of mathematical demonstrations, we are not prepared to say; that a very useful and interesting treatise may be thus composed, is sufficiently proved by the character of the present volume. The authorities used in composing this abstract, are ample and of the greatest weight; and the use made of them is quite as extensive as the limits of the undertaking will allow.

21. *Pantology, or a Systematic Survey of Human Knowledge; proposing a classification of all its branches and illustrating their history, relations, uses, and objects; with a Synopsis of their leading facts and principles; and a select catalogue of books on all subjects suitable for a cabinet Library; the whole designed as a guide to study for advanced students, in colleges, academies, and schools; and as a popular directory in literature, science and the arts.* By Roswell Park, A. M., Prof. Nat. Philos. and Chem. in the Univ. of Penn. Philadelphia, Hogan & Thompson, 1841. pp. 587, 8vo.

It is far from our purpose to enter on any analysis of the present work, albeit it covers with its ample title the field of human knowledge to the illustration of many important parts of which our pages are devoted. We cannot better set forth the author's classification than by transferring to our pages the order of growth of his tree of knowledge, which forms the frontispiece of the work. This plant is represented as springing from the soil of Noology and Cosmology; its trunk divides into two principal stems, Physiconomy the left, and Psychonomy the right; from the latter proceed various branches—Glossology, which ramifies into General Grammar, Oriental Languages, European Languages, Barbarous Languages; Psychology, under which are Rhetoric, Logic, Phrenics, Ethics, Education; Nomology, Political Philosophy, International Law, Constitutional Law, Municipal Law, Political Economy; Theology, Paganism, Mohammedanism, Judaism, Christianity; Mathematics, Arithmetic, Algebra, Geometry, Ancyclometry, Rhetometry; Acrophysics, Mechanics, Astronomy, Optics, Ceraunics, Chemistry; Idiophysics, Zoology, Botany, Mineralogy, Geology; Androphysics, Andronomy, Pharmacology, Thereology, Chirurgery. From the same main stem of Psychonomy, after this branch sets off, a prolongation extends itself to Ethnology, which again branches forth into Geography, Asiatic, European, African, North American, South American, Oceanic; Chronography, Euclassic, Oriental, European, American; Biography, Euclassic, Oriental, European, American. So much for one of the two principal stems into which this tree is divided. The other stem, (Physiconomy,) after lengthening out into Technology, gives forth Architechnics, viz. Hylurgy, Machinery, Architecture, Viatecture, Navitecture, Navigation; Chreotechnics, Agriculture, Horticulture, Domiculture, Vesticulture, Furniture, Commerce; Machetechnics, Hoplistics, Fortification, Geotactics, Strategy, Navitactics; Callotechnics, Printing, Painting, Sculpture, Music, Argics.

From the wide range of the author's subject, (what can be more comprehensive than the title *Pantology*?) it is not to be expected that much valuable information could be conveyed on any particular subject; but all are treated much in the same way, and to many readers no doubt it will be of service in pointing to the sources of knowledge, whether the author's peculiar classification is adopted or not.

22. *Notes on Gun powder, Percussion powder, Cannon and Projectiles, by* Lt. Miner Knowlton, instructor of Artillery in the United States Military Academy. pp. 73, 4to. Lithography of George Aspinwall, 1839.—This treatise was compiled with a view to the convenience of the class at West Point Military Academy. The subject is treated in a lucid, condensed, and very accurate way, and abundantly illustrated by drawings from the pencil of the author. It is remarkable for the thoroughness with which all the arts and manufactures, the materials and their preparation, and the tools and instruments required for these arts, are explained and rendered easy of comprehension by the pupil, so that the most untechnical cannot fail to obtain correct notions of the whole subject. The work is written solely for the use of the Military Academy, and to facilitate the ends of instruction in that institution.

23. *Meteorological Register for the years* 1826–1830 *inclusive; from observations made by the Surgeons of the army and others at the*

Military Posts of the United States. Prepared under the direction of Thomas Lawson, M. D., Surg. Gen. U. S. Army. To which is appended, the Meteorological Register for the years 1822, 1823, 1824, *and* 1825: *compiled under the direction of Joseph Lovell, M. D., late Surgeon General of the U. S. Army. Published for the use of the Medical Officers of the Army.* Philad. 1840. 8vo. pp. 161.

This valuable work contains observations on the thermometer, face of the sky, direction of the wind and the rain, at places ranging from N. lat. 27° 57′ to 46° 39′, and from W. long. 67° to 96°. It is introduced by judicious observations concerning the comparative temperature and climate of the different sections of the country, and is accompanied by a map of the U. S. indicating the positions of the various posts of observation. The collection will be of much interest to meteorologists, but its value would have been much greater had it comprised observations of the barometer.

Art. XIV.—*Proceedings of Learned Societies.*

British Association *for the Advancement of Science.*

The *eleventh* annual meeting of this body, was convened at Plymouth, on Thursday, the 27th of July. The meeting seems to have been one of great value to science. The proceedings of the various sections, as reported in the London Athenæum, are very full and interesting, and as far as published on the 28th of August, had occupied over one hundred and forty columns of that closely printed journal. We shall in our next number endeavor to give our usual condensed abstract, which has been, as far as we are informed, the only American channel by which a knowledge of the doings of this important body has come to men of science in this country.

We cannot resist the temptation to exceed the limits of our number, for the purpose of laying before our readers the following extract from the address delivered by the President, Prof. Whewell, at the opening of the meeting.

This address begins by an allusion to an imaginary Philosophical College, and that classic fable of which the great Bacon gives so remarkable a picture—*the New Atlantis.* The imaginary teacher whom he introduces as one of the sages of this Utopian region, describes to the inquiring traveller an institution which he calls *Solomon's House.* Of this institution he says, "The end of our foundation is the knowledge of causes and secret motions of things, and the enlarging the bounds of the human empire to effecting of things possible." A strikingly beautiful parallel is then drawn by the speaker between the British Association and the plan of such an institution, which should have for its business not to teach mainly, but to make discoveries—to extend our knowledge of every part of nature by all the appliances which experiment and theory, observation and calculation, ingenuity and perseverance can supply; and in addition to these, by more material resources, money and a multitude of fellow laborers. He then proceeds:—

"The British Association has now for ten years discharged the office of such an institution as we have spoken of. Considerable funds, raised by the contributions of its members, and expended under its direction, have been employed in furthering and verifying discoveries. It is true that we have not attempted to erect such edifices, and to make such preparations for the purposes of experiment, as Bacon introduces into his picture: but we have attained the same end more effectually, by procuring the use of many of the great establishments of manufacture and commerce which this empire possesses. We have had experiments carried on at furnaces and iron-works, on railroads and canals, in mines and harbors, with steam engines and steam vessels, upon a scale which no institution, however great, could hope to reach; but which has been placed in our power by the enlightened liberality and scientific zeal of the proprietors and directors of such means of research. We have not had various bodies of professors of the art of discovery employed in these inquiries—we have not attempted to form classes of mystery men and dowry men—collectors of facts and interpreters of nature; but we have found the most gifted and eminent cultivators of science in our own country, and several of those of other countries, ready and willing to undertake for us the office of exploring and interpreting nature—of extending and applying art. No institution, however formed, could have hoped to collect, as its active members, such a body of philosophers as have gladly come forward to labor for us, and have freely given us the resources of their vast powers and matured skill. Mathematicians, and astronomers, and geologists, and chemists, and naturalists, illustrious through Europe, have superintended the execution of our commissions with as much care as their own most favorite researches; and we have seen a co-operation of experimenters and calculators, observers and generalizers, such as might satisfy the wishes of Bacon himself.

