THE

AMERICAN JOURNAL

OF

SCIENCE AND ARTS.

CONDUCTED BY

PROFESSORS B. SILLIMAN, B. SILLIMAN, JR.,

AND

JAMES D. DANA,

IN CONNECTION WITH

PROF. ASA GRAY, OF CAMBRIDGE,
PROF. LOUIS AGASSIZ, OF CAMBRIDGE,
DR. WOLCOTT GIBBS, OF NEW YORK.

VOL. XXIX.—MAY, 1860.

SECOND SERIES.

WITH ONE PLATE AND THREE DIAGRAMS.

NEW HAVEN: EDITORS.
1860.

PRINTED BY E. HAYES, 50 CHAPEL ST.

CONTENTS OF VOLUME XXIX.

NUMBER LXXXV.

SCIENTIFIC INTELLIGENCE.

NUMBER LXXXVI.

SCIENTIFIC INTELLIGENCE.

NUMBER LXXXVII.

SCIENTIFIC INTELLIGENCE.

ERRATA.

P. 101, l. 4, for "ethyl," read "silver."
" 102, l. 13, for "43·5," read "44·8" (as the half sum of equivalents U and Ni.)
" 102, l. 25, for "Mg," read "Mn."
" 107, l. 8, for "C," read "B."
" 110, l. 2 from bottom, for "Co," read "Cr."
" 111, l. 1, for "No," read "Nb."
" 272, l. 8 from bottom, for "HENTZ," read "HEINTZ."

Vol. XXVIII, p. 135, l. 33 from top, for Vol. VI, read Vol. IV.—P. 356, line 20 from bottom, after the word "cultivation," insert a period, putting a capital W for the word "where." Same page, line 19 from bottom, omit "then."—P. 357. line 10 from top, for "deposition of sand," read "deposition of mud."

THE

AMERICAN

JOURNAL OF SCIENCE AND ARTS.

[SECOND SERIES.]

ART. I.—*On the Origination and Distribution of Species:—Introductory Essay to the Flora of Tasmania;* by DR. JOSEPH D. HOOKER.*

§ 1. *Preliminary Remarks.*

THE Island of Tasmania does not contain a vegetation peculiar to itself, nor constitute an independent botanical region. Its plants are, with comparativelv few exceptions, natives of extra-tropical Australia; and I have consequently found it necessary to study the vegetation of a great part of that vast continent, in order to determine satisfactorily the nature, distribution, and

* *To the Editors of the American Journal of Science, &c.*:—The sheets of this *Introductory Essay*, having been obligingly communicated to me in advance of the publication of the concluding part of the *Flora of Tasmania*, to which it belongs, I asked and have received the distinguished author's permission to reprint them, or a considerable portion of them, in your Journal, and now offer them for that purpose. This is in order that we may have before us, at the earliest date, an essay which cannot fail to attract the immediate and profound attention of scientific men; but which, if confined to the pages of the *Flora of Tasmania*, would be seen by very few American readers. To those who have intelligently observed the course of scientific investigation, and the tendency of speculation, it has for some time been manifest that a re-statement of the Lamarkian hypothesis is at hand. We have this, in an improved and truly scientific form, in the theories which, recently propounded by Mr. Darwin, followed by Mr. Wallace, are here so ably and altogether independently maintained. When these views are fully laid before them, the naturalists of this country will be able to take part in the interesting discussion which they will not fail to call forth.

To save room, a few paragraphs are omitted which do not directly bear upon the subject in hand. A. G.

affinities of the Tasmanian Flora. From the study of certain extratropical genera and species in their relation to those of Tasmania, I have been led to the far more comprehensive undertaking of arranging and classifying all the Australian plants accessible to me. This I commenced in the hope of being able thereby to extend our knowledge of the affinities of its Flora, and, if possible, to throw light on a very abstruse subject, viz. the origin of its vegetation, and the sources or causes of its peculiarity. This again has induced me to proceed with the inquiry into the oxigin and distribution of existing species; and, as I have already treated of these subjects in the Introduction to the New Zealand Flora, I now embrace the opportunity afforded me by a similar Introduction to the Tasmanian Flora, of revising the opinions I then entertained, and of again investigating the whole subject of the creation of species by variation, with the aid of the experience derived from my subsequent studies of the Floras of India and Australia in relation to one another and to those of neighboring countries, and of the recently published hypotheses of Mr. Darwin and Mr. Wallace. * * *

In the Introductory Essay to the New Zealand Flora, I advanced certain general propositions as to the origin of species, which I refrained from endorsing as articles of my own creed: amongst others was the still prevalent doctrine that these are, in the ordinary acceptation of the term, created as such, and are immutable. In the present Essay I shall advance the opposite hypothesis, that species are derivative and mutable; and this chiefly because, whatever opinions a naturalist may have adopted with regard to the origin and variation of species, every candid mind must admit that the facts and arguments upon which he has grounded his convictions require revision since the recent publication by the Linnean Society of the ingenious and original reasonings and theories of Mr. Darwin and Mr. Wallace.

Further, there must be many who, like myself, having hitherto refrained from expressing any positive opinion, now, after a careful consideration of these naturalists' theories, find the aspect of the question materially changed, and themselves freer to adopt such a theory as may best harmonize with the facts adduced by their own experience.

The Natural History of Australia seemed to me to be especially suited to test such a theory, on account of the comparative uniformity of its physical features being accompanied with a great variety in its Flora; of the differences in the vegetation of its several parts; and of the peculiarity both of its Fauna and Flora, as compared with those of other countries. I accordingly prepared a classified catalogue of all the Australian species in the Herbarium, with their ranges in longitude, latitude, and elevation, as far as I could ascertain them, and added what further information I could obtain from books. At the same time I made a careful study of the affinities and distribution of all the Tasmanian species, and of all those Australian ones which I believed to be found in other countries. I also determined as accurately as I could the genera of the remainder, and especially of those belonging to genera which are found in other countries, and I distinguished the species from one another in those genera which had not been previously arranged. In this manner I have brought together evidence of nearly 8000 flowering plants having been collected or observed in Australia, of which I have seen and catalogued upwards of 7000. About two-thirds of these are ascertained specifically with tolerable accuracy, and the remainder are distinguished from one another, and referred to genera with less certainty, being either undescribed, or described under several names, whilst some are members of such variable groups that I was left in doubt how to dispose of them.

To many who occupy themselves with smaller and better worked botanical districts, such results as may be deduced from the skeleton Flora I have compiled for Australia may seem too crude and imperfect to form data from which to determine its relations. But it is not from a consideration of specific details that such problems as those of the relations of Floras and the origin and distribution of organic forms will ever be solved, though we must eventually look to these details for proofs of the solutions we propose. The limits of the majority of species are so undefinable that few naturalists are agreed upon them;* to a great extent they are matters of opinion, even amongst those persons who believe that species are original and immutable creations; and as our knowledge of the forms and allies of each increases, so do these differences of opinions; the progress of systematic science being, in short, obviously unfavorable to the view that most species are limitable by descriptions or characters, unless large allowances are made for variation. On the other hand, when dealing with genera, or other combinations of species, all that is required is that these be classified in natural groups; and that such groups are true exponents of affinities settled by Nature is abundantly capable of demonstration. It is to an investigation of the extent, relations, and proportions of these natural combinations of species, then, that we must look for the means of obtaining and expressing the features of a Flora; and if in this instance the exotic species are well ascertained, it matters little whether or not the endemic are in all cases accurately distinguished from one another. Further, in a Flora so large as that of Australia, if the species are limited and

* The most conspicuous evidence of this lies in the fact, that the number of known species of flowering plants is by some assumed to be under 80,000, and by others over 150,000.

estimated by one mind and eye, the errors made under each genus will so far counteract one another, that the mean results for the genera and orders will scarcely be affected. As it is, the method adopted has absorbed many weeks of labor during the last five years, and a much greater degree of accuracy could only have been obtained by a disproportionately greater outlay of time, whilst it would not have materially affected the general results.

With regard to my own views on the subjects of the variability of existing species and the fallacy of supposing we can ascertain anything through these alone of their ancestry or of originally created types, they are, in so far as they are liable to influence my estimate of the value of the facts collected for the analysis of the Australian Flora, unaltered from those which I maintained in the 'Flora of New Zealand.' On such theoretical questions, however, as the origin and ultimate permanence of species, they have been greatly influenced by the views and arguments of Mr. Darwin and Mr. Wallace above alluded to, which incline me to regard more favorably the hypothesis that it is to variation that we must look as the means which Nature has adopted for peopling the globe with those diverse existing forms which, when they tend to transmit their characters unchanged through many generations, are called species. Nevertheless I must repeat, what I have fully stated elsewhere, that these hypotheses should not influence our treatment of species, either as subjects of descriptive science, or as the means of investigating the phenomena of the succession of organic forms in time, or their dispersion and replacement in area, though they should lead us to more philosophical conceptions on these subjects, and stimulate us to seek for such combinations of their characters as may enable us to classify them better, and to trace their origin back to an epoch anterior to that of their present appearance and condition. In doing this, however, the believer in species being lineally related forms must employ the same methods of investigation and follow the same principles that guide the believer in their being actual creations, for the latter assumes that Nature has created species with mutual relations analogous to those which exist between the lineally-descended members of a family, and this is indeed the leading idea in all natural systems. On the other hand, there are so many checks to indiscriminate variation, so many inviolable laws that regulate the production of varieties, the time required to produce wide variations from any given specific type is so great, and the number of species and varieties known to propagate for indefinite periods a succession of absolutely identical members is so large, that all naturalists are agreed that for descriptive purposes species must be treated as if they were at their origin distinct, and are destined so to remain. Hence the descriptive naturalist who believes all species to be derivative and mutable, only differs in practice from him who asserts the contrary, in expecting that the posterity of the organism he describes as species may, at some indefinitely distant period of time, require redescription.

I need hardly remark that the classificatory branch of Botany is the only one from which this subject can be approached; for a good system must be founded on a due appreciation of all the attributes of individual plants,—upón a balance of their morphological, physiological, and anatomical relations at all periods of their growth. Species are conventionally assumed to represent, with a great amount of uniformity, the lowest degree of such relationship; and the facts that individuals are more easily grouped into species limited by characters, than into varieties, or than species are into limitable genera or groups of higher value, and that the relationships of species are transmitted hereditarily in a very eminent degree, are the strongest appearances in favor of species being original creations, and genera, etc., arbitrarily limited groups of these.

The difference between varieties and species and genera in respect to definable limitation is however one of degree only, and if increased materials and observation confirm the doctrine which I have for many years labored to establish, that far more species are variable, and far fewer limitable, than has been supposed, that hypothesis will be proportionally strengthened which assumes species to be arbitrarily limited groups of varieties. With the view of ascertaining how far my own experience in classification will bear out such a conclusion, I shall now endeavor to review, without reference to my previous conclusions, the impressions which I have derived from the retrospect of twenty years' study of plants. During that time I have classified many large and small Floras, arctic, temperate and tropical, insular and continental: embracing areas so extensive and varied as to justify, to my apprehension, the assumption that the results derived from these would also be applicable to the whole vegetable kingdom. I shall arrange these results successively under three heads; viz., facts derived from a study of classification; secondly, from distribution; thirdly, from fossils; after which I shall examine the theories with which these facts should harmonize.

§ 2. *On the General Phenomena of Variation in the Vegetable Kingdom.*

1. All vegetable forms are more or less prone to vary as to their sensible properties, or (as it has been happily expressed in regard to all organisms), "they are in a state of unstable equilibrium."* No organ is exactly symmetrical, no two are exact

* Essays: Scientific, Political, and Speculative; by Herbert Spencer: p. 280.

counterparts, no two individuals are exactly alike, no two parts of the same individual exactly correspond, no two species have equal differences, and no two countries present all the varieties of a species common to both, nor are the species of any two countries alike in number and kind.

2. The rate at which plants vary is always slow, and the extent or degree of variation is graduated. Sports even in color are comparatively rare phenomena, and, as a general rule, the best-marked varieties occur on the confines of the geographical area which a species inhabits. Thus the scarlet Rhododendron (*R. arboreum*) of India inhabits all the Himalaya, the Khasia Mountains, the Peninsular Mountains, and Ceylon; and it is in the centre of its range (Sikkim and the Khasia) that these mean forms occur which by a graduated series unite into one variable species, the rough, rusty-leaved form of Ceylon, and the smooth, silvery-leaved form of the northwestern Himalaya. A white and a rose-colored sport of each variety is found growing with the scarlet in all these localities, but everywhere these sports are few in individuals. Also certain individuals flower earlier than others, and some occasionally twice a year, I believe in all localities.

3. I find that in every Flora all groups of species may be roughly classified into three large divisions: one in which most species are apparently unvarying; another in which most are conspicuously varying; and a third which consists of a mixture of both in more equal proportions. Of these the unvarying species appear so distinct from one another that most botanists agree as to their limits, and their offspring are at once referable by inspection to their parents; each presents several special characters, and it would require many intermediate forms to effect a graduated change from any one to another. The most varying species, on the contrary, so run into one another, that botanists are not agreed as to their limits, and often fail to refer the offspring with certainty to their parents, each being distinguished from one or more others by one or a few such trifling characters, that each group may be regarded as a continuous series of varieties, between the terms of which no hiatus exists suggesting the intercalation of any intermediate variety. The genera *Rubus*, *Rosa*, *Salix*, and *Saxifraga*, afford conspicuous examples of these unstable species; *Veronica*, *Campanula*, and *Lobelia*, of comparatively stable ones.

4. Of these natural groups of varying and unvarying species, some are large and some small; they are also variously distributed through the classes, orders, and genera of the Vegetable Kingdom; but, as a general rule, the varying species are relatively most numerous in those classes, orders, and genera which

are the simplest in structure.* Complexity of structure is generally accompanied with a greater tendency to permanence in form: thus Acotyledons, Monocotyledons, and Dicotyledons are an ascending series in complexity and in constancy of form. In Dicotyledons, *Salices*, *Urticeæ*, *Chenopodiaceæ*, and other orders with incomplete or absent floral envelops, vary on the whole more than *Leguminosæ*, *Lythraceæ*, *Myrtaceæ*, or *Rosaceæ*, yet members of these present, in all countries, groups of notoriously varying species, as *Eucalyptus* in Australia, *Rosa* in Europe, and *Lotus*, *Epilobium*, and *Rubus* in both Europe and Australia. Again, even genera are divided: of the last named, most or all of the species are variable; of others, as *Epacris*, *Acacia*, and the majority of such as contain upwards of six or eight species, a larger or smaller proportion only are variable. But the prominent fact is, that this element of mutability pervades the whole vegetable kingdom; no class nor order nor genus of more than a few species claims absolute exemption, whilst the grand total of unstable forms generally assumed to be species probably exceeds that of the stable.

5. The above remarks are equally applicable to all the higher divisions of plants. Some genera and orders are as natural, and as limitable by characters, as are some species; others again, though they contain many very well-marked subordinate plans of construction, yet are so connected by intermediate forms with otherwise very different genera or orders, that it is impossible to limit them naturally. And as some of the best marked and limited species consist of a series of badly marked and illimitable varieties, so some of the most natural† and limitable orders

* Mr. Darwin, after a very laborious analysis of many Floras, finds that the species of large genera are relatively more variable than those of small; a result which I was long disposed to doubt, because of the number of variable small genera and the fact that monotypic genera seldom have their variations recorded in systematic works, but an examination of his data and methods compels me to acquiesce in his statement. It has also been remarked (Bory de Saint-Vincent, Voy. aux Quatre Iles de l'Afrique) that the species of islands are more variable than those of continents, an opinion I can scarcely subscribe to, and which is opposed to Mr. Darwin's facts, inasmuch as insular Floras are characterized by peculiar genera, and by having few species in proportion to genera. Bisexual trees and shrubs are generally more variable than unisexual, which however is only a corollary from what is stated above regarding plants of simple structure of flower. On the whole, I think herbs are more variable than shrubby plants, and annuals than perennials. It would be curious to ascertain the relative variableness of social and scattered plants. The individuals of a social plant, in each area it is social upon, are generally very constant, but individuals from different areas often differ much. The *Pinus sylvestris*, *Mughus*, and *uncinata* are cases in point, if considered as varieties of one; as are the Cedars of Atlas, Algeria, and the Himalaya.

† It should be borne in mind that the term *natural*, as applied to orders or other groups, has often a double significance; every natural order is so in the sense of each of its members being more closely related to one or more of its own group than to any of another; but the term is often used to designate an easily limited natural order, that is, one whose members are so very closely related to each other by conspicuous peculiarities that its differential characters can be expressed, and itself

and genera may respectively consist of only undefinable groups of genera or of species. For instance, both *Gramineæ* and *Compositæ* are, in the present state of our knowledge, absolutely limited orders, and extremely natural ones also; but their genera are to a very eminent degree arbitrarily limited, and their species extremely variable. *Orchideæ* and *Leguminosæ* are also well-limited orders (though not so absolutely as the former), but they, on the contrary, consist of comparatively exceedingly well-marked genera and species. *Melanthaceæ* and *Scrophularineæ*, on the other hand, are not limitable as orders, and contain very many differently constructed groups; but their genera, and to a great extent their species also, are well-marked and limitable. The circumstance of a group being either isolated or having complex relations, is hence no indication of its members having the same characters.

Again, as with species, so with genera and orders, we find that upon the whole those are the best limited which consist of plants of complex floral structure: the orders of Dicotyledons are better limited than those of Monocotyledons, and the genera of Dichlamydeæ than those of Achlamydeæ.*

always recognized; these may be called *objective* orders; *Orchideæ* and *Gramineæ* are examples. Any naturalist, endowed with fair powers of observation and generalization, recognizes the close affinity between a pseudobulbous epiphytical, and a terrestrial tuberous-rooted Orchid, or between the Bamboo and Wheat, though the differences are exceedingly great in habit and in organs of vegetation and reproduction. Other orders are as natural and may be as well limited, but having no conspicuous characters in common, and presenting many subordinate distinct plans of structure, may be regarded as *subjective*. Such are *Ranunculaceæ* and *Leguminosæ*, of which a botanist must have a special and extensive knowledge before he can readily recognize very many of their members. No degree of natural sagacity will enable an uninstructed person to recognize the close affinity of *Clematis* and *Ranunculus*, or of *Acacia* and *Cytisus*, though these are really as closely related as the Orchids and Grasses mentioned above. We do not know why some orders are subjective and some objective; but if the theory of creation by variation is a true one, we ought through it to reach a solution.

* There are too many exceptions to this to admit of our concluding at once that it is attributable to any simple and uniform law of variation; but it may be explained by assuming that the degree or amount of variation is differently manifested at different epochs in the history of the group. Thus, if a genus is numerically increasing, and consequently running into varieties, it will present a group of species with complex relations *inter se;* if, on the contrary, it is numerically decreasing, such decrease must lead to the extinction of some varieties, and hence result in the better limitation of the remainder. The application of this assumption to the fact of the best limited groups being most prevalent among the higher classes (*i. e.*, among those most complicated in their organization), would at first sight appear an argument against progression, were it not for the consideration that the higher tribes of plants have in another respect proved themselves superior, in that they have not only far surpassed the lower in number of genera and species, but in individuals, and also in bulk and stature. And lastly, as all the highest orders of plants contain numerous species and often genera of as simple organization as any of the lower orders are, it follows that that physical superiority which is manifested in greater extent of variation, in better securing a succession of race, in more rapid multiplication of individuals, and even in increase of bulk, is in some senses of a higher order than that represented by mere complexity or specialization of organ.

Now my object in dwelling on this parallelism between the characteristics of individuals in relation to species, of species in relation to genera, and of genera in relation to orders, is because I consider (Introd. Essay to Fl. N. Z.) that it is to the extinction of species and genera that we are indebted for our means of resolving plants into limitable genera and orders. This view is now, I believe, generally admitted, even by those who still regard species as the immutable units of the vegetable creation; and it therefore now remains to be seen how far we are warranted in extending it to the limitation of species by the elimination of their varieties through natural causes.*

6. The evidence of variability thus deduced from a rapid general survey of the prominent facts elicited from a study of the principles of classification, are to a certain extent tested by the behavior of plants under cultivation, which operates either by hastening the processes of nature (in rapidly inducing variation), or by effecting a prolepsis or anticipation of those processes (in producing sports, *i.e.* better marked varieties, without graduated stages), or by placing the plant in conditions to which it would never have been exposed in the ordinary course of natural events, and which eventually either kill it or give origin to a series of varieties which might otherwise have never existed.†

* It follows as a corollary to the proposition (that species, etc., are naturally rendered limitable by the destruction of varieties), that there must be some intimate relation between the rate of increase and the duration of genera (or other groups of species) on the one hand, and the limitability of their species on the other. Thus, when a genus consists of a multitude of illimitable forms, we may argue with much plausibility that it is on the increase, because no intermediates have as yet been destroyed, and that the birth of individuals and the production of new forms is proceeding at a greater proportional rate than in an equally large genus of which the species are limitable.

† My friend Mr. Wallace treats of animals under domestication, not only as if they were in very different physical conditions from those in a state of nature, inasmuch as every sense and faculty is continually fully exercised and strengthened by wild animals, whilst certain of these lie dormant in the domesticated, but also as if they were subject to the influence of fundamentally different laws. He says, "No inferences as to varieties in a state of nature can be deduced from the observation of those occurring among domestic animals. The two are so much opposed that what applies to the one is almost sure not to apply to the other." But, in the first place, of the same species of wild animals some families must be placed where certain faculties and senses are far more exercised than others, and the difference in this respect between the conditions of many families of wild animals is as great as those between many wild and tame families; and secondly, other senses and faculties, latent and unknown in the wild animal, but which are as proper to the species as any it exercised in its wild state, are manifested or developed by it under domestication. An animal in a state of nature is not then, as Mr. Wallace assumes, "in the full exercise of every part of its organization;" were it so, it could not vary or alter with altered conditions, nor could other faculties remain to be called into play under domestication. The tendency of species when varying cannot be to depart from the original type in a wild condition and to revert to it under domestication, for man cannot invert the order of nature, though he may hasten or retard some of its processes.

7. Now the prominent phenomena presented by species under cultivation are analogous in kind and extent to those which we have derived from a survey of the affinities of plants in a state of nature: a large number remain apparently permanent and unalterable, and a large number vary indefinitely. Of the permanent there is little to remark, except that they belong to very many orders of plants, nor are they always those which are permanent in a state of nature. Many plants, acknowledged by all to be varieties, may be propagated by seed or otherwise, when their offspring retains for many successive generations the characters of the variety. On the other hand, species which have remained immutable for many generations under cultivation, do at length commence to vary, and having once begun, are thereafter peculiarly prone to vary further.

8. The variable cultivated species present us with the most important phenomena for investigating the laws of mutability and permanence; but these phenomena are so infinitely varied, complex, and apparently contradictory, as to defeat all attempts to elucidate the history of any individual case of variation by a study of its phases alone. It would often appear doubtful whether the natural operations of a plant tend most to induce or to oppose variation; and we hence find the advocates of original permanent creations, and those of mutable variable species, taking exactly opposite views in this respect, the truth, I believe, being that both are right. Nature has provided for the possibility of indefinite variation, but she regulates as to extent and duration; she will neither allow her offspring to be weakened or exhausted by promiscuous hybridization and incessant variation, nor will she suffer a new combination of external conditions to destroy one of these varieties without providing a substitute when necessary; hence some species remain so long hereditarily immutable as to give rise to the doctrine that all are so normally, while others are so mutable as to induce a belief in the very opposite doctrine, which demands incessant lawless change.

9. It would take far too long a time were I to attempt any analysis of the phenomena of cultivation, as illustrative of those of variability in a state of nature. There are however some broad facts which should be borne in mind in treating of variation by cross impregnation and hybridity.

10. Variation is effected by graduated changes; and the tendency of varieties, both in nature and under cultivation, when further varying, is rather to depart more and more widely from the original type, than to revert to it: the best marked varieties of a wild species occurring on the confines of the area the species inhabits, and the best marked varieties of the cultivated species being those last produced by the gardener. I am aware

that the prevalent opinion is that there is a strong tendency in cultivated, and indeed in all varieties, to revert to the type from which they departed; and I have myself quoted this opinion, without questioning its accuracy,* as tending to support the views of those who regard species as permanent. A further acquaintance with the results of gardening operations leads me now to doubt the existence of this centripetal force in varieties, or at least to believe that in the phrase "reversion to the wild type," many very different phenomena are included. In the first place, the majority of cultivated vegetables and cerealia, such as the Cabbage and its numerous progeny, and the varieties of wall-fruit, show when neglected no disposition to assume the characters of the wild states of these plants;† they certainly degenerate, and even die if Nature does not supply the conditions which man (by anticipation of her operations, or otherwise) has provided; they become stunted, hard, and woody, and resemble their wild progenitors in so far as all stunted plants resemble wild plants of similar habit; but this is not a reversion to the original type, for most of these cultivated races are not *merely* luxuriant forms of the wild parent. In neglected fields and gardens we see plants of Scotch Kale, Brussels Sprouts, or Kohl-rabi, to be all as unlike their common parent, the wild *Brassica oleracea*, as they are unlike one another; so, too, most of our finer kinds of apples, if grown from seed, degenerate and become crabs, but in so doing they become crab states of the varieties to which they belong, and do not revert to the original wild Crab-apple. And the same is true to a great extent of cultivated Roses, of many varieties of trees, of the Raspberry, Strawberry, and indeed of most garden plants. It has also been held, that by imitating the conditions under which the wild state of a cultivated variety grows, we may induce that variety to revert to its original state; but, except in the false sense of reversion above explained, I doubt if this is supported by evidence. Cabbages grown by the seaside are not more like wild Cabbages than those grown elsewhere, and if cultivated states disseminate themselves along the coast, they there retain their cultivated form. This is however a subject which would fill a volume with most instructive matter for reflection, and which receives a hundredfold more illustration from the Animal than from the Vegetable Kingdom. I can here only indicate its bearing on the doctrine of variation, as evidence that Nature operates upon mutable forms by allowing great variation, and displaying

* Fl. N. Zeal., Introd. Essay, p. x, and Flora Indica, Introduction, p. 14.

† Hence the great and acknowledged difficulty of determining the wild parent species of most of our cultivated fruits, cerealia, etc., and in fact of almost every member of our Flora Cibaria. This would not be so were their any disposition in the neglected cultivated races to revert to the wild form.

little tendency to reversion.* With this law the suggestive observation of M. Vilmorin well accords, that when once the constitution of a plant is so broken that variation is induced, it is easy to multiply the varieties in succeeding generations.

It may be objected to this line of argument that our cultivated plants are, as regards their constitution, in an artificial condition, and are, if unaided, incapable of self-perpetuation; but an artificially induced condition of constitution is not necessarily a diseased or unnatural one, and, so far as our cultivated plants are concerned, all we do is to place them under conditions which Nature does not provide *at the same particular place and time.* That Nature might supply the conditions at other places and times may be inferred from the fact that the plant is found to be provided with the means of availing itself of them when provided, while at the same time it retains all its functions, not only unimpaired, but in many cases in a more highly developed state. We have no reason to suppose that we have violated Nature's laws in producing a new variety of wheat,—we may have only anticipated them; nor is its constitution impaired because it cannot, unaided, perpetuate its race; it is in as sound and unbroken health and vigor during its life as any wild variety is, but its offspring has so many enemies that they do not perpetuate its race. In the case of annual plants, those only can secure the succession of their species which produce more seeds annually than can be eaten by animals or destroyed by the elements. Cultivated wheat will grow and ripen its seed in almost all soils and climates, and as its seeds are produced in great abundance, and can be preserved alive in any quantity, in the same climate, and for many years, it follows that it is not to the artificial or peculiar condition of the plant itself, and still less to any change effected by man upon it, that its annual extinction is due, but to causes that have no effect whatever upon its own constitution, and over which its constitutional peculiarities can exercise no control.

11. Again, the phenomena of cross impregnation amongst individuals of all species appear, according to Mr. Darwin's accurate observations, to have been hitherto much underrated, both as to extent and importance. The prominent fact that the stamens and pistil are so often placed in the same flower, and come to maturity at the same epoch, has led to the doctrine that flowers are usually self-impregnated, and that the effect is a conservative one as regards the permanence of specific forms. The ob-

* It is not meant by this that any character of a species which may be lost in its variety never reappears in the descendants of the latter, for some occasionally do so in great force; what is meant is, that the newly acquired characters of the variety are never so entirely obliterated that it has no longer a claim to be considered a variety.

servations of Carl Sprengel and others have, however, proved that this is not always the case, and that while Nature has apparently provided for self-fertilization, she has often insiduously counteracted its operation, not only by placing in flowers lures for insects which cross-fertilize them, but often by interposing insuperable obstacles to self-fertilization, in the shape of structural impediments to the access of the pollen to the stigma of its own flower.* In all these instances the double object of Nature may be traced; for self-impregnation (or "breeding in"), while securing identity of form in the offspring, and hence hereditary permanence, at the same time tends to weakness of constitution, and hence to degeneracy and extinction: on the other hand, cross-impregnation, while tending to produce diversity of form in the offspring, and hence variation and apparent mutability, yet by strengthening the offspring favors longevity and apparent permanence of specific type. The ultimate effect of all these operations is of course favorable to the hypothesis that variability is the rule, and permanence the exception, or at any rate only a transitory phenomenon.

12. Hybridization, or cross-impregnation between species or very well marked varieties, again, is a phenomenon of a very different kind, however similar it may appear in operation and analogous in design. Hybridizable genera are rarer that is generally supposed, even in gardens, where they are so often operated upon, under circumstances the most favorable to the production of a hybrid, and unfavorable to self-impregnation. Hybrids are almost invariably barren, and their characters are not those of new varieties. The obvious tendency of hybridization between varieties or other very closely allied forms (in which case the offspring may be fertile) is not to enlarge the bounds of variation, but to contract them; and if between very different forms, it will only tend to confound these. That some supposed species may have their origin in hybridization cannot be denied, but we are now dealing with phenomena on a large scale, and balancing the tendencies of causes uniformly acting, whose effects are unmistakable, and which can be traced throughout the Vegetable Kingdom. In gardening operations the number of hybridized genera is small, their offspring doomed, and since they are more readily impregnated by the pollen of either parent than by their own, or by that of any other plant,† they eventu-

* Thus, in *Lobelia fulgens*, the pollen is entirely prevented by natural causes from reaching the stigma of its own flower. In kidney beans impregnation takes place imperfectly except the carina is worked up and down artificially, which is effected by bees, who may thus either impregnate the flower with its own pollen or with that brought from another plant. I am indebted to Mr. Darwin for both these facts: see 'Gardener's Chronicle,' 1858, p. 828.

† A very able and careful experimenter, M. Naudin, performed a series of experiments at the Jardin des Plantes at Paris, in order to discover the duration of the

ally revert to one of their parents: on the other hand, the number of varieties is incalculable, the power to vary further is unimpaired in their progeny, and these tend to depart further and further in sensible properties from the original parent.

In conformity with my plan of starting from the variable and not the fixed aspect of Nature, I have now set down the prominent features of the Vegetable Kingdom, as surveyed from this point of view. From the preceding paragraphs the evidence appears to be certainly in favor of proneness to change in individuals, and of the power to change ceasing only with the life of the individual; and we have still to account for the fact that there are limits to these mutations, and laws that control the changes both as to degree and kind; that species are neither visionary nor even arbitrary creations of the naturalist; that they are, in short, realities, whether only temporarily so or not.

13. Granting then that the tendency of nature is first to multiply forms of existing plants by graduated changes, and next by destroying some to isolate the rest in area and in character, we are now in a condition to seek some theory of the *modus operandi* of Nature that will give temporary permanence of character to these changelings. And here we must appeal to theory or speculation; for our knowledge of the history of species in relation to one another, and to the incessant mutations of their environing physical conditions, is far too limited and incomplete to afford data for demonstrating the effects of these in the production of any one species in a native state.

Of these speculations by far the most important and philosophical is that of the delimitation of species by natural selection, for which we are indebted to two wholly independent and original thinkers, Mr. Darwin and Mr. Wallace.* These authors assume that all animal and vegetable forms are variable, that the average amount of space and annual supply of food for each species (or other group of individuals) is limited and constant, but that the increase of all organisms tends to proceed annually in a geometrical ratio; and that, as the sum of organic life on the surface of the globe does not increase, the individuals annually destroyed must be incalculably great; also that each species is ever warring against many enemies, and only holding its own by a slender tenure. In the ordinary course of nature this

progeny of fertile hybrids. He concludes that the fertile posterity of hybrids disappears, to give place to the pure typical form of one or other parent. "Il se peut sans doute qu'il y ait des exceptions à cette loi de retour, et que certains hydrides, à la fois très-fertiles et très-établis, tendent à faire souche d'espèce; mais le fait est loin d'être prouvé. Plus nous observons les phénomènes d'hybridité, plus nous inclinous à croire que les espèces sont indissolublement liées à une fonctions dans l'ensemble des choses, et que c'est le rôle même assigné à chacune d'elles qui en détermine la forme, la dimension et la durée." (Annales des Sc. Nat., sér. 4, v. 9.)

* Journal of the Linnean Society of London, Zoology, vol. iii, p. 45.

annual destruction falls upon the eggs or seeds and young of the organisms, and as it is effected by a multitude of antagonistic, ever-changing natural causes, each more destructive of one organism than of any other, it operates with different effect on each group of individuals, in every locality, and at every returning season. Here then we have an infinite number of varying conditions, and a superabundant supply of variable organisms, to accommodate themselves to these conditions. Now the organisms can have no power of surviving any change in these conditions, except they are endowed with the means of accommodating themselves to it. The exercise of this power may be accompanied by a visible (morphological) change in the form or structure of the individual, or it may not, in which case there is still a change, but a physiological one, not outwardly manifested; but there is always a morphological change if the change of conditions be sudden, or when, through lapse of time, it becomes extreme. The new form is necessarily that best suited to the changed condition, and as its progeny are henceforth additional enemies to the old, they will eventually tend to replace their parent form in the same locality. Further, a greater proportion of the seeds and young of the old will annually be destroyed than of the new, and the survivors of the old, being less well adapted to the locality, will yield less seed, and hence have fewer descendants.

In the above operations Nature acts slowly on all organisms, but man does so rapidly on the few he cultivates or domesticates; he selects an organism suited to his own locality, and by so modifying its surrounding conditions that the food and space that were the share of others falls to it, he ensures a perpetuation of his variety, and a multiplication of its individuals, by means of the destruction of the previous inhabitants of the same locality; and in every instance, where he has worked long enough, he finds that changes of form have resulted far greater than would suffice to constitute conventional species amongst organisms in a state of nature, and he keeps them distinct by maintaining these conditions.

Mr. Darwin adduces another principle in action amongst living organisms as playing an important part in the origin of species, viz., that the same spot will support most life when peopled with very diverse forms, as is exemplified by the fact that in all isolated areas the number of classes, orders, and genera is very large in proportion to that of species.

§ 3. *On the General Phenomena of Distribution in Area.*

Turning now to another class of facts, those that refer to the distribution of plants on the surface of the globe, the following are the most obvious:—

14. The most prominent feature in distribution is that circumscription of the area of species, which so forcibly suggests the hypothesis that all the individuals of each species have sprung from a common parent, and have spread in various directions from it. It is true that the area of some (especially cryptogamic and aquatic plants) is so great that we cannot indicate any apparent centre of diffusion, and that others are so sporadic that they appear to have had many such centres; but these species, though more numerous than is usually supposed, are few in comparison with those that have a definite or circumscribed area.

With respect to this limitation in area,* species do not essentially differ from varieties on the one hand, or from genera and higher groups on the other; and indeed, in respect of distribution, they hold an exactly intermediate position between them, varieties being more restricted in locality than species, and these again more than genera.

The universality of this feature (of groups having defined areas) affords to my mind all but conclusive evidence in favor of the hypothesis of similar forms having had but one parent, or pair of parents. And further, this circumscription of species and other groups in area, harmonizes well with that principle of divergence of form, which is opposed to the view that the same variety or species may have originated at different spots. It also follows that, as a general rule, the same species will not give rise to a series of similar varieties (and hence species) at different epochs; whence the geological evidence of contemporaneity derived from identity of fossil forms may be relied upon.

The most obvious cause of this limitation in area no doubt exists in the well-known fact that plants do not necessarily inhabit those areas in which they are constitutionally best fitted to thrive and to propagate; that they do not grow where they would most like to, but where they can find space and fewest enemies. We have seen (13) that most plants are at warfare with one or more competitors for the area they occupy, and that both the number of individuals of any one species and the area it covers are contingent on the conditions which determine these remaining so nicely balanced that each shall be able at least to

* It is a remarkable fact that there are some striking anomalies in the distribution of plants into provinces, as compared with animals. Thus there is no peculiarity in the vegetation of Australia to be compared with the rarity of placental mammals, nor with the fact of so many of the mammals, birds, and fish of Tasmania differing from those of the continent of Australia. Nearer home, we find the basin of the Mediterranean with a tolerably uniform flora on the European and North African sides, but these ranking as different zoological provinces. The much narrower delimitation in area of animals than plants, and greater restriction of faunas than floras, should lead us to anticipate that plant types are, geologically speaking, more ancient and permanent than the higher animal types are, and so I believe them to be, and I would extend the doctrine even to plants of highly complex structure.

hold its own, and not succumb to the enervating or etiolating or smothering influences of its neighbors. The effects of this warfare are to extinguish some species, to spare only the hardier races of others, and especially to limit the remainder both as to area and characters. Exceptions occur in plants suited to very limited or abnormal conditions, such as desert plants, the chief obstacles to whose multiplication are such inorganic and principally atmospheric causes as other plants cannot overcome at all; such plants have no competitors, are generally widely distributed, and not very variable.*

15. The three great classes of plants, Acotyledons, Monocotyledons, and Dicotyledons (Gymnospermous and Angiospermous), are distributed with tolerable equality over the surface of the globe, inasmuch as we cannot indicate any of the six continents (Europe, Asia, Africa, North and South America, and Australia) as being peculiarly rich in one to the exclusion of another. Further, the distribution of some of the larger orders is remarkably equable, as *Compositæ*, *Leguminosæ*, *Gramineæ*, and others; facts which (supposing existing species to have originated in variation) would seem to indicate that the means of distribution have overcome, or been independent of the existing apparent impediments, and that the power of variation is equally distributed amongst these classes, and continuously exerted under very different conditions. I do not mean that all the classes are equally variable, but that each displays as much variety in one continent as in another.

16. Those classes and orders which are the least complex in organization are the most widely distributed, that is to say, they contain a larger proportion of widely diffused species. Thus the species of Acotyledons are more widely dispersed than those of Monocotyledons, and these again more so than those of Dicotyledons; so also the species of *Thallophytes* are among the most widely dispersed of Acotyledons, the *Gramineæ* of Monocotyledons, and the *Chenopodiaceæ* of Dicotyledons. This tendency of the least complex species to be most widely diffused is most marked in Acotyledons, and least so in Dicotyledons,† a fact which is analogous to that already stated (4), that the least complex are also the most variable.

* Though invariable forms, they may be, and often are, themselves varieties or races of a species that inhabits more fertile spots, as *Poa bulbosa*, which is a very well-marked and constant form of *P. pratensis*, occurring in dry sandy soil, from England to Northwestern India, its "meadow" relative being a very variable species in the same countries, and always struggling for existence amongst other grasses, etc.

† Very much, no doubt, because of the difficulty in classifying Dicotyledons by complexity of organization; in other words, of our inability to estimate in a classificatory point of view the relative value of the presence or absence of organs in plants, where many are present, and where those of low morphological importance may have a comparatively high physiological significance.