"That I may not dwell on mere generalities, I will mention a few of the sums expended by the Association upon scientific researches; which, when it is understood that they have been spent under the direction and vigilant control of such men as I have spoken of, will show the amount of service which has been rendered to science by that body. In the first three years, the sums thus expended were small, the Association having been mainly employed in collecting information which might direct its future proceedings. In the fourth year 167*l.* was thus spent, and from this time the sum went on rapidly increasing. In the fifth year it was nearly 500*l.*; in the sixth and seventh nearly 1,000*l.* each year; in the eighth and ninth above 1,500*l.* each year; and it appears that during the past year we have expended in this manner, the sum of 1,240*l.* And these sums, it is to be observed, are only a part of what were voted; at Liverpool, in 1837, above 3,000*l.* was voted, of which 1,000*l.* only was applied for; at Newcastle 3,700*l.* was voted, and 1,600*l.* of this only paid; at Birmingham 2,800*l.* was voted, and 1,500*l.* paid; the sum voted at Glasgow last year, was 2,600*l.*, of which, as I have said, your treasurer has really paid 1,240*l.*

"These differences of the sums voted and paid in each year are evidence of the care with which the resources of the Association are husbanded; for the sums voted were to be had on application made by the persons to whom their disposal was intrusted; but they were not applied for, except in proportion to the scientific work which was done; and those who undertook these labors for us carefully confined their expenditure within the narrowest possible limits. It would occupy you too long if I were to mention in detail the subjects to which these sums have been applied; but I may state in general, that above 900*l.* has been expended by us in the furtherance of astronomy, mainly upon the object of reducing observations already made, into such a form that they can be directly compared with the theory. Above 800*l.* has been expended on tide observations; 250*l.* on experiments on waves; 500*l.* on experiments on the best form of vessels; 200*l.* on experiments on cast iron; about 400*l.* has been employed in various labors relative to meteorology; and above 300*l.* on the description of fossil fishes and reptiles. I shall not detain you by mentioning smaller sums which have been devoted to various objects: but I may call to your notice a work executed mainly in this country, upon which the Association expended about 550*l.* in 1838 and 1839. This work consisted in striking a level line from the north coast of Somersetshire to Exmouth, in order to determine whether the level of the sea is the same in the Bristol Channel and in the British Channel, and in order to afford a standard of reference in future times, if, from any cause the relative level of the land and the sea should change. This operation has already afforded us the means of determining, that the great land slip, which has recently taken place near Axmouth, was not accompanied by any permanent change in the level of the land itself, where a block of granite lies, which marks one of the extremities of our level line.

"Since the first institution of the Association, about 7,000*l.* has been expended on such objects as I have pointed out: but it is impossible for any one, who knows the nature of scientific researches, and the difference between the result of money expended in experiments by a good and a bad philosopher, to doubt that this sum has produced effects which many times the sum applied without the same advantages could not have obtained. Without the encouragement of the Association, these researches would never have been undertaken: without the aid of such men as have frequented the meetings of the Association, they would have been attempted to no purpose. It has been said of certain parts of Europe that they afford—

Iron and men, the soldier and the sword;

in like manner we may say of this Association, that it has supplied at the same time the philosophical soldier and the weapons with which he gains his victories over nature.

"But further, besides the expenditure of its own funds, the Association has been the means of procuring the appropriation of very large sums to scientific purposes from the national resources. At the suggestion or request of their body, the reduction of the observations of the planets made at Greenwich from the time of Bradley has been completed; and the reduction of the observations of the moon has been begun. Up to the present time, about 2,200*l*. has been expended in all. And by a letter from the Astronomer Royal, received since I came here, I am informed, that within a few weeks the Government expressed great willingness to advance more money for this purpose; and Mr. Airy adds, that next Monday he is to have twelve calculators employed upon the work. We have applied to the Government for the extension of the ordnance survey into Scotland, and have received a favorable answer. We have tendered our advice that the ordnance survey of England shall in future be conducted on a scale of six inches to a mile instead of two inches, and this advice is already acted on in the northern counties of England, where the survey is now proceeding.

"Above all, I must mention an undertaking, entered upon in pursuance of our repeated recommendations (a service which the philosophers of future ages will duly estimate),—the great Magnetical Survey of the terrestrial globe, by the combined operation of a naval expedition and fixed observatories in every quarter of the world, which is now carrying into effect;—a scientific work, this—far surpassing in the scale of its means, and in the completeness of its design, any ever yet attempted, and such as Bacon might have assigned to the sages of his New Atlantis, if he had, in imagination, extended their polity from the Atlantic to the Pacific, and from pole to pole.

"We most gladly bear our testimony to the liberality and spirit with which Her Majesty's Government have accepted and acted upon our suggestions; nor is this testimony at all weakened by our claiming for distinguished members of our own, the merit of having brought into view the importance of such an undertaking, laid before the English public the progress which the subject was making in other countries, planned the scheme of operations which our own exertions ought to follow, and animated the observers, by giving them the certainty that their observations will be well used and fully appreciated.

"When we can point to these numerous and valuable direct results of our exertions, we cannot at all waver in our conviction that those persons acted in the truest spirit of the age, and of the nation, who, eleven years ago, framed the design of a voluntary association for the advancement of science among the subjects of this empire: and that the hopes and expectations which such an institution might naturally exercise, have been fully verified by the course and progress, the labors and successes of the British Association.

"I do not doubt that the present Meeting will continue to uphold the character of the Association, and will be inferior to none of the preceding in the value and interest of its proceedings. We are not yet likely to want for matter to labor upon. The collection of facts and the reduction of them by various calculations is still required to a vast extent, in order that our knowledge may make the next step of progress to which its path invites our hopes.

"It is easy to point out vast fields of research, on which our resources and our energies may be applied with every prospect of a rapid increase of knowledge. For, in fact, how little has been done for science, by the collection of exact and long-continued series of observations, such as he must have before him who is to interpret nature. In astronomy, indeed, this has been done: sovereigns, and nations, and opulent individuals have thought their wealth well bestowed in providing costly instruments, and rewarding the astronomer through his daily and nightly toils. The stars have been well observed from the beginning of civilization; but, for the purposes of science, we ought to have observations as careful and as continued of all the other parts of nature as we have of the stars. The tides, the waves, the winds, and all the other changes of the air, pressure, temperature, moisture, magnetism, electricity, chemical changes, and even those of vegetable and animal life,—all these afford materials for researches full of importance and interest. For these, the time is, perhaps, not yet come, when they can be urged upon governments as a part of their business, in the same way in which astronomy is; except perhaps magnetism, which has already taken its place in our observatories by the side of astronomy, in our own and other countries. Those other subjects, then, are fitly cultivated by a voluntary association such as ours; and the occasions of fitly doing this will doubtless be suggested to us from time to time by our members. On the present occasion, a distinguished Belgian philosopher, one of our corresponding members, (M. Quetelet,) comes to us to invite us to take a part in determining, by extensive observations, the changes which atmospheric conditions produce in periodical phenomena,—such as the times of the leafing and flowering of plants, of the arrival of birds, and the like. He has obtained extensive co-operation in his own country, and no doubt will find fellow-laborers in ours. Meteorology, in its largest sense, is a subject, which, although great collections of observations have been made, is hardly yet a science: yet the interpreters of this part of the book of nature have already begun to spell out some phrases, which show that the language is not wholly unintelligible; and here, therefore, we may go on hopefully, recollecting always that the collection of facts is a matter of comparatively small value, except we can also trace in them some rule or order. The mere gathering of raw facts may be compared to the gathering of the cotton from the tree. The separate filaments must be drawn into a connected thread, and the threads woven into an ample web, before it can form the drapery of science.

"We ought to have meteorological observations and observers distributed over the face of the globe: and even this would not be enough; for we wish to know not only what passes on the earth's surface, but through the whole depth of the atmosphere; hence it would be desirable to have observations made at elevated points free from the action of the ground; such as can be attained only by the aid of balloons. Such an undertaking has been under consideration of a committee during the past year, and a report on the subject has come before the Physical Section. I trust that on this subject you will *soon* hear more. As other subjects on which we still want facts—that is, numerous and systematical collections of facts, and laws deduced from facts—I may mention the tides of the Pacific, the velocity of sea waves, and subterraneous temperature. Another class of inquiries well fitted for our labors, is the determination of the fundamental elements, or *constants*, of operations of engineering, as the constants of railroads, steam-engines, and other works of art, which form part of the wealth and resources of this great empire. These are already under investigation. The addition of a Section of Practical Mechanics and Engineering to the previous constitution of the Association, which took place at Bristol, showed the interest which such inquiries inspire; and various committees have collected much valuable information of this kind, and will, we trust, collect much more.

"There is also another Section of the Association, added to its plan at Cambridge, which has for its object researches of a highly interesting kind,—I mean the Section of Statistics; and we trust that there is ample employment for this Section, in subjects which can be dealt with in the same calm speculative spirit as the other sciences which we here cultivate.

"It may, perhaps, sometimes be useful to us to recollect that in many statistical subjects, the discussion and even the collection of facts is rather the office of a legislative than of a scientific body. The wise institutions of Bacon's New Atlantis would have assigned to the governors of the land, and not to the sages of Solomon's House, the collection of information respecting the habits, numbers, and education of the people; where the information is such as almost necessarily suggests legislation, or discussions having legislation for their natural end, and involving the deepest political and moral considerations. There may very fitly be voluntary associations, which aim directly at improving the intellectual, or moral, or social condition of our population; but we must ever remember that we are an association for a different purpose, namely, the advancement of science; and we are bound alike by our regard to the prosperity of our body, and by our most solemn and repeated declarations, to avoid the storm of opinions which is always raised when the parties which aim at social permanence and social progress are brought into conflict. The pursuit of scientific truth is, no doubt, a means of *indirectly* elevating man's intellectual and social condition; but we assemble in order to promote the *direct* pursuit of scientific truth; and we must not turn aside into the more wide and tangled paths of those who make its collateral effects their main object. Knowledge is power, we are told. Knowledge *is* power; but for us, it is to be dealt with as the power of interpreting nature and using her forces; not as the power of exciting the feelings of mankind, and providing remedies for social evils, on matters where the wisest men have doubted and differed.

"Being the person whose voice is first raised in addressing the meeting of the Association, I have thought that it was a part of my duty to use the opportunity in calling to our minds the fundamental character and principles of our Institution. There are other subjects which our constitution directs us to avoid; but none perhaps in which there is much danger or need of warning. We are in no great risk of deviating into literary, or metaphysical, or theological discussions. Some metaphysics and literary culture will of course show themselves in the addresses of those who possess such accomplishments, but are no direct objects of our attention. And in like manner, although we cannot dream of the slightest approach to the discussion of religious questions, heartfelt and real piety may be apparent even in the sentiments uttered at an association for the advancement of science. I am sure that many of you who attended the former meetings of this Association, must recollect occasions on which men's minds being excited, and yet solemnized, by the aspect of the assembled multitudes, and by the lofty views of nature which our philosophers had to present to them, the thoughtful and eloquent men who had to address you were carried by a spontaneous impulse, without plan or premeditation, into elevated strains of religious reflection; showing that those who take the lead in our meetings have their minds so tuned, that every voice which proclaims the wonders of nature, turns their thoughts to the Author of nature: that every new gleam of truth seems to them an effluence from the eternal fountain of truth. Long may such habits of thought prevail among the philosophers of this land; and then we need not fear but that knowledge, hallowed and elevated by the spirit in which it is pursued, will be every way a blessing to man,—to his soul as well as to his body—to his spiritual as well as to his intellectual being.

"To those of us who, knowing the institution by our attendance upon it, and our share in its labors, think thus of its value and its spirit, every new annual occasion of our coming together, must be an occasion of fresh gratification, an agreeable exercise of memory and of hope. In our present meeting at this place, there are many circumstances to give additional animation to our anticipations of pleasure. We come to a part of the empire hitherto unvisited by many of us, to a great maritime town, replete with objects of amusement, art, and interest. We know the love of science and the familiarity with its treasures which here prevail, for we are acquainted with the high character, the knowledge, zeal, and ability of the authorities of the Dockyard—the intelligence and activity of the Plymouth Institution;—we know and feel most gratefully, the kind and vigilant care with which preparations have been made for our reception; and we now see in this assembly, the look of cordial welcome and lively anticipation, of which I would say more, but that I would beg to leave the subject in abler hands. We hail with joy and confidence, the opening of the Plymouth Meeting of the British Association.

"Perhaps you will allow me the gratification of saying a word, respecting special personal reasons of my own, which make it a matter of pleasure to me to find myself here on this occasion. Besides that it brings me to the society of several valued and cherished friends, whose home is in this part of England, I have various ties of a scientific nature with this place and this region. The excellent observations of the tides made in this harbor, have been the subject of calculations involving considerable labors, which I have made or directed; and some curious traits in the laws of tidal phenomena here, which were noticed as early as the time of Newton, have, I trust, been followed out to a tolerably exact determination. An anemometer, which I had devised, has been erected here, with most valuable improvements, by Mr. Snow Harris, and has been for some time in operation. And when I consider, as we may do, this meeting as a meeting peculiarly intended to bring the Association in contact with the west of England, I find that Cornwall returns to my thoughts, with all the scientific zeal and intelligence, which from my own personal intercourse I know to exist among the miners of that county. Perhaps I have had very unusual opportunities of becoming acquainted with their merits, for in two different years (1826 and 1828) in the prosecution of certain subterraneous experiments, undertaken in conjunction with the present Astronomer Royal and other persons, I lived four months the life of a laboring miner, and learnt how admirable for skill and conduct is the character of all classes of the mining population in that region. If any of my Cornish friends are within hearing, I gladly bid them God speed, and claim once more their welcome to the west. And that I may no longer detain you, to all of you, gentlemen of the British Association, I bid God speed; and from all of you, gentlemen of Plymouth and its neighborhood, I seem to hear, Welcome to Plymouth!"

MISCELLANIES.

1. *Observations on the Shooting Stars of August* 9 *and* 10, 1841.—From the 8th to the 13th of August, 1841, the sky at this place was unfortunately too much overcast to permit any meteoric observations. The following statements, although not so full as could be wished, show a decided recurrence this year of the meteoric sprinkle which has so frequently been noticed about the 10th of August. It will be remarked, that the moon was in her last quarter on the 10th of the month.

1. *Pensacola*, *Flor.* N. lat. 30° 28′; W. long. 87° 12′. Dr. Joshua Huntington, U. S. N., has communicated to me his observations at this season, from which the following is an extract. "On the night of the 9th August, I kept watch for any unusual display of shooting stars. My field of vision included about a sixth part of the hemisphere, from S. E. to S. W., to an elevation of 65°, but was partly obscured by a bank of cloud. I took my station at midnight, and between that hour and one o'clock, saw *fourteen* of these meteors; and between one and two o'clock, *twenty three.* Most of them were small, and only five or six left luminous trains. They generally described very short arcs, and with a single exception, had a course towards the S. W. My position did not enable me to determine the radiating point, which must have been somewhere in the N. E. At two A. M., I watched in the north for half an hour, and saw two shooting stars only. Towards three A. M., I

watched again in the S. for twelve or fifteen minutes, and counted *ten* more, all of which had the same course as those before seen."

2. *Cincinnati*, *O.* N. lat. 39° 6′; W. long. 84° 27′. *Dr. John Locke* has published the following notice in the *Daily Gazette* of Cincinnati, Aug. 14, 1841. "*Meteors.*—Mr. Editor. Sears C. Walker, Esq., the astronomer of Philadelphia, has awarded to me the credit of having discovered in 1834, the radiant point of the meteors which appear annually on or near the 10th of August. I had also discovered in 1835, their periodical return. They have since been noticed by the philosophers of Europe. On the night of August 9, 1841, I observed the tracks of several, with an altitude and azimuth instrument, made for the purpose by Mr. James Foster, and found them to emanate from the same point, the constellation Perseus, and to converge to the same point, the constellation Lupus, as those of 1834. On the night of the 10th, observing from the top of the Bazaar, from nine to ten o'clock, myself and assistant counted sixty meteors, forty nine of which converged S. W. towards Lupus, and were mostly brilliant, rocket-like, and left a phosphorescent track. The remaining twenty one moved in a variety of directions, were small, and had a short track not phosphorescent. The forty nine parallel meteors had courses which mostly, if prolonged, would fall between α and β of Perseus, and in the opposite point in Lupus, towards which last point they all proceeded. Two only had a considerable deviation, one tending to a point about 20° E. of Lupus, and the other about 14° W. of the same. As myself and assistant could see only about one half of the visible heavens at once, the meteors may be reckoned at sixty per hour. The night was clear, a little hazy in the horizon; wind N. W., with a slight aurora in the north."

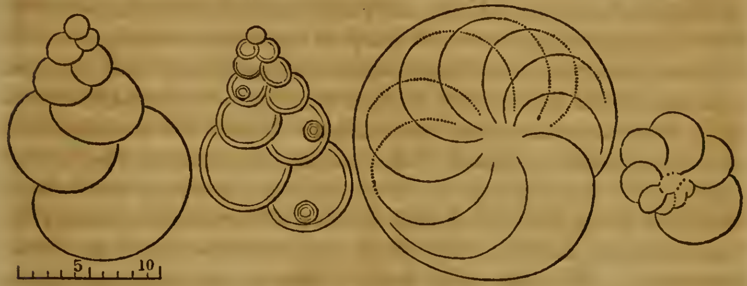
Had these observations been continued until 4 A. M., the meteors would undoubtedly have been found much more frequent. Between three and four A. M., they were probably five or six times as numerous as between ten and eleven of the evening previous. (See observations of Aug. 9, 1840; this Jour., Vol. xl, p. 329.) This being assumed, it results that on this occasion shooting stars were as abundant as on the preceding anniversary, or at least *six times* beyond the yearly average.