17. Though we rarely find the same species running into the same varieties at widely sundered localities (unless starved or luxuriant forms be called varieties), yet we do often find a group of species represented in many distant places by other groups of allied forms; and if we suppose that individuals of the parent type have found their way to them all, the theory that existing species have originated in variation, and that varieties depart further from the parent form, will account for such groups of allied species being found at distant spots; as also for these groups being composed of representative species and genera.

18. No general relations have yet been established between the physical conditions of a country and the number of species or varieties which it contains, further than that the tropical and temperate regions are more fertile than the polar, and that perennial drought is eminently unfavorable to vegetation. It is not even ascertained whether the tropical climates produce more species than the temperate.

19. Though we cannot explain the general relations between the vegetation and physical condition of any two countries that contrast in these respects, we may conclude as a general rule that those tracts of land present the greatest variety in their vegetation that have the most varied combinations of conditions of heat, light, moisture, and mineral characters. It is, in the present state of our knowledge, impossible to measure the amount of the fluctuations of these conflicting conditions in a given country, nor if we could can we express them symbolically or otherwise so as to make them intelligible exponents of the amount of variety in the vegetation they affect; but the following facts in general distribution appear to me to be favorable to the idea that there is such a connection.

There are certain portions of the surface of the globe characterized by a remarkable uniformity in their phænogamic vegetation. These may be luxuriantly clothed, and abound in individuals, but are always poor in species. Such are the cooler temperate and subarctic lake regions of North America, Fuegia and the Falkland Islands, the Pampas of Buenos Ayres, Siberia and North Russia, Ireland and Western Scotland, the great Gangetic plain, and many other tracts of land. Now all these regions are characterized by a great uniformity in most of their physical characters, and an absence of those varying conditions which we assume to be stimulants to variation in a locality. On the other hand, it is in those tracts that have the most broken surface, varied composition of rocks, excessive climate (within the limits of vegetable endurance), and abundance of light, that the most species are found, as in South Africa, many parts of Brazil and the Andes, Southern France, Asia Minor, Spain, Algeria, Japan, and Australia.

20. The Polar regions are chiefly peopled from the colder temperate zones, and the species from the latter which have spread into them are very variable, but only within comparatively small limits, particularly in stature, color, and vesture. Many of these polar and colder temperate plants are also found, together with other species closely allied to them, on the mountains of the warm temperate, and even tropical zones; to which it is difficult to conceive that they can have been transported by agencies now in operation.

21. The floras of islands present many points of interest. The total number of species they contain seems to be invariably less than an equal continental area possesses, and the relative numbers of species to genera (or other higher groups) is also much less than in similar continental areas.

The further an island is from a continent, the smaller is its flora numerically, the more peculiar is its vegetation, and the smaller its proportion of species to genera. In the case of very isolated islands, moreover, the generic types are often those of very distant countries, and not of the nearest land. Thus the St. Helena and Ascension forms are not so characteristic of tropical Africa as of the Cape of Good Hope. Those of Kerguelen's Land are Antarctic American, not African nor Indian. The Sandwich Islands contain many Northwest American and some New Zealand forms. Japan presents us with many genera and species unknown except to the *eastward* of the Rocky Mountains, in North America.* So too American, Abyssinian, and even South African genera and species are found in Madeira and the Canary Islands; and Fuegian ones in Tristan d'Acunha.

22. There is a strict analogy in this respect between the floras of islands and those of lofty mountain-ranges, no doubt in both cases owing to the same causes. Thus, as Japan contains various peculiar N. E. American species which are not found in N. W. America nor elsewhere on the globe, and the Canaries and Azores possess American genera not found in Europe nor Africa, so the lofty mountains of Borneo contain Tasmanian and Himalayan representatives; the Himalayas contain Andean, Rocky Mountain, and Japanese genera and species; and the alps of Victoria and Tasmania contain assemblages of New Zealand, Fuegian, Andean, and European genera and species. We cannot account for any of these cases of distribution between islands and mountains except by assuming that the species and genera common to these distant localities have found their way across the intervening spaces under conditions which no longer exist.

* Whilst these sheets are passing through the press, I have been informed by Professor Asa Gray that the flora of Japan and N. E. Asia is much more closely allied to that of the Northern United States than to that of America west of the Rocky Mountains.

23. There is much to be observed in the condition and distribution of the introduced or naturalized plants of a country, which may be applied to the study of the origin of its indigenous vegetation. The greater proportion of these are the annual and other weeds of cultivated land, and plants which attach themselves to nitrogenous soils; naturalized perennials, shrubs, and trees occur consecutively in rapidly diminishing proportions. I can find no decided relation between complexity of structure and proneness to migrate, nor much between facilities for transport or power of endurance or vitality in the seed, and extent of distribution by artificial means. I shall return to this subject (which I have elsewhere discussed at length with reference to the Galapagos Archipelago*) when treating of the naturalized plants of Australia.

24. I venture to anticipate that a study of the vegetation of islands with reference to the peculiarities of their generic types on the one hand, and of their geological condition (whether as rising or sinking) on the other, may, in the present state of our knowledge, advance the subjects of distribution and variation considerably. The incompleteness of the collections at my command from the Polynesian islands, has frustrated my attempts to illustrate this branch of inquiry by extending my researches from the Australian Flora over that of the Pacific. I may however indicate as a general result, that I find the sinking islands, those (so determined by Darwin's able investigations) characterized as atolls, or as having barrier reefs, to contain comparatively fewer species and fewer peculiar generic types than those which are rising. Thus, commencing from the east coast of Africa, I find in the Indian Ocean the following islands marked in Darwin's chart† as bounded with fringing reefs or active volcanos, and hence rising:—The Seychelles, Madagascar, Mauritius, Bourbon, Ceylon, the Andamans, Nicobar, and Sumatra; the vegetation of all which is characterized by great diversity and much peculiarity of generic type: whereas those marked as atolls or barrier reefs, as the Maldives, Laccadives, and Keeling Island, contain few species, and those the same as grow on the nearest continents. In the Pacific Ocean, again, the groups of islands most remarkable for their ascertained number of very peculiar generic types are the Sandwich group, Galapagos, Juan Fernandez, Loochoo and Bonin, all of which are rising, and most have active volcanos: those with the least amount of peculiarity are the Society group and Fijis, both of which are sinking. In the present state of our knowledge it is not safe to lay much stress on these apparent facts, especially as the New Hebrides and New Caledonia, which lie very close together, and both, I believe,

* Linn. Trans., xx, 235.

† See his works on volcanic islands and on coral reefs.

contain much peculiarity, are in opposite geological conditions, the Hebrides rising and Caledonia sinking; and the Friendly* and Fiji groups, equally near one another, and with, I suspect, very similar vegetation, are also represented as being in opposite conditions. On the other hand, in the whole of the group including the Low Archipelago and the Society Islands, extending over more than 2000 miles, I observe but one spot,† namely, Elizabeth Island, a mere speck of land, but which is the only known habitat of one of the most remarkable genera of *Compositæ*.‡

25. Many of the above facts in the general distribution of species cannot be wholly accounted for by the supposition that natural causes have dispersed them over such existing obstacles as seas, deserts, and mountain-chains; moreover, some of these facts are opposed to the theory that the creation of existing species has taken place subsequent to the present distribution of climates, and of land and water, and to that of their dispersion having been effected by the now prevailing aquatic, atmospheric, and animal means of transport.

Similar climates and countries, even when altogether favorably placed for receiving colonists from each other, and with conditions suitable to their reciprocal exchange, do not, as a rule, interchange species. Causes now in operation will not account for the fact that only 200 of the New Zealand flowering plants are common to Australia, and still less for the contrasting one that the very commonest, most numerous, and universally distributed Australian genera and species, as *Casuarina*, *Eucalyptus*, *Acacia*, *Boronia*, *Helichrysum*, *Melaleuca*, etc., and all the Australian *Leguminosæ* (including a European genus and species), are absent from New Zealand. Causes now in operation cannot be made to account for a large assemblage of flowering plants characteristic of the Indian peninsula being also inhabitants of tropical Australia, while not one characteristic Australian genus has ever been found in the peninsula of India. Still less will these causes account for the presence of Antarctic and European species in the Alps of Tasmania and Victoria, or for the reappearance of Tasmanian genera on the isolated lofty mountain of Kina-Balou, in Borneo.

* I find that there is a remarkable difference between the floras of the New Hebrides and Caledonia on the one hand, and those of the Fiji islands and those to the east of them on the other. In the former, New Zealand and Australian types abound; in the latter, almost exclusively Indian forms. The differences between the floras of Fiji, Samoa, Tonga, Tahiti, and that of India, are in species and not in genera, and many species are common to all.

† Mr. Darwin has left Aurora Island (another of the group) uncolored, on account of the doubtful evidence regarding it, which however is in favor of its being in the same condition as Elizabeth's Island. From a list of species communicated by Mr. Dana, it appears to contain no peculiar plants.

‡ *Fitchia*. See Lond. Journ. Bot. 1845, iv, p. 640, t. 23, 24. [A specimen of this plant was gathered by Prof. Dana on the mountains of Tahiti.—Eds.]

These and a multitude of analogous facts have led to the study of two classes of agents, both of which may be reasonably supposed to have had a powerful effect in determining the distribution of plants; these are changes of climates, and changes in the relative positions and elevations of land.

26. Of these, that most easy of direct application is the effect of humidity in extending the range of species into regions characterized by what would otherwise be to them destructive temperatures.

I have, in the 'Antarctic Flora,' shown that the distribution of tropical forms is extended into cold regions that are humid and equable further than into such as are dry and excessive; and, conversely, that temperate forms advance much further into humid and equable tropical regions than into dry and excessive ones; and I have attributed the extension of Tree-ferns, Epiphytal Orchids, Myrtaceæ, etc., into high southern latitudes, to the moist and equable climate of the south temperate zone. I have also shown how conspicuously this kind of climate influences the distribution of mountain plants in India, where tropical forms of Laurel, Fig, Bamboo, and many other genera, ascend the humid extratropical mountains of Eastern Bengal and Sikkim to fully 9000 feet elevation; and temperate genera, and in some cases species, of Quercus, Salix, Rosa, Pinus, Prunus, Camellia, Rubus, Kadsura, Fragaria, Æsculus, etc., descend the mountains even to the level of the sea, in lat. 25°. In a tropical climate the combined effects of an equable climate and humidity in thus extending the distribution of species, often amount to 5000 feet in elevation or depression (equivalent to 15° Fahr. of isothermals in latitude), a most important element in our speculations on the comparative range of species under existing or past conditions; and when to this is added that the average range in altitude of each Himalayan tropical and temperate and alpine species of flowering plant is 4000 feet, which is equivalent to 12° of isothermals of latitude, we can understand how an elevation of a very few thousand feet might, under certain climatic conditions, suffice to extend the range of an otherwise local species over at least 28° parallels of latitude, and how a proportionally small increase of elevation in a meridional chain where it crosses the Equator, may enable temperate plants to effect an easy passage from one temperate zone to the other.

27. To explain more fully the present distribution of species and genera in area, I have recourse to those arguments which are developed in the Introductory Essay to the New Zealand Flora, and which rest on geological evidence, originally established by Sir Charles Lyell, that certain species of animals have survived great relative changes of sea and land. This doctrine, which I in that Essay endeavored to expand by a study of the

distribution of existing southern species, has, I venture to think, acquired additional weight since then, from the facts I shall bring forward under the next head of Geological Distribution, and which seem to indicate that many existing orders and genera of plants of the highest development may have flourished during the Eocene and Cretaceous periods, and have hence survived complete revolutions in the temperature and geography of the middle and temperate latitudes of the globe.

28. Mr. Darwin has greatly extended in another direction these views of the antiquity of many European species, and their power of retaining their *facies* unchanged during most extensive migrations, by his theory of the simultaneous extension of the glacial temperature in both hemispheres, and its consequent effect in cooling the tropical zone. He argues that, under such a cold condition of the surface of the globe, the temperate plants of both hemispheres may have been almost confined to the tropical zone, whence afterwards, owing to an increment of temperature, they would be driven up to the mountains of the tropics, and back again to those higher temperate latitudes where we now find most of them. I have already (New Zealand Essay) availed myself of the hypothesis of an austral glacial period, to account for Antarctic species being found on the alps of Australia, Tasmania, and New Zealand; and if as complete evidence of such a proportionally cooled state of the intertropical regions were forthcoming as there is of a glacial condition of the temperate zones, it would amply suffice to account for the presence of European and Arctic species in the Antarctic and south temperate regions, and of the temperate species of both hemispheres on the mountains of intermediate tropical latitudes.

On the other hand, we have sufficient evidence of many of what are now the most tropical orders of plants having inhabited the north temperate zone before the glacial epoch; and it is difficult to conceive how these orders could have survived so great a reduction of the temperature of the globe as should have allowed the preglacial temperate flora to cross the Equator in any longitude. It is evident that, under such cold, the most tropical orders must have perished, and their re-creation after the glacial epoch is an inadmissible hypothesis.*

* The question of the state of the mean temperature of the globe during comparatively recent geological periods is yearly deriving greater importance in relation to the problem of distribution. Upon this point geologists are not altogether clear, nor at one with the masters of physical science. Lyell (Principles, ed. ix, chap. vii) attributes the glacial epoch to such a disposition of land and sea as would sufficiently cool the temperate zones; and he implies that this involves or necessitates a lowering of the mean temperature of the whole globe. Another hypothesis is, that there was a lowering of the mean temperature of the globe wholly independent of any material change in the present relations of sea and land, which cold induced the glacial epoch. A third theory is that such a redisposition of land and sea as would induce a glacial epoch in our hemisphere need not be great, nor necessitate a decrement of the mean temperature of the whole earth.

29. It remains then to examine whether, supposing the glacial epoch of the northern and southern hemispheres to have been contemporaneous, the relations of land and sea may not have been such as that a certain meridian may have retained a tropical temperature near the Equator, and thus have preserved the tropical forms. Such conditions might perhaps be attained by supposing two large masses of land at either pole, which should contract and join towards the Equator, forming one meridional continent, while one equatorial mass of land should be placed at the opposite meridian. If the former continent were traversed by a meridional chain of mountains, and so disposed that the polar oceanic currents should sweep towards the Equator for many degrees along both its shores, its equatorial climate would be throughout far more temperate than that of the opposite equatorial mass of land, whose climate would be tropical, insular, and humid.

30. The hypothesis of former mountain chains having afforded to plants the means of migration, by connecting countries now isolated by seas or desert plains, is derived from the evidence afforded by geology of the extraordinary mutation in elevation that the earth's surface has experienced since the appearance of existing forms of animals and plants. In the Antarctic Flora I suggested as an hypothesis that the presence of so many Arctic-American plants in Antarctic America might be accounted for by supposing that the now depressed portions of the Andean chain had, at a former period, been so elevated that the species in question had passed along it from the north to the south temperate zone;* and there are some facts in the distribution of species common to the mountain floras of the Himalaya and Malay Islands, and of Australia and Japan, that would well accommodate themselves to a similar hypothesis. Of such submerged meridional lands we have some slender evidence in the fact that, in the meridian of Australia and Japan, we have, first, the north-west coast of Australia sinking, together with the Louisiade archipelago to its north; then, approaching the line, the New Ireland group is sinking, as are also the Caroline Islands, in lat 7° N. Beyond this, however, in lat. 15° N., are the Marianne Islands (rising) of whose vegetation nothing is known; in 27° N., the

* The continuous extension of so many species along the Cordillera (of which detailed evidence is given in the Antarctic Flora) from the Rocky Mountains to Fuegia, is a most remarkable fact, considering how great the break is between the Andes of New Granada and those of Mexico, and that the intermediate countries present but few resting-places for alpine plants. That this depression of the chain has had a powerful effect in either limiting the extension of species which have appeared since its occurrence, or in inducing changes of climate which have extinguished species once common to the north and south, is evidenced by the fact that a number of Fuegian and South Chili plants extend northward as alpines to the very shores of the Gulf of Mexico, but do not inhabit the Mexican Andes, whilst as many Arctic species advance south to the Mexican Andes, but do not cross the intermediate depression and reappear in the Bolivian Andes.

Bonin Islands (also rising); and in 30° is Japan, with which this botanical relationship exists.

It is objected by Mr. Darwin to this line of argument (as to that on p. 15, concerning the Pacific Islands), that all these sinking areas are volcanic islands, having no traces of older rocks on them. But I do not see that this altogether invalidates the hypothesis; for many of the loftiest mountains throughout the Malayan Archipelago, New Zealand, and the Pacific Islands, are volcanic; some are active, and many attain to 14,000 feet in elevation, whilst the lower portions of some of the largest of these islands are formed of rocks of various ages.

(*To be continued.*)

ART. II.—*Some General Views on Archæology;* by A. MORLOT.*

A CENTURY scarcely has elapsed since the time when it would have been thought impossible to reconstruct the history of our globe, prior to the appearance of mankind. But, though contemporary historians were wanting during this immense pre-human era, the latter has not failed in leaving us a well-arranged series of most significant vestiges: the animal and vegetable tribes, which have successively appeared and disappeared, have left their fossil remains in the successively deposited strata. Thus has been composed, gradually and slowly, a history of creation, written, as it were, by the Creator himself. It is a great book, the leaves of which are the stratified rocks, following each other in the strictest chronological order, the chapters being the mountain-chains. This great book has long been closed to man. But science, constantly extending its realm and improving its method of induction, has taught the geologist to study those marvellous archives of creation, and we behold him now unfolding the past ages of our world, with a variety of details and a certainty of conclusions well calculated to inspire us with grateful admiration.

The development of archæology has been very similar to that of geology. Not long ago we should have smiled at the idea of reconstructing the by-gone days of our race, previous to the first beginning of history properly so called. The void was filled up, partly by representing that ante-historical antiquity as having been only of short duration, and partly by exaggerating the value and the age of those vague and confused notions which constitute tradition.

* This article is an introduction to a paper entitled, Geologico-Archæological Studies in Denmark and Switzerland, appearing in the *Bulletin de la Société Vaudoise des Sciences Naturelles,* for 1859, and of which a separate edition, comprising the present pages, will be published.

It seems to be with mankind at large as with single individuals. The recollections of our earliest childhood have entirely faded away, up to some particular event which had struck us more forcibly, and which alone has left a lasting image amidst the surrounding darkness. Thus, excepting the idea of a deluge, which exists among so many nations, and therefore appears to have originated before the migration of those same nations, the infancy of mankind, at least in Europe, has passed without leaving any recollection, and history fails here entirely: for what is history but the memory of mankind?

But, before the beginning of history, there have been life and industry, of which various monuments still exist, while others lie buried in the soil, much as we find the organic remains of former creations entombed in the strata composing the crust of the globe. The memorials of antiquity enact here a part similar to that of the fossils; and if Cuvier calls the geologist an antiquarian of a new order, we can reverse that remarkable saying, and consider the antiquarian as a geologist, applying his method to reconstruct the first ages of mankind, previous to all recollection, and to work out what may be called pre-historical history. This is archæology pure and proper. But archæology cannot be considered as coming to a full stop with the first beginning of history. For the further we recede in our historical researches, the more incomplete they become, leaving gaps which the study of the material remains helps to fill up. Archæology therefore pursues its course in a parallel line with that of history, and henceforth the two sciences mutually enlighten each other. But, with the progress of history, the part taken by archæology goes on decreasing, until the invention of printing almost brings to a close the researches of the antiquarian.

To pursue geological investigations we must first examine the present state of our planet and observe its changes; that is, we must begin by physical geography. This supplies us with a thread of induction, to guide us safely in our rambles through the passed ages of our earth, as Lyell has so admirably set forth. For the laws which govern the organic creation and the inorganic world are as invariable as the results of their combinations and permutations are infinitely varied; science revealing to us every where the perfect stability of the causes with the diversity of the forms.

So, to understand the past ages of our species, we must first begin by examining its present state, following man wherever he has crossed the waters and set his foot upon dry land: the different nations, which inhabit our earth at present, must be studied with respect to their industry, their habits and their general mode of life. We thus make ourselves acquainted with the different degrees of civilization, ranging from the highest summit of modern development to the most abject state, hardly surpassing that of the brute. By that means ethnography supplies us with what may be called a contemporaneous scale of development, the stages of which are more or less fixed and invariable, whilst archæology traces a scale of successive development with one moveable stage, passing gradually along the whole line.*

Ethnography is consequently to archæology what physical geography is to geology, namely a thread of induction in the labyrinth of the past, and a starting point in those comparative researches of which the end is the knowledge of mankind and of its development through successive generations.

In following out the principles above laid down, the Scandinavian savans have succeeded in unravelling the leading features in the progress of pre-historical European civilization, and in distinguishing three principal eras, which they have called the *stone-age*, the *bronze-age*, and the *iron-age.*†

This great conquest in the realm of science is due chiefly to the labors of Mr. Thomsen, director of the ethnographical and archæological museums at Copenhagen‡ and to those of Mr. Nilsson, professor at the flourishing University of Lund in Sweden.§

These illustrious veterans among northern antiquaries have ascertained that our Europe, at present so civilized, was at first inhabited by tribes to whom the use of metals was totally unknown, and whose industry and domestic habits must have borne a considerable analogy to what we now see practised among certain savages. Bone, horn and especially flint were then used instead of metal for manufacturing cutting-instruments and arms. This was the *stone-age,* which might also be called the first great phase of civilization.

The earliest settlers in Europe apparently brought with them the art of producing fire. By striking iron-pyrites (sulphuret of iron) against quartz, fire can be easily obtained. But this method can only have been occasionally used, and seems to have been confined to some native tribes in Tierra del Fuego.‖ The usual mode has evidently been that of rubbing two sticks together. But on further reflection it is easy to perceive that this was a

* Some naturalists see a correspondence of the same sort between embryology and comparative anatomy; for they consider the human embryo as passing during its development through the different stages of the scale of animal creation, or, at least, as passing through the different states of the embryos of the different stages of that scale.

† The history of Danish archæology has been sketched by T. Hindenburg. See *Dansk maanedsskrift*, i, 1859.

‡ *Ledetraad til nordisk Oldkyndighed, Kjoebenhavn*, 1836. Published in English by Lord Ellesmere, under the title, "A Guide to Northern Antiquities. London, 1848."

§ Nilsson: *Scandinaviska Nordens Urinvonare.* Lund, 1838—1843.

‖ Weddell: A Voyage towards the South Pole in 1822–1824. London, 1827, p. 167

most difficult discovery, and must at all events have been preceded by a knowledge of the use of fire, as derived from the effects of lightning or from volcanic action.

The stone-age was therefore probably preceded by a period, perhaps of some length, during which man was unacquainted with the art of producing fire. This, according to Mr. Flourens, indicates that the cradle of mankind was situated in a warm climate.*

The art of producing fire has been perhaps the greatest achievement of human intelligence. The use of fire lies at the root of almost every species of industry. It enables the savage to fell trees, as it allows civilized nations to work metals. Its importance is so great, that deprived of it man would perhaps scarcely have risen above the condition of the brute. Even the ancients were sensible of this, as is witnessed by the fable of Prometheus. As to their sacred perpetual fire, its origin seems to lie in the difficulty of procuring fire, thereby rendering its preservation essential.

In Europe the stone-age came to an end by the introduction of bronze. This metal is an alloy of about nine parts of copper and one part of tin.† It melts and moulds well; the molten mass in cooling slowly acquires a tolerable degree of hardness, inferior to that of steel, it is true, but superior to that of very pure iron. We therefore understand how bronze would long be used for manufacturing cutting-instruments, weapons and numerous personal ornaments. The northern antiquaries have very properly called this second great phase in the development of European civilization the *bronze-age*.

The bronze articles of this period, with a few trifling exceptions, have not been produced by hammering, but have been regularly cast, often with a considerable degree of skill. Even the sword-blades were cast, and the hammer (of stone) was only used to impart a greater degree of hardness to the edge of the weapon.

The bronze-age has therefore witnessed a mining industry, which was completely wanting during the stone-age. Now the art of mining is so essential to civilization, that without it the world would perhaps yet be exclusively inhabited by savages.

* Flourens: *De la longévité humaine.* Paris, 1855, p. 127. "Man, from the construction of his teeth, his stomach, and his intestines, is primitively *frugivorous*, like the monkey. But the frugivorous diet is the most unfavorable, because it constrains its followers perpetually to abide in those countries which produce fruit at all seasons, consequently in warm climates. But, when once the art of cooking was introduced, and applied both to vegetable and animal productions, man could extend and vary the nature of his diet. Man has consequently two diets; the first is primitive, natural and *instinctive*, and by it he is *frugivorous*, the second is artificial, being due entirely to his intelligence, and by it he is *omnivorous*."

† Bronze is still used for casting bells, cannon, and certain portions of machinery. It must not be confounded with common brass, which is a compound of copper and zinc, much less hard, and appearing only in the iron-age.

It is then worth our while to enquire more closely into the origin of bronze.

Copper was not difficult to obtain. In the first place, virgin copper is not particularly scarce. Then, the different kinds of ore which contain copper combined with other elements, are either highly colored, or present a marked metallic appearance, and are consequently easily known; they are besides not hard to smelt, so as to separate the metal. Finally, copper-ore is not at all scarce; it is met with in the older geological series of most countries.

Virgin tin is unknown, but tin ore is heavy, of dark color and very easy to smelt. However frequent copper may be, tin is of rare occurence. Thus the only mines in Europe which produce tin at the present day are those of Cornwall in England, and of the Erzgebirge and Fichtelgebirge in Germany.

But the question arises, whether, previous to the discovery of bronze, man, owing to the great rarity of tin, may not have begun by using copper in a pure state. If so, there would have been a copper-age between the stone-age and the bronze-age.

In America this has been really the case. When discovered by the Spaniards, both the two centres of civilization, Mexico and Peru, had bronze, composed of copper and tin, and used it for manufacturing arms and cutting-instruments in the absence of iron and steel, which were unknown in the New World. But the admirable researches of Messrs. Squier and Davis in the antiquities of the Mississippi valley* have brought to light an ancient civilization of a remarkable nature, and distinguished by the use of raw virgin copper, worked in a cold state, by hammering, without the aid of fire. The reason of its being so worked lies in the nature of pure copper, which when melted flows sluggishly and is not very fit for casting. A peculiar characteristic of the metal, that of occasionally containing crystals of virgin silver, betrays its origin, and shows that it was brought from the neighborhood of Lake Superior. This region is still rich in metallic copper, of which single blocks, attaining a weight of fifty tons, have lately been discovered. There was even found at the bottom of an old mine a great mass of copper, which the ancients had evidently been unable to raise, and which they had abandoned, after having cut off the projecting parts with stone hatchets.†

The date of that American copper-age is unknown. All we know is, that it must reach at least as far back as ten centuries, that space of time being deemed necessary for the growth of the

* Squier and Davis: Ancient Monuments of the Mississippi Valley. Smithsonian Contributions to Knowledge; Washington, 1848. It is one of the most splendid archæological works ever published.

† Lapham: The Antiquities of Wisconsin. Smithsonian Contributions to Knowledge, 1855, p. 76.

virgin forests now flourishing upon the remains of that antique civilization, of which the modern Indians have not even retained a tradition.

It is finally worthy of remark, that the *mound-builders,* as the Americans call the race of the copper-age, seem to have immediately preceded and prepared the way for the Mexican civilization, destroyed by the Spaniards; for, in progressing southwards, a gradual transition is noticed from the ancient earthworks of the Mississippi valley to the more modern constructions of Mexico, as found by Cortez.

In Europe the remains of a copper-age are wanting. Here and there a solitary hatchet of pure copper is found. But this can be easily accounted for by the greater frequency of copper, while tin had usually to be brought from a greater distance, so that its supply was more precarious.

As Europe did not witness a regular development of a copper-age, it seems, according to Mr. Troyon's very just remark, that the art of manufacturing bronze was brought from another quarter of the world, where it had been previously invented. It was most probably some region in Asia, producing both copper and tin, where those two metals were first brought into artificial combination, and where also traces of a still earlier copper-age are likely to be found.

An apparently serious objection might be started here by raising the question, how mines could be worked without the aid of steel. This however is sufficiently explained by the fact, that the hardest rocks can be easily managed through the agency of fire. By lighting a large fire against a rock, the latter is rent and fissured, so as considerably to facilitate its quarrying. This method was frequently employed when wood was cheaper, and is even practised at the present day in the mines of the Rammelsberg in Germany, where it facilitates the working of a rock of extreme hardness.

That metal of dingy and sorry appearance, but more truly precious than gold or the diamond—iron—at length appears, giving a wonderful impulse to the progressive march of mankind, and characterising the third great phase in the development of European civilization, very properly called the *iron-age.*

Our planet never produces iron in its metallic or virgin state, for the simple reason, that it is too liable to oxydation. But among the aerolites there are some composed of pure iron with a little nickel, which alters neither the appearance nor sensibly the qualities of the metal. Thus the celebrated meteoric iron discovered by Pallas in Siberia was found by the neighboring blacksmiths to be malleable in a cold state.* Meteoric iron has even been

* Pallas: *Voyages en Russie,* Paris, 1793, iv, 595. There was but one mass of this meteoric iron; it weighed 1600 pounds.

worked by tribes to whom the use of common iron was unknown. Thus Amerigo Vespucci speaks of savages near the mouth of the La Plata, who had manufactured arrow-heads with iron derived from an aërolite.* Such cases are certainly of rare occurrence, but they are not without their importance, for they explain how man may probably have first become acquainted with iron, and they also account for the occasional traces of iron in tombs of the stone-age, if indeed this fact be well established.

It is notwithstanding evident, that the regular working of terrestrial iron-ore must have been a necessary condition of the commencement and progress of the iron-age.

Now, iron-ore is widely diffused in most countries, but it has usually the look of common stones, being distinguished more by its weight than its color. Moreover its smelting requires a much greater degree of heat than copper or tin, and this renders its production considerably more difficult than that of bronze.

But, even when iron had been obtained, what groping in the dark and how much accumulated experience did it not require, to bring forth at will bar-iron or steel! Chance, if chance there be, may have played a part in it. But as chance only favors those privileged mortals who combine a keen spirit of observation with serious meditation and with practical sense, the discovery was not less difficult or less meritorious. We need not then be surprised, if man arrived but tardily at the manufacture of iron and steel, which is still daily improving.

In Carinthia traces of a most primitive method of producing iron have been noticed. The process seems to have been as follows: on the declivity of a hill was dug an excavation, in which was lighted a large fire; when this began to subside, fragments of very pure ore (hydrous-oxyd) were thrown into it and covered by a new heap of wood. When all the fuel had been consumed, small lumps of iron would then be found among the ashes.† All blowing apparatus was in this manner dispensed with; an important fact when we come to consider how much the use of a blast complicates metallurgical operations, because it implies the application of mechanics. Thus certain tribes in Southern Africa, although manufacturing iron and working it tolerably well, have not achieved the construction of our common kitchen bellows, apparently so simple; they blow laboriously through a tube, or by means of a bladder affixed to it.

The Romans produced iron by the so-called Catalonian process, and the remains of Roman works of that description have been discovered and investigated in Upper Carniola in Austria.‡

* Smithsonian Contributions to Knowledge, vol. ii, art. 8, p. 178.

† Communicated to the author by mining engineers in Carinthia.

‡ *Jahrbuch der k. k. geologischen Reichsanstalt.* Wien, 1850, ii, 199. Carinthia and Upper Carniola formed part of the Roman province *Noricum*, celebrated for its iron.

The Catalonian forge is still used in the Pyrenees, where it yields tolerable results, but it consumes a large quantity of charcoal, requires much wind, and is only to be applied to pure ore, containing but a very small proportion of earthy matter producing scoriæ; for the process consists in a mere reduction with a soldering and welding together of the reduced particles, without the metal properly melting. According to the manner in which the operation is conducted, bar-iron or steel are obtained at will. This direct method dispenses with the intermediate production of cast iron, which was unknown to the ancients, and which is now the only means of producing iron on a great scale.

Silver accompanies the introduction of iron into Europe, at least in the northern parts, while gold was already known during the bronze-age. This is natural, for gold is generally found as a pure metal, while silver has usually to be extracted from different kinds of ore by more or less complicated metallurgical operations—for example, by cupellation.

With iron appear also for the first time in Europe, glass, coined money, that powerful agent of commerce, and finally the alphabet, which, as the money of intelligence, vastly increases the activity and circulation of thought,* and is sufficient of itself to characterize a new and wonderful era of progress. From thence can we date the dawn of history and of science, in particular of astronomy.

The fine arts also reveal, with the introduction of iron in Europe, a new and important element, indicating a striking advance. Already in the stone-age, but more in the bronze-age, the natural taste for art reveals itself in the ornaments bestowed upon pottery and metallic objects. These ornaments consist in dots, circles, and zigzag, spiral, and S-shaped lines, the style bearing a geometrical character, but showing pure taste and real beauty of its kind, although devoid of all delineations of animated objects, either in the shape of plants or animals. It is only with the iron-age that art, taking a higher range, rose to the representation of plants, animals, and even of the human frame. No wonder, then, if idols of the bronze-age, as well as of the stone-age, are wanting in Europe. It is to be presumed that the worship of fire, of the sun and of the moon, was prevalent in remote antiquity, at least during the bronze-age, perhaps also during the stone-age.

The preceding pages constitute a sketch, certainly very rough and imperfect, of the development of civilization. They establish however in a striking manner the fact of a progress, slow,

* "The circulation of ideas is for the mind what the circulation of specie is for commerce, a true source of wealth." C. V. de Bonstetten: *L'homme du Midi et l'homme du Nord.* Genève, 1826, p. 175.

but interrupted and immense, when the starting point is considered. The physical constitution of man has naturally benefitted by it. The details contained in the treatise, of which the present paper forms the introduction, prove that the human race has been gradually gaining in vigor and strength since the remotest antiquity.* The domestic races also, the dog first, then the horse, the ox, the sheep, have shared in this physical development. Even the vegetable soil has been gradually improving since the stone-age, at least in Denmark.

And yet there are persons who deny all general progress, seeing everywhere nothing but decay and ruin, like that worthy specimen of a northern pessimist, who exclaimed, "see how man is degenerated, he has even lost his likeness to the monkey!"

Art. III.—*On a new genus of Patelliform shells from the Cretaceous rocks of Nebraska;*† by F. B. Meek and F. V. Hayden. (With a plate.)

Genus Anisomyon, M. & H.

Etym. ἄνισος, unequal; μυών, muscle; in allusion to the unsymmetrical muscular scar.

Plate I.

Generic characters.—Shell very thin, patelliform, or obliquely conical, with an ovate, oval, or circular base; margins entire; surface nearly smooth, or only marked by obscure lines of growth, crossed on some species by fine radiating striæ; summit more or less elevated, located between the middle and the anterior end, sometimes nearly central,—immediate apex very small, and abruptly curved backwards, but not spiral; interior without a projecting lamina or other appendage. Muscular scar irregularly horse-shoe shaped, enlarged at the extremities, with the open part directed towards the shorter end of the shell; becoming abruptly attenuate, or broken into a row of minute oval or circular spots on the right posterior side;—anterior extremities connected by a slender line, which usually passes across just in front of the summit.

* This agrees perfectly with the testimony of statistics. See Quetelet, *Sur l'homme et le developpement de ses facultés.* Paris, 1835, ii, 271. This work of first-rate merit is very near akin to archæology. Mr. Quetelet has just published a new work which will certainly be even more remarkable than the first, and which the author of the present paper regrets not having had within his reach.

† The specimens belong chiefly to the collections brought from Nebraska by Lieut. G. K. Warren, U. S. Top. Eng. Full illustrations and descriptions of the species will appear in his report.

On the left side of the shell, the anterior extremity of the muscular impression (*a*, fig. 2 and 3, of Plate I.) is generally not so much enlarged as on the right, but sometimes extends slightly farther forward; posteriorly it passes around in the form of a a band to the middle of the slope behind (*b*, fig. 2 and 3), where it is abruptly enlarged and curves upwards. From this point to the larger anterior termination on the right side, there is usually only a slender line (*c*, fig. 2), which is not always quite connected with the enlarged extremity of the band-like part coming around from the left side. Generally this slender line is nearly or quite entire, while in other specimens, even of the same species, it is broken into a series of minute scars as seen at *c*, fig. 3, and in some instances it seems to be entirely obsolete, so as to leave the enlarged anterior extremity on the right, quite isolated.

In most instances, the specimens are found with the small apex (*d*, of fig. 1) broken or worn away, in which condition its former existence would scarcely be suspected. In at least one species (*A. boreälis*) this small apex seems to be perforated in the end, the minute aperture being circular, and about large enough to fairly receive the point of a pin. This may be due to accident, but the thickened and smooth margin of this little opening, as seen under a magnifier, has very much the appearance of a natural orifice. We are not sure that this exists in the other species, but suspect it does. In two of the species *A. borealis*, and an undescribed form, there are six equidistant impressed hair lines radiating from the summit down the sides, nearly or quite to the margins, but as there are no traces of such lines on some of the others, presenting the same internal characters, we infer they can scarcely be regarded as a generic character.

From the foregoing description it will be seen that the group of shells we propose to include in this genus, although having the form of *Patella*, present striking differences in the unsymmetrical character of the muscular scar, indicating fundamental peculiarities in the structure of the animal, while they are all much thinner and smoother shells than we usually see in that genus. In some specimens, where there appears to be a complete break in the muscular scar on the right posterior side, there would seem to be some analogy to the genus *Siphonaria*, but as we observe no traces of a siphonal groove passing through this gap, nor any fold in the margin opposite it, and the slender portion of the muscular impression usually passes nearly or quite across, it is scarcely possible any organ such as exists in *Siphonaria*, could have been extruded there.

The more convex species, such as *A. borealis*, are somewhat similar externally to some species of *Hipponyx*, but to say nothing of other differences, the fact that the open extremity of the horse-shoe shaped muscular scar in our shells is always turned towards the shorter end, or in other words, that the apex is placed in front of the middle instead of behind it, shows they have no affinities to that or any allied genus.

They would then seem to be perhaps more nearly related to *Acmæa* and *Gadinia* than to any other of our existing mollusca, since in both these genera the animal is more or less unsymmetrical, the former having the branchial plume exserted from the right side of the neck, and the latter a siphon occupying a groove on the right just in front of the anterior extremity of the muscular scar, which is shorter on that side than on the other. Our shells, however, differ from these genera in the peculiar attenuate or interrupted character of the muscular impression on the right posterior side, and the folding back of the apex.* In the thinness of the shell and the nature of the surface, they are most like *Acmæa*, with which we at first thought them probably identical, but adopting the opinion of M. d'Orbigny that this genus is synonymous with *Helcium* of Montfort, we referred them provisionally to the latter as the older name. Not long afterwards we observed the peculiar character of the muscular impression on an internal cast of one of the species, but at first supposed it merely due to some accident; subsequently however, we ascertained that it exists in five clearly distinct species, and cannot be regarded as an accidental or specific character.

It is probable many of the Cretaceous and Jurassic species that have been referred by different authors to the genera *Patella*, *Acmæa*, *Helcium*, &c., will be found to possess the internal characters of this genus. Judging from the figures of the Cretaceous and Jurassic species of patelliform shells we have seen in published works, specimens showing the muscular scar, have rarely been found. We have observed the characters of this genus in the following Nebraska species:—

ANISOMYON BOREALIS, (=*Hipponyx borealis*, Morton, 1842=*Helcium carinatum*, Meek & Hayden, 1856).
A. SEXSULCATUS, (=*Helcium sexsulcatum*, M. & H.).
A. ALVEOLUS, (=*Helcium alveolum*, M. & H.).
A. PATELLIFORMIS, (=*Helcium patelliforme*, M. & H.).
A. SUBOVATUS, (=*Helcium subovatum*, M. & H.).

Washington, D. C., Nov. 20, 1859.

* Dr. A. A. Gould, the well known conchologist of Boston, to whom we sent sketches of these shells, writes that he concurs with us in regarding them as being clearly distinct from all the recent genera to which such fossil forms are usually referred.

ART. IV.—*General account of the results of the discussion of the Declinometer observations made at Girard College, Philadelphia, between the years* 1840 *to* 1845, *with special reference to the eleven year period;* by A. D. BACHE, Superintendent of the U. S. Coast Survey.

[Communicated to the American Association for the Advancement of Science, by authority from the Treasury Department.]

IT is proposed to give here in outline the results of an investigation of the magnetic observations made with the declinometer, between the years 1840 to 1845, at the Girard College observatory, with special reference to the eleven year period in the amplitude of the solar-diurnal variation and the disturbances of the magnetic declination. Prof. Henry, Secretary of the Smithsonian Institution, has kindly offered to publish the memoir in full in the Smithsonian Contributions to Knowledge. It is my intention to pursue the discussion by taking up the investigation of the lunar influence on the same magnetic element.

In coöperation with the scheme adopted at the British Colonial Observatories, a series of magnetic and meteorological observations were made at the Girard College observatory with instruments purchased under the direction of the trustees of the College, the observations being made under the patronage of the American Philosophical Society, and finally completed for the use of the Topographical Bureau of the War Department. These observations were made under my direction and superintendence. The series commenced in May, 1840, and with short interruptions terminated in June, 1845, thus furnishing a five years series of magnetic observations taken bi-hourly up to Oct. 1843, and after that date hourly. The readings of each magnetic element were united into means, and were also presented graphically (in the fourth volume of the record). This was done under my direction by J. Ruth, Esq., but owing to other laborious duties the record could not be submitted to a more complete reduction. I have now resumed the subject by the assistance of Charles A. Schott, Esq., Assistant in the Coast Survey, by whom, under my immediate direction, and as my assistant in this special matter, the present paper has been prepared.*

Although other magnetic observatories furnish by their judicious geographical location, a basis for the generalization of their results, it is nevertheless desirable to obtain results from intermediate observatories as confirmations or as corrections. In the investigation of the disturbance-law at Point Barrow, when compared with the same at Toronto, a very remarkable mutual

* It may be proper to state that this work has been performed out of Office hours, and at my own expense.

relation of the law at these stations resulted from such a comparison, and farther examination may bring to light other dependencies of a mutual character.

According to the latest determination the position of the Girard College observatory is in latitude 39° 58′ 23″ (north), and in longitude 75° 10′ 05″=5h 00m 40s·3 west of Greenwich. From it Toronto bears 38° 45′ west of north (true) and is distant about 334 statute miles.

It is proposed specially to investigate the law of the eleven year period, or, as it is more frequently called, the decennial period, there being yet an uncertainty as to its precise length. It is supposed to have some direct or indirect connection with the solar spot period, which correspondence, according to late investigations by Prof. R. Wolf, is so close as even to exhibit analogous disturbances. The following discussion will afford a contribution towards the determination of the epoch of the occurrence of a minimum in certain phases of the magnetic variation and disturbances, corresponding to a minimum of the solar spots. The method of reduction is substantially the same as that adopted by General Sabine. Earlier investigations of Dr. Lamont and those by Mr. Kreil differ from his in not including the discussion of the disturbances in connection with the period in question.

As long as the magnitude of the deflection remains the only criterion by which a disturbance may be recognized as such, the adoption of any limit of deviation from the normal value of the same hour, month and year, must necessarily remain in some measure arbitrary, or, in other words, there must always remain after the separation of the disturbances a certain small amount of their effect in the remaining regular diurnal progression. To effect the separation, Peirce's criterion has been used with entire success. After a preliminary investigation as to the number of disturbances separated, the limit, as pointed out by the criterion, or a deviation of 8 scale divisions (or 3′·6 of arc) has been adopted in the present discussion, as constituting a disturbed observation. Accordingly all observations differing by that amount or more from the mean monthly value of their respective hour were marked by a pencil line. Next a new hourly mean was taken, omitting values so marked, and each observation was again examined in reference to deviation from this new mean. This process was repeated when necessary, so that in all cases, values differing eight scale divisions or more from the mean were excluded. The last mean thus obtained for each observing hour and each month has been called "the normal." These values have been tabulated and are given for each month and year separately, together with such corrections as the omissions or interruptions demanded. The bi-hourly, and afterwards the hourly readings (and their means) were made $19\frac{1}{2}^m$ after the hour so as to correspond to an even Göttingen hour (diagram A).

For the purpose of comparing the annual means of the normals, or the mean march of the regular solar diurnal variation for each year, the results have been expressed analytically by means of Bessel's formula, and by the application of the method of least squares.

Probably owing to the several accidental changes in the suspension of the bar, and consequent uncertainty in the precise amount of scale correction, the mean readings of each year, when compared with one another, exhibit differences not actually due to irregularities occasioned by declination changes. Though this question does not directly bear upon the present investigation, which mainly depends on differences of readings, it will be proper to remark that the observed increase, giving the weight ½ to the mean of 1840 and 1845 (on account of incomplete record) is under the supposition of a uniform annual change between these years, equal to 4′·50. According to Mr. Schott's latest investigation of the secular change of the declination in Philadelphia supported by observations between the years 1701 and 1855·7, the annual increase between the years 1840 to 1845 is 4′·98, a result which accords tolerably well with actual observations. According to this formula the declination on the 1st of January, 1843, the mean epoch of the present series, is 3° 32′ West with a probable error of ±10′. This declination corresponds to the scale reading 560·31, which has been deduced by taking into account the weights of the annual means.

The expressions have been thrown into curves (diagram B), and the agreement between computed and observed values is shown by the introduction of dots giving the observed reading. The probable error of any single representation is ±0′·1. By means of the formulæ the following values were computed.

For	Epoch of maximum eastern deflection.	Correspond. scale reading	Epoch of maximum western deflection.	Correspond. scale reading.	Amplitude (in arc.)
	h m	d	h m	d	′
1840	7 26 A. M.	595·67	1 34 P. M.	575·71	9·08
1841	7 49	577·96	1 49	560·21	8·06
1842	7 36	571·24	1 37	553·96	7·83
1843	7 40	569·54	1 24	553·06	7·46m
1844	7 32	556·50	1 18	539·99	7·51
1845	7 34	536·65	1 16	517·81	8·53
Mean	7 36 A. M. ± 3m		1 30 P. M. ± 4m		

The inequality constituting the ten or eleven year period is plainly exhibited in the last column, the progression of the numbers being quite regular; the year 1843 is directly indicated as the year of the minimum range of the diurnal fluctuation. By means of a special formula, deduced by least squares, and representing a single value within $\pm 0'{\cdot}11$, the month of May, 1843, is indicated as the epoch of minimum amplitude.

The discussion of the disturbances, as far as they bear on the decennial inequality, next follows, taking in also some collateral results.

The total number of observations for changes of declination recorded and discussed amount to 24,566; of these, according to the preceding investigation, 2,357 were separated as disturbances. There is one disturbed observation in every 10·4 observations. The discussion of the disturbances is divided into two parts, that of the number, and that of the amount of the deflections. Omissions in the record have been supplied by the use of proper ratios showing the law as given by the full periods, and interpolated values are enclosed within brackets. The number of disturbances in each month of the year or the annual inequality in the distribution of the disturbances has been made out for each year, and the means and ratios are also given. The principal maximum occurs in October (at Toronto in September), the secondary in April; the two minima, nearly equal in amount, occur in February and June (the first one in January at Toronto). The ratios of the number of monthly disturbances to the average number are given in the following table, showing the same also divided into westerly and easterly values.

	RATIOS.				RATIOS.		
	W.	E.	Total		W.	E.	Total.
January,	1·27	0·42	0·77	July,	0·58	1·18	0·86
February,	0·70	0·46	0·52m	August,	2·00M	1·14	1·59M
March,	0·83	0·57	0·68	September,	0·93	1·86	1·36
April,	0·95	0·80	$0{\cdot}91M_2$	October,	1·58	2·50M	2·12M
May,	0·55	0·60	0·58	November,	0·96	1·12	1·08
June,	0·44	0·61	0·53m	December,	1·21	0·74	1·00
				Mean,	1·00	1·00	1·00

The ratios show a general correspondence in the numbers of westerly and easterly deflections; the westerly seem to occur most frequently in August, while the easterly predominate in October; the secondary maximum of both series is in April. For the total number the minima occur in February and June. The following table contains the number of disturbances in each year.

1840	483	weight ½	
1841	539		
1842	446		
1843	275	weight ⅔	Proportional number of western disturbance 937, of eastern 912; at Toronto the eastern predominate over the western in the proportion of 1·17 to 1.
1844	308		
1845	264	weight ½	

These numbers do not indicate the law of the eleven year period as plainly and systematically as found by the investigation of the diurnal amplitude, yet giving proper weight, (on account of deficiencies,) the minimum number of disturbances falls in the year 1843.

If we distribute the disturbances, 1,942 in number for the even hours, according to their respective hours of occurrence, we find the following ratios:

Philadelphia mean time.	RATIOS. W.	RATIOS. E.	Total number.
h m			
0 (19½)	0·82	1·20	162
2 "	1·18	1·16	189M
4 "	1·08	0·96	168
6 "	1·35	0·80	173
8 "	1·29	0·70	161
10 "	1·32	0·88	178
Noon "	1·13	0·71	150
14 "	0·95	0·67	133m
16 "	1·07	0·78	148
18 "	0·87	0·90	143
20 "	0·40m	1·66M	167
22 (19½)	0·54	1·58	170

The numbers in each column show a regular progression; the disturbances, irrespective of their direction, have a minimum at 2 P. M., and a maximum at 2 A. M., (at Toronto the respective hours are 2 P. M. and 22 P. M.). The principal contrast is between the hours of the day and the hours of the night. In the table given above the most striking result is that the westerly disturbances have their minimum precisely at the hour (8 P. M.) when the easterly have their maximum, and the exact coincidence of this result with that deduced by General Sabine for Toronto is not less remarkable. In connection with this subject, it may be remarked that the same distinguished magnetist found a singular mutual relation to subsist between the phenomena at Toronto and Point Barrow, on the shores of the Arctic sea,—the laws of the easterly deflection at one station being found to correspond at the same local hours with those of the westerly deflections at the other station, and vice versa. This contrast therefore holds good for Philadelphia as well as Toronto.

If we classify the disturbances according to their amount, we obtain the total aggregate and mean values of a single disturbance in the different years as follows:

	Aggregate amount in scale divisions.	Mean value.	Mean value. W.	Mean value. E.
	d	′	′	′
1840	7155·5	6·70	6·52	7·20
1841	7844·4	6·61	5·93	7·07
1842	6019·1	6·11	5·53	6·70
1843	2932·2	4·85	4·85	4·85
1844	4227·3	6·21	6·21	6·21
1845	3521·4	6·02	5·25	6·84

The eleven year period is well marked in the aggregate as well as in the mean values, and the precise epoch of the minimum was found by a special formula. It took place in August, 1843, and as a resulting epoch from this and the previous determination, June, 1843 may be adopted. This is graphically represented on diagram C.

The following table gives the ratios of the aggregate amount of disturbances in each month of the year:

	Total.	W.	E.		Total.	W.	E.
January,	0·72	1·21	0·33	July,	0·87	0·46	1·29
February,	0·54m_2	0·59	0·46	August,	1·61	1·95	1·28
March,	0·66	0·73	0·56	September,	1·56	0·99	2·12
April,	0·94M_2	1·04	0·84	October,	2·06M_1	1·67	2·46
May,	0·56	0·52	0·56	November,	1·06	1·09	1·02
June,	0·42m_1	0·37	0·47	December,	1·00	1·38	0·61

In reference to the first column, the maximum amount of disturbances occurs in October (at Toronto in September); the minimum in June (as at Toronto); the secondary maximum occurs in April (the same at Toronto); the secondary minimum occurs in February (and at Toronto in January). If separated into east and west deflections, maxima occur in September (mean of August and October) and April; and minima in June and January (same as at Toronto).

The arrangement according to the hours of the day gives the following ratios, to which is added, in the last column, the diurnal disturbance variation obtained by dividing the excess of aggregate westerly over easterly values by the total number of days of observation.

Philadelphia mean time.	RATIOS.			Disturbance, variation in arc.
	W.	E.	Both combined.	
h m				′
0 (19½)	0·83	1·24	1·04	−0·16
2 "	1·16	1·10	1·13*	−0·01
4 "	1·16	0·92	1·04	+0·05
6 "	1·46	0·67	1·06	+0·24
8 "	1·39	0·67	1·02	+0·22
10 "	1·22	0·77	0·99	+0·13
Noon "	1·03	0·63	0·83	+0·11
14 "	0·98	0·63	0·80m	+0·10
16 "	0·99	0·72	0·85	+0·07
18 "	0·84	0·89	0·84	−0·02
20 "	0·38m	1·88M	1·15	−0·53
22 (19½)	0·56	1·88	1·25M	−0·47

If we compare these ratios with the corresponding numbers in a preceding table showing the bi-hourly distribution in regard to number, we find, irrespective of the direction of the deflections, the 2 P. M. minimum preserved. The maximum is earlier and occurs at 10 P. M. (At Toronto these hours are respectively 1 P. M. and 9 P. M.) At Philadelphia as well as at Toronto the ratios are nearly invariable from 10 A. M. to 6 P. M., and again from 8 P. M. to 8 A. M. The easterly maximum and westerly minimum at 8 P. M. form again a marked feature.

The law governing the disturbances during a solar day is clearly shown, and is of a systematic character. The diurnal variation caused by the disturbances, if superposed on the regular diurnal variation, would produce what is known by the name "mean diurnal variation." If we plot the disturbance curve on the same scale, or actually superpose it on the curves of the regular diurnal variation, it would hardly show to the eye, and the compound curve of the mean variation would keep within the maximum distance of a dot from the regular curve in the diagram (D). The disturbance variation has but one maximum and one minimum. Its most prominent feature is the easterly deflection at 8 o'clock ($+19\frac{1}{2}^{m}$) P. M. (at Toronto it is at 9 P. M.); the maximum deflection amounts at that hour to 32″ of arc, and to 45″ at Toronto. The greatest westerly deflection occurs at 6^{h} ($19\frac{1}{2}^{m}$) A. M. and amounts to but 14″ (at Toronto the hour is 8 A. M. with 6″, and from a five years series of observation, with 31″ of deflection). The range of the disturbance variation equals 46″. From 3 in the morning till 5 in the afternoon, the mean effect of the disturbances is to deflect the north end of the magnet to the west, and during the remaining hours (principally night hours) to the east. The westerly and easterly disturbance deflections, during a day, balance within 0′·02.

The annual inequality in the amplitude of the diurnal disturbance variation cannot satisfactorily be shown on account of the short and partly interrupted series of observations.

It is my intention to continue the discussion of the observations made at the Girard College Observatory.

After the above was written No. 1185 of the Astronomische Nachrichten came to hand, containing Prof. R. Wolf's interesting results on the close connection of the variation in the frequency of the solar spots, and the corresponding inequality in the amplitude of the diurnal variation of the declination. He deduces for Munich the simple formula, $\beta = 6'{\cdot}273 + 0'{\cdot}051\,\alpha$, where α represents a relative number expressive of the frequency of the solar spots, directly derived from observation, and β the amplitude of the diurnal variation. He found a close correspondence between these phenomena, showing the observed and computed amplitude for the Munich observations between 1835 and 1850. The average length of the solar spot period is reaffirmed to be 11·11 years ± 0·04 years. For Philadelphia we obtain $\beta = 7'{\cdot}080 + 0'{\cdot}039\,\alpha$, which formula represents the observations as follows:

Year.	a From solar spot observations.	Amplitude derived from a.	Observed amplitude.	Difference.
1840	51·8	9′·10	9′·08	−0′·02
1841	29·5	8·23	8·06	−0·17
1842	19·2	7·83	7·83	0·00
1843	8·4	7·41	7·46	+0·05
1844	12·2	7·55	7·51	−0·04
1845	32·4	8·34	8·53	+0·19

The correspondence between the observed diurnal amplitude and the same computed from observations of the solar spots is further exhibited in the annexed diagram (E). The dotted curve is the approximate Toronto curve from observation specially introduced to show the agreement at the epoch of the maximum in 1848. By computation from the solar spot observations, the amplitude at that time would amount to 11′·00, the whole range of the inequality of the diurnal variation would therefore be 11′·00−7′·46=3′·54.

It is much to be desired that this interesting branch of physical enquiry be further studied, as it forms one of the links connecting terrestrial with cosmical phenomena.

Art. V.—*A Visual Method of effecting a Precise Automatic Comparison of Time between distant stations;* by Jonathan Homer Lane. (With a plate.)

The visual apparatus of which I here give a general description was invented several years ago, and is intended to supply the place, under certain circumstances, of the electric telegraph, in the determination of differences of longitude. Although the wires of the electric telegraph, when suspended in the air, appear to leave nothing to be desired, at least for distances of a few hundred miles, in situations where they are available, yet it has appeared to me that the visual method I propose may prove useful in many cases where the stations to be compared, particularly the astronomical stations in a trigonometrical survey, are removed to considerable distances from lines of electric telegraph. In those situations, also, where submarine or subterranean lines take the place of air lines, the visual method, on account of the comparatively slow velocity of the electric signal along the wires of such lines, and the open and irresolvable question whether the signal time might not be greater in one direction than the other, would be capable of furnishing a useful check upon the indications of the electric telegraph.

The general features of the method are the following:

First, an intense light shown at one station, A, and viewed at the other station, B, as a star.

Secondly, a uniform automatic movement at A, made to interrupt the light at regular minute periods of say one sixteenth of a second from the middle of one interruption to the middle of the next.

Thirdly, a uniform automatic movement at B, by which the star of light is made optically to travel around a circle at the rate of one revolution in the same period of one sixteenth of a second, the consequence of which is that the periodical interruption becomes visible to the eye as a break in the luminous circle produced by the motion of the star, and according as such break is seen upon one part or another of the circumference of the luminous circle, the relation of the movement at A to that at B becomes, inside of the recurring period of one sixteenth of a second, known in like manner as if they had been connected by a coupling shaft extended from one to the other.

Fourthly, a supplementary flash of the light at A, occurring at each whole second and during the interval of one of the interruptions before mentioned, which supplementary flash finds itself subject, in the movement at B, to an action by which it would be carried optically around a circle once only in one whole second, and by the position at which it occurs on the circumference of that circle indicates the sixteenths of the whole second. Also, a still further signal, by which account may be taken of the whole seconds, in ways too obvious to require special notice, and which will complete the knowledge of the relation of the movements at A and B to each other, or of the quantity by which one may be in advance of the other in its motion.

Fifthly, any of the known methods of effecting automatic comparison of the movement at either station with the clock at that station, by which means the comparison of the clocks at the two stations will be made to the minutest fraction of a second. Or, a single clock may be used, say at A, and any observation at B can by known methods be automatically referred to the movement at that station, and thus compared at once with the clock at A.

Further, if the automatic movement at B, besides giving optical motion to the star of light shown from A, is simultaneously made to produce periodical interruption of another intense light shown at B and seen at a third station, C, provided with a movement like that at B, the comparison from A to B may be extended directly onward from B to C, and from C onward to a fourth station, and so on, and such is the degree of precision which seems, so far as we can judge without direct experiment, attainable in the comparison between contiguous stations, that the probable error of a single comparison between the extremes of a line of twenty stations may, I believe, be made smaller than the hundredth part of a second.

An arrangement suitable for carrying the above plan into effect is illustrated by a sketch in the accompanying plate. At the station A, the rays of the signal light, diverging from their source L (Plate II, fig. 1, *a*), are converged by a lens C so as to meet and cross each other in the focus F of a telescope. Diverging from this point they traverse the object glass O, and issue from it in a nearly parallel beam directed to the station B. The light thus transmitted is periodically intercepted at the focus F by the projecting teeth of a rotating disc, or signal wheel, S, which is made to rotate uniformly at the rate of one revolution in about one second.

The telescope used at the station B for viewing the light shown at A, is furnished with a terrestrial eye-piece, and at that point where occurs the first image of the object glass, or crossing of all pencils of light that can pass through the telescope, is introduced a refracting glass prism P, shown in section in Fig. 1, *b*. The pencil formed by the light from A, on traversing the refracting prism, is turned aside so that the star image, which otherwise would be formed at *s*, is formed at *s′*, and the displacement is observed by the eye at E. Since this displacement is always in a plane at right angles to the edge of the refracting angle of the prism, if the latter be made to rotate on an axis *zz*, parallel to the axis of the telescope, the displaced star image *s′* will travel in a circle around *s* as a center. If the period of revolution be shorter than the duration of the luminous impression on the eye, and the light be unintermitting, the circle described by *s′* will appear to the eye as a continuous circle of light. The period of one sixteenth of a second may perhaps be taken as sufficiently small for continuity of luminous impression. Accordingly, if the prism be made to revolve about sixteen times per second, and precisely sixteen times during each revolution of the signal wheel S, and if the primary division of the latter be made by sixteen equidistant slotted openings in its border, then the luminous circle, which but for the interposition of the signal wheel would appear continuous and entire, will be seen in part obliterated, as shown in Fig. 3, the luminous part S′ D S′ having the same ratio to the obliterated part S′ G S′, that the width of one of the slotted openings of the signal wheel has to the width of a tooth. And as the luminous arc S′ D S′ appears on one part or another of the circumference, its angle of position, which may be observed by bringing the wire of a position micrometer into coincidence with the extremities of the arc, will determine the angle of position at which the prism arrives simultaneously with the arrival of the signal wheel at a given point of reference.

But for counting the whole sixteenths of a second it will be required to know also the simultaneous angle of position of a wheel geared with the prism P so as to revolve only once in the time of one revolution of the signal wheel. The circular prismatic piece is of a diameter many times greater than that of the pencil of light, and the latter traverses the former at its border. Around the prismatic piece is fastened a toothed ring, into which gears the driving wheel H from which the prism takes its motion. Between this wheel H and the regulator from which it derives its uniform motion, is interposed a satellite wheel arrangement, by means of which the observer, without disturbing the invariable velocity of the regulator, can set the wheel H, and the parts to which it gives motion, forward or back in their course, and then allow them to proceed *at once* with the same correct velocity as before. In this way the observer will have absolute control of the angle of position of the luminous arc S′ D S′, and it may be agreed that as this angle of position slowly changes in consequence of the want of *perfect* unison between the movements at the two stations, he shall, from time to time, bring the luminous arc back to near coincidence with a standard position, that for instance which is shown in Fig. 3, the exact angle of position to be measured, however, in the manner above mentioned. Provided, then, the arc be not allowed to stray far from its standard position, it will be obvious that one part of the border of the prismatic piece P will never be traversed by the light which passes the sixteen primary openings of the signal wheel and forms that arc. The part thus unused is made with parallel faces, as shown in the figure, and then any supplementary flash of light occurring midway between the primary ones, will pass through the parallel part of P unrefracted, and may be refracted by a second prism P′, that moment interposed. This second prism is made to revolve once in the period of the sixteen revolutions of P, and in the best mode of construction that occurs to me is one of sixteen prismatic pieces P′, P′, P′, &c., so cut out, and attached to the border of a wheel or disc K, made to revolve in that period, that the edges of the refracting angles of all of them shall be parallel to each other, the whole forming the equivalent of a single prism cut into a large toothed wheel. This wheel K, like P, takes its motion from H, and is so geared that during all the intervals of time in which a passing pencil would encounter the refracting part of P, it will have free passage through one of the spaces between the pieces P′, P′, P′, &c., which, during the alternate intervals of time, will in their turn be interposed in the path of the pencil. Any flash of light, therefore, that escapes through any supplementary opening, as *t*, in the middle of one of the sixteen primary teeth of the signal wheel, will, in traversing the telescope at B, be refracted by one of the prisms P′ alone, and not by P. And if it be recollected that the several prisms P′ are in effect parts of one prism, as distinctly indicated in fig. 1, *b*, and that the action of this differs in no respect from that of P except in its longer period of revolution, and that this period of revolution is the same with that of the signal wheel, it will be obvious that if several supplementary openings be made in the signal wheel, as for instance two, *t* and *u*, in two teeth diametrically opposite to each other, and two others, *v* and *w*, in teeth adjacent to one of these on each side of it, the flashes of light through these openings will be seen by the eye at E to occur at points t', u', v', w', fig. 3, distributed around the circumference of a circle concentric with S′D S′G, and at angular intervals from each other identical with those between the corresponding openings in the signal wheel. It will be further obvious that the observed angle of position of the diameter $u'\,t'$ will depend on the angle of position of the wheel K relatively to that of the signal wheel. The diameter $u'\,t'$ becomes, then, by aid of the more exact indication of S′D S′, an index by which we know the required angle of position at which the wheel K arrives simultaneously with the arrival of the signal wheel S at a given point of reference. As the most convenient mode of procedure in practice, the observer at B may operate the satellite wheel until the index diameter $u'\,t'$ is brought to a position, for instance the vertical one in the figure, made to denote zero, and the position micrometer for taking the angle of position of S′D S′ may be graduated to thousandths and ten thousandths of a second.

Instead of the above described arrangement there is a modification of it which I am disposed to prefer, the type of which is a pair of telescopes at station B, placed side by side so that one may contain the rapidly revolving prism and the other the more slowly revolving one, each prism being in this case unintermitting in its action, and the supplementary openings of the signal wheel being replaced by the filling up of a single one of the primary openings. The omission of the flash of light from this one would be observable through the slow prism and give the required indication, while it would not probably injure in any material degree the distinctness of the arc of light seen through the fast prism. Instead of a pair of complete telescopes, the equivalent of a pair of eye-pieces with a sliding object-glass to alternate between them at pleasure, would answer the same purpose. In this arrangement no rectification of the prisms by the observer would be necessary, it being always possible to observe the total deviation. This would be a great advantage on a line of very numerous stations, in which case it would, on the first described plan, be a somewhat critical matter to bring all the instruments on the line into the required correspondence for simultaneous observation.

As before intimated, it would be possible to employ but a single clock on the whole line of stations, but as this would require signal observations for every time observation at any other than the clock station, it would be more convenient to employ a clock at every astronomical station.

The question of the feasibility of the process described in this paper will depend primarily on the practicability of securing, with telescopes of moderate aperture, a sufficiency of light for such distances as from fifty to eighty miles, and next on the attainment of sufficient precision of rate in the uniform motion employed. I do not anticipate serious difficulty in either of these things. For the uniform motion, considering especially the light work it will have to do, the Fraunhofer regulator would I presume be everything that is required, or an electro-magnetic regulator, similar to that described by me in a paper presented to the American Association at their meeting at Montreal, may be used if found reliable. From what a scientific friend has told me of his experience with distant lights, I think we are justified in anticipating the easy attainment of sufficiency of light.

A similar optical means can also be used for comparing a mean time clock at one station with a sidereal clock at another, by the method of coincidences, without other mechanism than the clocks themselves, though with diminished power of precision on a long line of stations. The pendulum of the one clock is made to carry in the focus of the telescope at its station, an opaque disc with a narrow slit, through which, at each oscillation, a flash of light is allowed to escape to the other station, and through the focus of the telescope at that station oscillates a wire carried by the pendulum of the other clock, which eclipses the flash of light at each coincidence of the two pendulums. Or the pendulum at the observing station may carry a mirror, in which either a flash or an interruption of light from the other station may be observed by reflection, and the coincidence noted when the flash or the break is seen at the same point of the field of view where it is observed with the pendulum at rest.

I have already observed that the visual method proposed in this paper might prove useful as a check, at least, upon the indications of submarine or subterranean lines of electric telegraph. But it seems less liable to uncertainty in its indications than even the air lines, the signals of which occupy a very appreciable and more or less ambiguous time in passing, and therefore on very extensive surveys it would be very instructive at least, and might be found to give increased accuracy, to add to the comparisons made by the telegraph wires, further comparisons by means of a sufficient number of the visual instruments to reach across the whole extent of the survey. In case it should ever be undertaken, as has been proposed, to measure an extensive arc of the equator, the idea of such a visual method for the accurate determination of the differences of longitude, would be well worth considering.

I will close by suggesting one more obvious application of the method, and that is, the determination of the velocity of light, which, with a sufficiently high velocity of revolution of the prism and signal wheel, might be done with considerable accuracy by transmitting, in the same manner as before described from a second station to a third, a return signal from the second station to the first.

ART. VI.—*On Osmious Acid, and the position of Osmium in the list of Elements;* by J. W. MALLET, Prof. of Chemistry, &c., Univ. of Alabama.

IN most chemical text-books it is stated, on the authority of Berzelius, that there are five oxyds of osmium—OsO, Os_2O_3, OsO_2, OsO_3, and OsO_5—of which however the second and fourth have not been isolated, although compounds containing them are known. To these may be added a blue substance, first obtained by Vauquelin and supposed by Berzelius to consist of OsO united to either Os_2O_3 or OsO_2, and the highest oxyd, probably OsO_5, the existence of which was announced by Frémy in 1854.

While preparing osmium from some black platinum residues I have accidentally obtained, a substance which there is some reason to believe may be osmious acid—the hitherto unisolated teroxyd—mixed indeed with osmic acid, but still permitting certain of its properties to be observed.

Three or four ounces of the platinum residue were treated by a modification of the original process of Wollaston, now seldom adopted. The powder was mixed with three times its weight of nitre, the mixture was fused for some time in an iron crucible, and then poured out upon an iron plate. While still warm the fused cake was broken into fragments and put into a flask fitted with a cork, through which passed a tube two feet long, bent at right angles, and a funnel-tube, the latter drawn out to a very small bore at the lower end, and reaching to the bottom of the flask. The bent tube was well cooled, and *undiluted* oil of vitriol was very cautiously poured, by a few drops at a time, into the funnel.

The acid produced intense heat on coming in contact with the cake of potash salt, and oily drops of a bright *yellow* color began to make their appearance in the cooled tube. These drops very slowly congealed to a solid resembling unbleached bees-wax.

By the time the sulphuric acid had been added in slight excess a considerable quantity of this yellow substance had collected in the tube and in a receiver attached. By gentle heating the whole was obtained in the receiver, and united under a little water to a single mass. Towards the end of the distillation *colorless* needles and fused drops of the well known osmic acid came over, and doubtless a considerable portion of the yellow mass in the receiver consisted of the same.

At first it seemed probable that the yellow color of the latter was due merely to some impurity, and it was therefore cautiously resublimed, but it again collected of the same tint as before. It appeared to be even more fusible and volatile than osmic acid; it took a long time to congeal under a stream of cold water flowing over the outside of a tube in which it had been melted.

The water in which it was fused acquired a bright yellow color, and gave off fumes, the odor of which seemed to me somewhat different from that of osmic acid, and which irritated the eyes so insufferably that it was scarcely possible to finish work with the acid and put it up for preservation. It was removed as a single cake from the water, and sealed up hermetically in a glass tube which had been previously cleansed with care from all traces of dust or other organic matter. The water in which it had been fused was mixed with caustic potash, and gave a solution of very dark brown-red color, such a tint as would probably result from a mixture of the red* *osmite* of potash discovered by Frémy with the orange-brown *osmiate* of potash.

The sealed tube containing the fused cake or stick of yellow acid was allowed to remain upon a table exposed to the direct rays of the sun. The acid immediately began to sublime upon the sides of the tube, not in long needles and prismatic crystals like osmic acid (which seems to be monoclinic), but in feathery crusts like sal-ammoniac, which under a lens had somewhat the appearance of minute octahedrons grouped together. The color was still bright yellow, but in a short time the sublimed acid began to turn black, and in twenty-four hours the whole inner surface of the tube was perfectly black and opaque. A tube containing pure colorless osmic acid has been exposed in a similar way to the sun for three weeks without any such blackening taking place. A tube closed by a cork, or one from which dust has not been carefully removed will often cause osmic acid to turn dark, but never exhibits anything like the absolute blackness and opacity of the whole tube noticed in the present instance.

* A rose-red color is also characteristic of the salt supposed by Berzelius to be the ammonio-*terchlorid* of osmium, corresponding in the chlorine series to *osmite* of ammonia.

It is easy however to imagine the cause of this change undergone by the yellow acid if it be in fact the teroxyd of osmium (mixed with osmic acid). The teroxyd probably broke up into osmic acid and one of the lower oxyds of osmium or perhaps the metal itself. We might have

$$2OsO_3 = OsO_4 + OsO_2$$
$$5OsO_3 = 3OsO_4 + Os_2O_3$$
$$3OsO_3 = 2OsO_4 + OsO,$$

or,

$$4OsO_3 = 3OsO_4 + Os.$$

In order to ascertain, if possible, which of the above changes had taken place, the tube was opened two or three months after it had been sealed, and the contents were examined. The fused stick of acid was found to be black and partially friable; on heating in another glass vessel most of it sublimed, leaving a little black powder behind, and condensed in needles, still slightly yellowish, but differing little in appearance from common osmic acid. The inner surface of the original tube was found coated with a thin filmy, adherent crust, of a black color and considerable lustre. This was scraped off, and a portion of it gently heated in a stream of dry carbonic acid gas until all traces of adherent osmic acid were driven off. After cooling, the carbonic acid was replaced by dry hydrogen, and heat was again applied. Water condensed on the tube beyond the heated part, thus proving that an oxyd of osmium, not the metal, was under examination. Replacing again the hydrogen by oxygen, osmic acid was produced and carried off with the stream of gas. The black powder scraped off from the original tube was heated with hydrochloric acid, and seemed to be but slowly acted on; the acid however assumed a green color, and hence it is probable that the osmium existed as protoxyd.

It is not easy to see, without further investigation, how osmious acid could have replaced in part osmic acid in the attempt to prepare the latter as above described. Is there a particular stage of the decomposition of nitre by heat at which osmium may replace nitrogen in nitrite of potash (KO, NO_3)? From the relations of the two elements, to be noticed presently, this would seem probable, and in fact Frémy has noticed the crystallization of osmite of potash from a solution in hot water of the fused cake of nitre and iridosmium. A reason for osmic acid (OsO_4) being usually obtained from the latter, instead of osmious (OsO_3), might perhaps be found in the fact that the chemists who of late years have worked upon osmium recommend the use of nitric or nitro-muriatic acid to neutralize the potash—sulphuric acid, to which Wollaston had recourse in his early experiments, is now seldom employed. Thomson, in his "Chemistry of Inorganic Bodies," published many years ago, observes that osmic acid has sometimes a tint of yellow.

It does not seem likely that the cork closing the neck of the flask used for distillation had anything to do with the production of osmious acid, if such took place; the cork itself did not show any appearance of being acted on, and there was no blackening of its surface until some time after the experiment was ended.

The reduction of osmic acid generally results in the formation of the basic oxyds; Berzelius, however, observed that on adding sulphurous acid to a solution of osmic acid the latter passed through various shades of color—*yellow*, *orange-yellow*, brown, green, and at last blue; he attributed these tints to the successive formation of sulphates of the bin-oxyd, sesqui-oxyd, and blue oxyd; but may not the first step in the reduction have been osmious acid, giving the yellow color?

Another and altogether different view of the nature of the volatile yellow substance above described was suggested as possible by some remarks of Claus in a recent paper on the tendency to reduction of salts of iridium (Ann. d. Chem. u. Pharm., Aug., 1858, S. 129). This author has shown that the platinum metals fall naturally into these groups, in each of which are contained two metals resembling in general habit and relations each other more closely than members of the remaining groups. Platinum and palladium constitute the first of these pairs, iridium and rhodium the second, osmium and ruthenium the third. The atomic weight of the first-mentioned member of each pair is higher than (nearly double) that of the second.

In the paper quoted Claus remarks that the metal of lower atomic weight in each of these groups is much more easily reduced than the other from superior to inferior grades of combination with chlorine; thus the bichlorid of palladium is reduced with much greater ease to proto-chlorid than is the corresponding compound of platinum; and, for the same season, probably, the bi-chlorid of rhodium is not known, but only the sesqui-chlorid, while both salts of iridium can be easily obtained. On this same principle Claus explains the fact that no oxyd of ruthenium homologous with osmic acid has been obtained, while he gives the following reasons for suspecting the existence of such an oxyd: "This opinion is based upon the fact, that in my preparation of compounds of ruthenium, which can be obtained only by energetic processes of oxydation, the material worked upon, notwithstanding all my care and economy, gradually diminished, and yet I have never succeeded in collecting a volatile product. Once only, when I had fused ruthenium, perfectly free from osmium, with caustic potash and nitre, dissolved the mass in water, and decomposed it with nitric acid, I observed a peculiar odor, quite distinct from that of osmic or nitrous acid; and afterwards, having covered the beaker, which was smeared on the edge with tallow, with a plate of glass, I remarked an unmistakable blacking of the tallow, caused by the reduction of a volatile metallic compound."

It seemed possible that the volatile yellow substance to which the present paper refers might have been an acid oxyd of ruthenium*—RuO_3, RuO_4, or RuO_5—and reducible with extreme facility, Claus and others having already noticed the reducing effect of light upon salts of the platinum metals. A portion of the crust from the sides of the tube of yellow acid was carefully examined for ruthenium, the various tests given by Claus as well as that recently proposed by Dr. Gibbs being made use of, but no proof of the presence of this metal could be obtained.

The properties of osmium and its compounds are very remarkable, and render it a matter of no little interest to trace the analogies of this rare substance and fix its place among the other elements. It is described in most chemical works along with platinum and its associated metals, mainly on the ground of community of origin, for in many respects it is unlike the platinum, palladium, rhodium, &c., with which it always occurs in nature. All these metals are commonly thought of as very infusible, of great density, very slightly affected by reagents, and very easily reduced from their compounds to the metallic state; when more closely examined they are found to differ from each other in many of their other properties. The arrangement by Claus of the platinum metals in these groups, each containing one metal of high and one of low atomic weight, viz.

Platinum,	Iridium,	Osmium,
Palladium,	Rhodium,	Ruthenium,

has been alluded to above; the two members of each group are more closely related to each other than to any of the rest. Osmium and ruthenium are clearly the most electro-negative of the series. Graham has inferred the isomorphism of platinum, palladium, iridium and osmium, from the fact that their potassio-chlorids all crystallize in the form of the regular octahedron; the corresponding compound of ruthenium has since been added to the list, while that of rhodium is still unknown. The occurrence of two salts under the same form, *in the regular system*, of

* If such a compound exist, an explanation may be found for the process by which Frémy has obtained a lower oxyd of ruthenium—probably the bin-oxyd—in crystals. He roasts the powder of platinum-residue in a stream of air drawn through a porcelain tube at a bright red heat; osmic acid volatilizes, and is said to carry with it *mechanically* the oxyd of ruthenium, which deposits upon fragments of porcelain placed in the cooler part of the tube. But the oxyd is in distinct crystals, and can therefore scarcely be conceived of as a powder borne along in a merely mechanical way by a stream of vapor; and, moreover, there is no reason for oxyd of ruthenium only being so borne along, while other substances of no greater density remain behind. Is it not more likely that a volatile and very easily reducible homologue of osmic acid is formed, and almost immediately afterwards decomposed, depositing the bin-oxyd of ruthenium?

course does not of itself suffice to establish the relation of isomorphism between them; iridio-chlorid of potassium seems however to be capable of crystallizing in all proportions with the platino- and osmio-chlorids.

The interesting fact has been discovered by Claus that osmio-cyanid and ruthenio-cyanid of potassium are strictly isomorphous with the well-known ferro-cyanid, crystallizing with it in all proportions, and even giving very similar precipitates with various metallic solutions; so that, in these double cyanids, osmium and ruthenium are capable of taking the place of *iron.*

In the greater number of its relations, however, osmium presents itself as a member of the *arsenic group* of elements. This has been noticed by some recent authors, as by Prof. Dana in the arrangement of the elements adopted in his System of Mineralogy, and by Prof. Miller, who says in his lately published Elements of Chemistry that "it presents more analogy with arsenic and antimony than with the noble metals." Frémy too compares osmium in platinum ore to arsenic in the native arseniurets.