New Haven, Conn., Aug. 25, 1841. E. C. Herrick.

2. *American Polythalmia from the Upper Mississippi, and also from the cretaceous formation on the Upper Missouri.*—Prof. Bailey, whose microscopical observations on American infusoria are so well known to all our readers, at home and abroad, has recently made an examination of a light cream-colored marl from a mission station on the Upper Mississippi, called there "*prairie chalk.*" In a letter to the

junior editor, of Sept. 6th, Prof. Bailey remarks, "I have given to the '*prairie chalk*' which you sent me, from the Upper Mississippi, a somewhat hasty examination; but I found it, as I ventured to predict, full of the 'elegantly little.' It is indeed richer in the beautiful forms of the polythalmia than any American specimen I have yet seen, and many of the forms are entirely different from those of New Jersey, Alabama, and Upper Missouri. The following are rude outlines of some of the most common forms, sketched hastily with camera lucida and microscope.

Fig. 1. Fig. 2. Fig. 3. Fig. 4.



[The scale represents $\frac{10}{100}$ millimetre magnified equally with the figures.]

"In some of the cells are spots, (see fig. 2.) I am ignorant of their nature, whether ova or animalcules I cannot decide. I cannot make out the nature of the large brown fragments in the same specimen, nor of the fragment sent in the little box, and which you compared to vertebræ of fish with the spines broken off—they are all evidently of animal origin."

Prof. Bailey has also found very interesting forms of polythalmia in the specimens brought by Mr. J. N. Nicollet from the 'far west;' it will be remembered that in our last number, we published under the proceedings of the associated geologists at Philadelphia, an interesting narrative of this gentleman, giving an account of his important observations in that region. We had hoped to insert in the present number, some more extended notice of these matters, showing in what an interesting and unexpected manner the observations of Prof. Bailey on the limestones of Alabama, and the localities above named, had connected themselves with similar observations by Prof. Ehrenberg.

Mr. Thos. Weaver has published (L. E. and D. Phil. Mag. for May, June, 1841,) a paper on the organic bodies composing the chalk and chalk marl, as drawn from observations of Dr. Ehrenberg, with some notice of the researches of M. D'Orbigny; a condensed abstract of this interesting memoir was prepared for our present number, but the crowded state of our pages requires its postponement.

3. *Prof.* Geo. Forchhammer, *of Copenhagen, on new substances from the peat mosses.*—We have received a letter from Prof. Forchhammer, dated Oct. 6th, 1840, stating that he was about to send a paper for this Journal on some new compounds, which he has discovered in the peat mosses of Denmark. "They are *Tekoretin* $= C^5 H^9$; *Phylloretin* $= C^5 H^6$; *Zyloretin* $= C^{40} H^{66} O^4$; and *Baloretin* $= C^{40} H^{64} + H^{10}O^5$. The three first substances crystallize in beautiful white crystals; the last is a pulverulent body. It is partly want of time at this moment, partly because I have still some analyses to make on the compounds of Zyloretin with chlorine, which prevent me from sending the paper at present; but I hope soon to be able to send it."

Geology of Denmark.—"I have just returned," continues Prof. F. "from a tour through the greatest part of Denmark, principally upon an enquiry into the nature of our bowlder formations. I have been able to trace three periods in this vast formation. 1. The oldest contains fossils agreeing with the sub-Appenine formation of Dartona and several other places in upper Italy. 2. Contains no fossils, is very seldom stratified, but consists of a yellow loam, and blue and yellow marls, with numerous bowlders of enormous size. 3. Contains the fossils of our present sea, in stratified gravel and bowlders, very often hardly any thing but bowlders.

"The theory of the bowlders being carried by ice, either in form of glaciers or ice islands, does not appear to me to agree with our formation. I am preparing a work on the geology of Denmark which will soon appear in German, which I shall have the pleasure of sending you, since I hardly dare hope that the Danish will be understood."

4. *Gilding by Electrography.*—Mr. Dent, of London, whose name is well known as a skillful artist in chronometers, as well as by various articles in this and other scientific journals on his own subject, has recently sent us by the hand of Mr. Blunt, a very delicate steel chronometer hair-spring, about three fourths of an inch in length and one quarter of inch in spiral diameter, *perfectly gilt* with pure gold, by the electrographic process. Perfect protection from rust is thus secured, and probably some advantage is also gained in the relative changes of electrical condition thus obtained, although Mr. Dent remarked that some portion of the elasticity was lost. The same process has also been applied to the protection of magnetic needles.

Mr. Joseph Saxton of the United States' mint at Philadelphia, has made much use of the electrographic art in the prosecution of his duties at the mint. He last spring presented us with a medallion of Flora and Pomona in copper, which he had gilded most perfectly, by simple immersion for a few moments in a very weak neutral solution of the chloride of gold. The deposite was perfectly uniform, and took place without any other electrical agency, than such as was excited by immersing the copper in the neutral chloride.

5. *Shooting Stars of December* 7, 1838.—At a session of the Royal Academy of Brussels, Feb. 6, 1841, M. Quetelet communicated the following extract of a letter from M. A. Bravais, officer of the French marine, and engaged in the French scientific expedition to the north. "The meteoric shower of December 7, 1838, of which Mr. Herrick speaks, (report of session of Oct. 17, 1840,*) was noticed by us at Bossekop. I am not able to state the whole number of meteors seen at that time, but I well remember that nearly all diverged (apparently) from the same point of the heavens, situated between the constellations of Perseus, Cepheus and Andromeda, about seven o'clock in the evening. This point could be determined with great exactness, as we noted the places of origin and extinction of each meteor. We have then a phenomenon seen at the same time in New Haven, China, England, North Cape, and in France, (Toulon, by M. Flaugergues,) that is, over nearly all the northern hemisphere; which argues a great extent in the meteoric cloud."—*L'Institut*, No. 385, May 13, 1841.

6. *Scientific Visit of Charles Lyell, Esq. of London, to the United States.*—This eminent geologist and gifted writer, arrived at Boston early in August with his lady. He will, by invitation, deliver a course of lectures on geology in the Lowell Institute, beginning October 19, and continuing through six weeks. We doubt not that this effort, sustained, as we understand it will be, by large and splendid drawings, will be worthy of the distinguished reputation of Mr. Lyell, and of the interest and dignity of the science. We are gratified to learn that he will spend a year in this country, occupied in viewing it *geologically*. He has already visited the States of Massachusetts, Connecticut, New York and Pennsylvania, and after the conclusion of his course of lectures, he will proceed south for the winter, to return to the north in the spring. We are happy to learn, that Mr. Lyell's impressions of the geological interest of this country, and of the zeal, science, and acumen of its geologists, are such as will make it a pleasure to the gentlemen engaged in these pursuits in our different states and territories, to facilitate his researches, which we cannot doubt will advance the common cause; while we feel assured that a social intercourse with the accomplished strangers who are now beginning to visit us in increasing numbers, will promote every good national and personal feeling.

7. *Shower of red matter like blood and muscle.*—We are indebted to Prof. Troost, of Nashville, Tennessee, for an interesting notice of a

* See this Journal, Vol. xxxv, p. 361, and Vol. xxxvi, p. 355.

remarkable event. It appears from communications made to that gentleman, that on Friday, August 17, between one and two o'clock, P. M., the negroes of Mr. Chandler, near Lebanon, Wilson County, Tennessee, came in and reported that it had been raining blood in the tobacco field where they had been at work; that near noon there was a rattling noise like rain or hail, and drops of blood, as they supposed, which fell from a red cloud which was flying over. Intelligent men visited the ground, and observed drops apparently of blood on the upper surface of the tobacco leaves, and portions of flesh and fat—one piece one and a half inches long, emitting a very offensive smell over the field.

The drops evidently fell perpendicularly over a space from forty to sixty yards broad, and six or eight hundred yards long. Some particles appeared to have been clear blood uncombined with any thing else; others, blood united with muscular fibre and fat. Dr. Troost, after visiting the place, is decidedly of the opinion that it was animal matter, but he thinks not blood; although he distinctly distinguished muscular fibres, on maceration of the matter in water, which separated longitudinally, as in the case of dried beef; they were of a reddish brown color. The pieces supposed to be blood were brown and resembled glue. There was a distinct smell of animal matter in a state of putrefaction.