Nitrogen, phosphorus, arsenic, antimony, and bismuth are generally recognized as forming a distinct and natural group of elements, and into this group it seems from many considerations that osmium, and probably ruthenium, ought to be introduced. They have some analogies with other natural families, just as arsenic is allied to sulphur in native sulph-arseniurets, and nitrogen and chlorine exhibit some resemblance in the nitrates and chlorates, but here appear to lie their closest relations. It may be interesting to notice some of the principal points of resemblance to or difference from this group.

Iridosmine occurs in crystals closely related in form to those of arsenic, antimony, and bismuth in the metallic state. The analyses of iridosmine are not yet sufficiently numerous or accurate to enable us to decide upon its normal composition, but it seems probable that the two metals occur in variable proportions, and are in this mineral isomorphous, thus establishing, as noticed by Dana, a connection between the arsenic group and that of the distinctly basic metals, as the arsenic and sulphur groups are united through homœomorphous bismuth, tetradymite, and tellurium. Dana places iridium in the same section with iron, among the metals whose most stable grades of oxydation are the prot-oxyd and sesqui-oxyd, but the statement of Claus that the *bin-oxyd* of iridium is the most stable and easily prepared compound with oxygen would remove this metal, as also perhaps platinum and palladium, from the iron section to that containing tin and titanium, and the propriety of this transfer may be supported by the relationship of Frémy's crystallized oxyd of ruthenium (doubtless the bin-oxyd) examined by Sénarmont: This was found to be homœomorphous with stannic and (the rutile form of) titanic acid. The bi-chlorid of tin and potassium too is reported as crystallizing in regular octahedrons, like the corresponding salts of iridium, platinum, and palladium.

The arsenic section, as given by Dana, includes nitrogen, phosphorus, arsenic, antimony, bismuth, osmium, and tellurium. The last-named is marked as doubtful, and should decidedly be placed with sulphur and selenium, to which it is analogous in by far the greater number of its compounds.

In one of the interesting memoirs lately published by Dumas on the numerical relations subsisting among the atomic weights of the elements, the arsenic series is thus given:

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

and the parallelism of this series with that of chlorine, iodine, &c., is supposed to be shown in the following lines:

F (19)	Cl (35·5)	Br (80)	I (127)
N (14)	P (31)	As (75)	Sb (122)

in which a common difference of 5 is assumed between the two members in each of the vertical columns (a difference not strictly brought out in the case of phosphorus and chlorine), and in which antimony is given a higher atomic weight than in the preceding table. Osmium is not included, but in a supplemental note since published we find it placed, with an equivalent somewhat higher than that usually adopted, in the *sulphur* group, serving to complete the following two lines of equivalents:

Mg (12·25)	Ca (20)	Sr (43·75)	Ba (68·5)	Pb (103·5)
O (8)	S (16)	Se (39·75)	Te (64·5)	Os (99·5)

between the paired members of which a common difference of 4 is supposed to exist.

Let osmium and ruthenium be brought into the arsenic group, and the series of atomic weights will then stand thus:

	Atomic weights.
Nitrogen, - - -	14
Phosphorus, - - - -	14+17=31
Ruthenium, - - -	14+17+22=53
Arsenic, - - - -	14+17+44=75
Osmium, - - - -	14+17+66=97
Antimony, - - - -	14+17+88=119
Bismuth, - - - -	14+17+176=207

The atomic weights of ruthenium and osmium are here assumed as 53 and 97, numbers not differing more widely from those commonly received—52·2 (Claus) and 99·6 (Berzelius)—than do several of those assumed by Dumas. Our knowledge of these two equivalents is based upon very limited data, and can but be looked on as approximative merely. As regards osmium, Frémy says that in several experiments he has obtained an equivalent number lower than that given by Berzelius, and the vapor-density of osmic acid, which we shall notice presently, points to an equivalent close to 97. A re-determination of this equivalent is very much to be desired.

Taking the series as given above, we find ruthenium and osmium to fall in between phorphorus and arsenic—arsenic and antimony; the numbers from phosphorus to antimony increasing by 22—44—66—88, just as in the following group given by Dumas:

	At. wts.
Chromium, - - -	26
Molybdenum, - - -	26+22=48
Vanadium, - - -	26+44=70
Tungsten, - - -	26+66=92

and we may arrange the two series in parallel lines,

P (31)	Ru (53)	As (75)	Os (97)
Cr (26)	Mo (48)	V (70)	W (92)

These numerical relations are of very little importance in themselves, when we employ the small numbers of the hydrogen scale of equivalents, and especially when we permit ourselves to alter the numbers themselves to any extent, however small, but they acquire more interest when they present us with groupings of elements which we acknowledge on other grounds to be naturally related. In such cases, when the homology is distinctly marked, we may even be justified in taking some liberties for the moment with the numbers standing, often with but slender evidence to support them, for the equivalents of the less-known elements; and we may, perhaps, thus be directed to errors of determination which future experiments will clear away.

The bodies named in each of the two lines just given are homologous in many respects besides that of atomic weight, and a connection between the two series, through vanadium, has lately been shown by Schafarik. There is a clear resemblance running through the formulæ and properties of their oxyds. In the chromium series—a very natural one—the most important oxyds are the metallic acids of the composition MO_3; we have also in each case a bin-oxyd, MO_2; but the sesqui-oxyd is prominent only in the case of chromium itself, and indicates the relation of this metal with iron.

In the arsenic series the known oxyds are the following:

	P_2O					
NO	PO	RuO	AsO(?)	OsO	SbO(?)	BiO(?)
		Ru_2O_3(?)		Os_2O_3(?)		
NO_2		RuO_2		OsO_2		
NO_3	PO_3	RuO_3	AsO_3	OsO_3	SbO_3	BiO_3
NO_4	PO_4(?)			OsO_4	SbO_4	BiO_4
NO_5	PO_5		AsO_5	OsO_5	SbO_5	BiO_5(?)

The prominent compounds in the table are the acids, MO_3 and MO_5; with respect to the separate columns the following facts are noticeable.

The oxyds of nitrogen are well known; the regularity observable in this column causes it to be frequently used as an illustration of the "law of multiples." NO and NO_2 are usually said to be neutral, but the latter plays the part of a base in contact with sulphuric acid, as in the crystals of the oil of vitriol chambers, and possibly the former may do so too in the nitrosulphates (KO, NO, SO_3 and NH_4O, NO, SO_3?) obtained by Davy by bringing nitric oxyd in contact with an alkaline sulphite. NO_3 and NO_5 are well-known acids. It is doubtful whether hypo-nitric acid (NO_4) is capable of combining with bases and forming salts; in contact with the alkalies it yields a mixture of nitrites and nitrates, yet, *when out of contact of bases, it seems to be a body of more stability than either* NO_3 *or* NO_5 *(anhydrous)*.

In the column of the oxyds of phosphorus, we have first the very anomalous sub-oxyd (P_2O), which is probably the only marked exception to the homology running through the whole table. Before the discovery of red (amorphous) phosphorus by Schrötter this substance was, no doubt, to some extent confounded with phosphoric oxyd, and may even now throw some doubt upon the cases in which the latter seems to have been obtained pure and to have yielded a formula supported by trustworthy analyses. PO, unlike the other protoxyds of the series, is usually considered an acid, but as it has not been obtained in the separate state, and all the hypophosphites contain water, it may be reasonably assumed that the formula of the acid should include hydrogen. PO_4 is doubtful; this may, perhaps, be the composition of Pelletier's phosphorous acid, produced by the slow combustion of phosphorus, a body which undergoes no further oxydation by prolonged exposure to the air, and which, in contact with bases, yields mixed phosphites and phosphates. The last term in the column—phosphoric acid—is well known.

The existence of a distinct protoxyd of arsenic, as of antimony and bismuth, is doubtful. Arsenious acid is *a feeble, volatile, me-*

tallic acid—feebler in its relations as an acid than arsenic acid, and volatilizing at a lower temperature than the latter. Arsenious acid, moreover, volatilizes at a temperature below that required by metallic arsenic.

In the antimony column, the oxyd SbO_3 is usually viewed as a weak base, but seems also to be capable of uniting as a feeble acid to the alkalies, and even of expelling carbonic acid from their carbonates (Liebig). The iso-dimorphism of SbO_3 and AsO_3 is well established. SbO_3 is volatile at quite a moderate temperature, while metallic antimony requires at least a white heat to vaporize it. SbO_5 is a body of distinctly acid properties. *Both* SbO_3 *and* SbO_5 *are converted by heating in the air into* SbO_4 —the so-called antimonious acid—which seems therefore to be the most stable oxyd when strong bases and acids are not present. It is most probable that, as Frémy maintains, SbO_4 is not itself an acid, but that a so-called alkaline antimonite is in fact a mere mixture of an antimoniate with the compound of antimonic oxyd and alkali ($2SbO_4=SbO_3+SbO_5$).

In the bismuth column, the teroxyd is homologous as a base with teroxyd of antimony, but shows little tendency to play the part of an acid with even the strongest bases. This oxyd and the metal itself are volatile at high temperatures. BiO_4 also seems to be devoid of acid properties, but the compound BiO_5 probably exists, and is homologous with SbO_5, forming alkaline salts of little stability.

Comparing now ruthenium and osmium with the above recognized members of the arsenic group, we find first that both metals form protoxyds, which are feeble bases, as are probably the corresponding compounds of the other members of the group. We next meet with the sesquioxyds, whose formula is exceptional in the series, but for neither metal has this grade of oxydation been obtained in the free state and pure, and in the case of osmium its existence may be gravely doubted. Anhydrous Ru_2O_3 is supposed by Claus to be formed during the roasting of metallic ruthenium in the air at a high temperature, but only on the ground that the absorption of oxygen slackens when *about* enough has been taken up to form this compound, and that the proportion necessary for the binoxyd is never fully attained. Claus, however, describes a sesquichlorid with which double salts are formed by the chlorids of potassium and ammonium, and we must therefore assume a sesquioxyd also. Sesquioxyd of osmium is quite unknown in the separate state, and the belief in its existence is founded solely upon the preparation by Berzelius of a dark brown substance, supposed to consist of the sesquioxyd united to ammonia, which, dissolved in hydrochloric acid, yields a brown compound, supposed to be the sesquichlorid of osmium and ammonium. Neither of these, however, can be crystallized, nor has the constitution assigned to either been supported by an analysis. The so-called ammonio-sesqui-oxyd detonates when heated, (sometimes with much violence, as I have noticed in removing by heat the deposit of this substance which forms on the end of a retort-neck during the distillation of osmic acid into a receiver containing ammonia,) and hence is probably analogous to fulminating platinum, containing perhaps the binoxyd of osmium. The binoxyd itself is a feeble base, the characteristic color of whose salts in solution is yellow as is the case with the corresponding compounds of iridium. Similar remarks apply to the binoxyd of ruthenium—probably the body obtained, as we have shown, by Frémy in crystals. The teroxyd of osmium is the body supposed to have been isolated in the experiment described at the beginning of this paper. Its position as a feeble acid, capable, however, under some circumstances, of playing the part of a base, its fusibility and volatility (greater, apparently, than those of osmic acid, as nitrous acid is more fusible and volatile than hypo-nitric), its probable crystallization in octahedrons of the regular system, in which arsenious acid and teroxyd of antimony are also found, all tend to indicate homology with the other teroxyds of the arsenic group. The general relations of ruthenic acid, so far as these are known, place it in a similar position. Just as we find hyponitric acid (NO_4) and antimonious acid (SbO_4) to be the most stable of the higher oxyds of nitrogen and antimony, so the well known osmic acid (OsO_4) seems to be the grade of oxydation which osmium most readily assumes and retains when not in contact with bases. OsO_3 and OsO_5 (the latter as described by Frémy) seem scarcely capable of existing in the separate state; when set free from their salts they soon pass into OsO_4; while it may as well be doubted that the latter ever exists as a distinct acid in combination with bases as that NO_4 or SbO does so. No so-called osmiate has ever been analyzed; the saturating capacity of the acid, if it be such, is unknown; when free and in solution in water it has no acid reaction, it does not displace carbonic acid from the carbonates, and it is itself expelled by heat from most of its supposed compounds, and is separated in part by water even from potash and soda. No compound of OsO_4 with a base has been obtained in crystals, while Frémy states that he has crystallized the alkaline salts of both OsO_3 and OsO_5. RuO_4 and RuO_5 are as yet unknown.

The tendency throughout the whole arsenic group is manifestly to the production of the acid compounds MO_3 and MO_5, the former the more fusible and volatile body, the latter the stronger acid. In addition we have some cases of the protoxyd (MO), a feeble base, and the binoxyd (MO_2), a body of still more feebly basic properties, verging upon the acids. All other grades of oxydation, so far as they exist at all, may perhaps be correctly viewed as compounds of the preceding *inter se.* The stability of the oxyd (MO_4) in the separate state is remarkable—its formula is one of rare occurrence.

The affinity of all the elements of the group for oxygen is considerable; it is so even in the case of osmium and ruthenium, usually placed among the noble metals. Dumas (*Traité de Chim. app.*) states that osmium does not oxydize at common temperatures, nor even at 100° C., but I have obtained conclusive evidence that oxydation may go on slowly even at the ordinary atmospheric temperature. The paper label and the cork of a tube containing pure metallic osmium have in the course of several years become blackened, precisely as organic matter is by the fumes of osmic acid, the black tint on the paper decreasing from the mouth of the tube along the outside. A piece of white paper in which some black platinum residue had been wrapped, was strongly stained in the immediate neighborhood of the powder in the course of a few weeks. *The same effect is distinctly observable even upon the paper label placed inside a tube of native iridosmine* (Siberian) *in the usual coarse grains*—a specimen which has lain among other minerals, and has never been placed near any artificial preparations of osmium. Osmium, like arsenic and antimony, is clearly capable of slowly taking up oxygen at common temperatures. At a red heat, roasting in a current of air affords, as is known, a good method of obtaining osmic acid from the iridosmine of platinum residues—just as by similar roasting arsenious acid is prepared from the native arseniurets.

It would be a matter of much interest to compare osmium with its supposed homologues under circumstances in which we should expect it to play an electro-negative part. Frémy has announced his belief in the existence of an *osmiuretted hydrogen*, but such a body has not yet been isolated and described. Compounds of the metal with ethyl, methyl, &c., would be well worth examination, and it is not unlikely that such might be prepared from a body which in some states of combination exhibits such a high degree of volatility.

The earlier experiments of Deville and Debray upon the platinum metals seemed to have shown that both osmium and ruthenium could be volatilized, at exceedingly high temperatures, without previous fusion; if this were confirmed, a strong point of resemblance with arsenic would be made out, but it appears from a more recent paper that osmium at least may be fused and obtained as a perfectly compact mass, the apparent volatility of the metal being due doubtless to previous oxydation, the crucibles used being permeable to air. We have seen, as regards arsenic and antimony, that their oxyds are more volatile than the metals themselves.

It is lately stated that osmium may be obtained in crystals by the same means as those used for boron and silicon, but I have as yet seen no account of the form which it assumes.

Deville has furnished another interesting fact with respect to osmium, by determining the density of the vapor of osmic acid, which he has found = 8·88. This, if we take the generally received atomic weight for osmium, gives the atomic volume $\frac{131·6}{8·88}=14·82$, indicating a condensation to 2 vols. If we now calculate back to the theoretical atomic weight we get $(14·57\times 8·88)-32=97·38$, a number closely approaching 97, which, as we have seen, brings the equivalent of osmium into simple and harmonious relation with those of the other elements of the arsenic group.

The specific gravity of fused metallic osmium having been lately determined by Deville =21·4, there can be little doubt that all the metals of the platinum family possess the same atomic volume when in the free state, about 4·6 or 4·7; the specific gravity of ruthenium is not yet known with accuracy, but such experiments as have been made render it improbable that it will prove an exception. This number is about one-fourth the mean of the at. vols. of the long recognized members of the arsenic group, but these latter differ so widely among themselves* that the comparison is of little or no value. It would be desirable to get a good determination of the density of osmic acid in the solid state, so that its at. vol. might be calculated and compared with that of antimonious acid.

The specific heat of osmium, so far as its value as a physical character goes, *opposes* the introduction of this element into the arsenic group. It has been determined by Regnault =·03063; multiplying now by the equivalent 97, we have the product, 2·9711, thus placing osmium in the list of the elements (including the majority) for which the product of sp. ht. by at. wt. is nearly 3, while for phosphorus, arsenic, antimony and bismuth the product thus obtained is twice as great, or about ·6. In this respect, however, osmium probably resembles nitrogen—the latter examined, as it necessarily is, in the gaseous form.

It is to be hoped that the conducting power for heat and electricity of compact osmium will soon be examined; nothing is as yet known of these characters.

* Phosphorus, - - - -	$\frac{31}{1·83}$ (*Schrötter*)	=16·94
Arsenic, - - - -	$\frac{75}{5·67}$ (*Herapath*)	=13·23
Antimony, - - - -	$\frac{119}{6·7}$ (*Karsten*)	=17·76
Bismuth, - - - -	$\frac{207}{9·8}$ (*Marchand & Scheerer*)	=21·12

Lastly, as regards the magnetic relations of the element—it is placed, with some doubt, by Faraday in the paramagnetic class; the metal and its protoxyd were found to act feebly in this sense, while pure osmic acid is said to have shown itself clearly *diamagnetic.* The strongly diamagnetic character of phosphorus, antimony and bismuth would render a re-examination of this point interesting. Arsenic, however, is said to be very feebly diamagnetic, and is placed by Faraday close to osmium in the list of metals examined, though on the opposite side of the line of magnetic neutrality or indifference.

Reviewing, now, the united physical and chemical characters of osmium, and comparing them with those of the generally recognized members of the "arsenic group," we are, I think, justified in concluding that here this curious metal should be placed in a natural arrangement of the elements—while important distinctions seem to separate it from some, at least, of the platinum metals, with which it is usually associated and described.

Tuscaloosa, Ala., Nov. 1, 1859.

Art. VII.—*The Comas and Tails of Comets;* by Prof. W. H. C. Bartlett, U. S. Military Academy at West Point.

Comets have, at all times, been objects of curiosity and wonder; and the question in regard to the nature of their luminous appendages, has exercised the speculative ingenuity of philosophers from the earliest records of astronomy. Everything written about them is read with interest, and the most extravagant theories in respect to their constitution and the laws of their being find a ready favor with the public. They are still among the enigmas of the heavens. Among the recent and remarkable efforts at solution, is one by the ablest mathematician of the country, perhaps of the age: and granting the premises, there is no avoiding the conclusions of the comprehensive and searching analysis for which this eminent man is so remarkable. But the assumption, that the attractive energy which summons a comet from the depths of space to the presence of the sun, retains its nature unchanged and strengthens with the diminution of distance for a part of the approaching mass, and yet reverses its character and becomes repulsive for another part, in order to obtain material to build up the tail, appears so unsupported by the analogies of nature as to give to his results the taint of improbability. Indeed, a theory which demands such an exercise of faith in matters of science, and from such friends, can only inspire doubt, and should yield the place it has too long occupied to some other, founded in better ascertained laws of matter. The question is not one of pure mathematics, but of physics.

The material elements of all bodies of which we have any knowledge, are united by some conditions of aggregation, determined by the reciprocal action of molecular forces; and the circumstances of their relative motion will come from the equation

$$\Sigma m.\left[\left(\frac{d^2x}{dt^2}-\mathrm{X}\right)\delta x+\left(\frac{d^2y}{dt^2}-\mathrm{Y}\right)\delta y+\left(\frac{d^2z}{dt^2}-\mathrm{Z}\right)\delta z\right]=0;$$

in which m is the mass of an element, xyz its coördinates of place, XYZ the sums of the components of impressed accelerations in the direction of the axes xyz, respectively.

The conditions of aggregation may be expressed in some functions of the coördinates of molecular places. As three coördinates determine the place of a single molecule, there will be three times as many coördinates as molecules; and if μ be the number of molecules and λ the number of equations that give the conditions of aggregation, then will $3\mu-\lambda=n$ be the number of coördinates which, if given, would reduce the number of unknown coördinates to the number of equations. These unknown coördinates could then be found, and the places of the molecules at the corresponding instant determined.

Denote the λ coördinates by xyz, $x'y'z'$, &c., and the n coördinates by $\alpha\beta\gamma$, $\alpha'\beta'\gamma'$, &c. The former of these coördinates, as also the forces, may be expressed in functions of the latter, and both eliminated from the general equation of motion. And if $\xi\eta\zeta$, $\xi'\eta'\zeta'$, &c., be the increments of $\alpha\beta\gamma$, $\alpha'\beta'\gamma'$, &c., at any instant and due to any transmitted initial disturbance, it is easily shown that

$$\begin{aligned}\xi &= \Sigma\,\mathrm{R.N}_\xi.\sin(t.\sqrt{\varrho}-r),\\ \eta &= \Sigma\,\mathrm{R.N}_\eta.\sin(t.\sqrt{\varrho}-r),\\ \zeta &= \Sigma\,\mathrm{R.N}_\zeta.\sin(t.\sqrt{\varrho}-r),\\ \xi' &= \text{\&c.}\qquad \text{\&c.}\qquad \text{\&c.}\end{aligned}$$

In which there are n terms comprehended by the sign Σ, and in which ϱ will, in general, have different values from one term to another. When these values of ϱ are real and positive, the different terms in the values of $\xi\eta\zeta$, &c., will disappear periodically, the precise times of disappearance being given by

$$t.\sqrt{\varrho}-r=a\pi;\quad t'\sqrt{\varrho'}-r'=a'\pi;\ \text{\&c., \&c.}$$

or

$$t=\frac{a\pi+r}{\sqrt{\varrho}};\qquad t'=\frac{a\pi+r'}{\sqrt{\varrho'}};\ \text{\&c., \&c.}$$

in which a is any whole number. The intervals of disappearance will be

$$\frac{\pi+r}{\sqrt{\varrho}};\quad \frac{\pi+r'}{\sqrt{\varrho'}};\quad \text{\&c.}\quad \text{\&c.};$$

and if these intervals be commensurable, all the terms will disappear simultaneously, and $\xi\,\eta\,\zeta$, &c., reduce to zero, at equal intervals of which the duration is

$$t_i = \frac{\pi + r}{\sqrt{\varrho}} \times \frac{\pi + r'}{\sqrt{\varrho'}} \times \text{\&c.} \times \text{\&c.}$$

The vast atmosphere of ether which pervades all space is ever busy transmitting luminiferous waves from the sun and other heavenly bodies. Its molecules are ever on the move with velocities and in orbits determined by the relative places and intensities of existing wave sources. Any cause which will perturbate the etherial molecules of any limited volume of ether from these orbits, regarded as initial, and by the quantities $\xi\,\eta\,\zeta$, $\xi'\,\eta'\,\zeta'$, &c., will make such volume *self-luminous;* and if the perturbation be great enough, it will be visible from all directions.

Comets are known to exist in a state of great tenuity, their densities being almost insignificant in comparison with that of the fleecy clouds that float in the upper atmosphere. The luminiferous waves from the sun, entering such bodies with great ease, their intromitted greatly preponderate over their reflected components. The former of these components modify and determine the internal motions of comets, and make them *self-luminous.* The internal cometary elements become so many centres of disturbance. They throw their waves in all directions, and are simultaneously sources of molecular perturbations to the surrounding ether, each giving rise to a term $\text{R}.\text{N}.\text{S}in\,(t.\sqrt{\varrho} - \gamma)$, in the general value of the perturbating functions $\xi\,\eta\,\zeta$, &c., and thus making the ether also *self-luminous.* The degree of illumination will vary with the maximum values of the perturbating functions. These will result, in any case, from the extent of the initial disturbance and the distance, at right angles thereto, over which the disturbance may have been propagated; decreasing, according to the principle of wave divergence, as the square of this distance increases. The components of the initial disturbance perpendicular to the line drawn from the comet to the sun, is, from the principle of wave propagation, much greater than in any other direction; and hence the much greater extent of the illumination on the side of the comet opposite the sun. The comet's head can have no phases, from its self-luminosity; neither can the coma and tail have sharply defined outlines, from the gradual degradation of molecular perturbations towards their borders.

This view denies the presence of cometary material in the coma and tail altogether, and regards these appendages but as phenomena due to the reciprocal action of the etherial and cometary molecular forces. According to it, the coma and tail become, as it were, *a luminous shadow,* a part of which is literally "cast before," and the dark cap which envelopes the head and stretches away through the tail, a region of *wave interference.* No wonder, then, that comets turn their tails from the sun, and, at perihelion, whisk them, though of enormous lengths, through celestial arcs well nigh equal to a semi-circumference, in a few hours. This is no more surprising than that opaque bodies throw their shadows from the luminous sources whose light they intercept. The curvature, which is so remarkable a feature in the tail, is but the simple effect of the comet's orbital velocity, and the progressive motion of light.

If the principles here cited be well founded, then will the zodiacal light find an easy solution; and the great oblateness of its spheroidal figure must be taken as evidence that the component molecular motions in the sun are greater in the direction of the solar axis than in any other.

West Point, Oct. 25th, 1859.

Art. VIII.—*On Sodalite and Elæolite from Salem, Massachusetts;* by J. P. Kimball, Ph.D.

For a knowledge of this locality of the occurrence of the two rare silicates, sodalite and elæolite, we are greatly indebted to Gilbert L. Streeter, Esq., of Salem, as well as to several other gentlemen of the same city. Fortunately, Mr. Streeter very carefully observed their mode of occurrence, and, together with G. F. Cheever, Esq., and Rev. S. Johnson, Jr., collected choice specimens of them. The best of these are in the possession of the Essex Institute, Salem, to the curator of which, Dr. Henry Wheatland, I owe in a great measure the privilege of examining them.

The locality in which the minerals were found is "a pit or quarry, a short distance below the Almshouse upon the road passing along the northern side of the Neck, towards Hospital Point."* They were first noticed in a "block of compact syenite resting upon the bank, the end of which presented a beautiful coloring of blue and greenish white, with specks of black. Upon examination these conspicuous minerals were seen to be in a vein, a portion of which was connected with the block of syenite."†

Mr. Streeter subsequently discovered what undoubtedly was the continuation of the same vein. This traversed an erratic block of the same rock, imbedded in the drift, of which the small block, just mentioned, was a fragment. The vein is described to have been about six feet in width, and to have dimin-

* G. L. Streeter: Essex Institute Proceedings, ii, 153. † Ib.

ished in thickness "wedge-like to a mere line at the termination." Although this vein-stone was identical in its character with that of the smaller block, its yield of fine specimens of sodalite and elæolite was less abundant than that of the latter. Unfortunately enough, the discovery of the minerals was not made in time to rescue this precious vein-stone from the hands of the quarrymen. Large masses of it including, it is believed, the best specimens, had been carted away and buried deep beneath a littoral road along Collins' Cove. The quarry was opened in the autumn of 1855. I visited it last spring when it was not being worked, and found amongst the debris of the quarry very good specimens of both minerals. At that time a portion of the boulder which contained the vein was still left. This I identified as a syenitic porphyry (Quarzfreier Porphyr of Senft). It is characterized by remarkably entire crystals of oligoclase of a greenish color, which make up the base of the rock. Thickly disseminated through the base are minute grains of hornblende and scales of mica.

Besides the sodalite and elæolite, the vein-stone is composed of orthoclase for the greater part; biotite in black tabular prisms; small crystals of zircon in octahedral prisms; fine stellate brownish-yellow flakes of xanthosiderite; and (probably) albite in small, irregular, reddish, granular masses.

At Litchfield, Me.—the only other known locality in America where sodalite and elæolite occur together—these minerals are further associated with cancrinite and, as at Salem, with zircon; but instead of occupying a vein as in the Salem instance, exist as accidental constituents of a granitic rock composed of quartz, feldspar and black mica,* thus constituting a miascite analogous to that of the Ilmen mountains.† The Litchfield rock, to be sure, is found only as erratic blocks; but the absence of cancrinite in the Salem boulder, and the dissimilarity between this and the Litchfield rock as to petrographic character, tend to preclude the possibility of the two having a common source.

Sodalite.—The sodalite from Salem has quite the same character as that from Litchfield and the Ilmen Mts., with specimens of which I have been able to compare it, excepting that the former in common with the elæolite, is contaminated with minute particles of what appears to be mica, thus rendering it very difficult to glean perfectly pure mineral for analysis. It is in crystalline, sub-translucent masses having an indistinct cleavage. Its lustre is greasy, and its color beautiful lavender blue.

Three separate determinations of its specific gravity were made with different portions of the mineral, giving the results as follows: 2·294, 2·303, 2·314.

* J. D. Whitney: Poggendorff's Annalen, lxx, 434.
† Senft: Classification und Beschreibung der Felsarten, 218.

Two portions of the mineral were used for the analysis. The one portion was treated with nitric acid, and the chlorine determined as chlorid of silver. The other portion was treated with diluted hydrochloric acid, whereby the silica, alumina, lime and soda were determined according to the customary methods.

Prof. J. D. Whitney,* in his analyses of the sodalite and its associated minerals from Litchfield, has so fully observed their chemical properties as to render superfluous here any remarks on the same subject.

Calculating all the sodium as soda, the following results are obtained:

Silica,	37·33
Alumina,	32·70
Oxyd of iron,	*trace.*
Soda,	24·31
Chlorine,	6·99
	101·33

But on the other hand, assigning to the percentage of chlorine enough of sodium to form chlorid of sodium in accordance with von Kobell's formula of this mineral ($\dot{N}a^3\dddot{S}i+3\bar{\dddot{A}}l\dddot{S}i+NaCl$), we have 18·17 for the percentage of soda in combination with silica.

Hence the analysis will stand thus:

Silica,	37·33	
Alumina,	32·70	
Soda,	18·17	
Sodium,	4·57	NaCl
Chlorine,	6·99	
	99·76	

These results are sufficiently in agreement with the established formula.

Elæolite.—The elæolite from Salem possesses all the constant physical properties of this variety of nepheline. Its color is dull green, its lustre greasy, and its fracture sub-conchoidal. It is sub-translucent, and in structure massive. Its specific gravity is 2·629. Its chemical composition I find to be as follows:

	I.	II.
Silica,	44·31	44·07
Alumina,	32·80	
Peroxyd of iron,	*trace*	
Lime,	·40	
Soda,	16·43	
Potash,	5·50	
Ignition,	1·47	
	100·91	

Schenectady, N. Y., September, 1859.

* Poggendorff's Annalen, lxx, 433.

ART. IX.—*Description of Nine new species of Crinoidea from the Subcarboniferous Rocks of Indiana and Kentucky;* by SIDNEY S. LYON and S. A. CASSEDAY.

PTEROTOCRINUS. *Lyon and Casseday.*

Asterocrinus, Lyon, Geol. Rep. Ky., vol. iii, p. 472.

SINCE our description of Asterocrinus was published in the 3d vol. of the Kentucky Report, we find the name to have been appropriated previously. We therefore propose Pterotocrinus as the name of our genus, which has now four well authenticated species.

Generic formula.

Basal pieces,	2	Mouth central,	1
Radial pieces, 1st series,	5	Column round, (?)	
" " 2nd series,	10	Arms ciliated and single,	20
" " 3rd series,	20	Wings or lobed pieces,	5 { variously formed.
Anal piece,	1 or more.		

Pterotocrinus depressus, n. s.

Body, depressed, subconical, twice as wide as high; below the free arms it presents the form of a very shallow, flattened cup, the pieces composing it smooth and of equal thickness, in some cases nearly a plane, the margin curving suddenly upward at the junction of the arms; the vault rises from the arms at a very low angle, rapidly increasing towards the centre, where it is nearly perpendicular. *Column*—very small, formed near the body of circular pieces of unequal size and thickness. *Basal* pieces, two, similar in size and shape; not prominent. When joined, they together form an irregular pentagon, raised a little above the general surface of the cup; slightly indented at their junction with the column, which has one or more pieces buried in the pit in which it is attached.

Radial pieces of the first series, five; subquadrangular, similar in form and size, nearly twice as long as high. Radial pieces of the second series, ten, subquadrangular, differing slightly both in form and size, resting near the centre of the outer margin or the first radials, a little more than half covering them. The radials of the third series are twenty in number, differing slightly in size, subquadrangular, nearly twice as wide as high, ten of them resting upon the radials of the second series, the other ten resting partly on these and partly on the first radials. The radials of the third series support from four to five brachials of quadrangular form, four times as wide as high; from the summits of this last series the arms, which before had been horizontal, become quite erect, and are composed of a double row of pieces which join by angular sides in the center of the arms.

Anal piece, one; obtusely angular below, fitting into a depression of the basal pieces, rounding to a point above, rising above the first radials.

The vault is divided into five triangular spaces by the wings, five in number; each space is covered with seven pieces of a compressed hexagonal form, three rising from the arms from the first row; upon these rest two; in the angular space at the summit of these rests the sixth, similar in size to those below it; it is squarely truncated above, thus becoming pentangular; its upper margin supports a small quadrangular piece; there are some quite small pieces above this last, in our specimen, probably one row which may form the mouth; between each of the five fields of the vault is a long lanceolate piece as wide as the other pieces of the vault, and about three times as long as those in the fields; the wings are attached to these long pieces by an articulating joint, and extend beyond the arms which embrace the vault, diminishing in thickness from their attachment outward, and terminating in a thin knife-like edge, equal in width to twice the height of the vault, the upper and lower margins nearly parallel; obtusely rounded at their outer extremity.

The arms are single, twenty in number, lying in sets of four between the wings, which, being placed immediately above the centre of the first radials, divide them into pairs, two from each of the adjacent radials falling into the spaces between each pair of wings. The arms are provided with a row of cilia for each side, formed of short joints, placed immediately in contact one above the other, filling the length of the arms which rise above the wings, about one sixth of their length. The dermal coating has been lost from all the specimens we have seen. The sides of the wings are strongly marked by a muscular attachment, by which they were moved. (?)

By reference to the figures it will be evident that this differs from all heretofore described species; by the bipartite basal pieces it is related to *Dichocrinus.*

Dimensions of Specimen of Medium Size.

Height of base, - - - -	·20 inches.
" " vault, - - - -	·30 "
Diameter of arms, - - - -	·75 "
Length of " - - - -	·70 "
Greatest width across the wings, - -	1·69 "
Height from base to top of wings, - -	·85 "

Geological Position and Locality.—Several specimens have been found in the third intercalated limestone, of the Millstone grit, Grayson Springs, Grayson County. Same horizon, near Dr. Baker's furnace, Edmonson County, Ky.

Pterotocrinus pyramidalis, sp. nobis.

Body without the arms and winged appendages:—the vault is a pyramidal pentagon, nearly twice as high as the greatest diameter at its junction with the calyx. The calyx is vasiform, the rim of the vase slightly reflected downwards: four times as wide as high. Columnar pit deep, irregularly oval, the longest diameter transversely disposed with reference to the anal side. Columnar facet round, perforation not visible (in any specimen that has come under our observation).

Basal pieces, two; prominent, outer margins thick and rounded, joined by a straight line to each other, the posterior margin having a deep angular notch, while the anterior side is but slightly indented at the junction of the pieces; both pieces are irregularly pentagonal, nearly of the same form; that on the right of the anal piece being a little the largest.

First radial pieces, five; broad, sides diverging from below upwards, three times as wide as high, the ends of the upper margins parallel to the lower; about one-half of the length of the upper margin circularly depressed for the reception of two of the second radials to each.

Second radial pieces, ten; small, obscurely quadrangular, resting in the circular depression at the summit of the first radials, each pair being separated one from the other by a very minute anomalous piece, which rests on the center of the first radials between them; it is nearly round, and rises under the suture which marks the separation of the second radials, and is about one fourth the height of those pieces.

Third radials, twenty; small, differing both in size and form; nearly as high as wide, divided into five groups, disposed on a curved line, the exterior pair of each group resting partly on the first radial and partly against the oblique outer margins of the second radials. The central pair of all the groups rest upon the second radials, against those on either side and against each other. The articulating surfaces to which the arms were attached are slightly concave; the upper side deeply indented by a perforation into the body, partly in the arms and partly in the pieces immediately above them.

Anal piece, one; large, as compared with other species of this genus, long, hexagonal, obtusely pointed below, resting in the deep angular notch in the basal pieces; diminishing gradularly upward, supported on either side by the first radials; acutely pointed above, reflected toward the body, and supporting on either side one of the third radials.

Our specimens are weathered to such an extent that all surface markings (if they ever existed) have been removed.

Summit.—The summit above the arms is divided into five fields, nearly of the same form and size, that above the anal side being a little the largest; they are covered by hexagonal or pentagonal pieces higher than wide. The largest field has four pieces in the first row, the other fields having only three each; the outside of each of the first series is articulated with a piece at the base of the wings, the second range and the superior margins of the third radials. Dividing the first range of pieces are five projecting angular pieces inserted between the groups of the first range, touching the third radials at one of its angular points, rising from them by a line slightly curved outward; they articulate with the first and second range of the five fields and with the wings, in the same manner as *P. depressus* (nobis). The second range consists of three long pentangular pieces, in the field above the anal piece, and two to each of the others. The third range consists of one lanceolate piece to each of the regular and two (?) to the irregular side. Between the fields of the summit is a broad articulating surface, about as wide as the pieces covering the fields, formed by the reflected margins of those pieces, and the supporting piece at the base of the wings. *Wings*—none have been found attached; great numbers are found loose, which we refer to this species. They are of various forms. The articulating surface applied to our specimen precisely fills and fits the articulating surface upon it; they are thick near the junction with the body, curved both above and below, gradually running to a point, five times as long as thick, broad on the upper surface, gradually thinning downward; the outer end round and pointed; they are frequently found bifurcate near the end and double pointed: affixed to the specimen they radiate regularly and horizontally, the points being about as high as the summit of the specimen.

Mouth, central.

Arms, twenty; form unknown.

Column, unknown.

Dimensions.

Greatest diameter of calyx, - - -	·80 inch.
" " " basal pieces, - -	·49 "
" " " columnar facet, - -	·05 "
Height of calyx, - - - -	·18 "
Height of specimen, small part lost, - -	·75 "
Length of wing articulations, with body, -	·45 "
Width " " " " -	·12 "
Height of first radial pieces, - - -	·12 "
Width " " " - - -	·35 "

Geological Position and Localities.—Found only in beds near the top of the third limestone of the Millstone grit series of Edmondson, Grayson and Breckinridge counties, Kentucky.

Fragments very abundant, good specimens rare. Beds from one to two feet thick are found composed of a mass of the remains of this crinoid cemented together, forming a distinctive and characteristic bed of the 3rd limestone.

Pterotocrinus rugosus, nobis.

The condition of our specimen is such that a particular description cannot be made: the arrangement of the parts, however, is evidently quite similar to that of *P. depressus*. The basals, first, second and third radials, are present, together with parts of the wings and a portion of one of the arms. This species differs remarkably from *P. depressus*, in the greater thickness of the pieces, prominence of the base, the knobby protuberances upon it and upon the first radials, the depth of the columnar pit, as well as by its roughness and more robust appearance.

Geological Position and Locality.—A single crushed and imperfect specimen was found in the lowest siliceous mud bed,* at the Falls of Rough Creek, Breckinridge county, Ky. Fragments of this species are quite abundant.

The beds at Rough Creek, Grayson Springs, Grayson county, and Baker's Furnace, Edmondson county, are doubtless the equivalents of each other. In the western edge of Breckinridge county, they are separated by a thick sandstone, where the upper division of the limestone appears to be the equivalent of the beds above enumerated. The size and proportions of this species is about the same as *P. depressus.*

Zeacrinus ovalis, sp. nobis.