Both the muscular part and that which had been called blood, were heated in a glass tube, and were similarly affected as beef would have been in the same circumstances; there was a movement in the mass, a brown fluid rose, and a black animal charcoal remained. Dr. Troost concluded, that without doubt this is animal matter, and belongs to our globe. He cites many instances of red rain, red dust, red sand, red snow, showers of blood, so called, &c. in various centuries from 472 of our era to 1814, and gives the authorities. There is now no room to relate or discuss these statements, and it remains only to give the conclusion of Dr. Troost.

After alluding to the well known power of wind to raise materials high into the atmosphere and to transport them to the distance of many miles, (and even in some cases, as in volcanic eruptions, hundreds of miles,) he observes: "Such a wind might have taken up part of an animal which was in a state of decomposition, and have brought it in contact with an electric cloud, in which it was kept in a state of partial fluidity or viscosity. In this case, the cloud which was seen by the negroes, as well as the state in which the materials were, is accounted for."

Dr. Troost gives many cases of transported seeds, pollen, and similar things—which have been taken for showers of sulphur. When we remember that even fishes have fallen in showers, we cannot doubt that whirlwinds may elevate and transport parts of animals and deposit them in distant places.

INDEX TO VOLUME XLI.

M.

N.

O.

P.

Q.

R.

S.

T.

V.

W.

Z.

ACKNOWLEDGMENTS TO CORRESPONDENTS, FRIENDS AND STRANGERS.

Remarks.—This method of acknowledgment has been adopted, because it is not always practicable to write letters, where they might be reasonably expected; and still more difficult is it to prepare and insert in this Journal, notices of all the books, pamphlets, &c., which are kindly presented, even in cases, where such notices, critical or commendatory, would be appropriate; for it is often equally impossible to command the time requisite to frame them, or even to read the works; still, judicious remarks, from other hands, would usually find both acceptance and insertion.

SCIENCE.—FOREIGN.

Arsberättelse om Framstegen i Fysik och Kemi, afgifven den 31 Mars, 1838, af Jac. Berzelius, K. V. Acad. Sec. Stockholm, 1838. 8vo.

Arsberättelse om Botaniska Arbeten och Upptäckter för är 1837, afgiven den 31 Mars, 1838, af Joh. Em. Wikstrom. Stockholm, 1839. 8vo.

Arsberättelse om Technologiens framsteg, afgiven den 31 Mars, 1838, af G. E. Pasch. Stockholm, 1839. 8vo.

Kongl. Vetenskaps-Academiens Handlingar för ar 1837 and 1838. Stockholm, 1839. 8vo. These four last from M. Berzelius.

Tal om Juridisk Statistik och gneriderne för Lagstiftrimgen, Hallet uti Kongl. Vetenskaps-Academiens, den 8 April, 1840, af Grefve M. Rosenblad. Stockholm, 1840.

Tal om R. Seraphimer Ordeno Lazarettet i Stockholm, Hallet, 1 Kongl. Vetenskaps-Academiens vid Præsidii Nedlaggande den 7 April, 1838, af Dr. C. J. Ekstromer. Stockholm, 1849.

Three Lectures on Agriculture, delivered at Oxford in 1840 and 1841; by Charles Daubeny, M. D., F. R. S., &c. Oxford, 1841. 8vo. From the Author.

Proceedings of the Royal Irish Academy, No. 26. From Prof. Bache.

On the Geological Structure of the northern and central regions of Russia in Europe; by Roderick I. Murchison, F. R. S. and E. De Verneuil. London. From the Author. Three copies.

Notice of two Geological Models and Sections forming a part of the Mineral Basin of South Wales; by R. C. Taylor, Esq.; from the Geological Society's Transactions, 1830. From the Author. May, 1841.

Transactions and Proceedings of the London Electrical Society, from 1837 to 1840. London, 1841. 4to. pp. 206. From the Councillors.

Etudes sur les Glaciers, par L. Agassiz ; ouvrage accompagné d'une atlas de 32 planches, folio. Neuchatel, aux frais de l'Auteur. 1840. 8vo. pp. 346.

Description des Echinodermes Fossiles de la Suisse ; par L. Agassiz. Premiere et Seconde parties, contenant Spatangoides, Clypeastroides et Cidavides. Planches 22, 4to. pp. 101 and 108. Neuchatel, 1839 and 1840.

Mémoire sur les Moules de Mollusques Vivans et Fossiles, par L. Agassiz. Premiere partie—Moules d'Acéphales vivans. Neuchatel, 1839. 4to. pp. 48, plates 9. From the Author.

Etudes critiques sur les Mollusques fossiles, par L. Agassiz. Premiere livraison, contenant les Trigonies du Jura et de la Craie Suisses. Neuchatel, aux frais de l'Auteur. 1840. 4to. pp. 58, planches 11.

Catalogus Systematicus Ectyporum, Echinodermatum fossilium, Musei, Neocomensis, &c., auctore L. Agassiz, Necomi, Helvetorum sumptibus auctoris. 1840. 4to. ab auctore.

SCIENCE.—DOMESTIC.

Objections to the nomenclature of Berzelius, with suggestions respecting a substitute ; by Robert Hare, M. D. From the Author.

Discourse on the object and importance of the National Institution for the promotion of Science ; by J. R. Poinsett, Secretary of War. From the Author.

Exposition of the Science of Mechanical Electricity ; by Robert Hare, M. D. 8vo. 1841. From the Author.

Compendium of the Course of Chemical Instruction in the Medical Department of the University of Pennsylvania, by Robert Hare, M. D. 8vo. 1841. From the Author.

Monograph of the genus Sciurus, with descriptions of new species and their varieties. By J. Bachman, D. D., from the Magazine of Natural History for 1839. From the Author.

Proceedings of the American Philosophical Society, Nov. Dec., 1840, No. 14. From the Society.

Supplement to No. 1 of "A Monograph of the Limniades of fresh-water univalve shells of North America ;" by S. S. Haldeman. Three copies. From the Author.

Catalogue of Philosophical and Astronomical Instruments manufactured and sold by N. B. Chamberlain, Boston.

Annual Report of the State Geologist of Michigan, (Dr. Douglass Houghton,) for 1839-40. From Dr. Houghton.

Discourse by Dr. Darlington on the character, properties, and importance to man of the natural family of plants called Gramineæ. 1841. 2 copies from the Author.

Synopsis of the Course of Lectures and Instructions in Miami University, &c.; by Prof. J. W. Scott. 24mo. From the Author.

Report of the Alleghany Company's Coal Lands, Maryland; by Prof. Walter R. Johnson. From the Author.

Final Report of the Geology of New Jersey; by Prof. Henry D. Rogers. Philadelphia, 1840. 8vo. pp. 301. Two copies. From the Author.

Letters descriptive of the Bedford Springs. Philadelphia, 1836. 18mo. pp. 148. By and from Philip H. Nicklin.

Prof. Amos Eaton's Geological note-book, for the Troy class of 1841. From the Author.

Fifth Annual Report of the Geological Survey of Pennsylvania, by and from Prof. Henry D. Rogers, State Geologist. Harrisburg, Pa. 1841. 8vo. pp. 179.

Report on the Geological Survey of the State of Virginia, for 1840; by Prof. William B. Rogers, of the University of Virginia. 1841, 8vo. pp. 131. From the Author.

Annual Report of the Geologist of Maryland, J. T. Ducatel, for 1840. 8vo. pp. 59. From the Author.

Monograph of the Limniades of North America; by S. S. Haldeman. No. 2, 1841. From the Author.

Proceedings of the Academy of Natural Sciences, Philadelphia. March and April, No. 1. May, 1841, No. 2. Two copies. From the Society.

H. Milne Edwards' Anatomy and Physiology, translated from the French by Dr. J. F. W. Lane. Boston, 1841. 8vo. pp. 312. C. C. Little and J. Brown. From the Publishers.

Fossils from the Medial Tertiary of the United States; by T. A. Conrad. No. 2. May 7, 1840. From the Author.

Monography of the family Unionidæ; by T. A. Conrad. No. 12. Two copies. Philadelphia, 1840. From the Author.

Report on the manufacture of iron, addressed to the Governor of Maryland. By Prof. J. H. Alexander, Annapolis, Md. 8vo. pp. 270, with plates. From the Author.

Annual Report of the State Geologists of New York, for 1840. Albany, 1841, 8vo. pp. 184. From Dr. L. C. Beck.

Observations on magnetic intensity at twenty one stations in Europe; by Prof. A. D. Bache, LL. D. From the Author.

On the perchlorate of the oxide of ethule or perchloric ether; by Clark Hare, and Martin H. Boyè. Philadelphia, 1841. Two copies. From the Authors.

Report on the Invertebrate Animals of Massachusetts, comprising the Mollusca, Crustacea, Annelida, and Radiata, by Augustus A. Gould, M. D. in accordance with an order from the Legislature by the Commissioners on the Zoological and Botanical Survey of the State. 8vo. pp. 373, with 15 plates. Boston, 1841. From Dr. Gould.

Description of new Fossils of Tennessee, &c. by Prof. G. Troost, with lithograph figures; from the Transactions of the Geological Society of France. From the Author.

Letter of Albert Stein to S. J. Peters on the improvement of the navigation of the Mississippi. From Dr. Troost.

Dr. Troost's fifth Geological Report on Tennessee, with a colored map and section. From the Author.

MISCELLANEOUS—FOREIGN.

German Catalogues. From Messrs. Nestler & Melle, Hamburgh.

Extrait du Catalogue des livres des fonds de la Libraire de Jent et Gassmann a Soleure en Suisse.

Catalogue of a collection of rare Books (4500 vols.) to be sold in Hamburg Sept. 27, 1841, by the auctioneer Brödermann. 12mo. pp. 372. Hamburg. From Nestler & Melle.

MISCELLANEOUS.—DOMESTIC.

Inaugural Address of Thomas Corwin, governor of Ohio. From Roswell Marsh. Also, Annual Message of Governor Corwin, 1841.

Address by Charles Miner at the democratic whig festival, Wilkes-barre, Penn.

Catalogue of Western Reserve College for 1841. From Prof. St. John. Do. from Prof. Day.

Speech of Mr. Huntington of Connecticut in the House of Representatives of the United States, 1841, on the prospective pre-emption bill. From the Author.

Prospectus of the American Eclectic, conducted by Absalom Peters, D. D. From Dr. Peters.

Annual Report of the Superintendent and Inspector of Salt in Onondaga county, N. Y. From J. P. B. Storer.

Annual Report for 1841 of the Trustees of the Perkins Institution and Massachusetts Asylum for the Blind. From S. J. Howe, M. D.

Dr. Ticknor's Address to the candidates for degrees and licenses in the Medical Institution of Yale College, 1841.

Rev. A. R. Wright's Address before the Alumni of Western Reserve College. From Prof. St. John.

Speech of C. M. Clay, of Fayette, in the House of Representatives of Kentucky, upon the bill to repeal the law of 1832–3, prohibiting the importation of slaves into that state.

Correspondence and remarks relative to the recent attempt to exclude Unitarians from the Young Men's Bible Society of Cincinnati. From T. C. Lea.

Semi-Annual Catalogue of Westfield Academy, 1840–41. From A. Parish.

Twenty Fourth Annual Report of the American Colonization Society.

Twentieth Annual Report of the Board of Directors of the Mercantile Library Association, New York, 1841. 2 copies.

Otis, Broaders & Co. Literary Advertiser, Boston.

Second Annual Report of the Directors and Superintendent of the Ohio Lunatic Asylum, 1841. From E. M. Sill. Do. from Roswell Marsh.

Catalogue of British, French, and American Books, for sale by Wiley & Putnam, New York.

Supplement to the General Catalogue of the Library of the Linonian Society, Yale College, New Haven.

Catalogue of Books for sale by Little & Brown, Boston.

American Tract Magazine, March, 1841.

Yale Literary Magazine, Nos. 3 and 4. From the editors.

A Report of the special agent of the lead mines relative to the sale or future management of the mineral lands of the United States. From W. W. Boardman, M. C.

Dr. Allen on Mental Philosophy. From the author.

Southern Literary Messenger, with a commendatory notice of the American Journal of Science. By and from Lt. Maury, Richmond, Virginia.

Catalogue of the Philorhetorian Society.

Description of the Missourium. From Geo. Silliman.

Catalogue of the officers and students of the Louisville Medical Institute. From H. C. Banks.

Bulletin of the Proceedings of the National Institution for the promotion of Science. From Francis Markoe, Jr.

Eighth Annual Report of the Trustees of the State Lunatic Hospital at Worcester, 1841. From Dr. Woodward.

Address on the study of Natural History, by J. G. Morris, and a Poem on the Sovereignty of Mind, by John N. McJilton. Baltimore, 1841. From the Authors.

Address by Daniel Ullman, delivered at the Broadway Tabernacle, N. Y. From the Author.

Register of the Officers and Cadets of the Virginia Military Institute at Lexington, Va. 1840.

Mr. Cleveland's dedication sermon, Stoneham. From the Author.

The proper test of Christian character, by Henry Colman. Boston, 1824. From the Author.

Lecture on Phrenology, by Frank Hamilton, M. D. Rochester, 1841. From the Author.

Report of the engineer upon the several definite locations of the Hartford and Springfield Railroad. From Charles Buck.

Eulogy on Washington, by Rev. D. C. Banks, Louisville, Kentucky, 1841. From the Author.

Sermon on the death of Lt. J. A. Underwood and midshipman Henry, of the U. S. N. by Jared J. Elliot, chaplain. Delivered on board the U. S. ship Vincennes. Printed at the mission press, Honolulu, Oahu. From J. D. Dana, of the Exploring Expedition.

Sermon on the death of Joseph May, aged 81, by F. W. P. Greenwood. From S. May.

Catalogue of the University of Pennsylvania, for Yale College Library. From P. H. Nicklin.

Report made to the Trustees of the University of Pennsylvania, by P. H. Nicklin, for Yale College Library. From P. H. Nicklin.

Report of the committee of investigation appointed by the Stockholders of the Bank of the United States.

Remarks by President Quincy, of Harvard University, on the voluntary system in the studies of Greek and Latin. From President Quincy.

Syllabus of a course of popular lectures on Physiology, by Reynell Coates, M. D. From Dr. Coates.

Discourse before the Georgia Historical Society, Savannah, by Wm. Bacon Stevens, M. D. From the Author.

Funeral Sermon delivered in the Presbyterian church, Washington, on the Sabbath after the decease of William H. Harrison, late President of the United States, by Rev. C. Van Rensselaer, of Burlington, N. Y. From the Author.

Twenty Second Annual Report of the Directors of the New York Institution for the Instruction of the Deaf and Dumb, 1840. From J. H. Pettingill.

Commendations of Dr. N. Webster's Books.

Catalogue of English and American Books to be sold at auction May 3, by Bangs, Richards & Platt.

Prince's select Catalogue of double Dahlias, 1841.

Perkins' Higher Arithmetic. From the Author.

Eulogy on William Henry Harrison, by Rev. J. Richards. From the Author, Windsor.

Address before the Boston Natural History Society, by J. E. Teschemacher, May 5, 1841. 2 copies from the Author.

The History of Harvard University, by Josiah Quincy, LL. D. 2 vols. 8vo. C. C. Little and James Brown. From the Author.

History of the Belville Settlement, by S. P. Hildreth, M. D. Cincinnati, 1839. From the Author.

An introductory lecture before an Education Convention, by John T. Plummer, M. D. Springfield, Ind. May 11, 1841. From the Author.

New Hampshire Medical Institution Annual Circular, Hanover, N. H. 1841. From Prof. Hubbard.

Mr. Vinton's Sermon on the National Fast Day, May 14, 1841. From Lt. Wm. Rogers Taylor, U. S. N.

Seventeenth Annual Report of the Directors of the Hartford Insane Retreat, Hartford, 1841. From Dr. A. Brigham.

The History of Harvard University, by President Josiah Quincy, LL. D. 2 vols. 8vo. with illustrations. 2 copies, one for the Library of Yale College. From the Author.

History of Long Island, by Benjamin F. Thompson, Esq. 1 vol. 8vo. with illustrations. From the Author.

Sketches of Iowa, with a map, by John B. Newhall, Esq. From the Author.

SPECIMENS.—FOREIGN.

Seventy Mineralogical Specimens from Faroe and the north of Europe. From Prof. George Forchhammer, of Copenhagen.

Two hundred Geological and Mineralogical Specimens. From Dr. Fr. Tamnau, Berlin, Prussia.

Minerals of Nova Scotia. From Rev. Costar Parrsborn, N. S.

Models and specimens of several Saurian reptiles from the lias of England. From Wm. Stutchbury, Esq. Bristol Institution.

Agates and Carnelians from Singapore. From Rev. J. T. Dickinson.

Fossil Fish of Mt. Libanus, and Salt from near the Dead Sea. From Rev. Eli Smith.