Body—When the arms are closed the body is ovoid, the length being equal to about twice the diameter; concave at the base.

Basal pieces, five; minute, forming a pentagon, slightly indented at the sutures marking the division of the pieces. When the column is present, the basals and about half of the subradials are concealed. *Sub-*

* Equivalent to limestone No. 3, Millstone grit.

radial pieces, five; four of which are of the same form and size; lanceolate, obtusely pointed at the superior extremity; the fifth is large, irregularly pentagonal; one of its sides supports two of the anal pieces. *First radials*, five; differing considerably in size; the anterior one being symmetrical and the largest, the other four are unsymmetrical, the inferior side toward the anal field being longer than the inferior anterior side; they diminish regularly from the anterior to the posterior side. The *second radials* in all but the anterior ray are pentagonal, twice as wide as long, on the upper beveled sides of each supporting two arms. The second radial of the anterior ray is twice as wide as high, supporting on its upper side a third radial similar in size and form to the second radial of the other four rays, and like them it also supports a pair of arms. The *arms* are ten in number. The antero- and postero-lateral rays are composed of three quadrangular and one pentagonal piece each; one of the antero-lateral rays has the same form and number of pieces as the other and the anterior rays are composed of two quadrangular and one pentagonal piece each, somewhat larger than the pieces of the other rays. The inner upper margin of the last piece in all the rays support each one division of the ray, which is composed of about forty-five quadrangular pieces each, gradually diminishing in size to the end of this division of the ray, which is thus terminated in a point. The outer margins each support a branch which is bifurcated on the fourth, fifth or seventh piece, similar in form and arrangement to the first branch of the rays, the pieces being nearly as high and about half as wide as those below. The inner beveled upper margin of each last piece supports a branch which is of the same form as the inner branch below, composed of quadrangular pieces, terminating at the same height as the other branches. The outer margin of the second bifurcation of the anterior ray has in like manner an undivided branch of the same form and length; all the other rays are again bifurcated on the fifth or sixth piece, each division bearing two branches similar in form to the upper part of the inner divisions below; the anterior ray in its last division consisting of six fingers, all the others having eight fingers each, 46 fingers in all. The *fingers* are ciliated their entire length with a row on their inner margins.

Anal pieces, eight; the two inferior pieces rest partly on the subradials and partly against the first radials on either side, one rising nearly as high as the upper margin of the first radial against which it rests; upon these two rest two of the second range, and on the left side one quite minute, making the upper margin of the field nearly horizontal; the last two support one subquadrangular piece (the largest in the anal field) above which are three of the same form rapidly diminishing in size, thus running the anal field to a point.

Columns delicate pentagonal (near the body), composed of alternately larger and smaller thin pieces.

Dimensions.

Total length,	1·25 inches.
Greatest diameter,	·62 "
Height of calyx,	·05 "
Greatest diameter of calyx,	·50 "

Length of finger, first bifurcation, - -	·94	inch.
" " second " - -	·80	"
" " third " - -	·45	"
Depth of columnar pit, - - -	·05	"

Geographical Position and Locality.—Found in the third limestone of the Millstone grit beds of Grayson, Breckinridge, Edmondson and Todd counties, Kentucky.

It has not been found in any other geological horizon in the Millstone grit beds of Western Kentucky, and may be classed amongst the distinguishing fossils of this particular bed of limestone.

Remarks.—Our species, in the arrangement of the calyx, approaches *Z. Worthenii* (Hall), from which it differs in having three instead of four primary radials; also in the number of pieces in the first branches of the rays. It differs from *Z. magnoliæformis* (Troost) in the arrangement of the calyx, the first radials and subradials being much shorter in our species.

Cyathocrinus dekadactylus, nobis.

Calyx, vasiform, spreading rapidly from the base, comprising all the pieces up to the second radials. The pieces forming it are thick, tumid, with long strong arms. Vault unknown. Column obtusely pentangular, covering nearly one half of basals; articulating surfaces crenulated on their borders.

Basal pieces, five; pentangular, forming together a marked stellate figure; upper facets prolonged into nearly acute angles; under surface slightly excavated.

Sub radials, five; hexagonal or heptagonal, somewhat irregular in size; they alternate with the basals; very tumid and thick.

Radials, five; the first are large, various in their shapes, generally septangular, sometimes broader than high, in other instances as high as broad; they support each two other radials, the first of which are thin, parallelogramic pieces much rounded on their outer surfaces; the last radial pieces are pentagonal, rather acuminate, more than twice as wide as high, and support on their bevelled edges two arms each, making $5\times2=10$ arms in all.

Arms.—The arms are apparently without any subdivisions throughout their whole length. They come off immediately from the radials; the first two armpieces are anchylosed, the arms becoming free and separate on the second armpieces. They are composed of thick pieces, rounded on their backs, about equal in size, and regularly superimposed upon each other. They are about one half as large as the last radials, and have a shallow sulculus (ambulacral groove,) (?) on their inner surface.

Anal piece, one; trapezoidal in shape, the upper margin reflected, pentangular, the middle portion of the piece intumescent. In two well preserved specimens we cannot trace any pieces superposed upon the anal piece.

At the juncture of any three pieces composing the calyx, there is a depression, which attains its greatest depth along the suture lines of the pieces. These depressions are an easily distinguishable character of this species. The surface markings and proboscis (if one existed) are not preserved in any examples before us.

The spaces between the radials supporting the arms are quite extended and present the förm of a Doric arch.

Geological Position and Locality.—Sub-Carboniferous beds of Montgomery county, Indiana. Rather rare.

Cyathocrinus hexaductylus, sp. nob.

Body, compressed, crateriform, robust, with long arms; surface covered with minute granulæ, arranged apparently in rows parallel to the margins of the pieces. The column is comparatively small, composed of alternating thick and thin pieces; the articulating surfaces are crenated, the larger and thicker pieces project over the thinner ones, the projecting edges being distinctly rounded.

Basis—Composed of five sublanceolate pieces forming together an irregular pentagonal figure. The columnar pit is small, occupying about one-third of the basis, slightly depressed, with a large central opening.

Subradials—Three of them are nearly equal in size, hexagonal, generally higher than wide; the remaining two are larger than the others, not constant in their shape, being quadrangular, pentangular and hexangular in different individuals. They alternate with the basals, are very convex, from their centres project two obscure folds which proceed to the radial pieces, where they join similar folds, thus enclosing a triangular depression, which is considerably deepened at their junction with any two radial pieces. These plicatures are faintly marked, and can be seen only on specimens when in a good state of conservation.

Radials—The first are large, wider than high, fitting into the retreating angles of the subradial pieces, equal in size; near the middle of the upper surfaces of each, and at the juncture of the two adjoining folds from any two adjoining subradial pieces, come off the remaining radials, which are two in number, small, the last axillary. The second radials fit into a facet which is scooped out of the upper part of the face of the first radials, and are bordered by a small emarginate plicature, slightly raised and reflected outwards.

Anal pieces, two; the first is obscurely pentangular, the fifth facet being hardly perceptible; it is hemmed in entirely by the surrounding pieces, much smaller than they are; the second is irregularly shaped, about the size of the subradials, and bears upon its upper edge an arm.

We have never been able to procure a specimen which had a vault preserved, and can therefore give no description of it. We doubt the existence of a proboscis. The figure* is drawn the size of nature.

Arms.—Following the large first radials are three small, rounded pieces, two quadrangular, the third axillary, giving off two branches, which are long, tapering, composed of a single row of stout pieces; three branches give off at intervals on either side, long filamentous pinnulæ, which extend the full length of the arms. These are composed of small pieces resembling those forming the arms, and are similarly arranged. There are five pairs, rising from the regular rays. From the anal field rises a single, long, tapering arm, not bifurcated as the others are.

* To be given hereafter.

None of the family of Cyathocrinidæ have yet been described in which the anal field always bore an arm. We supposed at first that this sixth arm was only an anomalous condition of the individual, but having in our possession six or seven specimens, all showing this arrangement, its actuality is placed beyond cavil.

Hall has described (Iowa, vol. ii, p. 625, pl. 18, fig. 7, 8,) *C. spurius*, to which our species is most closely related; it differs in the sixth arm, which alone constitutes it a distinct species. See also *P. Meekianus*, Shumard (Geol. Rep. Missouri, p. 188, pl. A, fig. 7, a, b).

Geological Position and Locality.—Found in sub-carboniferous beds of Clear Creek, Hardin county, Ky., and Montgomery county, Indiana.

Actinocrinus Indianaensis, nobis.

Calyx, subglobular, ornamented with hieroglyphic sculpture. Proboscis long and thin, arms very long, articulations similar in size.

Basal pieces, three; large, extending beyond the supra columnar oint. The margin of the basis projected into a flange.

Radial pieces, 5×3; the first is large, heptagonal, its central portion marked by a double sigmoid carina; its upper facet is angularly notched, into which depression the second radial pieces fit; they are longer than wide, pentagonal, supporting the third radials, which are axillary. These last give off on either facet two radials of the second series, the last of them axillary, and support two rows of three brachials each, the last ones of which are large, tumid, and quite protuberant; here the free arms come off. This is the case except in the antero-lateral ray, where the radials of the second series support only one row of brachials on each beveled edge, thus giving $4\times4+2=18$ pairs of arms.

A prominent carina commencing on the first radial piece extends over the middle of all the pieces, distinctly marking the ramifications of each ray.

Interradials.—Three are interposed between each two rays, except of course, on the anal side, (a large hexagonal one followed by two smaller ones) their middle portions rise into mammiform protuberances.

Anal pieces, seven; hexagonal, of nearly the same size. First one, followed by a row of three, then two; above them a single one, which completes the pyramid. The ornature of these pieces is the same as that of the interradials. We have one specimen showing elaborate hieroglyphic markings; doubtless this was the character of the original ornature of the surface.

Vault—about equal in size to the calyx, composed of many small polygonal pieces, which are intumescent, sometimes prolonged into small sharp thorns. The whole is surmounted by a thin, long proboscis, composed of very minute pieces, and closed at its extremity, where it widens out into a fusiform shape.

Arms.—When in a normal condition there are eighteen pairs of arms, long, dividing into two branches almost immediately after leaving the brachials, each branch running out to the end without further bifurcations. They are formed of a double row of pieces, which at their juncture present a serrated appearance; towards their outer extension they become gradually smaller, until at their end they are quite attenuate and generally curved inward. They are deeply sulcate on their inner surfaces, and bear on their lower outer edges long filamentous frimbriæ, which are set closely together, one to each piece forming the arm; these are composed of five or six joints, each having a direction outwards or downwards. *De Koninck and Le Hon* figure (Recherch. Crinoid. foss., Pl. IV, fig. 3,) a bundle of arms which so nearly resemble the arms (with head attached) of one of our specimens, that we cannot but regard them as perfectly identical. They refer them to *Actinocrinus stellaris*, to which species they certainly do not belong. A *stellaris* has 20 arms, (our species 18) arranged in 5 rays of 4 each, with comparatively large interstitial spaces, while the arms figured are thickly crowded together, coming off as they did in almost a closed circle, without any interstices of moment to divide them into separate bundles of arms.

Geological Position and Locality.—Sub-carboniferous beds of Montgomery county, Indiana. It is quite an abundant fossil, and generally found in a good state of preservation.

Actinocrinus Coreyi.

Body, globular; the vault about one third higher than the calyx tumid; vault surmounted by a large knob.

Basal pieces, three; forming together an irregular hexagon, scooped out, producing a depression occupying about one-half of the superfices.

Radials.—The first are large, irregularly hexagonal, wider than high; their upper surfaces are hollowed, receiving the second radials, which have a corresponding convexity; they are thin but wide, flabelliform, and bearing the third radials, which are flattened pentagons, axillary, and support on each beveled edge three unsymmetrical brachial pieces, five or six times wider than high; thence the free arms.

Interradials, three; the first is very large, generally an elongated octagon; above this are two irregular, thin, high pieces, each having a small depression near the center of their lower surfaces, and interposed between the arms.

Anal pieces, seven; the first is large, having a depression corresponding to that described in the radials; in a row with the first radials, but a little larger than them, hexagonal, its vertical diameter is greatest on its three upper facets, bearing three pieces; they are smaller than the first anal piece; the one on the superior facet is irregularly hexagonal, the lateral ones are long, rather unsymmetrical, and adjoining the neighboring radials and brachials, which nearly enclose them; upon these are three considerably smaller polygonal pieces, which complete the calyx. All of the pieces described above are tumid, without any visible external markings.

This intumesence renders the sutures of the pieces very marked. The arms are ten in number, very strong and thick, coming off in five pairs; we cannot describe them farther, as they are not preserved, and this is the only example we have of this species.

Vault.—The vault is surmounted by a very prominent knob, from which are projected in the directions of the arms, five plicatures, formed of four or five (generally four) massive tumerous pieces; they are much larger than any of the other pieces of the vault, with the exception of the central knob. Between each two of the plicatures is an interradial field, containing from eight to ten polymorphous pieces; they are generally arranged in a pyramidal form; often the vertex being a piece considerably larger than any of the others; the basal pieces of the pyramid are in most cases thinner and longer than any of the others; these, as well as the remainder of the pieces of this species, are turgid and massive, and like those of the calyx, are destitute of ornament.

On the side of the vault, above the anal field, is a considerable ovoid intumescence, composed of nearly thirty small pieces, whose surfaces are quite plain and level in contrast to the other pieces of the vault; they are arranged in nearly parallel rows, as follows, commencing with the lowest: 1–3–4–5; on the fifth row (which nearly completes this field—there are but two more rows) supervenes an ovoid opening, about one line in length, without a proboscis. This is the only opening upon the vault.

We have named this elegant species after Mr. O. W. Corey, to whom we are much indebted for many favors.

Dimensions.

Diameter of base, - - - -	·32 inch.
Greatest diameter, - - - -	1·30 "
Height to arms, - - - -	·35 "
Total height of body, - - -	1·10 "
Diameter of axillary articulation, - -	·30 "

Geological Position and Locality.—Rare in sub-carboniferous limestone near the top of the knob stone bed, Hardin and Allen counties, Ky., and same geological horizon, Washington county, Indiana.

Genus Onychocrinus, nov. gen. *Lyon & Casseday.*

Generic Formula.

Basal pieces, - - - - -	3
Subradial, - - - - -	5
Radial, - - - - - -	5×5
Brachial, - - - - -	4 or 6×5
Interradial, - - - - -	20 or 25×4
Anal, - - - - - -	5 to 7
Interaxillary, - - - - -	1 to 3×5
Arms, - - - - - -	5 pairs.

Generic description.—This genus, in the shape of some of the pieces and in its general form, resembles more closely *Forbesiocrinus* than any other; yet they are so widely different in other respects that it will require no great perception to distinguish between them. The column near the calyx is cylindrical, large, composed of very thin articulations similar in size to each other; perforation small.

The radials are large, and form, together with the brachials, a continuous line: the arms are quite robust, furnished with strong pinnulæ.

Interradial fields, triangular in general shape; an anomalous one on the anal side. Anal field—long and narrow: one to three small interaxillary pieces on each ray.

Basal pieces small, subradials large, pentangular, alternating with the radials.

Onychocrimus exculptus, n. s.

Calyx vasiform, spreading rapidly to the base of the free arms, together with the arms resembles much the talons of a bird, whence its generic name; surface ornamented with minute granules.

Basal pieces, three; their under surfaces concave, forming a saucer-shaped depression, which was wholly filled by the column; they are low, rather thick, upper surfaces prolonged into an obtuse angle; the column facet marked by small short striæ on the outer margin.

Subradials, five; large, four are pentagonal, two of which are larger than the remaining two; their upper articulating surfaces form quite sharp angles; the fifth is hexagonal, its superior surface parallel to the inferior, smaller than the remaining pieces.

Radials, generally five in each ray; the first row are very large, heptagonal, except in the postero-lateral rays, where they are hexagonal, having but one facet on the side next the first anal piece instead of two; their lower surfaces rest on the retreating angles formed by the subradials, with which they alternate; their superior margins are horizontal, on which are imposed the second row of radial pieces; these are smaller than the first, hexagonal, nearly twice as wide as high. The third and fourth rows are similar in form, but become gradually smaller; the fifth rows are heptagonal, axillary, and support on each beveled edge a row of two or three brachials, which are smaller than the radials, obscurely hexagonal, bounded by wave-like lines; near the center of their inferior borders they are prolonged into minute uvulæ, or little tongue-like projections, similar to those found in some species of *Forbesiocrinus*, (and described by Hall as patelloid pieces,) which fit into corresponding depressions in the adjoining pieces.

Upon the last brachial pieces rest two arms; they are as long as the body, robust, composed of exceedingly stout pieces, grooved by a deep sinus, decreasing gradually in size to their outer extension; they are similar in form to the brachials described above; from either side of these arms, alternately disposed, are stout, short pinnulæ, composed of thick pieces stretching in a direction outwards and upwards, without tentaculæ. Commencing with the first two pieces of the arms, they come off in two pairs, then on the next two pieces come off again two pairs, but on the alternate side from the first two; this arrangement is continued throughout their whole length, the pinnulæ becoming smaller and more closely crowded together toward the termination of the arms.

Interradials.—These vary from twenty to twenty-five, according to the age of the individual. The first is large, septagonal, situated between the first two radials of each row, followed by three pieces about half the size of the first; the middle piece of the three is an elongated pentagon, smaller than the other two, which are hexagonal, upon the outer upper facet of each of which, and lying against the radials, are one or two smaller pentagonal pieces; then follow (in the specimen we have before us) fifteen yet smaller pentagonal pieces; they are arranged in the form of a hemispherical arch, depressed towards the disc of the stomach, the sides of the arch being extended up along the radials and brachials as far as the commencement of the free arms.*

* See also under description of Vault.

Interaxillaries.—In a line with the radial, and between the two opposite arm pieces, is one to three quite small pentagonal interaxillary pieces; sometimes two yet smaller pieces occur.

Anal pieces, one; small, quadrangular; one is superimposed on the subradial, followed by three pieces of like form and size, and similarly disposed.

Vault.—The vault of this remarkable crinoid resembles so much some of the *asteriada* that we may consider it as one of the connecting links between the crinoidea and the star-fishes. The specimens in our possession do not show distinctly the whole of the vault, so that a description must necessarily be imperfect until better examples are found. The plates which we have described above as interradials, form the greatest portion of the perimeter of this upper surface; the remainder of the perimeter is formed of a row of pieces lying on the anal side; their relative position is as follows: viewing it from the anal side, in the position in which the animal grew, we see a row of small pieces (interradials?) which extend up along the radials and brachials of the left ray, fitting into the serrated depressions existing at the junctures of any two pieces. This is found only on the left ray, the pieces forming the right being squarely truncated and without other pieces attached in any way to them.

The general surface of the vault is depressed, the edges being raised and curved inwards. From the centre, and in the direction of the arms, extend five rays, composed of two rows of large granular pieces, one row alternating with the other. We cannot discover pores in any of these pieces, which are most probably analagous to the *ambulacra* of the *asteriada*.

The interstitial pieces lying in the fields bounded by the five rays of larger ones are very small, granulose, and arranged without any apparent order.

It is impossible, from the fragmentary portions in our cabinets, to trace the farther similarity between this genus and the star-fish, as the central portions of all are so concealed that we cannot make them out clearly.

Geological Position and Locality.—Found at Clear Creek, Hardin county, Ky., in sub-carboniferous rocks near the upper part of the sandy mud-beds of the knobstone. In beds of similar age in Montgomery county, Ind. Good specimens are rare.

Louisville, Ky., Nov. 1, 1859.

ART. X.—*Theoretical Determination of the Dimensions of Donati's Comet;* by Prof. W. A. NORTON.

I HAVE recently undertaken to bring the theory developed in a previous number of this Journal,* in an article entitled "Dynamical Condition of the Head of a Comet," to the test of numerical computations, by determining, by calculation, the theoretical dimensions of the great Comet of 1858. The more important results may be briefly stated; the complete discussion is reserved for a subsequent number of the Journal.

* Vol. xxvii, [2], 86.

It appears from the investigation that, confining our attention to the outer bright envelope, the process of ejection of nebulous matter from the nucleus was mostly confined to a certain portion of its illuminated side lying nearest to the sun, and that the limiting angle of inclination of the jets to the line connecting the centre of the nucleus with the sun, was 25°. From this circumstance it resulted that the envelope had nearly a circular form. The lateral dispersion of these luminous jets, as they were flowing away into space, under the influence of the sun's repulsion, or, in other words, the breadth of the tail, was partially due to the directions, more or less inclined to the radius vector, in which they originally issued from the nucleus; but another and highly efficient cause coöperated to produce that result. If, as we conceive, certain portions of the cometic matter, at the surface of the nucleus, were brought by some action of the sun into the condition to be repelled by both the nucleus and sun, we may make two suppositions with regard to the forces of repulsion thus developed; that the force exerted by either body was of the same intensity for all the particles acted upon, or that it varied from one particle to another. It is not easy to decide, upon *à priori* grounds, which of the two suppositions is the most probable. The latter is certainly no less so than the former. If we adopt this as a fundamental hypothesis, we have an efficient cause in operation adequate, in connection with that already mentioned, to the development of the tail of the comet, in all its vast proportions; and which may incidentally have produced the special phenomena observed,—as the supernumerary tails, and the alternate bright and dark bands seen to traverse a certain portion of the principal tail. The ejected particles that are unequally repelled, by both the nucleus and sun, do not part company in consequence, while they are in the vicinity of the nucleus, nor materially while within the limits of the envelope, for the reason that the ratio of the repulsive forces of the two bodies remains constantly the same; but as soon as they pass out of the sphere of influence of the nucleus they are analyzed by the solar repulsion, and driven off by it into space, in separate and diverging paths. The various susceptibilities to repulsion possessed by the particles have accordingly no sensible effect upon the dimensions or form of the envelope, but may give rise to a wide lateral dispersion of the flowing streams that make up the tail of the comet. The particles that are most energetically repelled go to make up the preceding or convex side of the tail.

I have made the calculations, regarding the solar repulsion as varying between certain prescribed limits. The determinations of the breadth of the tail, at various points of its length, accord with the results of observation; at the same time that the tail is found to have the form and positions actually observed.

The supernumerary tails observed were but lines of receding particles subject to much greater forces of repulsion than the other particles ejected from the nucleus. All such collections of matter would, of necessity, be in advance of the principal tail, and lie in a curve that would approach more nearly to a straight line. Their position makes known the intensities of the repulsive forces to which they owe their separate existence.

An interesting result of the investigation is that the alternate bright and dark bands so distinctly seen to traverse a certain portion of the tail of Donati's comet, in nearly parallel directions, on the evening of Oct. 10th, had each the position of a line connecting particles which started from the region of the nucleus at a certain previous date, and at the same instant of time. They accordingly find their natural explanation in corresponding alternations in the quantity of nebulous matter given off simultaneously from the nucleus. The most probable cause for such alternations of discharge that can be conjectured is that the nucleus turns about an axis, and so presented periodically different sides to the sun, which were unequally influenced by his inciting action. If this be the true explanation of the phenomenon, we have in the observed distance between contiguous bright bands, the means of determining the period of rotation; or, at least, the shortest interval of time in which the rotation can be completed. If we take this distance at 1°, the period of rotation comes out about 24 hours.

There was a special cause of lateral dispersion at work in the case of the cometary particles that, on their return, came very near the nucleus. Such streams of particles must have been repelled off obliquely, and may very well have presented the appearance of luminous jets issuing from the sides of the nucleus, and have formed curved terminations to the inner envelope. From the dispersion thus produced resulted an absence of matter, or a dark space, behind the nucleus, whose varying boundary was determined by the intersections of lines of particles unequally repelled by the sun.

The indications are, that the formation and gradual expansion of one envelope after another, may have arisen from the process of ejection beginning in all instances high up in the photosphere surrounding the nucleus, and gradually extending downward to the vicinity of the solid surface. It appears, upon investigation, that if this descending action were to proceed according to a certain uniform law, the outline of the envelope would recede from the nucleus at a uniform rate. This process of evolution of cometary matter, in whatever it may consist, is probably auroral in its origin and character, and has its counterpart on both the earth and sun.*

New Haven, Nov. 30, 1859.

* To render the investigation more complete, I have considered the case of the cometary matter being projected from the nucleus, without experiencing any repulsion from its mass.

Art. XI.—*Geographical Notices.* No. X.

The Mountains of Western North America.—Having previously spoken of the publication of Warren's memoir to accompany the map of the western territory of the United States, and also of the map to which it refers, we now copy the following paragraphs from the conclusion of the Memoir, in order to meet the erroneous opinions which are prevalent in respect to the "Rocky Mountains," and the erroneous presentation of their direction which is given in most of the popular maps.

"The mountains in our territory west of the Mississippi river, from where they rise above the horizontal strata of recent geological formations on the east to their disappearance under the waters of the Pacific Ocean, form a nearly continuous mass of upheaved ridges, with occasional intervening level plateaus. The direction of the central line of this mass between the 32d and 49th parallels of north latitude, is about north 20° west. The greatest width perpendicular to this direction is along the line passing from the vicinity of San Francisco through that of the Great Salt Lake to Fort Laramie. This distance is about 1,000 miles, or, if we include the Black Hills of Nebraska, 1,125 miles.

"The great mountain mass, of which that in our territory forms but a part, extends with varying breadth nearly on the line of a great circle of the globe from Cape Horn north to Behring Straits, and thence south along the western part of Asia to the island of Sumatra. Its length is about 240 degrees, or 18,560 miles, being two-thirds of the circumference of the earth.

"The area occupied by and included in this mountain mass in our territory, is about 980,000 square miles. Large as this is, it is probably only a small portion of the upheaved formations between the 32d and 49th parallels. A few ridges and peaks projecting above the surface of the Pacific as islands, or above the level tertiary and cretaceous strata of the eastern plains, give evidence of the existence of vast areas whose extent must forever remain unknown. Throughout the portions now visible, proofs are abundant of great abrasions; in some cases whole ridges even, having been swept away or broken into separate portions.

"Already enough has been learned to establish the existence in these mountains of the equivalents of many of the geological formations; and it is probable, when investigations have been carried to the same extent as in the civilized portions of the earth, that the geologist will find here new and still more complex fields for research.

"The classification of the separate parts of this mountain mass, so as to present its physical characteristics clearly to the mind, is a great desideratum. It has in part been attempted at various times, but as yet unsuccessfully from the want of sufficient information; the theorist's idea being often proved to be wrong by new discoveries almost as soon as uttered." * * *

"The publication of the Pacific Railroad maps will probably change some of the former ideas of these mountains, and give rise to new speculations as to their directions, equivalents, and connexions of different parts. Every one knows how easy it is to generalize ideas where facts are few, and in accordance with this, those who have travelled most in the region have theorized the least, having seen the immensity of the subject and the difficulties which must be overcome to comprehend it. Those who have investigated merely the travels of others, have had only the imperfect representations of the latter on which to theorize.

"It may not be inappropriate here to give some of the general ideas which have successively prevailed in regard to these mountains.

"In the earlier periods of North American discovery it was known that there were mountains in the interior at its northern and southern parts, and rivers flowing from them to the two great oceans east and west. It was natural to connect these mountains by hypothesis, and to consider them as one great chain, separating the sources of these streams. Such an idea prevailed at the time of Humboldt's New Spain. Even now many well informed persons consider that a road has but one mountain summit to cross from the Mississippi river to the Pacific Ocean.

"When, after the publication of the charts of Vancouver, map makers became aware of the extent of the mountains near the Pacific coast, nothing seemed more natural than to suppose two great mountain chains—one near the Pacific and one in the interior. If this theory were true, we should find a great longitudinal valley between the ranges similar to that separating the interior mountains from the Alleghanies, and we should have but two mountain summits to pass between the Mississippi and the Pacific. This idea is practically as erroneous as that of one summit, although it still prevails. Such a prominent place did this longitudinal valley hold, in the opinions of geographers of earlier times, that we find in Humboldt's New Spain: 'M. Malte Brun has started important doubts concerning the identity of the Tacouche Tesse and the river Columbia. He even presumes that the former discharges itself into the Gulf of California: a bold supposition, which would give the Tacouche Tesse a course of an enormous length. It must be allowed that all that part of the west of North America is still but very imperfectly known.'

"The explorations of Lewis and Clarke proved that the Tacouche Teche did not empty into the Gulf of California, and that it was probably the source of the Columbia. Without considering the character of the pass of the Columbia river through the Cascade range, the belief now became general that the overland route in this latitude crossed but one summit, and was therefore more favorable than any other. This erroneous idea with some still prevails.

"The idea of rivers traversing great mountain chains, now known to be so common in the mountains west of the Mississippi, was so repugnant to the opinions of even philosophers in earlier times, that we find Humboldt saying, 'every geographer who carefully compares Mackenzie's map with Vancouver's will be *astonished* that the Columbia, in descending from the Stony mountains, which we cannot help considering as a prolongation of the Andes of Mexico, should traverse the chains of mountains which approach the shore of the great ocean, whose principal summits are Mount St. Helen and Mount Rainier.'" * *

"The distinguished explorers, Lewis and Clarke, having determined that the Columbia river broke through the Cascade range, considered from the size of the Willamette at its mouth, that it also broke through this chain, having its source in the Rocky mountains, near the position of the Great Salt lake. We then see the American maps representing mountains surrounding the valleys of the Columbia and Colorado, and separating them from that of the San Joaquin and Sacramento. On the English maps of that date, the Sierra Nevada is not represented, and two or three great rivers are made to flow from large lakes in the interior to the Pacific; nearly all of their compilers making false applications of the principles of hydrography laid down by Humboldt.

"The first map which represented these rivers and lakes correctly was that of Captain Bonneville, of which I have given a reduced copy. There we see the Great Salt lake and Bear river and Utah lake forming one basin; to the west lies the Mary or Ogden's river, with its lakes forming another enclosed basin; the San Joaquin and Sacramento rivers in their right position; and the Willamette reduced to its proper length. The positions given on this map are not geographically correct, nor are their many mountains indicated; but it gives the first correct idea of the hydrographic character of the country; and by giving too little rather than too much, escapes the errors into which others had fallen.

"The explorations of Captain Frémont fixed these great rivers and basins in their proper geographical positions; but his maps have given rise to many erroneous impressions in regard to the mountain ranges. Still, making a 'false application of the principles of hydrography,' he represented all the basins as if surrounded with mountains or 'rims,' and thus introduced mountain chains which have no existence in nature.

"Since Frémont's expedition began, a large portion of the area of these mountains in the territory of the United States has been examined, and many new attempts have been made to systematize the knowledge acquired. The most important theory advanced is that of parallelism in the ranges, the foundation of which I shall briefly indicate.

"On the map of Lewis and Clarke the Rocky mountain ranges are represented parallel to each other with a northwest trend. That this was their theory is evident, from the fact that they indicated the Black Hills about the source of the Shyenne as having this same trend, though they never saw them, and only knew of their existence from hearsay.

"The maps of Captain Frémont showed a parallelism and general north and south direction of the mountain ranges from the Wasatch, east of the Salt Lake, to the Sierra Nevada, including all the numerous intermediate ranges.

"The maps made by Major Emory, near the 32d parallel, and in New Mexico, showed again a remarkable parallelism of the mountain ridges, those in this latitude having a northwest trend nearly parallel to the Rocky mountains, as shown by Lewis and Clarke.

"The maps of Lieutenants Abert and Peck, of Lieutenant Simpson, of Lieutenant Beckwith, Lieutenant Williamson, and Lieutenant Parke, have all shown a local parallelism to exist in different parts of the mountains. The systems of ridges have courses varying from a few degrees north of east to north 45° west.

"The idea has lately begun to prevail that this local parallelism is the characteristic of the great mountain mass throughout its whole extent. Whether this idea has been true or not it has been attended with some practical advantages. Instead of one or two main summits for an overland road to pass, it shows us that we must expect many. On every route explored across the continent, at least four well-defined summits have been discovered, and on some of them many more. Some of these ridges enclose interior hydrographic basins. Others are traversed by rivers, but the passes thus made are generally impracticable, and, for the purposes of travel, might almost as well never have existed.

"In many places, however, the mountain ridges have not this local parallelism, of which a few instances will be cited. The Uintah mountains, east of the Great Salt Lake, trend nearly east and west; the Wind River mountains about north 45° west; and the Humboldt range about north 20° east; these three ranges being comparatively near to each other.

"Humboldt, in speaking of the Sierra Nevada, says, 'it soon separates into three branches.'

"Lieutenant Abbot, in the sixth volume of the Pacific Railroad Reports, says: 'Shasta Butte, although generally considered a peak of the western chain of the Sierra Nevada, is, in truth, the great centre from which radiate, beside several smaller ridges, the Cascade range, the Coast range, and the western chain of the Sierra Nevada.'

"There are many other portions of this mountain region from which the ridges seem to radiate. Such as Long's Peak, the junction of the Sierra Nevada, and Coast ranges in Southern California, &c., as is evident on an inspection of the map. The parallel system of ridges has been considered a matter of importance, as being in accordance with some supposed laws of mountain formation, but that of centres of upheaval are not less consistent with those laws. At any rate it does not appear that we are at liberty to assume a parallelism of ridges till examination has shown this to be the case.

The Sources of the Nile.—At a recent meeting of the Royal Geographical Society of London (May 9), Captain Speke gave the following narrative of his journey with Capt. Burton into the interior of Africa. His remarks were called out by a paper of Mr. Macqueen's, the object of which had been to prove that the mountain Kilimandjaro is actually snow covered but that it has no connection with the River Nile.

Capt. Speke's remarks are thus reported in the Proceedings of the Society, vol. iii, No. 4:

"After arriving at Zanzibar, we had to wait a considerable time, some months, until the masika, or rainy season, would be over, before we could penetrate into the interior. It was generally advised that we should do so. During the interim Captain Burton and myself made a short coast tour, first to Mombas, and then proceeded down the coast to Pangani. Leaving that place, we ascended the Pangani river, and arrived at Chongwe, a small military station belonging to Prince Majid. Here we were supplied with a small escort of Belooch soldiers, who accompanied us across some hills, by an upper route, to Fuga, the capital of Usambara, where we were hospitably entertained by King Kimwere, a great despot reigning there. After visiting him for one day, the shortness of our supplies compelled us to retrace our steps by a forced and rapid march, following down close along the banks of the same river until we again arrived at Pangani. Thus ended our initiatory tour in Eastern Africa. The rainy season or masika was spent by us at Zanzibar, in constructing the equipment of a caravan. There is a singular tribe of negroes in the interior of Africa, called Wanyamuezi—the literal translation of which signifies people of the moon. These strange people are professionally voluntary porters: they annually bring down ivory to the coast in barter for themselves, or otherwise for the Arabs. At the close of the rainy season Captain Burton and myself left Zanzibar, with a caravan mustering about eighty men, having previously sent on some supplies in anticipation of our arrival. Unable to collect a sufficient caravan for the conveyance of our kit, we purchased a number of donkeys (about thirty). Thus completed, and with an escort of twelve Belooch soldiers, given us by Prince Majid, we commenced our journey westward, and arrived (by slow degrees travelling over a low alluvial plain, up the course of the Kingani river) at Zungomero, a village situated under the coast range, which struck us as bearing a good comparison with the western ghauts of India. We might call this range the Eastern Ghauts of Africa. There we were detained by severe illness a considerable time. Afterwards we crossed these eastern ghauts, the maximum altitude of which I ascertained to be about 6000 feet. On the western side of this longitudinal chain of hills we alighted on an elevated plateau, an almost dead flat, ranging in level from 3000 to 4000 feet above the sea. Here we had cold easterly winds, continuing through the entire year. Proceeding onwards, we arrived at the Tanganyika Lake, called by the Arabs Sea Ujiji, a local name taken from the country on the eastern margin of the lake, whither they go to traffic for ivory and slaves. This lake is in a singular synclinal depression; I found its elevation to be only 1800 feet; whereas the surrounding country (the plateau), as I said before, averaged from 3000 to 4000 feet. The lake is encircled at its northern extremity by a half-moon shaped range of hills, the height of which I estimated (for I could not reach its summit) to be at least 6000 feet. They may extend to a height much greater than that; however, we could not take any observations for determining it. After exploring this lake we returned by the former route to Unyanyembe, an Arab depôt, situated in latitude 5° south, and about 33° east longitude. My companion, Captain Burton, unable to proceed farther, remained here; whilst I, taking just sufficient provisions for a period of six weeks, made a rapid march due north, to latitude 2° 30′ south; and there discovered the southern extremity of the Nyanza, a lake, called by the Arabs Ukerewe, a local name for an island on it, to which the merchants go in quest of ivory. The altitude of this lake is equal to the general plateau (4000 feet), even more than the average height of all the plateau land we traversed. In reverting to the question asked, why I consider the Lake Nyanza to be the great reservoir to the Nile, my answer is this: I find, by observation, that its southern extremity lies in east longitude 33°, and south latitude 2° 30′. By Arab information, in which I place implicit confidence, I have heard that the waters extend thence, in a northerly direction, certainly from five to six degrees. Notwithstanding they can account for a continuous line of water to this extent, no one ever heard of any limit or boundary to the northern end of the lake. A respectable Sowahili merchant assured me that, when engaged in traffic some years previously to the northward of the line and the westward of this lake, he had heard it commonly reported that large vessels frequented the northern extremity of these waters, in which the officers engaged in navigating them used sextants and kept a log, precisely similar to what is found in vessels on the ocean. Query, Could this be in allusion to the expedition sent by Mahamad Ali up the Nile in former years? Concerning the rains which flood the Nile, the argument is simple, as I have said before: a group of mountains overhang the northern bed of the Tanganyika Lake. The Arabs assure us that from the north and northeastern slopes of these hills during the rainy season immense volumes of water pour down in a northeasterly direction, traversing a flat marshy land, intersected by some very large, and many (they say 180) smaller streams. Again, on the western side, we hear from Dr. Krapf, that the snow-clad mountain, Kœnia, is drained by rivers on its western slopes in a direction tending to my lake.

"During the rainy season, which I know, by inspection, commences in that region on the 15th of November, and ends on the 15th of May, the down-pour is pretty continuous. Supersaturation, I should imagine, takes place later on the northern than on the southern side of the aforesaid moon-shaped mountain, systematically in accordance to the ratio of seasonal progression; but this, in so mean a distance, could not be very great. Suffice it to say, that I saw the Malagarazi river, which emanates from near the axis of these hills, to be in a highly flooded state on the 5th of June. The Nile at Cairo regularly swells on the 18th of June.

"Farther, it would be highly erroneous to suppose that the Nile could have any great fluctuations from any other source than periodical rains. Were the Nile supplied by snow, as some theorists suppose, its perennial volume would ever be the same. There would be no material fluctuations observable in it, in consequence of its constant and near approximation to the path of the sun.

"By these discoveries, the old and erroneous hypothesis of a high latitudinal range of mountains extending across the continent of Africa from east to west, in the vicinity of the line, and known as the Mountains of the Moon, is therefore now annihilated. However, it is worthy of remark, that the crescent-shaped mountain, which we visited to the northward of the Tanganyika, lies in the centre of the continent of Africa, immediately due west of the snowy peaks Kilimanjaro and Kœnia, and is west beyond the Unyamuezi, or Country of the Moon.

The Wanyamuezi tribe has from time immemorial been addicted to journeying, and at all periods has constantly visited the eastern coast of Africa. It would not be beyond legitimate and logical supposition, to imagine that these hills, lying beyond their Moon Country, should have given rise to the term Mountains of the Moon, and from misunderstanding their relative position with the snowy Kœnia and Kilimanjaro, should have been the means of misguiding all ancient inquirers about that mysterious mountain.

"My positions were fixed by astronomical observations, certainly under painful and considerable difficulties, owing to my constantly impaired general state of health: weakness and blindness not being the least of these difficulties which I had to contend with. My latitudes were taken by the altitude of stars, at nearly every stage, on an average from ten to fifteen miles apart. I also fixed some crucial stations, the principal points for delineating the country by lunars, on which I place great reliance, as the means of the masses of them which I took show so little deviation. The intermediate distances I compassed very closely; the altitudes of the country I traversed I determined by boiling thermometer, on which I also place very great reliance. We had a thermometer and pedometer, and several chronometers. The performance of these instruments was anything but satisfactory; indeed, finally, I had to rig up a string and bullet pendulum to beat time whilst taking my lunars in the latter stage of the journey. There now can be no doubt that this great lake, the Nyanza (Captain Speke now pointing to the map) is the great reservoir of the Nile, and that its waters indubitably extend northwards from the position visited by me on its southern extremity to 3½° north latitude, lying across the equator, and washes out that supposed line of mountains, called the Mountains of the Moon, which stands so conspicuously in all our atlases."

Baikie's Niger Expedition.—The latest intelligence we have received from this important expedition is contained in the following extract from Sir R. I. Murchison's late anniversary address before the Roy. Geog. Soc. of London:

"The unfortunate shipwreck of the *Pleiad* on the rocks near Rabba, and the check given to the expedition under Dr. W. B. Baikie, which left England early in 1857, were alluded to in my last year's address. I now learn from Mr. D. T. May,* R.N., who has returned to England, that less than twenty miles above Rabba the River Niger, or Quorra, divides into several rocky, intricate channels. Consul Beecroft in the *Ethiope*, in 1845, safely navigated the most available of these passages; but the voyagers of 1857 were not so fortunate, and the steamer was totally lost on the rocks. Most of the property was, however, saved, and the neighboring bank became the head-quarters of the expedition for a whole year. The rocks forming the banks of the river where the shipwreck took place are composed of highly-inclined strata of hard sandstone. All the specimens of this rock which I have examined, whether brought home by Mr. May or sent by the Admiralty, belong to the same light-colored, hard, sub-crystalline, pinkish sandstone, with very fine flakes of white mica; the successive layers (which are much foliated) being strikingly covered by thin elongated crystals of black tourmaline. The rock has altogether the appearance of having undergone considerable metamorphosis, and much elevation and disturbance. Geodes of pure white quartz, with large micaceous coatings, also occur. As soon as the party had become somewhat settled, it was determined to make a direct overland communication by Yóruba with Lagos, and Mr. May offering himself for this service, accomplished it satisfactorily, as explained in a notice laid before the Society. In the mean time Lieut. Glover made journeys up the river, visiting Wawa and Busa, and definitely ascertained the impracticability of navigating the river for a few miles beyond the spot of the encampment, a waterfall at Waru being an impassable barrier even for canoes in any season.

"Mr. May having waited on the sea-coast, expecting another steamer from England, at last returned to the encampment through Yóruba, and then set out on a more extended journey, with a view to exploring the country, and of establishing postal communication in a line from Lagos to the confluence. Having first travelled to Hadan (the road between Lagos and Hadan being well known and used), he passed eastward, and journeyed for many weeks through the previously unvisited districts of Ife, Ijesha, Igbouma, Yagha, &c., being warmly received, and observing everywhere that the people were quiet, orderly, and industrious; though these good qualities are here and there broken in upon by marauding or slave-catching armies, sent into the Yóruban country by powerful neighbors. The details of this journey were communicated to the Foreign Office in January last, and will, I presume, soon reach the Society.

"Approaching to within fifty or sixty miles of the confluences of the Quorra and Chadda Rivers, Mr. May was compelled to alter his route, and proceed northwards, visiting the ruined famous town Ladi, crossing the Quorra at Shaw, and journeying thence on the north side of the river through Núpe to Rabba.

"Lieutenant Glover had during this time also visited the coast by Mr. May's first route, and was now there waiting to pilot up the river the steamer which was at last coming to the relief of the party. Dr. Baikie and the other members of the expedition had been chiefly employed during the year in cultivating a good understanding with their neighbors, reducing their language, &c., whilst the energies of Mr. Barton were amply occupied on the botany of this part of Africa. In October, 1858, just a twelvemonth after the settlement of the expedition at the spot in question, the *Sunbeam* steamer arrived, the whole party were then embarked, and proceeded down the river to Fernando Po, there to recruit the health of the officers and men, and make arrangements for farther exploration. During the twelvemonth's residence in Núpe the most friendly relations were maintained with the king, his brother, and chiefs, and the natives generally; supplies being often received overland from Lagos.

"At Fernando Po (November, 1858), a re-organization having taken place, and the preparations being completed, the party again set out, now in the steamer *Rainbow*, built and sent for the purpose, and endeavored to re-ascend the river. But it was then found that this vessel, which draws four feet of water, could not ascend the Niger even in the month of January; the waters subsiding until June, when they increase. In consequence, the party was obliged again to return to the sea, and since have set out upon the land-journey from Lagos to Rabba (upon the route opened up by Mr. May), whence it is purposed to proceed with an expedition, the friendly objects of which must by this time have made a due impression on the native chiefs, and from which we may anticipate the gain of much knowledge when all the acquisitions of Dr. Baikie and his associates are unfolded."

KHANIKOFF'S EXPEDITION IN CENTRAL ASIA.—At a recent meeting of the American Oriental Society in New York, the Corresponding Secretary, Prof. Wm. D. Whitney of Yale College, presented a letter from the Chev. N. Khanikoff, dated Kerman, Persia, April 7–19th, 1859, in which he speaks as follows of the journey which he has just made.

"I have just completed, or nearly so, a very interesting journey through Khorassan, Western Afghanistan, and Northern Seistan. The whole region traversed by the scientific expedition which I have had the honor to conduct has been carefully surveyed, the situation of its principal points has been fixed astronomically, for more than thirty points ascertainment has been made of the magnetic coördinates, and of the intensity of magnetism corresponding therewith, and the profile of the territory has been determined by barometrical observations and trigonometrical measurements. The botanical researches have been made by Prof. Bunge, and the geological investigations by M. Göbel; the oriental literature, archæology, and numismatics have fallen to my share, and I hope soon to have the pleasure of communicating to the Society a succinct view of the results at which I have arrived." D. C. G.

ART. XII.—*The Great Auroral Exhibition of August 28th to September 4th*, 1859.—(2d ARTICLE.)

IN our last Number* we gave some observations of this grand auroral exhibition, from a number of stations widely distant from each other. We now put on record some facts observed respecting the influence of the Aurora upon the wires of the electric telegraph. We hope in our next Number to be able to communicate additional intelligence respecting this Aurora.

1. *Observations made at Boston, Mass., and its vicinity by* GEORGE B. PRESCOTT, *Telegraph Superintendent.*

My attention was first called to the possibility of the Aurora Borealis affecting the telegraph wires in 1847, while operating the Morse (electro-magnetic) telegraph at New Haven; but I was not fortunate enough to observe it until the winter of 1850. At this time I became connected with Bain's (electro-chemical) telegraph in this city, and observed some effects of the aurora; but, owing to the feeble displays, only to a limited extent.

In September, 1851, there was a remarkable display of the Aurora Borealis, which completely took possession of all the telegraph lines running out of the city, and effectually prevented any business being done over them during its continuance.

The following winter there was another remarkable display, which occurred upon the 19th of February, 1852. I furnish from data recorded in my journal at that time the following particulars in regard to this phenomenon.

The system of telegraphing used upon the wires during the observations of February, 1852, was Bain's electro-chemical. The circuit was what is known as the open circuit,—that is, the key, which throws the current from the battery upon the line, was always open when a message was being received from a distant station, and the current passed through the chemically prepared paper to the earth without uniting with the home battery. Each station was furnished with its own battery, the negtive pole of which was invariably connected with the earth, and the positive, by the depression of the key, with the line.

The line extended in a direction nearly northeast and southwest. The paper was prepared with a solution of cyanid of potassium, made after the following recipe. Six parts prussiate potassa dissolved in water; two parts nitric acid; two of ammonia. This solution will scarcely color the paper, while it will render it quite sensitive to the action of the electric current. The stylus was made of No. 30 iron wire. A battery of ten

* Vol. xxviii, p. 385–408.

cups Grove, with the line well insulated, will decompose the salts, and uniting with the iron stylus, leave a bright blue mark upon the paper, at a distance of 230 miles.

The positive pole only produces a colored mark; the negative bleaches the paper.

When there is no electric current upon the wires, the pen leaves no impression upon the paper; but the slightest current will produce decomposition; and the color of the mark depends upon the strength of the current.

Free or common electricity produces no color upon the paper. It emits a bright spark in passing from the stylus to the moistened paper; produces a quick, sharp noise, like the snapping of a pistol and disappears. This effect is totally unlike that of the Aurora Borealis, as will be seen from the following.

Thursday, February 19, 1852. Towards evening a blue line appeared upon the paper, which gradually grew darker and larger, until a *flame of fire* followed the pen, and burned through a dozen thicknesses of the prepared paper. The paper was set on fire by the flame, and produced considerable smoke. The current then subsided as gradually as it came on, until it entirely disappeared, and was then succeeded by a negative current, which bleached instead of colored, the paper; this also gradually increased, until, as with the positive current, it burned the paper, and then subsided, to be followed by the positive current again. This state of things continued during the entire evening, and effectually prevented any business being done over the wires. The current came in waves of varying intensity—light at first, then stronger, until having attained to the volume and intensity of at least two hundred Grove cups, it subsided, and was followed by a current of the reverse polarity. This *invariably occurred*, and may be set down as an established fact, that the currents from the Aurora Borealis always change their polarity during every wave.

I have seen the auroral current produce magnetism, decompose chemicals, and produce heat and fire.

The effects of the magnetic storm of August 28th, 1859, were apparent upon the wires during a considerable portion of Saturday evening, and during the entire day, Sunday. At 6 P. M. the line to New Bedford (60 miles in length, running a little west of south) could be worked only at intervals, although, of course, no signs of the Aurora Borealis were visible to the eye at that hour. The same was true of the wires running east through the state of Maine as well as those running north to Montreal. The wire between Boston and Fall River had no battery connected with it on Sunday, and yet there was a current upon it during the entire day, which caused the keepers of the electro-magnets to open and close as the waves came on and receded.

Upon the lines which had batteries connected Sunday evening, it was observed that the poles changed during every wave of the aurora—each wave occupying from fifteen seconds to half a minute. When the poles of the Aurora were in unison with the poles of the battery upon the line, the effect was to increase the current; but when they were opposed, to neutralize it. I will give my proofs of this farther on. It is to be observed that the effects I have illustrated in relation to the Aurora of August 28th, 1859, were observed upon the Morse (electro-magnetic) system. The same were, however, observed upon the House and Hughes lines running out of the same office.

It is not true that there is any difference in the effect produced upon the wires by the Aurora Borealis, whether they run east and west, or north and south. Lines running to every point of the compass diverge from the office here and were equally affected. Even the short wire running from the office in State street to the observatory in Cambridge (five miles long) was sensibly affected.

In an article which I published in the Boston Journal, August 31st, I stated that the current from the Aurora Borealis could have been used for telegraphic purposes, but I did not imagine it would be so soon verified by the actual fact.

On Friday, September 2d, 1859, upon commencing business at 8 o'clock A. M. it was found that all the wires running out of the office were so strongly affected by the auroral current as to prevent any business being done, except with great difficulty. At this juncture it was suggested that the batteries should be cut off, and the wires simply connected with the earth. The Boston operator accordingly asked the Portland operator to cut off his battery and try to work with the auroral current alone. The Portland operator replied, "I have done so. Will you do the same?" Boston operator answered, "I have cut my battery off and connected the line with the earth. We are working with the current from the Aurora Borealis alone. How do you receive my writing?" "Very well indeed," rejoined the Portland operator; "much better than with the batteries on. There is much less variation in the current, and the magnets work steadier. Suppose we continue to work so until the Aurora subsides?" "Agreed," said the Boston operator. "Are you ready for business?" "Yes; go ahead," was the reponse. The Boston operator then commenced sending private dispatches, which he was able to do much better than when the batteries were on, and continued to use the wire in this manner for about two hours, when, the Aurora having subsided, the batteries were resumed.

While this singular phenomenon was taking place upon the wires between Boston and Portland, the operator at South Braintree—Miss Sarah B. Allen—informed me that she was working the wire between that station and Fall River—a distance of about forty miles south—with the auroral current alone. Since then I have visited Fall River and have the statement verified by the intelligent operator upon the railroad line at the dépôt in that village.

The office at the dépôt is about half a mile from the regular office in the village. The battery is kept at the latter place, but the operator at the dépôt is provided with a button or switch, by which he can throw the battery off the line, and put the wire in connection with the ground at pleasure. The battery at the other terminus of the line is at Boston, but the operator at South Braintree is furnished with a similar switch, which enables her to dispense with its use at pleasure. There are no intermediate batteries; consequently if Fall River operator puts his wire to the earth, and the South Braintree operator puts her wire to the earth, the line is without battery, and of course, without an electrical current.

Such was the state of the line upon the 2d of September last, when for nearly two hours, they held communication over the wire with the aid of the celestial batteries alone!

I have restricted myself in this article to facts observed by myself. I have stated nothing which I am not absolutely certain of, and which, if necessary, can be proved by a number of reliable witnesses.

2. *Observations made at White River Junction, Vt., communicated by* J. H. Norris, *Telegraph Superintendent.*

During the forenoon of Sept. 2d, an unusual current of varying intensity was present most of the time on the wires of the Vermont and Boston telegraph. The polarity of this current appeared to change frequently, sometimes being opposite to and nearly or quite neutralizing the battery current when an attempt was made to use the line; at other times much increasing the force of the battery current. The auroral current produced the same marks upon our chemical paper (we use the Bain or chemical system of telegraph) as those produced by the use of the battery. Signals and messages were transmitted between Boston and Manchester *by the sole use of the auroral current.*

3. *Observations made at Springfield, Mass., by* J. E. Selden.

On the evening of Aug. 28th, upon the Boston and New York circuit, at one moment there was a very heavy current on the wire, and the next none at all. On the Albany and Springfield circuit, *a flash* passed across from the break key of the telegraph apparatus to the iron frame, the flame of which was about half the size of an ordinary jet of gas. It was accompanied by a humming sound similar to a heavy current passing between two metal points almost in contact. The heat was sufficient to cause the smell of scorched wood and paint to be plainly perceptible.

[The observations at Springfield, as well as those at New York and Washington were communicated by Messrs. Lewis and Lovett of the New York telegraph office.]

4. *Observations made at New York by* J. C. Crosson, *Telegraph operator.*

On the evening of Aug. 28th, at 7½ o'clock, I experienced considerable difficulty in working, on account of the variation of current. I could work south by constantly altering the adjustment of my magnets, but the magnetism on the eastern circuit was so nearly destroyed that I could do nothing. About ten o'clock I could see nothing of the Aurora in the southern hemisphere, yet the same variations of current were manifest upon the line for an hour afterward. There was during this time a very strong turning current from the east, which resembled a reversed current so much that I disconnected my battery and put on a 'ground,' but I could not then get magnetism sufficient to work a simple armature. At 12^h 30^m the current from the east assumed a new feature, producing enough magnetism to work quite well, yet wavering and varying in intensity.

5. *Observations made at Philadelphia, communicated by* H. Emmons Thayer, *Telegraph Superintendent.*

On the evening of Aug. 28th, about 8 o'clock, we lost current on all our four wires running from Philadelphia to New York, and we had strong circuit as if from a near ground connection; but there was no interruption on wires running south to Baltimore and Washington. At 9^h 10^m the wires were relieved to a great extent from the influence of the Aurora, giving us our usual working current.

On testing wires at 8 o'clock on the morning of Sept. 2d, I found two of our wires, those running *via* Camden and Amboy to New York, strongly under the influence of an Aurora. The effect was different from that of Aug. 28th. There was an intensity of current which *gave a severe shock* when testing, giving a reversed current, neutralizing our batteries, and destroying magnetism. On removing the batteries, we had a very strong circuit, giving powerful magnetism, but could not raise New York. On the line running from this city to Pittsburgh, the operator, Mr. Steacy, succeeded in transmitting a business message to Pittsburgh *wholly on the auroral current.* The current was changeable, suddenly disappearing and returning at intervals of from five to ten minutes. The signals were distinct and the conversation lasted four or five minutes, the operators exchanging remarks as to the singularity of the phenomenon. At 9 A. M. all the wires were relieved from the effects of the Aurora, and worked well as usual.

6. *Observations made at Washington, D. C., by* FREDERICK W. ROYCE, *Telegraph operator.*

On the evening of Aug. 28th I had great difficulty in working the line to Richmond, Va. It seemed as if there was a storm at Richmond. I therefore abandoned that wire, and tried to work the northern wire, but met with the same difficulty. For five or ten minutes I would have no trouble, then the current would change, and become so weak that it could hardly be felt. It would then gradually change to a 'ground' so strong that I could not lift the magnet. The Aurora disappeared at a little after 10 o'clock, after which we had no difficulty. During the auroral display, I was calling Richmond, and had one hand on the iron plate. Happening to lean towards the sounder, which is against the wall, my forehead grazed a ground wire. Immediately I received *a very severe electric shock,* which stunned me for an instant. An old man who was sitting facing me, and but a few feet distant, said that he saw *a spark of fire* jump from my forehead to the sounder.

7. *Observations made at Pittsburgh, Pa., communicated by* E. W. CULGAN, *Telegraph manager.*

During the Aurora of Aug. 28th the intensity of the current evolved from it varied very much, being at times no stronger than an ordinary battery, and then suddenly changing the poles of the magnets it would sweep through them, charging them to their utmost capacity, and compelling a cessation of work while it continued.

On the morning of Sept. 2d, at my request the Philadelphia operator detached his battery, mine being already off. We then worked with each other at intervals as long as the auroral current continued, which varied from thirty to ninety seconds. During these working intervals we exchanged messages with much satisfaction, and *we worked more steadily when the batteries were off than when they were attached.*

On the night of Aug. 28th the batteries were attached, and on breaking the circuit there were seen not only sparks (that do not appear in the normal condition of a working line) but at intervals regular *streams of fire,* which, had they been permitted to last more than an instant, would certainly have fused the platinum points of the key, and *the helices became so hot* that the hand could not be kept on them. These effects could not have been produced by the batteries.

ART. XIII.—*On Numerical Relations existing between the Equivalent Numbers of Elementary Bodies;* by M. CAREY LEA, Philadelphia. Part I.

(1.)

THE determination of the chemical equivalents of the simple substances seems with each new approach to accuracy, to destroy more and more the numerical relations once supposed to exist between the equivalent numbers of certain series of elements nearly related to each other by their properties. If we except the series formed by oxygen, sulphur, selenium and tellurium, there probably remains none of those usually recognized in which the numerical relation is rigorously exact. Chlorine, bromine and iodine are represented by the numbers 35·5, 80 and 127, where the difference between the first and second term is 44·5, between the second and third, 47. Lithium, sodium and potassium have the numbers 6·95, 23, 39·2; difference of first and second terms, 16·05, of second and third, 16·20. Calcium, strontium and barium have 20, 43·77 and 68·6; difference of first and second, 23·77, of second and third, 24·83. It can be demonstrated that other relations exist, approaching quite as near to exactness as these, and some where the accuracy is perfect.

Few of the so-called elements present more directly marked analogies than nitrogen, phosphorus, arsenic and antimony, and the very interesting discoveries of Cahours and Hofmann respecting the phosphorus bases have shown that phosphorus stands in every respect intermediate between nitrogen and arsenic, forming compounds of the type $3(C_4H_5)PHCl$, &c., like the nitrogen compounds as well as those of the type $3(C_4H_5)PO_2$, &c., like those of the arsenic and antimony groups. These authors further observe that the equivalents of phosphorus, arsenic and antimony differ by nearly the same number (44 to 45), but that nitrogen does not exhibit this relation.*

Beyond the fact of the approximate equality of these two differences, the analogy has never been extended. The following considerations will show that this relation not only extends to nitrogen but may be carried with exactness to other elements.

If we form a descending arithmetical series beginning with antimony =120·3, and diminishing by a common difference of 45 (45·3 in one instance, 44 in several) we shall find that such a series does not cease with the third term, P=31, but gives for a fourth −14, the exact equivalent of nitrogen with a negative sign. The fifth term will be −59, the exact equivalent of tin, with a negative sign. The sixth will be −104, or very nearly the equivalent of lead (also with a negative sign). The seventh

* Kopp u. Will, Jahresbericht für 1857.

−149, very nearly the double of the equivalent of arsenic, a previous term in the same series. These results are exhibited in the following table.

Differences.	Calculated equivalents.		Received equivalents.
	120	- -	Sb = 120·3
45	75	- -	As = 75
44	31	- -	P = 31
45	− 14	- -	N = 14
45	− 59	- -	Sn = 59
45	−104	- -	Pb = 103·5
45	−149	- -	2As = 150
45	−194	- -	—— ——
45	−239	- -	2Sb = 240·6
45	−284	- -	—— ——
44	−328	- -	= 2×164
44	−372	- -	—— ——
44	−416	- -	2Bi = 416

It will be seen presently that the number 164, the eleventh term in the above table, occurs also in the ascending positive series, and may represent the equivalent of a metal existing but as yet unknown.

If we examine the position occupied by antimony, arsenic, phosphorus and nitrogen in the electro-chemical scale of Berzelius we shall find that in proportion as their equivalent numbers diminish, their properties become more and more electro-negative; a corresponding change is also visible in the organic radicals which these elements are capable of forming by their union with carbon and hydrogen. The passage from the positive to the negative sign in the interval between phosphorus and nitrogen is accompanied by a marked change in the nature of the organic radicals into which these elements enter—$3(C_4H_5)N$ does not possess the power of combining directly with oxygen, chlorine and sulphur which $3(C_4H_5)P$, $3(C_4H_5)As$, $3(C_4H_5)Sb$ exhibit in so high a degree. The methyl compounds show the same differences as the ethyl. Standing between nitrogen and arsenic, phosphorus is every way more closely allied to the latter of these substances, not only by the analogies of its organic radicals, but also by the polybasic nature of phosphoric acid.* Although tin and lead represent the further members of the same series in reference to their equivalent numbers, it is evident that the increase of electro-negative relations does not extend to them. Moreover bismuth, antimony, arsenic, phosphorus and nitrogen at their maximum of oxydation combine with five equivalents of oxygen and chlorine, whereas tin unites with but two of each, and lead at most with two of oxygen and one of chlorine.

Again, if we begin with phosphorus, and form an ascending series with a common difference of 44 (except in one instance), we shall find both the number 164, the double of which constituted the eleventh term of the preceding table, and also the equivalent of bismuth, the double of which formed the thirteenth term of the same table.

Differences.	Calculated equivalents.		Received equivalents.
	31	- -	P = 31
44			
	75	- -	As = 75
45			
	120	- -	Sb = 120·3
44			
	164	- -	— —
44			
	208	- -	Bi = 208

These four elements exhibit strong analogies and are all isomorphous with each other.

If, taking mercury as a starting point, we subtract the number 44 from each term to find the following one, we shall obtain the series—

Differences.	Calculated equivalents.		Received equivalents.
	100	- -	Hg = 100
44			
	56	- -	Cd = 56
44			
	12	- -	Mg = 12
44			
	−32	- -	Zn = 32·6

The salts of the protoxyds of the three last of these metals are isomorphous. It may seem forced to place mercury in the same group, but its analogies with zinc are perhaps as strong as those which it exhibits with silver, next to which it has usually been classed, principally because the oxyds of both metals are reduced by heat. Mercury, like zinc, cadmium and certain other metals, is capable of replacing an atom of hydrogen in hydrid of ethyl, C_4H_5H, and of producing in this manner the conjugate

* See the very interesting paper of Cahours and Hofmann above referred to.

organic radical mercur-ethyl, C_4H_5Hg, analogous to zinc-ethyl, C_4H_5Zn. This compound has recently been isolated by Buckton.* Silver does not appear to be capable of a similar substitution; when chlorid of ethyl is made to react upon zinc-ethyl the products are ethyl, chlorid of zinc and metallic silver. It has lately been shown by Hallwachs and Schafarik that magnesium forms an ethyl-magnesium.† These metals are likewise all volatile, and it has been shown by H. St.C. Deville that magnesium, like the others, is readily distilled.

It is not a little curious that the numerically negative members of this series lead into the positive members of the foregoing; if we continue the subtraction of 44, we find for the fifth number 76, or nearly the equivalent for arsenic, for the sixth 120, very nearly that of antimony, for the seventh 164, corresponding, as before remarked, with a possible undiscovered metal, and for the eighth 208, or exactly the equivalent of bismuth. The two series thus naturally lead to each other. The equivalent for arsenic given by L. Gmelin, viz. 75·4, accords more nearly with some of these series and renders them more exact than that adopted by the Jahresbericht which has been here employed.

The members of these two analogous series are further united by the fact that all of them, eleven in number, are capable of uniting with the hydro-carbons of the methyl, ethyl, &c. type to form powerful organic metals, and that this capacity appears to be limited to these elements alone.‡

(2.)

The magnesia group includes a well marked natural family of metals, whose oxyds having the constitution RO are related with each other by isomorphism. The equivalents of these metals, according to the most recent determinations, are as follows:

Magnesium,	12	Chromium,	26·7
Manganese,	27·5	Zinc,	32·6
Iron,	28	Cadmium,	56·
Cobalt,	29·5	Copper,	31·7
Nickel,	29·6	Lead,	103·5
Uranium,	60		

[Some of the elements here enumerated are found also in the foregoing or following series. It need scarcely be remarked that no absolute classification into groups is possible; lead for instance is in some of its combinations isomorphous with the barium group, in others with the magnesia, copper in grey cop-

* See this Journal, [2], xxviii, 146. † This Journal, [2], xxviii, 147.

‡ There appears some reason to believe that an ethyl-aluminum may exist, which would form an exception to this law. If so, the same property may possibly be extended to others of the metals of the earths.

per ore is isomorphous with silver, while its carbonate, sulphate and seleniate are isomorphous with those of the magnesia group.] The equivalents of the above metals are related in the following manner by the number 44:

Cu 31·7	Zn 32·6	Cd 56
Mg 12·	Mg 12	−Mg 12
43·7	44·6	44
U 60	U 60	U 60
Mn 27·5	Fe 28	Co 29·5
87·5, $\frac{1}{2}$=43·75	88, $\frac{1}{2}$=44	89·5, $\frac{1}{2}$=44·75
U 60	U 60	Pb 103·5
Ni 29·6	Cr 26·7	−U 60
89·6, $\frac{1}{2}$=43·5	86·7, $\frac{1}{2}$=43·35	43·5
Cd 56	Cd 56	Pb 103·5
Zn 32·6	Cu 31·7	Fe 28
88·6, $\frac{1}{2}$=44·3	87·7, $\frac{1}{2}$=43·9	131·5, $\frac{1}{3}$=43·8

and so likewise by adding to the equivalent of lead, the equivalents of manganese, cobalt, nickel, and chromium, we obtain in each case a number almost exactly equal to three times the number 44.

To sum up: with Cu and Mg, Zn and Mg the sum of each pair is 44 nearly.

With Cd and Mg, Pb and Ur, the difference of each pair is 44 or nearly.

With U and Mg, U and Fe, U and Co, U and Ni, U and Cr, Cd and Zn, the mean term is 44 or nearly.

With Pb and Mn, Pb and Fe, Pb and Ni, Pb and Co, Pb and Cr, the sum of each pair is three times 44 nearly.

It has been before pointed out that the strong analogy existing between Mg, Cd and Zn extends to their equivalents, that of Mg being added to that of Zn gives the number 44 nearly, subtracted from that of Cd, 44 exactly.

(3.)

The following metals may be classed together as tending to form acids:

Tin,	59	Vanadium,	68·8
Titanium,	25	Molybdenum,	48
Tantalum,	68·6	Tellurium,	64
Tungsten,	92	Niobium,	48·82*

Relations depending upon the number 44 exist between these equivalents.

* By the recent determination of H. Rose, Poggendorff, civ, ext. in this Journal, [2], xxvii, 127.

If from Ta 68·6 we subtract Ti 25, the remainder is 43·6, 44 nearly.

If from V 68·8 we subtract Ti 25 the remainder is 43·8, 44 nearly.

If from W 92 we subtract Mo 48 the remainder is 44 exactly.

If from W 92 we subtract Nb 48·82 the remainder is 43·18.

If to Te 64 we add Ti 25 the sum 89 is twice 44 nearly.

If to Te 64 we add Ta 68 the sum 132·6 is three times 44 very nearly.

If to Te 64 we add V 68·8 the sum 132·8 is three times 44 very nearly.

If the equivalent of Sn=59 be multiplied by $\frac{3}{4}$ the result is 44·25, in other words 3Sn=4×44·25.

If we add Sn 59, Ta 68·6, W 92, V 68·8 and Te 64, the result is 352·4, 8×44=352.

If we add Ta 68·6, Nb 48·82, Sn 59, we have 176·42, 4×44=176.

If we add Ta 68·6, Mo 48, Sn 59, we have 175·6, 44×4=176.

If we add W 92, 2Te 128, we have 220, 5×44=220.

If we add Ta 68·6, Mo 48, 2Sn 118, V 68·8, Nb 48·82, we have 352·22, 8×44=352.

These analogies, though many are very complex, approach exceedingly near to absolute exactness; in the last for instance the difference amounts to only $\frac{1}{1601}$ of the whole amount.

(4.)

If commencing with gold, Au=197, we form a diminishing series with a common difference of 44·5, we shall find for its terms—

Differences.	Calculated equivalents.		Received equivalents.
	197	- -	Au=197
44·5	152·5	- -	— —
44·5	108	- -	Ag=108
44·5	63·5	- -	Ꞓu = 63·4

The equivalent of Cu has been here taken at double that usually employed, or that which results from taking the first oxyd of copper as CuO, a view formerly entertained by Berzelius, L. Gmelin and other distinguished chemists.* It is doubtful if this be not the true equivalent; in grey copper ore, if we consider Cu as 32·7 we must admit that one equivalent of silver is replaced by two of copper, which is improbable. Moreover the two sulphides, AgS and ꞒuS (Ꞓu=63·4) are isomorphous.†

* Thomson, Inorganic Ch., i, 592.

† L. Gmelin, Handbook, i, 95. The recent experiments of Cannizaro give additional reason for doubling the equivalent of copper.

Viewed in this light the above series approaches very near to exactness, and may probably be wholly so, for the equivalent of copper is by no means positively known. Dumas' recent researches were too unconcordant to permit him to offer a positive opinion. It will be shown further on that these metals, as well as many others here grouped together by relations depending upon a number approximating to 44·5, are also united by analogies of atomic volume.

The second number in the above series, 152·5, does not correspond with the equivalent of any known element, and it, like the number 164, which occurs twice in the nitrogen series, may represent the equivalent of some elementary body as yet unknown. In the second part of this paper, an additional argument will be presented in favor of the possible existence of two elements having these equivalent numbers.

The same relation may be extended with more or less approximation to the platinum group, which naturally subdivides itself into two families: the members of each family have very nearly the same equivalents, and the difference between the equivalents of the two families approaches to 45.

Rhodium,	52·2	Platinum,	98·7
Ruthenium,	52·2	Iridium,	99
Palladium,	53·3	Osmium,	99·6

The difference between the equivalents of platinum and palladium is 45·4, the rest vary a little more.

The elements, carbon, boron and silicium, are united by the number 44 in the following manner: if we take Gerhardt's equivalent for carbon Ɵ=12, the sum of the equivalents of these three substances amounts to 44 exactly.

Carbon (Ɵ),	12
Boron,	11*
Silicium,	21·
	44

With respect to the metals of the earths and alkaline earths, magnesium and calcium have been considered elsewhere, yttrium, erbium and terbium, and the cerite metals have not yet been sufficiently examined, the equivalents of the three first have in fact not yet been determined, thorium exhibits a relation which will be hereafter pointed out; there remain therefore glucinum, aluminium and zirconium. The equivalents of these metals, viz. 4·7, 13·7 and 22·4 are evidently too small to follow precisely the same law which governs the various series previously considered, but that it extends also to them becomes evident

* As determined by Deville by analyses of bromid of boron.

by multiplying their equivalents by 5, when they form a series analogous to the foregoing.

Differences.			
	23·5	- -	5G = 23·5
45			
	68·5	- -	5Al = 68·5
45			
	113·5	- -	5Zr = 112·

(5.)

If we take the equivalent of nitrogen, add to it three times 45 and divide by 8, we shall have nearly the equivalent of fluorine, and by continuing nearly in the same manner, we obtain those of chlorine, bromine and iodine.

$$\frac{14+3\times45}{8} = 18{\cdot}62, \quad Fl = 19$$

$$\frac{14+6\times45}{8} = 35{\cdot}5, \quad Cl = 35{\cdot}5$$

$$\frac{14+14\times45}{8} = 80{\cdot}5, \quad Br = 80$$

$$\frac{2\times14+22\times45}{8} = 127{\cdot}25, \quad I = 127$$

(6.)

Several elements, taken in pairs, have equivalents which differ from each other by a number approximating to 45.

Na	23	+ 45·6	=	Ba	68·6
Be	4·7	+ 43·3	=	Di	48
Ti	25	+ 43·6	=	V	68·6
Cl	35·5	+ 44·5	=	Br	80
Al	13·7	+ 45·9	=	Th	59·6
Zr	22·4	+ 46·4	=	Ba	68·6

(7.)

At the commencement of this paper reference was made to the well known numerical relations existing between Li, Na, K; Ca, Sr, Ba; Cl, Br, I. Inexact as are these (with the exception of the first which approaches tolerably near), the strong analogies which exist between the substances themselves, give to their relations an interest which they would not otherwise possess, and it is to be remarked that they all belong to that class which it has been the object of this paper to develop, viz., those depending upon the number 44–45.

1. The equivalent of sodium is well known to be nearly a mean between those of lithium or potassium—if the equivalent

of lithium be added to that of potassium, or if that of sodium be doubled, a number is obtained in either case approaching nearly to 45.

2. The equivalent of strontium is nearly a mean between those of calcium and barium: Sr=43·75, half the sum of Ba 68·6 and Ca 20 is 44·3; in either case 44 nearly.

3. The equivalent of chlorine, 35·5, subtracted from that of bromine, 80, gives 44·5. The equivalent of bromine subtracted from that of iodine gives 47.

(8.)

With respect to three of the groups already considered, a further relation exists. For greater clearness the groups in question have been arranged in the following table:

Group A.

Differences.		
	100	Hg=100
44		
	56	Cd= 56
44		
	12	Mg= 12
44		
	−32	Zn = 32·6

Group C.

Differences.		
	92	W= 92
44		
	48	Mo= 48

Group B.

Differences.		
	208	Bi =208
44		
	164	— —
44		
	120	Sb =120·3
45		
	75	As = 75
44		
	31	P = 31
45		
	−14	N = 14
45		
	−59	Sn = 59
44·5		
	−103·5	Pb =103·5

If to the equivalent of the substances forming series A, we add successively to each term the equivalent of fluorine, we shall obtain four corresponding terms of series B.

Mercury	100	Cadmium	56	Magnesium	12	Zinc	−32·6
Fluorine	19	Fluorine	19	Fluorine	19	Fluorine	19
	119		75		31		−13·6
Antimony	120·3	Arsenic	75	Phosphorus	31	Nitrogen	14

Further: If to two corresponding terms of series A we add the equivalent of chlorine, we shall obtain the two terms of series C.

Cadmium	56	Magnesium	12
Chlorine	35·5	Chlorine	35·5
	91·5		47·5
Tungsten	92	Molybdenum	48

And if to two terms of the same series A we add the equivalent of bromine, we also find the terms of series C.

Magnesium	12	− Zinc	− 32·6
Bromine	80	Bromine	80
	92		47·4
Tungsten	92	Molybdenum	48

If we add respectively the four terms of series A with the four corresponding terms of series C, we shall in each case obtain a number which is very nearly twice forty-four; that is, the mean of each pair of series is 44 nearly.

− Zinc	− 32·6	Magnesia	12	Cadmium	56	Mercury	100
Antimony	120·3	Arsenic	75	Phosphorus	31	− Nitrogen	− 14
	88·7		87		87		86

It will be remarked that where a series leads to the equivalent of an element, but with a negative sign, that negative sign has been in all cases preserved in the further examination of its numerical relations.

(9.)

It is evident that the number 44—45 plays an important part in the science of stoichiometry, and the relations which depend upon it are supported, in some cases at least, in a remarkable manner, by analogies of atomic volume. That such analogies are a support becomes evident from the following considerations.

Solids and liquids are very far from being governed by the laws which determine the combinations of gases, in volumes either equal or having some very simple relation to one another. Therefore, if we find that in some few instances such a relation does hold good with solid substances, we may naturally expect to find a close relationship existing between those substances thus united. We may even be permitted, by way of hypothesis, to advance a step further, and finding that a given volume of silver unites with a given volume of oxygen, and that the same volume of gold unites with precisely the same volume of oxygen, to conjecture that gold may differ from silver only by a third substance, which unites with the silver without increasing its volume, or affecting the amount of oxygen which it is capable of saturating, but which, on the other hand, alters its chemical equivalent, its specific gravity, and other physical characters.

Moreover, if we find that by subtracting from the chemical equivalent of silver, half the difference between the equivalents of silver and gold, we obtain the equivalent of a third metal, copper (Ɇu=63·4), which also, under equal volumes, combines with a quantity of oxygen expressed by a very simple relation with that capable of saturating gold and silver, we may at least speculate that the three may form a series consisting of two substances combined in different proportions. It is true that we must be extremely cautious about venturing upon hypotheses involving a compound constitution of bodies which all our efforts have hitherto proved ineffectual to decompose, but on the other hand it must be admitted that when we find so-called elements arranging themselves into a series of terms having a common difference, and when we find the terms of these series united by equality or simple relation of atomic volume, we cannot grant that their elementary nature has been absolutely established.

The following substances combine relations of chemical equivalents already pointed out, with analogies of atomic volume:

Differences of Equivalents.		Atomic† volume.	Relation of At. vol.
	Nitrogen,	14·42	2
45*			
	Phosphorus (vapor),	7·01	1
44			
	Arsenic (vapor),	7·07	1
	Lead,	9·09	1
44·5*			
	Tin,	8·09	1
45*			
	Nitrogen,		
45			
	Phosphorus,	17·7	2
44			
	Arsenic,	12·58	$\frac{3}{2}$
45·3			
	Antimony,	18·—	2
87·7 or twice 43·85			
	Bismuth,	21·18	$\frac{7}{3}$

(Where phosphorus, arsenic, &c., are compared in the solid state, the unit of relation is of course different). It has been already remarked that in point of chemical relations generally, lead and tin are less closely united with the series than the other members composing it, but the relation between the atomic volumes of lead and antimony, the latter almost the last term at the other end of the series, is almost absolutely exact. Nitrogen is of course omitted in the second table, as we do not know what would be its atomic volume in the solid state.

* In the cases of nitrogen, tin and lead, the equivalents are taken with a negative sign, as before explained.

† The numbers here given for the atomic volumes are calculated from the specific gravities adopted in Gmelin's Handbook, and the latest and most reliable determinations of chemical equivalents.

Differences.		Atomic Volume.	Relation of At. Vol.
89 or twice 44·5	Gold,	10·26	1
44·6	Silver,	10·35	1
	Copper (€u=63·4),	7·27	$\frac{3}{4}$

It will be observed that the difference between the equivalents of copper and silver approaches very near to absolute exactness with half the difference between the equivalents of silver and gold, and as the equivalent of copper is by no means positively settled, the relation may be rigorously exact. If we take the mean between the number adopted by L. Gmelin and that adopted by the Jahresbericht (always considering cuprous oxyd as €uO), we shall have for the difference between the equivalents of copper and silver the number 44·5, half the difference between the equivalents of silver and gold, with mathematical exactness.