Minerals from the Sandwich Islands. From Rev. Mr. Judson.

Minerals from the vicinity of Mt. Vesuvius. From J. E. Teschemacher, Esq.

SPECIMENS.—DOMESTIC.

Native Steel Ore of Duane, N. Y. From M. H. Read.

Tubular Concretions of Iron sand, from Florida coast. From Lieut. James Gerry.

Phosphate of Uranium, from Chesterfield, Mass. From Mr. J. E. Teschemacher.

Red Pipe Stone, (Catlinite,) from Côteau des Prairies—also quartz geodes. From J. N. Nicollet, Esq.

A box of fine Rock Salt, from Abingdon, Va. From the Rev. Stephen Taylor. (See p. 214, of this number.)

NEWSPAPERS.—DOMESTIC.

The Country Advertiser.—Marietta Intelligencer. From the Editor.—Union Agriculturalist. From the Editors.—New England Christian Advocate.—St. Louis Republican, with notice of La Motte lead mines.—New York Journal of Commerce.—The Future.—Rochester Daily Advertiser. From Thomas H. Hyatt, Editor.—Nantucket Enquirer. From S. M. Jenks, Editor.—American Standard, Albion, N. Y. From T. C. Strong, Editor.—Ohio Observer. From E. P. Barrows, Editor.—Le Roy Gazette. From C. B. Thomson, Editor.—Fredonia Censor. From E. Winchester, Editor.—Charleston Observer, Charleston, South Carolina.—Cincinnati Gazette.—Constitution, Middletown. From Professor Johnston, with an account of the explosion of the steam-boiler in the sash and blind factory of Messrs. Pike & Perry.—Boston Daily Advertiser.

From Dr. Wyman.—Literary Pearl, Charlton, Saratoga Co. N. Y. From the Editor, with an account of a meteor.—Republican Standard, Bridgeport. From James H. Linsley.—Savannah Daily Republican. From Dr. Watkins.—Macon Georgia Telegraph.—Vicksburg Daily Whig. From George W. Niles, with a meteorological Table.—Rochester Republican. From the Editor.—Greensburg Sentinel.—Scioto Gazette.—Indiana Gazette. From Dr. Plummer.—Louisville Journal. From H. C. Banks.—St. Louis New Era, February 27, 1841, containing meteorological table for December, 1840. From C. C. Whittelsey.—The Constitutionalist, Bath, Maine, March 10, 1841, notice of parhelia.—York County Herald.—" Olive Leaf," New York, Providence, Boston.—New York Evangelist.—Northern Light, Albany.—Mohawk Advertiser. From J. A. Lapham.—Charleston Courier.—Rutland Herald.—Bloomington Herald, I. T. From J. S. Parvin.—Maumee River Times.—Cincinnati Chrouicle.—Vermont Chronicle.—Greensburgh Sentinel, Pa.—Milwaukie Sentinel.—Cattskill Messenger.—The Constitution, Middletown. From Dr. Barratt with a statement showing the falls of snow at Middletown, Conn. from Oct. 1840, to April, 1841.—Democratic Herald, Ann Arbor.—The Daily Picayune, New Orleans.—Sentinel and Witness, Middletown, with an obituary notice of Dr. Miner, by Dr. Barratt.—Christian Observer, Philadelphia.—Jewett's Advertiser.—Kentucky Farmer, containing an analysis of Kentucky soil, by Dr. Peter.—Maumee River Times, with a notice of the Am. Journ.—New York Sun.—Louisville Public Advertiser.—Daily Evening Gazette, St. Louis.—Greensburg Sentinel.—The Boston Atlas.—Self Instructor, Extra, New York.—The Free Press, Xenia, Ohio.—Niles' National Register, Baltimore.—Union Agriculturalist, Chicago.—Leonard Town Herald.—Commercial Advertiser, Buffalo. From R. W. Haskins, with a notice of that gentleman's astronomy for schools.—United States, Philadelphia.—New York American, June 16, 1841. —Zion's Advocate, June 16 and 23, 1841, with a notice of the American Journal of Science.—Various numbers of the Ohio Observer, containing Meteorological Tables. From Prof. E. Loomis.

NEWSPAPERS—FOREIGN.

Halifax (Nova Scotia) Morning Post, with an account of the Mechanics' Institute.—Oxford University, England, with report of the communication of Prof. Buckland on the animalculæ in limestone.—Sussex Advertiser.—Montreal Transcript, June 3, 1841.—Anti-Slavery Reporter, London, in series.—The Witness, Edinburgh, extra royal sheets for May 29, and June 1, 1841. From John Dunlop, Esq.

THE

AMERICAN JOURNAL

OF

SCIENCE AND ARTS.

CONDUCTED BY

PROFESSOR SILLIMAN

AND

BENJAMIN SILLIMAN, Jr.

VOL. XLI.—No. 1.—JULY, 1841.

FOR APRIL, MAY, AND JUNE, 1841.

(TO BE CONTINUED QUARTERLY.)

NEW HAVEN:

[illegible] MALTBY and [illegible] W. NOYES—*Philadelphia*, CAREY & HART, [illegible] LITTELL, and ORRIN ROGERS [illegible] —*Baltimore*, [illegible] HICKMAN—*New York*, CARVILL & Co., [illegible] Broadway; THEODORE FOSTER, [illegible] and Broadway; and [illegible] No. [illegible] Wall St.—*Boston*, C. C. LITTLE & Co. [illegible] WILEY & PUTNAM, No. 35 Paternoster Row, *London*, and [illegible] Broadway, New York.—*Paris*, HECTOR BOSSANGE & Co. No. 11, Quai Voltaire.—*Hamburgh*, Messrs. NESTLER & MELLE.

PRINTED BY B. L. HAMLEN. [illegible]

Published at New Haven, July [illegible], 1841.

TO CORRESPONDENTS.

Papers from Prof. J. W. Bailey, Mr [illegible] Prof. [illegible] Dr. [illegible] Dr. A. Gray, Prof. E. Loomis and J. M. M'C[illegible], [illegible] Prof. J. [illegible] and [illegible] will appear in our next.

Some of the [illegible] authors.

Communications have been [illegible] Mr. [illegible] Samuel Adams, Mr. T. L. [illegible]

We have received the following valuable [illegible] which were prepared for the [illegible] number, [illegible]

Three Lectures on A[illegible] delivered at [illegible] Daubeny, M. D. F. R. S., &c. [illegible] Professor [illegible] versity of Oxford.

Report on the manufacture of Iron, by order of the [illegible] to the Governor, by J. H. Alexander, T[illegible] ohs, 1840. This is an important report and will [illegible]

Report on the Geological Survey of Virginia, for the year [illegible] Rogers. Richmond, 1841, pp. 132.

Fifth Annual Report on the Geological Survey of Pennsylvania, by Prof. H. D. Rogers, State Geologist. Harrisburgh, 1841, pp. [illegible]

Final Report on the Geology of the State of New [illegible] Philadelphia, 1840, pp. 301.

Annual Report on the Geology of Maryland for [illegible] Baltimore, 1841, pp. 59.

Annual Report for 1840 of the State Geologist of New York, 1841. [illegible] 184.

Dr. Troost's fifth Geological Report to the Assembly of Tennessee. [illegible] 1840, pp. 75.

Flora of North America, by Dr. John Torrey and Dr. Asa Gray, [illegible]

Discourse by Dr. Darlington on the characters, [illegible] of the natural family of plants, called [illegible]

The titles of communications and of their authors, [illegible]

Authors should always specify at the head of their [illegible] copies of their communications which they may wish to have printed.

Return proofs, not to be mailed, [illegible]

Notice always to be given when communications sent to this Journal, have been, or are to be, published also in other Journals.

NOTICE.

The public are [illegible] Journal, except to such persons as are authorized to receive it. [illegible] See the Prospectus on the third page of this cover.

THE

AMERICAN JOURNAL

OF

SCIENCE AND ARTS.

CONDUCTED BY

PROFESSOR SILLIMAN

AND

BENJAMIN SILLIMAN, JR.

VOL. XLI.—No. 2.—OCTOBER, 1841.

FOR JULY, AUGUST, AND SEPTEMBER, 1841.

(TO BE CONTINUED QUARTERLY.)