Difference.		Atomic volume.	Relation of At. vol.
44	Tungsten,	5·28	1
	Molybdenum,	5·58	1

In the series Hg, Cd, Mg, Zn, similar analogies are not well marked: the atomic volumes of the three first metals are not very far apart,

	Atomic volume.
Hg,	7·37
Mg,*	6·85
Cd,	6·48

but the atomic volume of zinc differs considerably; it is 4·72.

In each of these different series, each term differs in its equivalent by the number 45 or a number approaching very near to 45, and yet the addition of this large amount of matter is in most cases accompanied by no change in volume, or when a change takes place, it is expressed by some simple relation to the original volume. Some of these relations of atomic volume are well known and are only here presented in view of the confirmation which they afford of the series here established, but it is believed that the connection of the atomic volume of copper with those of gold and silver, and of those of tin and lead with those of the elements of the antimony group are pointed out for the first time.

Ammermüller has noticed a fact not wholly dissimilar from this in the case of protoxyds of copper, mercury, tin and lead, which combine with a second equivalent of oxygen without change of atomic volume, the density alone increasing. But according to L. Gmelin, the specific gravities on which he based

* Taking the sp. gr. of magnesium at 1·75 as determined by Deville.

his calculations are too unreliable to render the fact well established.

The numbers adopted for the equivalents in the foregoing calculations are those obtained by the latest and most reliable determinations; they are taken from the table contained in the Jahresbericht der Chemie of Kopp & Will for 1857, published in August, 1858, and the last which the author has been able to obtain at the time of concluding this paper, and have been in no case altered or modified in the slightest degree with a view to preserve or increase numerical relations, which by slight changes of this kind can be often rendered much more symmetrical. Dumas, in one of his highly interesting papers on this subject* (Comptes Rendus, XLV, 709, extracted in Kopp and Will's Jahr. 1857) in his series $a+xd+yd'$, adopts the equivalents N=14, P=31, As=75, Sb=119, Bi 207 (see Jahresbericht, p. 35, where the equivalent of Bi is erroneously printed 108, by substitution of the values given for a, x, y, d, and d', 207 is obtained): whereas the equivalent of Sb as lately found by R. Schneider, confirmed by H. Rose, and adopted by Kopp & Will is 120·3. In another place (Comptes Rendus, XLVII, 1027) Dumas has taken the equivalent of the same metal at 122, thus adopting alternately the numbers 119 and 122, neither the true one, according to the exigencies of the two series. The equivalent number of bismuth in the series just mentioned is taken at 207, whereas it should be 208. In the series $a+xd$ we find Mg=12, Ca=20, Sr=44, Ba 68, Pb 104—the last three should be Sr 43·77, Ba 68·6, Pb 103·5. So with Li, Na, K, V, Zr, &c.

In the foregoing tables the calculated and received equivalents are placed by side of each other for comparison. The differences rarely exceed the possible errors in the determination of chemical equivalents, respecting some of which there is still much doubt. Dumas, in the paper above referred to, gives the results of many new determinations by himself, and arrives at the number 26 for both chrome and manganese, instead of the ordinarily received Cr=26·7, Mn=27·5. For copper his results disagreed too much to lead him to any positive conclusion.

The analogies here presented, all depending upon the same or approximately the same number, extend therefore—

To the series Pb, Sn, N, P, As, Sb, Bi.

To the series Hg, Cd, Mg, Zn.

To the series Au, Ag, Cu.

To the magnesia group, including Mn, Fe, Co, Ni, U, Co, and some of the metals also classed in the three preceding series.

* The interesting paper of Prof. Cooke (Memoirs Amer. Ac., 2d ser., vol. v) to which the author's attention has been called since concluding this paper, will be more particularly referred to in the Second Part.

To the metals belonging to the group Ti, Ta, W, V, Mo, Te and No; Sn belongs also to this series as well as to the first.
To the platinum group, Rh, Ru, Pd, Pt, Ir, Os.
To Є, B, Si.
To G, Al, Zr.
The differences between Cl and Br, Br and I, approximate to the same number, as likewise do the relations between Li, Na and K, and between Ca, Sr and Ba.

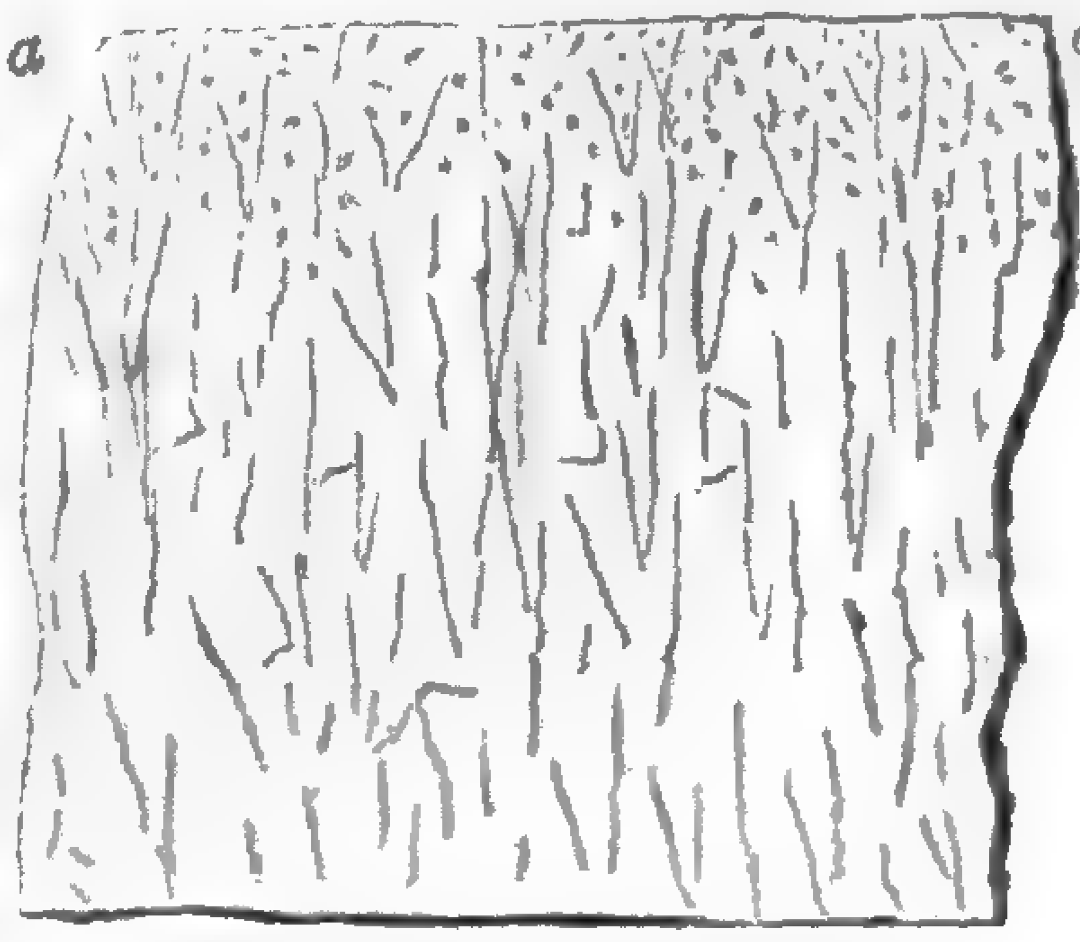
This relation, therefore, extends to no less than forty-eight ot the elementary bodies: to all except those as yet imperfectly understood, most of which may yet range themselves under the same law, and except the oxygen group, oxygen, sulphur, selenium and tellurium, substances which stand alone and unmistakably apart from the other elements.

Philadelphia, Nov. 10, 1859.

ART. XIV.—*Remarks on the Dissolution of Field Ice;* by CHAS. WHITTLESEY, of Cleveland, Ohio.

THE interesting paper of Col. Totten, U.S. A., in the November number of this Journal for 1859, upon the rapid disappearance of ice in the northern lakes, recalls some observations that I had an opportunity to make on Lake Superior a few years since.

On the 8th of March, 1855, the inhabitants of Eagle River, a village in Haughton County, situated upon the most northerly part of Point Kewenaw, were engaged in procuring ice for their summer use. The severity of winter in that latitude (47° 22′ north) had so far relaxed, that the surface of the field was slightly porous from the direct action of the sun. There had been no rain; the atmosphere was clear and cool, but on the sunny side of houses and other objects the snow melted rapidly in the day time.



Block of ice 30 inches thick.
a a, upper surface.

Below the soft and moist surface, at a few inches in depth, the ice was solid and pure to the bottom, its thickness being thirty inches. The blocks which the people were cutting out, were taken about 1000 feet from the shore. One of them nearly in the form of a cube, of thirty inches on each face, was suffered to lie upon the unbroken ice, its natural surface uppermost, as represented in the figure here inserted.

I was thus enabled to take a direct view of the progress of its decay, as no doubt others have done many times, upon these lakes. As the force of the sun increased, the porous part on the surface increased rapidly in depth, lines or planes of separation extending downward from it into the hitherto transparent and homogeneous mass. There were not at any time horizontal planes visible, indicating layers or lamination, in the original structure. A thin film of matter followed each newly formed crevice downwards, and bubbles of air rose continually through the same to the surface. These planes of division converged below, giving the block the appearance above represented, of inverted spikes or rudely formed pyramids, with their bases upward.

By ten o'clock A. M. the upper half of the block was divided in this manner. The figures were somewhat regular and were principally triangular and rectangular, reminding me of the imperfectly columnar red trap of the north shore of Lake Superior. By noon the block was so far disintegrated that it fell to pieces under a single blow, and remained a pile of roughly formed spikes, pyramids and prisms of various lengths. After this as so much new surface was exposed to the sun it melted very fast. The newly cut ice was still solid and clear except a few inches at the surface.

There seemed to be in the block that had so suddenly lost its form and solidity, a process of contraction, arising from an increase of temperature. I presume that this appearance can be thus accounted for. No doubt the planes of division existed in the solid ice, as results of the crystallization in freezing. The general law of structure in all masses slowly crystallizing from a state of fusion is the production of a prismatic structure perpendicular to the cooling surfaces. Basalt assumes its polygonal figures in obedience to the same law, and the structure of ice is quite in accordance with it. Its effects are not wanting even in some pastes, like starch and domestic cake.

This structure exists often where it is concealed. An ingot of block tin shows no crystalline structure, but by slow fusion the amorphous parts melt and run out leaving a skeleton of crystalline prisms. Ice is in the same predicament, and since in freezing water expands one-seventh of its volume, the first result of the fusion of a part of it is to dissect out the prismatic masses, leaving them standing isolated by reason of their being on a larger scale than the fluid volume from which they were formed. In this process the air bubbles no doubt materially assist by opening channels of escape for the ice-water.

What I have stated may assist in explaining why immense fields of fresh water ice disappear in a single gale of a few hours duration. When the temperature rises above 32° the ice soon loses its cohesion, and the first agitation breaks it up. In popular phrase it sinks, and is thus lost sight of suddenly; but in truth it is dissolved by the warmer water acting upon the fragments in the shape of little columns and pyramids such as Col. Totten saw strewed along the shore of Lake Champlain.

SCIENTIFIC INTELLIGENCE.

I. CHEMISTRY AND PHYSICS.

1. *On Platinum and the metals which accompany it.*—H. St. Claire Deville and Debray have published a very interesting and valuable memoir on the platinum metals, considering the subject rather from a metallurgical than from a purely chemical point of view. For the details of the processes employed we must refer to the original memoir, which rarely admits of abridgment, and which is in the highest degree instructive. The authors employ exclusively the *dry* method of investigation and operate at temperatures much higher than any which have hitherto been obtained upon a working scale. By a new arrangement of the oxy-hydrogen blowpipe most of the members of the platinum group may be fused—platinum even in larger quantities than was accomplished by Dr. Hare. By the same apparatus properly employed, the authors purify the metals and their alloys from more volatile elements with which they may be mixed. Osmium has a density of 21·3 to 21·4, and when dissolved in tin exhibits traces of a crystalline structure. It is bluish white, has no odor, and gives off vapors of osmic acid only above the temperature of melting zinc. At the temperature of melting ruthenium, osmium is sensibly volatilized but it does not fuse, and hence resembles arsenic in having its boiling point lower than its point of fusion. Two determinations of the density of the vapor of osmic acid gave 8·88 and 8·89, corresponding to 2 vols. Next to osmium ruthenium is most difficult of fusion, but may yet be obtained in small fused masses when its density is from 11 to 11·4. The authors give analyses of the protoxyd of ruthenium and of the crystallized deutoxyd isomorphous with stannic acid. They also describe a beautiful alloy of ruthenium and tin having the formula $RuSn_2$ and crystallizing in cubes. Palladium fuses even more readily than platinum, and volatilizes at the temperature at which iridium melts. It also absorbs oxygen when fused like silver without becoming oxydized. Its density at 22·5° is 11·4. With tin it forms an alloy crystallizing in small brilliant plates having the formula Sn_2Pd_3. Rhodium fuses less easily than platinum; it has about the color of aluminum and when pure is malleable and ductile; its density is 12·1. It forms crystalline alloys with zinc and tin.

Platinum may (as first shown by Dr. Hare) be fused in large quantities before the oxy-hydrogen blowpipe. Deville and Debray give a detailed description of the apparatus by which this metal may be fused in quantities of not less than 12 to 15 kilograms at an expense of from 0·24 fr. to 0·40 fr. per kilogram. (The late Dr. Hare fused 28 ounces at one operation.) The fusion of platinum is best accomplished in crucibles of lime, which serve to refine the metal by absorbing the impurities. When fused and refined, platinum is as soft as copper; it is whiter than ordinary platinum and free from porosity; its density is 21·15. With tin platinum forms a beautiful crystallized alloy having the formula Pt_2Sn_3. Iridium requires an extremely high temperature for its fusion, but when fused has a pure white color, and is brittle under the hammer like a crystalline metal; its density is the same as that of platinum, namely 21·15.

(Dr. Hare, who long ago succeeded in fusing iridium, found its density 21·83.) Iridium forms with tin a beautiful alloy crystallizing in cubes having the formula $IrSn_2$.

The authors remark that the alloys of platinum with iridium and rhodium are much more valuable in the arts than pure platinum, many of them resisting the action of aqua-regia, and possessing a considerable degree of hardness and rigidity. The memoir contains in addition numerous elaborate analyses of different specimens of platinum ore and of osmiridium, as well as new processes for the treatment of platinum ores upon the larger scale, the preparation of pure platinum for industrial purposes, and of alloys of platinum with its associate metals possessing useful properties. For these we must refer to the original.—*Ann. de Chimie et de Physique*, lvi, 385, Aug. 1857.

[*Note.*—A memoir read before the Am. Association for the Advancement of Science at its meeting in August 1859, and shortly to appear in the 12th volume of the Transactions of the Smithsonian Institution, contains entirely new processes for the separation of all the platinum metals in a state of absolute purity. These processes are in the wet way; they are very simple and easy of execution, and they not only apply to the separation but to the qualitative analysis of mixtures of the different metals of this group in almost any proportions. The methods in question involve the preparation and properties of a new and remarkable series of salts, and will I hope be found to remove completely the difficulties which have hitherto surrounded the subject.—W. G.]

2. *Blowpipe experiments.*—Bunsen has contributed some very interesting additions to our knowledge of the use of the blowpipe in quantitative as well as qualitative analysis. The author employs the peculiar form of gas burner, first introduced by him, and now used in all laboratories, instead of the blowpipe. The lower part of the flame is surrounded by a conical sheet iron chimney, 30^{mm}. in diameter above, and 55^{mm}. below, so that the burner tube is in the axis of the chimney and 45^{mm}. below the upper base of the cone. The cock is to be regulated so that the apex of the inner non-luminous cone of gas within the flame exactly reaches the level of the upper base of the chimney. In this manner we obtain a flame of perfectly constant dimensions which is immovable, sharply defined in all its parts, and which may always be obtained of uniform character. The outer cone of flame has a very faint sky-blue color, which is invisible even by feeble daylight. The inner cone of flame is less intensely blue than the outer. The object to be submitted to the action of the flame must never be larger than the half or the third of a grain of millet seed. It is to be introduced into the flame by means of a little loop on the end of a platinum wire which is attached to a holder by which it may be moved gently and steadily, so that the object may be introduced into any part of the flame. The loop is to be moistened with water, when a grain or a little of the powdered substance will readily adhere to it.

The author remarks that the temperature which the flame is capable of producing depends principally upon the constitution of the gas consumed. The temperatures corresponding to gas analyzed in the Heidelberg laboratory on four different occasions were 2369 C., 2352 C., 2391

C., 2386 C., or as a mean, 2350 C., so that the temperature of the flame where the quantity of air is exactly sufficient for the combustion of the gas, may be assumed in round numbers as 2300 C. It is easy to see, however, that the temperature will vary in different parts of the flame. The author gives a simple and elegant method of determining the point of maximum temperature by introducing a platinum wire into the flame, and determining at what point the light emitted by this is most intense. In this manner it is found that the zone of maximum temperature lies in the external cone of flame, a few millimetres above and below the apex of the internal non-luminous cone, which is on a level with the upper base of the chimney. The author employs this zone to investigate the action of a temperature of 2300 C. upon different substances, and terms it the *melting space.* The outer border of this *melting space* acts as an oxydizing flame, the inner as a reducing flame, the reduction being most powerful immediately above the apex of the innermost cone. The great constancy which the flame exhibits in all its parts allows us to observe and estimate the volatility of substances at the very high temperature of 2300 C. For this purpose a mass of matter having a measured diameter of 1 millimeter is introduced into the flame, and the time required for complete evaporation determined by means of a seconds pendulum or a metronome. The size of the mass introduced upon the platinum wire is easily regulated by adding new substance or by evaporation in the flame, and may be measured under the microscope. The author takes the volatility of carbonate of soda as unity, and gives a table of the comparative volatility of different substances in terms of this unit: thus the volatility of chlorid of potassium is 15·33; of chlorid of sodium 6·57; of phosphoric acid 23·00. Other substances are more or less completely decomposed at the temperature of 2300 C.

Besides its use in experiments on volatility the flame may also be employed for a series of other very valuable blowpipe reactions, among which the author cites the quantitative determination of soda in the presence of potash and lithia.

For the simple recognition of soda in its volatile salts, it is sufficient to introduce a small bead of the substance into the melting space and then to illuminate a crystal of bicromate of potash with the light produced. The salt appears perfectly colorless, transparent, and with a diamond lustre so long as the rays of the soda flame fall upon it, and this even by ordinary lamplight or daylight. A still more delicate reaction is obtained by using paper, covered with iodid of mercury, a square centimeter of which may be attached to the chimney in front of the flame by a movable arm. If we introduce the smallest quantity of a soda compound into the melting space, the red paper appears white, with a faint tinge of tawny yellow. When the soda salt is in solution the loop of the fine platinum wire may be flattened under the hammer to a little ring. This ring introduced into the liquid will take up a drop which must be gently evaporated to dryness and then tested as before. In this manner $\frac{4}{10,000}$ of a milligram of common salt may be easily detected.

Volatile potash compounds, as is well known, communicate a bluish violet tint which is completely concealed by small quantities of soda. In this case the potash may easily be detected by means of Cartmell's reaction, that is, by looking at the flame through a deep blue cobalt glass, when a violet or ponceau-red color appears. This potash test is even more delicate than that for soda, $\frac{2}{10,000}$ parts of chlorid of potassium may be detected with perfect distinctness.

The detection of lithia in the presence of potash and soda is best effected by looking at the flame through a hollow prism filled with a solution of sulphate of indigo. The carmine-red color of the lithia flame disappears when a certain thickness of solution is reached, and if a mark be put upon the prism, all the layers of liquid above the mark will allow only the red potash-rays to pass through. Lime and soda have no influence on this reaction. When potash and lithia are both present the flame from the salt to be tested should be compared with that of pure potash. The flame containing lithia and potash appears through thin layers of liquid redder than the potash flame alone; through thicker layers the potash flame appears scarcely weakened. In this manner some thousandths of lithia may the discovered in potash salts.

To render these processes available for silicates Bunsen mixes the mineral with pure gypsum and heats in the melting space, when silicate of lime and volatile sulphate of potash are formed. By comparing the intensity of the color produced by a mineral to be tested with that of a series of silicates whose percentages of alkali are known, it is easy even with very small fragments to determine the relative quantities of potash, soda and lithia with tolerable approximation.

The author in the first place determines whether the mineral to be examined contains lithia or not by the method already explained. The minerals of the first group are those containing no lithia, and the author arranges a scale of minerals for comparison according to the content of soda in each. These are—

	NaO	KO
1. Lazulite,	9·09	—
2. Nepheline,	15·44	4·94
3. Albite,	10·06	—
4. Orthoclase,	7·08	7·03
5. Sanidin,	4·0	8·0
6. Labradorite,	2·55	1·06
7. Anorthite,	1·13	0·62
8. Leucite,	—	2·2

These are to be ignited, pulverized and preserved with their numbers as blowpipe reagents. One of these and the mineral to be tested, with or without gypsum, are to be introduced into the flame together so that small equal ends of wire are ignited; the iodid of mercury paper then appears more or less bleached. Remove the test from the flame, if now the paper shows a reddish tint, the test contains more soda than the mineral used for comparison. If the paper however becomes paler the contrary is the case. In this manner it is easy to determine between what two minerals the test lies, so that the percentage of soda may be estimated within a few per cent. The substances to be compared must be as nearly as possible in equal quantities, the ignited lengths of wire be the same and the soda-flame of the same size and form. The eye must be accustomed to distinguish the different degrees of brightness of the same tint from actual differences of tint. When the iodid paper is intensely bleached it may also be illuminated by a candle flame so that the soda-flame produces with this foreign light a white tint nearer to red.

The potash color-test is not as accurate quantitatively as that for soda. It is sufficient for all purposes to distinguish *a slight*, *a strong*, and a *very strong* potash reaction, using for comparison in succession the flame of oligoclase, orthoclase and leucite heated with gypsum in the same flame with the test. The indigo prism is to be used and the dimensions, color and duration of the red flames observed.

Lazulite gives a stronger soda reaction than nepheline because it contains sulphuric acid, and it is always necessary to determine beforehand whether the test contains sulphuric acid, chlorine or fluorine. This is best accomplished by the common blowpipe.

For further details we must refer to the original memoir, which must create an entirely new department in blowpipe analysis.—*Ann. der Chemie und Pharm.*, cxi, 257.

[*Note.*—It is easy to see that Bunsen's memoir contains the solution of many chemical and physical problems of great interest. Thus it is easy to produce at will a flame which shall have any required temperature, at least between certain limits. Since according to Bunsen's calculations (Gasometrische Analyse, p. 254) the flame of hydrogen burning freely in the air has a temperature of 3259° C., and that of olefiant gas has a temperature of 5413° C., we may as readily experiment at these temperatures as at 2300° C. Very much higher temperatures are of course produced when these gases burn with pure oxygen—in the case of hydrogen 8061° C.; in that of olefiant gas 9187° C. Hence by burning mixtures of these gases with oxygen and varying the proportion of nitrogen arbitrarily, we can make a scale of flames the temperatures of which shall range from less than 2000° C. up to at least 9000° C. and the melting point as well as degree of volatility of almost all metals and mineral substances may be thus assigned within quite narrow limits. It is easy to calculate the percentage of nitrogen or carbonic acid to be added in each case to the combustible gas in order to produce the required temperature. It appears probable that at high temperatures the radiating power of a body for heat is proportional to its radiating power for light. By directly comparing the intensity of the light radiated from platinum heated in a furnace with the intensity of the light radiated when the platinum is heated to a known temperature in a gas-flame, the temperature of the furnace might be approximately determined. Interesting results could also be obtained as to the exact temperature at which bodies become luminous and as to the relative quantities of light which different substances emit when heated to the same temperature, or the same substance at different temperatures. With respect to Bunsen's scale or series of minerals containing different but known proportions of soda I will suggest that perhaps a series of *glasses* could be made containing perfectly definite quantities of either soda or potash from 2 up to 30 per cent, so that each number would contain 2 per cent more alkali that the next lowest number. These would then serve as universal standards of comparison and give much precision to blowpipe observations.—W. G.]

W. G.

II. GEOLOGY.

1. *Review.—On some points in the Geology of the Alps.**—For nearly a century the great problem of the structure of the Alps has engaged the attention of geologists, and although much still remains to be done, we are gradually attaining to a clearer notion of the age and the mode of formation of this great mountain region. Among the most remarkable conclusions deducible from modern investigations is that of its very recent upheaval, and the age, geologically modern, of a large portion of its rocks. The great mass of nummulitic strata, with the overlying series of slates, limestones, marls, and fucoidal sandstones (once regarded as of transition age), which constitute the *flysch*, and belong to the eocene division of the tertiary rocks, has been upheaved vertically more than two miles, and penetrated by intrusive granites. Then took place the great movements of folding, inversion, and lateral and vertical displacement, the slow sinking of the northern side of the chain permitting the accumulation of the conglomerate of the *molasse* or the *nagelflue* (a formation partly freshwater and partly marine), which belongs to the miocene, and in part perhaps to the pliocene, and attains in some parts a thickness of from 6000 to 8000 feet. After the subsidence which allowed this vast accumulation, we find it raised by a vertical displacement of many thousand feet, and dipping towards the older rocks, so that the upper beds of the *nagelflue* seem to pass beneath the nummulitic limestones. (Murchison, *Quar. Jour. Geol. Soc.*, v, 241.) These great foldings and dislocations have often given rise to lateral movements by which the older strata have actually been forced over and made to rest unconformably upon younger rocks.

The Messrs. Rogers have shown that these great movements of the Alps present a close resemblance with those observed by them in the Alleghanies, and we are persuaded that a farther study of our older mountain chains, rendered more easy of investigation from the smaller number of formations involved, and from the regions being less elevated and more accessible, will throw still farther light upon the structure of the Alps and of mountain chains in general.

The vast thickness of the sedimentary deposits of secondary and tertiary age over the area now occupied by the Alps, when compared with their volume in other parts of Europe, serves to exemplify the relation which Prof. James Hall has so well pointed out, of the apparent connection of mountain elevations with original deposition (*Geology of Iowa*, i, 41). At the same time the weight of evidence now tends to show that the crystalline nucleus of the Alps, so far from being an extruded mass of so-called primitive rock, is really an altered sedimentary deposit more recent than many of the fossiliferous strata upon their flanks, so that the Alps as a whole have a general synclinal structure.

In the memoir before us the learned professor of geology in the Academy of Geneva, already well known by his researches in Alpine geology, has devoted himself to the study of a number of sections which exhibit

* *Memoire sur les terrains liassique et keuperien de la Savoie, par* ALPHONSE FAVRE. (Extrait du tome xv *des Memoires de la Société de physique et d'histoire naturelle de Genève*, 1859. Quarto, pp. 92, with three plates.)

the anthracitic and jurassic formations of Savoy. In these sections which he has given with lists of their organic remains, he includes (1) under the general name of the jurassic system both the oolitic or jurassic proper and the lias. Beneath these he distinguishes (2) a succession of limestones, chiefly dolomitic (*cargneules*), with interstratified beds of gypsum, underlaid by red and green marls and a silicious sandstone or arkose, the whole series, which attains a thickness of 2500 feet, being destitute of fossils, and reposing upon (3) shales and sandstones which enclose beds of anthracite and plants of the carboniferous epoch. Succeeding these comes the conglomerate of Valorsine (4), which, with the preceding group, he refers to the carboniferous system, while beneath all these are (5) reddish crystalline schists and gneiss, passing into a rose-colored protogine or talcose granite.

The third division is regarded by Favre as representing the *keuper* or triassic system, which, although known in the Tyrol and in Lombardy, has not hitherto been recognized in Savoy. Its identification is regarded by our author as highly important, inasmuch as it establishes a well defined geological horizon throughout this region. To the triassic system he refers the gypsums which are found encircling Mt. Blanc, and the salt deposits of Bex, of the Tarentaise, and elsewhere. Favre however recognizes the existence of more recent gypsums, with *cargneules*, in a fucoidal sandstone overlying the nummulitic limestone. The *cargneule*, which is here a constant associate of the gypsum, is according to the recent analyses of Marignac (p. 11) a cellular limestone or tufa containing about nine per cent of carbonate of magnesia, while the cells are filled with a nearly pure pulverulent dolomite. Massive, compact and crystalline dolomites are however frequently associated with these cargneules, which Haidinger conceives to have been formed by the alteration of dolomites under the influence of a solution of gypsum. We are however inclined to regard the cargneules as analogous to the tufas which we have elsewhere described as often associated with true dolomites. (This Journal, xxviii, 372.) To this subject we propose soon to return, but we take occasion to correct an error into which Mr. Favre has fallen in attributing to Sir William Logan the view that magnesian limestones owe their origin to the action of solutions of carbonate of soda upon sea-water (p. 42). This has arisen from an oversight in quoting from the *Literary Gazette*, where some observations made by us at the meeting of the American Association at Montreal in August, 1857, are reported in immediate connection with a paper of Sir W. E. Logan. We have endeavored to show that carbonate of magnesia has been formed in nature from the decomposition of soluble magnesian salts either by solutions of bicarbonate of soda or bicarbonate of lime, in the latter case sulphate of magnesia being concerned and sulphate of lime being also a product. (*Comptes Rendus de l'Acad.*, xlviii, 1003, and this Journal, xxviii, 170.)

Mr. Favre has not endeavored to identify the Permian system in this region, although according to Pidancet it occurs with its characteristic flora, beneath the variegated sandstones of the trias, in the adjacent department of the Jura. (*Bul. Soc. Geol. de Fr.*, [2], xii, 149.) The trias, according to our author, rests directly upon the shales and sandstones which are regarded by him as belonging to the carboniferous system, and at other times upon crystalline schists, which however he suggests may be no other than the carboniferous rocks so highly altered as not to be distinguished from the older crystalline schists.

In the view which he takes of the relations of the jurassic and anthracitic rocks of the Alps, Mr. Favre is so completely at variance with most of those who have preceded him that it will be necessary to enter into some details with regard to this so-called anthracitic formation, which has been hitherto regarded as intercalated with and forming a portion of the jurassic system of the Alps. The conglomerate of Valorsine, which Favre classes with the palæozoic series, is according to Sismonda, no other than the *verrucano* of Savi, and the lowest member of the liassic series, beneath which at Jano in Tuscany is an anthracitic formation associated with a strictly palæozoic fauna. (Ibid, p. 635.) In various localities in Savoy, on the contrary, and particularly at Petit-Cœur in the Tarentaise and near Briançon, the anthracitic beds, consisting of sandstones and shales with impressions of ferns and layers of anthracite, appear to be intercalated in the jurassic system. This fact, announced by Elie de Beaumont in 1828, has been confirmed by a great number of subsequent observers, among which may be mentioned Sismonda, Murchison, Gras and Rozet. The fossil plants of this formation were by Brongniart and subsequently by Heer identified with those of the carboniferous system; according to Mr. Bunbury, who has since examined the fossils of Petit-Cœur, which are very much distorted and replaced by a film of talcose matter, their identification is difficult, and although some are undoubtedly carboniferous species, others are doubtful. (*Geol. Journ.* v, xxix, 130.) Elie de Beaumont, with Murchison and Rozet, conceives that the evidence of the age of a formation furnished by fossil plants, to be far less reliable than that afforded by its fauna, and the latter imagines that during the jurassic period the climate may have permitted the development, upon islets in the midst of the sea, of a flora like that of the coal period. The observations of Elie de Beaumont in 1828 led him to refer the ferns and anthracites of Petit-Cœur as well as those of the neighborhood of Briançon to the summit of the lias. Both Gras and Sismonda distinctly recognize two horizons of anthracitic beds with fossil ferns, the one at the summit and the other at the base of the great anthracitic system; but while Sismonda (grounding himself upon the inferiority of the true coal formation to the conglomerate of Valorsine,) agrees with Elie de Beaumont and Murchison in regarding this system as really of jurassic age, although including a flora like that of the coal, Gras is inclined to look upon it rather as a formation of carboniferous age, with a liassic fauna, which has not however afforded a single palæozoic species.

Notwithstanding the concurrent evidence of so many observers to the fact of the intercalation of the anthracitic beds over a large area, with liassic fossils, there have not been wanting those who have endeavored to explain this association by supposed foldings, which may have involved the liassic beds with others belonging to an underlying carboniferous formation. This explanation, long since suggested by Voltz, was afterwards brought forward by Favre in 1841, since which time it has been discussed by various geologists, although generally with little favor. In the present memoir our author examines the section of Petit-Cœur, and finds on one side, intercalated between the beds with belemnites and those containing ferns, a mass of cargneule which he considers the representative of the triassic system, separating the carboniferous and liassic series; the cargneule has not however been detected on the other side of the supposed fold, where the belemnite beds are in direct contact with the anthracite series. It might farther be objected that the mass of cargneule, the usual associate of gypsum, is not alone sufficient proof of the existence of the triassic system, since, according to Gras, gypsums accompanied by yellow altered limestones (in one case declared to be dolomite,) occur at two distinct horizons in the anthracitic series.

The observations of Mortillet on the section of Petit-Cœur are important; he not only concludes from his examinations that it is impossible to admit the existence of a fold in the strata at this locality, but asserts that the belemnites which underlie the fossil plants are different from those above. In the lower beds there is found but a single species, the *Belemnites acutus* (Miller), which as well as an associated ammonite, *A. bisulcatus*, belongs to the lower lias. In the beds above the coal fossils, on the contrary, there are several species of belemnites, not readily identified, but differing in form from that just mentioned, and apparently belonging to the middle or upper lias. He farther cites the existence in the same specimen of a belemnite and a fossil fern. (*Bull. Soc. Geol. de France*, [2], x, 18.)*

The testimony of Sir Roderick Murchison, after a minute examination of the section at Petit Cœur, is also most decisive as to the impossibility of admitting any folding or inversion of the strata, and he confirms the opinion of Elie de Beaumont and Sismonda as to the liassic age of the coal flora in question. As regards the vertical range of certain fossil plants he refers to a species of *Calamites* which extends from the carboniferous series through the permian and trias, and an *Equisetum* which is common in the trias of Germany and yet abounds in the oolite of England. (*Geol. Journal*, v, 178.)

In the Alps of Lombardy, Omboni has described a succession of rocks similar to that given by Favre; beneath the lias, according to him, are red and green marls with limestones containing the fossils of the muschelkalk, underlaid by red and green sandstones, the whole being referred to the triassic system. Lower still a series of limestones, dolomites and sandstones underlaid by black shales, the whole without fossils, and resting upon crystalline schists and protogines, is referred to the permian and carboniferous formations. These sandstones he considers as identical with the verrucano, which according to him contains in Tuscany the fossils of the coal period. (*Bull. Soc. Geol. de France*, [2], xii, 517.) We have already seen that Sismonda regards the verrucano as belonging to the base of the lias, and although associated with the anomalous liassic flora, as overlying the true palæozoic coal measures.

* For fuller details see Gaudry's sketch of the various papers on this subject, *Bull. Soc. Geol. de France*, [2], xii, 580 and 637. Sismonda, Ibid., p. 631. Rozet, *Memoire sur les Alpes Françaises*, ibid., 204; Gras, *Sur le terrain anthracifère des Alpes, Ann. des Mines*, [5], v, 475, and d'Archiac *Histoire de la Géologie*, vii, 107–157. Also Lyell, *Geol. Journal*, 1849, p. xxxviii, and Portlock, ibid., 1856, p. lxvii.

Connected with the view which supposes the continuation of a palæozoic flora up to the epoch of the lias is a question raised by our author (p. 29) regarding the association (established by comparison of considerable lists of species from various localities), of the fossils of the different stages of the lias in a single bed. Analogous cases are frequently met with at different geological horizons, showing that the causes which have limited the vertical range of certain animals are so far local, that under somewhat modified conditions the duration of a species may be prolonged after its disappearance from the adjacent seas. As our author has well said, it is in the continuation of similar chemical and physical conditions, generally dependant upon local accidents of the surface, that we must look for the cause of these apparent anomalies in the distribution of fossils. The investigations of Prof. Safford in Tennessee and Sir William Logan in Canada have made known a remarkable example of this in the Lower Silurian series of North America. In the State of New York the fauna of the Black River and Trenton limestones are almost totally distinct, so that the division between the two formations constitutes a well-marked palæontological horizon, while in Canada and in Tennessee the fossils of the two formations are so completely intermingled that it is impossible to distinguish between the Black River and Trenton limestones. (Billings, *Report Geol. Survey of Canada*, 1857, p. 152.) The genus *Catenipora* in North America also presents a remarkable instance of anomalous distribution; this coral throughout New York and Western Canada is Upper Silurian and is unknown below the Clinton Group, while at Lake St. John on the Saguenay it is abundant in the Black River limestone, a position which corresponds with its geological horizon in Europe.

The anthracitic system of the Alps has been described by Gras with great minuteness in the memoir cited above. He estimates the total thickness of the system at between 25,000 and 28,000 feet, and divides it into an upper and lower series, which are unconformable. The former with a thickness of about one-tenth of the whole, is referred to four subdivisions, and consists chiefly of argillo-calcareous shales terminated by a considerable mass of limestone, immediately beneath which occurs a bed of spilite, associated with gypsum and a yellow altered dolomite. The inferior division also consists in great part of similar shales, interstratified with gneissoid and talcose rocks to be noticed farther on; gypsum and a yellow altered limestone (dolomite?) also occur in this portion. In both the upper and lower divisions sandstones and shales are found with coal plants and layers of anthracite, the latter sometimes changed into graphite, while the associated calcareous shales contain in various localities, and at different horizons, belemnites and ammonites of liassic age.

Sismonda confirms the accuracy of the observations of Gras, and admits an upper anthracitic series, resting unconformably upon the lower, and of oolitic age, the lower only being referred by him to the lias proper, while Studer concludes the discussion of the subject as follows: "The intercalation of the lower anthracitic slates sometimes with gneissoid and talcose schists, and sometimes with the belemnitic shales, the layer of jurassic limestone which separates the lower from the upper anthracites, the extraordinary thickness of this calcareous layer and the anthracitic zone which covers it, the presence of the verrucano (or talco-quartzose conglomerate) between these two formations and the quantity of talc disseminated in all these rocks, present us problems which science is not able to solve completely."—(Studer, cited by Laugel, *Bull. Soc. Geol. de France*, [2], xii, 576.)

In the metamorphic rocks of the anthracitic system Gras distinguishes what he calls a protogine or talcose formation, consisting of granite, gneiss and mica slates, generally more or less talcose, containing also schistose diorites, eurites and leptynites. This protogine formation is intimately associated with the argillo-calcareous shales of the lower anthracitic series, which are found interstratified with and even passing into gneiss and other feldspathic schists. Gras concludes that this somewhat irregular interpenetration of the two classes of rocks is due to an irregular metamorphism, portions of the argillo-calcareous sediments having, as he supposes, been profoundly altered by emanations from below, and he hence regards the protogine formation as a portion of the inferior anthracitic series. Similar crystalline schists also occur in some parts of the upper anthracitic series, and in both the upper and lower there are found serpentines, euphotides, porphyries and spilites, all of which the author regards as of sedimentary origin, and as having undergone *in situ* a profound metamorphism which has often effaced the marks of stratification; this view he declares is the only one which appears to agree with the observed facts. This protest against the theory of the igneous origin of serpentines is in accordance with the results obtained by the Geological Survey of Canada; in his report for 1848 Sir William Logan insisted upon the sedimentary origin of the serpentines which occur in the altered palæozoic strata of the Green Mountains.