NEW HAVEN:

Sold by B. & W. NOYES—Philadelphia, CAREY & HART, J. S. LITTELL and ORRIN ROGERS, No. 67 So. Second St.—Baltimore, Md., N. HICKMAN—New York, CARVILL & Co., No. 108 Broadway, THEODORE FOSTER, corner of Pine St. and Broadway, and G. S. SILLIMAN, No. 40 Wall St.—Boston, C. C. LITTLE & Co.—London and New York, WILEY & PUTNAM, No. 35 Paternoster Row, London, and 161 Broadway, New York—Paris, HECTOR BOSSANGE & Co., No. 11, Quai Voltaire.—Hamburgh, Messrs. NESTLER & MELLE.

PRINTED BY B. L. HAMLEN.

Published at New Haven, October 1, 1841.

TO CORRESPONDENTS.

Papers will appear in our next, from [illegible], Dr. [illegible], Prof. [illegible]vel, Dr. Locke, Dr. H. C. Perkins, [illegible], Prof. [illegible], Dr. J. Lawrence Smith, Dr. T. M. [illegible], [illegible], Mr. [illegible] H. Blake, and others.

"A Constant Reader" requests [illegible] of the Daguerreotype." We would willingly gratify him, but such [illegible] advance of the process, [illegible] for a more advanced and mature [illegible] of this elegant art. [illegible] count of the process published in the Journal, Vol. [illegible], p. [illegible]

Papers have been received from Prof. [illegible], [illegible] Mr. F. G. Woodward, Dr. J. [illegible], Mr. [illegible]

Prof. Dewey's article, promised in our last, [illegible] could not be published for want of the plates illustrating [illegible] postponed with his consent to January. We owe [illegible] correspondents for our unfulfilled obligations; the [illegible] Prof. Hitchcock's Address before the Association of Geologists, [illegible] of short communications which we had hoped to [illegible]. It is in vain that we endeavor to clear our pocket of valuable papers; and our friends [illegible] enough to believe that the delay which sometimes occurs, is the result of [illegible] and not of intention.

Reply of Sir Michael Faraday *to* Dr Hare.—We [illegible], that a letter addressed to us by Sir Michael Faraday, and intended for this Journal, has been mislaid. In substance, however, its object was to [illegible] discussion of the points at issue—referring [illegible] to the [illegible] or which may hereafter appear on the subject. We have learned with regret, that the health of Sir Michael Faraday has declined under his [illegible], and that relaxation has become indispensable to his restoration. [illegible] Dr. Hare, being now abroad in Europe for [illegible], we [illegible] both a speedy recovery of health, to enable them to resume their [illegible] and successful researches.

The titles of communications and of their authors, must be fully given.

Authors should always specify at the head of their MSS. the number of extra copies of their communications which they may wish to have printed.

Return proofs, not to be sealed, or inclosed with any thing [illegible].

Notice always to be given when communications [illegible] to this Journal, have been, or are to be, published also in other Journals.

Erratum.—On the Plate to Prof. [illegible], [illegible] *diaceæ.*

NOTICE.

The public are cautioned not to make payment [illegible] to this Journal, except to such persons as are authorized to receive it. [illegible] See the Prospectus on the third page of this cover.

THE

AMERICAN JOURNAL

OF

SCIENCE AND ARTS,

CONDUCTED BY

PROF. B. SILLIMAN AND B. SILLIMAN, JR.

OF YALE COLLEGE.

THIS JOURNAL embraces in its plan, *the entire circle of the Physical Sciences*, and their applications to the arts. It was begun in July, 1818, and the forty-first volume is now completed.

While it has prospered it has been sustained by original American efforts, and being devoted to important national and universal interests, it is in that character known and accredited, both at home and abroad. It has elicited many valuable researches and discoveries; its miscellaneous department has presented a great variety of topics of general interest, and a large part of the work is quite intelligible to the reading public.

Avoiding local, personal, and party interests and prejudices, it forgoes the support of popular feeling, and relies mainly upon the intelligent and the patriotic.

The Foreign Journals (many of them sent in exchange,) often quote from its pages, which are in turn enriched by theirs; and it has thus become identified with the science and Arts of the present day.

The American Journal of Science and Arts is published at New Haven, Connecticut. Each number contains at least 200 pages, closely and handsomely printed on good paper, and fully illustrated by engravings. The subscription is [illegible] *in advance*, by mail. The price being twice the usual price of the literary quarterlies is indispensable, on account of a more limited patronage, and the great expense of engravings.

Remittances should be made, if possible, in Eastern money, but if that cannot be obtained, the best other which can be had may be substituted.

Subscribers will remember the regulations of the Post-Office department, by which *Postmasters are authorized to remit payments for periodicals free of postage, if the letter containing the remittance is subscribed by themselves.*

POSTAGE PAID.

The postage is paid by the Editors on all copies sent *directly from them by mail* to subscribers whose accounts are not in arrears. Their object in this arrangement is to place those persons who cannot take the work through an agent, and therefore free of charge of transportation, on the same footing in this respect with city subscribers. Subscribers by mail have heretofore paid from $1 to $1.37½ per annum for postage, which has been a sufficient consideration to induce many to decline taking the work. Now this objection is removed, upon the simple condition of punctual remittance. This plan has been in operation since January, 1841, and has already had the effect of increasing the number of subscribers to the work, and also of rendering all more attentive to the condition of their accounts, that they may avail themselves of the free postage, as the Editors strictly adhere to their rule of paying *no postage* on Nos. sent to those who are in arrears; and they make an appeal to their present supporters and contributors, for aiding their endeavors, and to enable them to sustain the experiment, by making this notice more public, and inducing their friends to subscribe.

Experience has proved that the mail is by far the best means of conveyance to distant subscribers—the most safe and most speedy; and all attempts to establish agencies at a distance, and away from the great lines of transportation have utterly failed,—delay and dissatisfaction, and often abandonment of the work, being the result.

New Haven, Conn., October 1, 1841.

THE AMERICAN JOURNAL, &c.

TERMS.

Six dollars per annum; published in four Quarterly numbers, making two volumes a year, of from eight to nine hundred pages for both volumes, which are fully illustrated by plates.

The Editors will draw on the agents, semi-annually, at ninety days.

Postmasters can frank all remittances: sums of $5 or $10 are convenient, as they can be credited *in account.*

Money remitted will in all cases be acknowledged by a bill or receipt in the next No. of the Journal or by letter: if no such paper or letter is received, it may be presumed that the remittance has miscarried, (a very rare occurrence.)

Complete sets, now 41 vols., are furnished to order.

The extra dollar per annum charged on this Journal beyond the price of the literary reviews, is indispensable on account of a more limited patronage and greater expense.

This work is most safely and expeditiously transmitted to distant subscribers *by mail. Postage paid to all who receive this work directly from the Editors, provided their accounts are square.* This No. contains 13 sheets; postage, under 100 miles, 20 cents, over 100 miles, 32½ cents.

Notice always to be sent of discontinuance, removals and deaths of subscribers.

AGENTS.

MASSACHUSETTS.

BOSTON, Otis, Broaders & Co.; W. H. S. Jordan.
SALEM, Henry Whipple.
NEW BEDFORD, William C. Taber.
AMHERST, J. S. & C. Adams.
LOWELL, D. Bixby.
WORCESTER, Clarendon Harris; Dorr, Howland & Co.

RHODE ISLAND.

PROVIDENCE, B. Cranston & Co.

CONNECTICUT.

HARTFORD, Robbins & Folger.
MIDDLETOWN, Luke C. Lyman.

NEW YORK.

NEW YORK, A. T. Goodrich.
ALBANY, W. C. Little.
TROY, Elias Gates.

MARYLAND.

BALTIMORE, N. Hickman.

DISTRICT OF COLUMBIA.

WASHINGTON, Frank Taylor.

NORTH CAROLINA.

CHAPEL HILL, Prof. E. Mitchell.

SOUTH CAROLINA.

CHARLESTON, Ebenezer Thayer.

GEORGIA.

SAVANNAH, Wm T. Williams.
AUGUSTA, Th. I. Wray.

C. W. UPSON, travelling agent for Connecticut.

ADVERTISING SHEET
OF THE
AMERICAN JOURNAL OF SCIENCE.

Advertisements will be inserted on the covers or Advertising Sheet of this Journal, (for terms see Advertising Sheet,) and its circulation both in Europe and America renders it a favorable vehicle for that purpose to the authors and publishers of both countries.

Publishers' advertising sheets will be stitched up with the numbers if desired, so far as they are not transported by mail, and then, if the publisher will pay the increase of postage.

Advertisements intended for insertion in this Journal, should be forwarded as early as the 20th of the last month in each quarter. They may be sent direct to the Editors, or to Wiley & Putnam, 161 Broadway, N. Y., and 35 Paternoster Row, London.