Rozet, in his investigations of the Alps, has referred to the liassic and jurassic periods the great system of gneiss, with micaceous and talcose schists, which makes up Mts. Cenis and Pelvoux, and a great part of the mountains of the frontier of Piedmont, while, according to Fournet, the jurassic rocks of the Valais have been altered in like manner. (Coquand, *des Roches*, pp. 300–301.)

Our author however supposes that all these altered strata are of carboniferous age, and remarks that the fossils in the limestone of the lower lias show but slight marks of metamorphism, from whence he concludes that "in this region of the Alps there are no highly metamorphosed jurassic rocks," and that the metamorphic action "which took place beneath the sea before the elevation of these mountains" ceased to be powerful after the deposition of the Valorsine conglomerate, (which he regards as of carboniferous age,) the paste of which is often converted into a crystalline talcose schist. At the same, although the liassic and so-called triassic rocks are comparatively unaltered, the jurassic shales are described as more or less talcose and greasy to the touch, but the alterations of these argillaceous rocks our author regards "as rather mechanical than chemical." (p. 76.)

The question before us is then, whether in that series of rocks which embraces liassic and jurassic beds with gypsum, dolomite, anthracite, and a carboniferous flora, and which geologists have generally referred to one great system not older than the lias, we have really in these mountains, as Favre supposes, the whole succession of formations from the oolite to the carboniferous inclusive, so involved by foldings and inversions that it has hitherto been impossible to determine their real structure.

But even if we admit with Favre the palæozoic age of the protogine formation described by Gras and Rozet, we cannot agree with him in limiting to the rocks of that period the action of the metamorphic process. The development of a talcose character in the jurassic shales we cannot regard as the result of a mechanical process, and we have besides evidences in the Alps of the metamorphism of still higher rocks.

As early as the year 1834 Keferstein had asserted that the granites of Mont Blanc are nothing more than altered strata of *flysch*, (*Naturgeschichte des Erdkörpers*, i, 286–292; *Bul. Geol. Soc. de France*, [1], vii, 198,) and in 1850 Sir Charles Lyell in his address before the Geological Society of London suggested that the protogines of the Alps might be of tertiary age. This is so far true that both Studer and Murchison have shown that portions of the eocene flysch have been converted into crystalline gneiss, mica slate and even granitic beds; Murchison, like Gras, whom we have already cited, remarks that the metamorphism seems irregular, some bands of the rock being apparently much more altered than others. (*Geol. Journal*, v, 164, 210.) The intercalation of wedge-shaped masses of fossiliferous limestone of liassic age among the gneissic strata of the Alps has been well described by Studer (*Bul. Soc. Geol. de France*, [2], iv, 208), and similar phenomena observed in various other metamorphic regions present a problem the right understanding of which is most important in its relations to the theory of metamorphism, and one which we propose to consider at an early day.

In conclusion we hasten to say, that although dissenting from some of the views of Mr. Favre, we are not less grateful for his very suggestive memoir, which with its carefully prepared lists of fossils and its beautiful sections is a valuable contribution to alpine geology.

T. Sterry Hunt.

2. *The Geological Structure of the "Jornada del Muerto," New Mexico, being an abstract from the Geological Report of the Expedition under Capt. John Pope, U. S. Top. Eng., for boring Artesian Wells along the line of the 32d parallel;* by G. G. Shumard, M.D., Geologist of the Expedition, (Trans. Acad. Sci. St. Louis, vol. i, No. 3, p. 341. 1859).—This paper gives a description of the geological structure of a district of country lying immediately east of the Rio Grande, and between the 32d and 34th parallels. The "Jornada del Muerto" is described as a gently sloping plane, of an elliptic form, from twelve to forty miles in breadth, and extending from near the southern extremity of the Doña Ana Mountains eighty or ninety miles in a N.N.W. direction. It is bounded on the east and west by ranges of mountains, varying in their elevation from two or three hundred to one thousand feet above the plane, and seem to be mainly composed of dark colored limestones of Upper Carboniferous age, dipping towards the interior of the intermediate plane; these rocks however, in the western range, were observed at some places, surmounted by shales and sandstones referred to the Cretaceous epoch. Igneous rocks form a belt of low hills along the eastern side of the eastern range, and also occur between the other and the Rio Grande. From the structure of

the surrounding country, the "Jornada" is supposed to be a great synclinal depression, in which water could probably be obtained by artesian borings, through heavy detrital deposits.

The fossils found in the Carboniferous rocks here, and in the Organ Mountains, are all Upper Carboniferous types, many of them being identical with species almost everywhere common in the western Coal Measures. Those mentioned in the Cretaceous beds are *Inoceramus* and *Cardium*. The paper also contains interesting local details in regard to the igneous and metamorphic rocks of the several mountain ranges explored. M.

3. *Notice of Fossils from the Permian strata of Texas and New Mexico, obtained by the United States Expedition under Capt. John Pope, for boring Artesian Wells along the 32d parallel, with descriptions of new Species from these strata and the Coal Measures of that region;* by B. F. Shumard, M.D., (Trans. Acad. Sci. St. Louis, vol. i, Part 3d. 1859).—This is an important paper, containing descriptions of many new species, with an enumeration of others identified with forms known in the Kansas rocks, and of a few which are, by the author, supposed to be identical with foreign species; it is also illustrated by an excellent plate of twenty-seven figures by Leopold Gast & Brother of St. Louis.

Most of these fossils are from an extensive deposit of white limestone, and inferior beds of sandstone and darker colored limestone, in the Guadalupe Mountains, referred by Dr. S. to the Permian System. The new species described from these rocks are—

Campophyllum? Texanum, Chonetes Permiana, Spirifer Guadalupensis, Terebratula perinflata, Rhynchonella indentata, R. Texana, Camerophoria Swalloviana, Crania Permiana, Axinus securis, Turbo Guadalupensis, Pleurotomaria Halliana, and *Chemnitzia Swalloviana.* Those from the beds regarded as Carboniferous, are *Turbo Texanus, Straparollus cornudanus, Pleurotomaria Proutiana, P. obtusipira, P. perornata* and *Machrocheilus Texanus.*

All of which appear to be described with the author's well known care and accuracy.

Dr. S. had previously described from the beds he places in the Permian,

Phillipsia perannulata, Fusulina elongata, Productus Mexicanus, P. pileolus, P. Popei, Strophalosia (Aulosteges) Guadalupensis, Spirifer Mexicanus, S. sulciferus, Spiriferina Billingsii, Terebratula perinflata, Rhynchonella Guadalupæ, Camerophoria bisulcata, Retzia papilata, R. Meekiana, and *Myalina recta,*—

several of which are well illustrated in the plate accompanying the paper now under consideration. He also gives the following list of forms from these rocks, regarded by him, with more or less confidence, as identical with species occurring in the Permian and Upper Carboniferous beds of Kansas, viz:

Acanthocladia Americana, Productus Calhounianus, P. Norwoodii, Spirifer cameratus, Streptorhynchus (Orthisina) Shumardianus, Edmondia suborbiculata, and *Pleuraphorus occidentalis;* while he thinks he recognizes the following foreign species in the same association:—*Chætetes Mackrothii, Productus semireticulatus,* var. *antiquatus, P. Leplayi?, Terebratula elongata, Camerophoria Schlotheimi?, Myalina squamosa, Monotis Speluncaria,* and *Turbo helicinus?.*

As Dr. S. finds a *Phillipsia* and a *Fusulina*, in these rocks, neither of which genera are known to range up into Permian beds in the old world, and *Spirifer cameratus* is a characteristic Coal-measure species, from Pennsylvania to the Rocky Mountains; while *Productus semireticulatus*, is regarded by most authors as peculiar to the Carboniferous system—and a large proportion of the other species mentioned as common to the New Mexican and Kansas rocks, are known to occur in the latter territory in beds containing even a majority of well marked Coal-measure species, we may infer that in New Mexico, as in Kansas, there is a considerable blending of Carboniferous and Permian types; so that it becomes a matter of doubt and difficulty to determine at what particular horizon the line of demarkation should be drawn between these two Systems, if indeed there is any such natural break in our upper Palæozoic series of this country. It is to be hoped Dr. S. will continue his investigations of the fossils occurring in these formations, which he will doubtless have an opportunity to do, in connection with the geological survey of Texas, under his charge. M.

4. *Observations on the Geology of the County of Ste. Geneviève, being an extract from the Report made to the Missouri Geological Survey,* in 1859; by B. F. Shumard, M.D., (Trans. St. Louis Acad. Sci., vol. i, part 3, page 404, 1859.)—In addition to information respecting the iron and lead mines, building materials, &c., of the county, this extract contains some facts having an important bearing on mooted points in the classification of the Lower Carboniferous Series of the West. Dr. S. found this series to be composed of the following members, in the descending order:

1st. The Upper Archimedes Limestone, characterized by *Pentremites pyriformis, P. sulcatus, Agassizocrinus dactyliformis, Spiriferina spinosa, Spirifer trigonalis,* and species of *Archimedipora.*

2d. The Ferruginous Sandstone, in which no fossils were found,—estimated thickness, 80 to 100 feet.

3d. The Ste. Genevieve Limestone, a second Archimedes bed, in which the following fossils were found:—*Rhynchonella trimela, R. Wortheni, Spirifera hirsuta, Retzia Marcyi, Spiriferina spinosa, Spirifera Leidyi, Productus elegans, P. bisulcatus, Murchisonia vermicula, Pentremites florealis,* and one or more species of *Archimedipora.*

4th. The Saint Louis Limestone—containing *Lithostrotion mammillaria, Archæcidaris* and *Pentremites conoides,*—thickness 100 feet or more.

5th. The Third Archimedes Limestone, containing *Pentremites laterniformis, P. conoides, Archimedipora, Dichocrinus simplex, Spirigera hirsuta, Productus Indianensis, Rhynchonella subcuneata,* and *Holopea Prouti,*—thickness from 100 to 150 feet.

6th. The Encrinital Limestone (= Burlington Limestone), with its usual fossils—being the lowest member of the great Carboniferous series.

Of the Devonian rocks he recognizes the Chemung Group, Hamilton Group, and the Oriskany Sandstone. Of the Silurian—1st. Lower Helderberg Series, 2d. The Niagara Group, 3d. The Hudson River Group, 4th. The Receptaculite Limestone, 5th. The Trenton Limestone, 6th. The Blackriver and Birdseye Limestones. Then comes the five members of the Great Magnesian Limestone Series, which represents the Calciferous, and possibly portions of the Potsdam and Chazy Limestones of New York; and last of all, eruptive rocks.

Of the Coal measures, only thin outliers cap the hills half a mile above St. Mary's on the Mississippi. The beds are alternations of shale and sandstone, surmounted by a thin bed of hard siliceous limestone. M.

5. *Third Series of Descriptions of Bryozoa, from the Palæozoic Rocks of the Western States and Territories;* by H. A. Prout, (Trans. Acad. Sci. St. Louis, vol. i, part 3, p. 443, 1859.)—Every geologist who has

worked amongst western rocks, must have regretted that Palæontologists have generally given so little attention to the remains of *Polyzoa* characterizing these formations. These delicate forms of life existed in great profusion during portions of the Palæozoic era, especially during the deposition of some of the lower members of the great Carboniferous series, and are often met with in a good state of preservation, where no other organic remains are to be seen. Consequently when accurately classified, and the species and genera are fully described and illustrated,—due care being taken to determine the exact geological position of each,—they will at once become an important guide in the identification of strata.

The task of classifying, describing, and illustrating these remains occurring in the western rocks, has been undertaken by Dr. Prout of St. Louis, who has produced several valuable papers on this subject, previous to the publication of that now under review. His last paper, mentioned at the head of this notice, contains full descriptions of nine new species, and two new genera, with four beautiful plates illustrating these and some of the species described in his former papers. These plates are engraved on stone by Leopold Gast and Brother, of St. Louis, from drawings by Dr. Prout and Mr. Gast, and bear evidences of skill and accuracy. The new genera described in this paper are *Semicoscinium* and *Septopora*, and the new species, *Semicoscinium rhomboideum*, *Fenestella hemitrypa*, *F. banyana*, *Limaria falcata*, *Flustra spatula*, *F. tuberculata*, *Septopora Cestriensis*, and *Polypora tuberculata*.

Dr. S. also thinks he has identified a Permian species, *Polypora biamica* of Keyserling, in the Upper Archimedes Limestone, a member of the Lower Carboniferous Series,—at any rate no essential differences were observed in the specimens compared. It is probable however, that when better specimens are obtained, showing all the characters of this Lower Carboniferous form, it will prove distinct from the Permian species. M.

III. BOTANY AND ZOOLOGY.

1. *Collections of Cuban Plants.*—Mr. Charles Wright revisited the eastern part of Cuba in the autumn of the year 1858, where he still remains, engaged in botanical explorations in that little-known region. His collections of dried plants, up to last autumn, have already been received; and the Ferns, which form a large and very attractive part of them, have been distributed into sets. A number of these sets, not yet appropriated, are offered for sale. The fullest of these sets contain about 180, the smallest 120 species, which may be increased by further collections to a moderate extent. Sets can be obtained from Professor Gray, Cambridge, at $10 per hundred specimens. Of phænogamous plants, soon to be distributed, a very few sets are still open to subscribers, at the same price. It is expected that the species will be named very soon. The names of the ferns are about to be published by Mr. Eaton of New Haven, our principal Pteridologist. The rich collections in the lower *Cryptogamia*, made by Mr. Wright in his former visit to Cuba, along with those of the present exploration, are now in course of study, the *Musci* and *Hepaticæ* by Mr. Sullivant, the *Lichenes* by Prof. Tuckerman, the *Fungi* by the Rev. Dr. Curtis, preparatory to their distribution in

named sets. Those who desire to secure full sets should make early application to Prof. Gray, who will have them in charge during Mr. Wright's absence. A. G.

2. *Systematic Arrangement of the Species of the Genus Cuscuta, with critical remarks on old species, and descriptions of new ones;* by George Engelmann, M.D. (Extr. from Trans. Acad. Science of St. Louis, vol. i, pp. 453–523; separate issue pp. 73,) 8vo, 1859.—It is well known to botanists that Dr. Engelmann has for many years been making a special study of *Cuscuta*, and that, besides his own and other American collections, those of the principal European herbaria have been rendered to him for examination. His recent visit to Europe enabled him to extend and to revise his study of this genus. The study of the *Cactaceæ* of North America, which so long interrupted the former investigation, having been brought to a conclusion, Dr. Engelmann has at length been able to publish his revision of the whole genus *Cuscuta*. The thirty-eight species more or less known to Choisy when he elaborated this genus for the Prodromus are doubled in the present enumeration, although an equal number, including several of Choisy's, and of his own formerly proposed, are reduced to synonyms, or arranged as varieties. Many species prove to be remarkably polymorphous, and require an array of varieties and subvarieties to express their manifold diversities in systematic form. The work has involved immense labor, and bears the marks throughout of the most patient and conscientious treatment. Sound views prevail in the generalities as well as in the details; the Linnæan genus is preserved intact, but disposed in three primary groups or subgenera,—*Cuscuta* proper, *Grammica*, and *Monogyna*, and these in nine sections. The species of the first group are all indigenous to the Old World; those of the second are mostly American and Eastern Asian; of the third principally Asian, but two species extend into Europe, one is Texan, and one South African. A. G.

3. *On the Distribution of the Forests and Trees of North America, with Notes on its Physical Geography;* by J. G. Cooper, M.D. pp. 40, 8vo. Extr. from the Report of the Smithsonian Institution for 1858.—The tables illustrating the geographical distribution of our trees and larger shrubs bring together and systematize a great amount of valuable information. As a first essay, it appears to be all that could be expected; and the author himself, having taken up the subject with great zeal and good opportunities, will doubtless perfect it in the future reports for which he is collecting materials. Since the catalogue purports to be one of the *trees* of the United States, we should not have included shrubs except those of the largest class, and a few which belong to genera characteristically arboreous, such as *Quercus*, *Acer*, &c. Such depressed shrubs as *Prunus maritima* and *Cerasus pumila* are surely out of place. The incongruity is all the more serious in the catalogue of the trees of the regions of the Rocky Mountains and westward, which regions being "comparatively poor in trees," the standard is there still farther reduced, "since shrubs become more valuable where trees diminish in number." This confusion of economical with scientific considerations has the effect of representing the western part of the continent to be far richer in trees than it really is. Of the 108 species in this list only a moiety would

seem to make good their claim to be called forest trees, or trees at all. And in this connection we venture to suggest that the *minimum* as well as the *maximum* height which the trees attain should be given. The *average* height is the more important to be known; the maximum is rather a matter of curiosity. "The reason for giving the maximum heights [only] is, that it is thought the cultivation of trees will become some day a matter of national interest, and I wish to show what they are under the best natural circumstances, supposing that, with cultivation, they will at least equal this standard." We do not suppose so. Under the *same circumstances*, the Lambert-pines of future ages might indeed aspire to 300 feet in height, and the giant *Sequoia* to 450 feet; but planted trees, with room to spread as they should, are never drawn up as in primæval forests.

The delineation of *natural provinces* and *regions* in North America, according to the distribution of our arborescent vegetation, opens wide questions upon which we must not here enter. We should incline to broader views and fewer subdivisions, as preferable for exhibiting the general facts of the case, and more likely to be stable.

The Floridian region is said to have "about thirty-two characteristic and thirteen peculiar trees," and accordingly would appear to be far more strongly marked than any other of the nine. The erroneous impression which this may give would be removed by an expansion of the statement that "Florida appears rather to belong to the West Indian province." Nearly all these peculiar or characteristic trees belong to the Keys, and are Bahaman or West Indian species. Probably no other part of North America is really so destitute of peculiar species of plants as southern and eastern Florida.

In conclusion, while we heartily thank Dr. Cooper for this interesting and useful essay, and expect still better results from his continued investigations, we simply enter a protest against the anachronism of appending an early author's name to a species under a genus which he never heard of, and against the bad taste of writing the names of persons without a capital initial,—both innovations from which botanical writings have thus far been nearly free. A. G.

ZOOLOGICAL NOTICES.—

1. Prof. J. VICTOR CARUS, of Leipsic, writes to the Smithsonian Institution, July 27, 1859, as follows: "During the last two years, I have been collecting materials towards a general catalogue of zoological literature, from 1750 up to the present day, including not only all the separately published works, books and pamphlets, (most of which are to be found in Engelmann's Bibliotheca Historico-Naturalis,) but also, and especially, all the papers, articles and notices contained in periodicals, the number of which is increasing every year. All the titles and references will be arranged systematically, not according to the alphabet of the authors, but within the classes and groups according to the alphabet of the genera; so that at a glance one will find the whole literature of any particular genus." Prof. Carus calls for aid in obtaining access to American scientific papers, many of which are found in periodicals which are difficult to obtain, as he wishes to make his work as nearly complete as possible. Any who wish to further his laudable object would confer a favor by forward-

ing to him synopses of the contents of the more obscure publications, copies of minor papers, etc.

2. *Die Klassen und Ordnungen des Thier-Reichs, wissenschaftlich dargestellt in Wort und Bild.* Von Dr. H. G. Bronn, Prof. an der Univ. Heidelberg. Leipsic and Heidelberg; C. F. Winter, 1859. 8vo. with plates.—Of this excellent work now in the course of publication, five parts have appeared, containing the Amorphozoa and part of the Actinozoa. The subject is treated in a masterly manner, and the work will undoubtedly be far more complete in its character than any general treatise on Zoology yet published. It commences with an introduction, giving a short general account of the nature of the Animal Kingdom, a history of its investigation by zoologists, and a tabular view of the characteristics of the five subkingdoms, (which are named Amorphozoa, Actinozoa, Malacozoa, and Entomozoa and Spondylozoa,) in regard to their radical form, radical number, embryonic development with reference to the "Primitive-Theil" and position of the yolk, organic system, the head, nervous system, structure of skeleton, teeth, circulatory system, and blood.

In the body of the work each class is treated of under the following heads: I. Introduction, an account of the name and literary history of the class; II. Organic structure; III. Chemical composition; IV. Vitality, and Embryology or Development; V. Classification; VI. Geographical and Topographical Distribution; VII. Geological evolution; and VIII. Signification in the Economy of Nature. In the *Introduction* a full bibliography is given for each class. Under the head of *Classification*, a systematic review or synopsis is given of the sub-classes, orders, suborders, families, sub-families, relationships, (sippschaft) and genera, (sippen); more than the usual number of subdivisions being introduced. In this synopsis, such characters only are given as are necessary to discriminate between known genera, but the work is carefully brought up to the present state of the science in a more thorough manner than has been attempted before in any general work. Some useful abbreviations are introduced which will tend to a convenient shortening of descriptions, although it would have been perhaps better to have formed them from Latin rather than German words. (See p. 44, Vol. II.) The plates are well done, and illustrate not only the structure and development, but a considerable number of the generic forms of each class. The wood-cuts are less remarkable for artistic excellence.

W. S.

3. *Description of Oceania (Turritopsis) nutricula, n. s. and the embryological history of a singular Medusan Larva found in the cavity of its bell;* by Prof. John McCrady, (Proc. Elliott Soc. Nat. Hist. of Charleston, S. C., vol. i, p. 55–90); and *The Gymnophthalmata of Charleston Harbor;* by the same author. (Ibid., i, p. 103–221. Plates 4–12.)—These articles, particularly the latter, form the most valuable contribution to the history of our American *Hydroidea* that has appeared since Agassiz' papers in the Memoirs of the American Academy. Like the latter, they are written in that easy, interesting style which is so much more agreeable to the reader than the dry description of details which constitutes the bulk of biological writings. The author has put forth some new views, and enlarged upon the suggestions of his predecessors, in a manner well worthy of the attention of scientific zoologists.

Prof. McCrady states that there is no essential difference between the so-called alternation of generations and a regular metamorphosis, which he calls a *homogony*, after Gegenbaur. The hydroid larvæ therefore of the Gymnophthalmata should no longer be separately named and described, but their history should be included in that of the species to which they belong, as is done in the case of Insects. For the group the name *Hydroidea* is retained, following the rule of priority, *Cryptocarpæ* of Eschscholtz being rejected on account of its erroneous significance. In subdividing the group the author seizes upon an excellent character, founded upon the mode of growth in the medusa-buds, which has been overlooked by previous investigators. In *Tubularia*, *Coryne*, etc., the outer covering of the bud becomes the disc of the medusa, and the digestive tube is enclosed from the first. In *Campanularia* and its allies, on the contrary, the digestive tube projects at first freely from the bud, and becomes afterward enclosed by the over-arching disc, which grows outward from its base. Upon this ground the Hydroidea are separated into two sub-orders—*Endostomata* and *Exostomata*. This subdivision is confirmed by characters derived from the full-grown Medusæ:—in the first group these are deeply bell-shaped, sometimes even sub-cylindrical, with no sinuses in the radiating tubes, and a long digestive cylinder pendant from the disc; in the second they are generally broad and shallow, or saucer-shaped, with many sinuses in the radiating tubes, and a short digestive cavity imbedded in the disc. In the former group are included the Corynidæ, Velellidæ, Tubularidæ and Siphonophoræ. Recent investigations have shown that the Velellidæ and Siphonophoræ are true Hydroidea and the free medusa (*Chrysomitra*) of Velella has been observed and described by Huxley, Vozt, Kolliker and others. The development of the medusa-buds in these two families is after the manner of the Endostomata. In the latter group, Exostomata, the author includes the Campanularidæ, Sertularidæ and Æginidæ, with several genera the family connections of which are uncertain.

In the description of the Hydroidea of Charleston Harbor, Prof. McCrady has more than doubled the number of species known to exist on our shores. He describes 32 species belonging to 30 genera, 8 of which are new, as follows;—among the Endostomata, *Turritopsis*, *Corynitis*, and *Dipurena*; among Exostomata, *Encheilota*, *Entima*, *Epenthesis*, *Phortis*, and *Persa*. In the genus *Nemopis* the author has failed to discover the ocelli described by Agassiz as existing in the tips of the upturned tentacles, and says that the darker hue of these tips is occasioned by their greater thickness. The *Globiceps tiarella* of Ayres (*Eucoryne elegans*, Leidy) is found in Charleston Harbor, and is placed in the genus *Pennaria* by Prof. McCrady, who describes its medusa. The parasitic medusa found in the bell of *Turritopsis nutricula*, and described in the first paper as the young of that species, is afterward considered to be the young of a *Cunina*. The paper closes with a discussion of the geographical distribution of the American Hydroidea. W. S.

4. *On the zoological affinities of Graptolites;* by Prof. John McCrady. (Proc. Elliott Soc. Nat. Hist., i, p. 229.)—These paradoxical fossils are regarded by Prof. McCrady as similar to the toothed rods of the larvæ of Echinoderms, described by Joh. Muller. The great discrepancy in size

he explains by suggesting that these embryo-like forms were fully developed animals characteristic of the early geological period at which they existed. W. S.

5. *Letters from Alabama, chiefly relating to Natural History;* by Philip Henry Gosse, F.R.S. London, 1859, pp. 306: small 8vo.—Another popular scientific work of this prolific writer. It consists of a series of letters written many years ago, when the author was teaching school in the interior of Alabama. In his usual interesting style, Mr. Gosse describes the habits and points out the beauties of many of our Southern plants, insects, reptiles and birds, with numerous illustrations on wood. W. S.

6. *Sketch of a revision of the genera of Mithracidæ;* by Wm. Stimpson.—The old genus *Mithrax* was divided into three groups by Milne-Edwards, (Hist. Nat. des Crust., i. 318), *Mithrax triangulaires, M. transversaux,* and *M. déprimés.* These groups are now considered of generic value. For the first group De Haan has proposed the name *Dione,* which cannot however be adopted, as it was previously applied to a genus of Lepidoptera, by Hübner. It is also used for a bivalve shell. To the third group White gave the name *Mithraculus,* which was adopted by Dana.

In the Proceedings of the Zoological Society of London, 1847, p. 222, Mr. Adam White gives a wood-cut of a maioid crustacean, to which he applies the name *Schizophrys,* with the following description: "Carapax oval, depressed, somewhat attenuated behind; beak deeply cloven; upper orbit deeply cloven, with a strong tooth in the middle of the cleft; under orbit an elongated appendage on the inside, with two teeth at the end. Tale of male with 7 joints, the sides nearly parallel. Fore-legs shortest. Fingers without teeth." This genus, in the catalogue of the British Museum, is placed between *Othonia* and *Pericera.* But if we consider that by the "elongated appendage on the inside" of the orbit, Mr. White probably means the basal joint of the external antennæ, and observe how closely the figure corresponds to young crabs belonging to De Haan's genus *Dione,* the conclusion is unavoidable that *Schizophrys* is really synonymous with the *Mithrax triangulaires* of Milne-Edwards, and as such we here adopt it. In the following synopsis new characters are introduced; some new genera are described, and a list given of the species known up to the present date.

A. Maxillipedis externi merus margine antico integer.

Mithraculus, White. (*Mithrax,* pro parte, De Haan, Fauna Jap. Cr. 82.) Carapax depressus, rostro brevissimo vel nullo.—*M. sculptus,* (M.-Edw.,) *M. nodosus,* (Bell,) *M. denticulatus,* (Bell,) *M. coronatus,* (Herbst.) *M. cinctimanus,* Stm.

B. Maxillipedis externi merus angulo antero-interno excisus, ad palpum incipiendum.

a. Frons angusta. Orbitæ parvæ, profundæ, oculos pæne operientes.

Mithrax, Leach. Carapax plus minusve transversus. Orbita multifissa, margine dentibus vel tuberculis armata.

1. Antennæ externæ articulus basalis spinis tribus armatus.—*M. verrucosus,* M.-Edw., *M. aculeatus,* (Herbst.)

2. Antennæ externæ articulus basalis spinis duabus armatus.—*M. spinosissimus,* (Lamk.), *M. hispidus,* (Herbst,) *M. ursus,* Bell, *M. rostratus,*

Bell, *M. pygmæus*, Bell, *M. cornutus*, De S., *M. armatus*, De S., *M. tuberculatus*, Stm.

Teleophrys, nov. gen. Carapax antice triangularis, postice et lateraliter rotundatus, sulco cervicali sat profundo. Orbita margine supero externoque integra, nec dentata.—*T. cristulipes*, Stm.

b. Frons lata. Oculi majores. Orbitæ grandes, non profundæ, late fissæ, oculos non operientes.

Schizophrys, White. (*Dione*, De Haan.) Carapax ovato-triangularis. Rostrum longum, bifidum, cornibus bidentatis.—*S. dichotoma*, (Latr.) *S. affinis*, (De Haan,) *S. aspera*, (M.-Edw.), *S. serrata*, White, *S. spinigera*, White, *S. dama*, (Herbst.)

Cyclomaia, nov. gen. Carapax orbiculatus, antrorsum quam retrorsum vix angustior. Rostrum brevissimum, bifidum, cornibus acutis, non dentatis. Oculi grandes, sat breves. Antennæ externæ articulus basalis trispinosus, spinis superne conspicuis. Maxillipidis externi merus apice interno profunde sinuatus. Pedes mediocres.—*C. suborbicularis*, Stm.

Cyclax, Dana. *Cyclomaiæ* carapacem affinis. Oculi longi; pedes longi, tenues.—*C. Perryi*, Dana.

Mithraculus, *Mithrax*, and *Teleophrys* are American; *Schizophrys*, *Cyclomaia* and *Cyclax*, old-world types.

7. *Archiv für Naturgeschichte*, vol. xxiv, for 1858; Berlin, 1859:—contains the following articles of special interest:—Anatomy and development of Copepoda, with 2 plates; *Claus.*—Descriptions of new Chilian Vertebrates by *Dr. Philippi.*—Revision of the Gadidæ, Soleinæ, and Plagusinæ; *Kaup.*—(Mr. Kaup considers our *Morrhua pruinosa* and *M. americana* as one species and identical with *M. vulgaris!*)—Geographical and historical remarks on certain mammals; *Martens.*—On the species of Velutina; *Martens.*—On Annelides of the Brazilian Coast, 2 plates; *F. Müller.*—Enthelminthica, 2 plates; *Wagener.*—Anatomy and Histology of some Trematodes; *Walter.*—New Batrachians, *Günther.*—On the hard-cheeked Acanthopterygians; *Kaup.* (The number of genera very much reduced.)—Critical remarks on Castelnau's Siluroids; *Kner.*

W. S.

8. *The Natural History Review and Quarterly Journal of Science*, a periodical published at London and Dublin, containing reviews of works relating to Natural History, and also the proceedings of the Dublin Scientific Associations, as follows:—the Geological Society, the Natural History Society, the University Zoological and Botanical Association, the Royal Irish Academy, and the Royal Society. In giving a synopsis of the more important zoological papers read before these five Associations, we shall, for the sake of convenience, cite the "Quarterly Journal" instead of their own regular publications:—

Vol. v. 1858, contains—p, 134, On new genera and species of Polyzoa, 4 plates; *W. Thompson.*—p. 148, Cambrian fossils, Histioderma, n. g. (annelide); *Kinahan.*—p. 168, Steropis, a new genus of Carboniferous Crustacea, allied to Limulus; *Baily.*—p. 194, On some Oniscoidea, with a plate, *Kinahan.*—p. 202, New forms of Diastylidæ (wood-cuts); *Bate.*—p. 207, On Oldhamia, a Cambrian fossil; *Kinahan.*—p. 276, Ancient and modern races of Oxen in Ireland, (wood-cuts); *Wilde.*—Vol. vi., 1859, p. 108, On the urticating organs of Actinia; *M'Donnell.*—p. 113, Irish

—p. 191, Crimean Fossils; *Bailey.*—p. 199, New Irish Orthocerata; Actiniadæ; *Wright.*—p. 125, Platyarthrus, Brandt, and allied genera; *Kinahan.*—p. 152, Anatomy of the brain in some small Quadrupeds, 2 plates; *Garner.*—p. 161, Subterranean Gammaridæ; *Bate* and *Hogan. Haughton.*—p. 237, Morphology of the Hydrozoa, with reference to the constitution of the sub-kingdom Cœlenterata; *Greene.* (Mr. Greene includes the Lucernaridæ with Hydrozoa, and considers the Medusidæ—"Animal consisting of a polype suspended from the under surface of a natatorial organ"—an order distinct from the Hydridæ and Tubularidæ!)

9. *An essay on Classification*, by Louis Agassiz.—This work, forming the introduction to Prof. Agassiz' Contributions to the Natural History of the United States, has been published in a separate form, a convenient octavo, by the Longmans and Trübner & Co., London.

10. *On the genus Synapta;* by Woodward and Barrett, (Proc. Zool. Soc. of London, xxvi, 360. Plate xiv).—A short historical and anatomical account of the interesting family of Holothurians in whose skin are found the miniature anchors and wheels, which form such elegant objects for the microscope. They give detailed descriptions of *Synapta digitata* and *S. inhærens*, and add one new species, *S. bidentata*, from China. They also give what purports to be a list of the known species of the family, to which, however, the following should be added:—*Synapta oceanica* Less., *S. doreyana* Quoy and Gaimard, *S. punctulata* Q. & G., *S. bachei* Pourt., *S. tennis* Ayres, *S. pellucida* Ayres, *S. dolabrifera* Stm., *Chirodota fusca* Q. & G., *C. rubeola* Q. & G., *C. tenuis.* Q. & G., *C. rufescens* Brandt, *C. rotifera* Pourt., *C. pallida* Ayres, *C. australiana* Stm., *C.* (*Myriotrochus*) *brevis* Huxley.

W. S.

Proceedings of the Elliott Society of Natural History of Charleston, S. C.—Vol. I. 1856.—p. 50, On Cicadæ; *J. Lee.*—New and rare Phænogamous Plants found in the State of South Carolina; *H. W. Ravenel.*—p. 55, Description of Oceania (Turritopsis) nutricula, nov. sp., and the embryology of a Medusan Larva found in its bell; with four plates; *J. McCrady.*—1857, p. 91, On the past and present condition of Niagara Falls; *L. R. Gibbes.*—p. 101, Notice of an ore of Argentiferous Galena; *Frampton.*—p. 102, On the fruit of Yucca gloriosa; *L. R. Gibbes.*—p. 103, Gymnophthalmata of Charleston Harbor, with five plates, (noticed in this Volume, p. 130); *J. McCrady.*—p. 222, On Specific Form; *J. McCrady.*—p. 223, On a Bolina found in Charleston Harbor; *J. McCrady.*—p. 225, Description of Ranilia muricata, M. Edw., with a plate; *L. R. Gibbes.*—p. 229, On the zoological affinities of Graptolites (noticed in this volume, p. 131); *J. McCrady.*—p. 237, Gigantic Orthoceras from Minnesota; *L. A. Frampton.*—p. 238, On a Cactus from Eding's Bay, S. C.; *L. R. Gibbes.*—p. 239, Medusæ of Port Royal Harbor, S. C.; *J. McCrady.*—p. 241, Botany of Eding's Bay; *L. R. Gibbes.*—p. 251, Preparation of Metallic Cobalt; *W. Sharswood.*—p. 254, Development of two species of Ctenophora found in Charleston Harbor, with a plate; *J. McCrady.*—1858, p. 272, Cacti of S. Carolina; *L. R. Gibbes.*—p. 275, Instance of incomplete longitudinal fission in Actinia cavernosa; *J. McCrady.*—p. 278, A new locality for Rutile; *W. Sharswood.*—p. 280, Three new Univalves; *E. Ravenel.*—p. 282, New genus of fossil Echini, Ravenelia, allied to Pygorynchus; *J. McCrady.*—p. 287, Antidote for Arsenious Acid; *W. Sharswood.*—p. 288, Phenomena of the Earthquake of Dec. 19, 1857; *L. R. Gibbes.*—p. 291, On a convenient form of Aspirator; *L. R. Gibbes.*

Proceedings Boston Soc. Nat. Hist., 1859.—p. 49, On the priority of discovery of the fossil footmarks of the Connecticut Valley; *T. T. Bouvé.*—p. 54, Trilobites from Newfoundland; *C. T. Jackson.*—p. 58, Japanese plant-wax; *W. B. Rogers* and *C. T. Jackson.*—p. 60, On the Infusorial deposit in the Tertiary of Virginia and Maryland; *W. B. Rogers.*—p. 64, Mineral resources of the Rocky Mountain Chain;

W. P. Blake.—p. 74, On the frozen well of Brandon, Vt.; *C. Stodder.*—p. 75, Paradoxides Harlani, *C. T. Jackson.*—Habits of marine animals observed at West Yarmouth, Mass.; *T. Lyman.*—Diatomaceæ from Milwaukee; *A. M. Edwards.*—p. 81, Report on the frozen well at Brandon, Vt.; *Jackson* and *Blake.*—p. 89, On collecting, preparing and mounting Diatomaceæ; *A. M. Edwards.*—p. 102, A list of Birds seen at the Bahamas, from Jan. 20th to May 14th, 1859, with descriptions of new or little known species; *H. Bryant.*

Proceedings Philadelphia Acad. Nat. Sci., 1859.—p. 162, On fossil teeth and bones collected by Prof. Emmons: Ontocetus Emmonsi, nov. sp., founded on the tooth of a Cetacean; *J. Leidy.*—Notice of Humboldt; *I. Lea.*—p. 164, On a specimen of Hydaticus zonatus; *C. A. Helmuth.*—p. 165, Description of new generic types of Cottoids from the Collection of the North Pacific Expedition; *T. Gill.*—p. 167, Description of a new species of Callianidea; *T. Gill.*—p. 168, Entomacrodus, a new genus of Salarianæ; *T. Gill.*—p. 169, Herpetological Notices; *C. Girard.*—p. 170, Twelve new Uniones from Georgia; *I. Lea.*—Catalogue of Birds collected on the Rivers Camma and Ogobai, W. Africa, by Mr. P. B. Duchaillu, with notes and descriptions of new species; *J. Cassin.*—p. 177, On fresh-water shells, *I. Lea.*—Fossils from the Post-Pliocene of S. C., with Dr. Leidy's paper on the fossil Horse, and Prof. Agassiz' letter; *Holmes.*—p. 187, Four new exotic Unionidæ; *I. Lea.*—p. 188, Notes on American land-shells, No. 5; *W. G. Binney.*—p. 189, Catalogue of birds collected in the vicinity of Fort Tejon, Cal., with description of a new Syrnium; *J. Xantus.*—p. 194, Freyia Americana, a new animalcule from Newport Harbor; *J. Leidy.*—p. 195, Evorthodus, a new genus of Gobioids; *T. Gill.*—p. 196, Pimeletropis, a new genus of Siluroids from South America; *T. Gill.*—p. 197, New genera and species of N. American Tipulidæ with short palpi, with an attempt at a new classification of the tribe, (pp. 59 and three plates); *R. von Osten Sacken.*—p. 255, A spider catches a fish; *E. A. Spring.*—p. 256, Contributions to American Lepidopterology; *B. Clemens.*—p. 262, On a deformed fragmentary Human Skull from Jerusalem; *J. A. Meigs.* Supplement—Catalogue of the Invertebrate Fossils of the Cretaceous Formation of the United States; *W. M. Gabb.*

Journal Philadelphia Acad. Nat. Sci., Vol. VI, Pt. II.—Contains the following papers:—Synopsis of the North American Sphingidæ, by B. Clemens, M.D.; and New Unionidæ of the United States, by I. Lea, LL.D.

Canadian Naturalist and Geologist, Oct. 1859.—A new Gasterosteus, G. gymnetes; *Dawson.*—Glacial Phenomena of Canada and the North-eastern United States during the Drift Period; *Ramsey.*—On Ozone; *Smallwood.*—Fossils of the Calciferous Sandrock, etc.; *Billings.*—New Trilobites; *Billings.*—On the Aurora of 28th of August; *Smallwood.*

Annals and Magazine of Natural History, London, Oct. 1859.—Cellulose in starch-grains; *H. von Mohl.*—New spiders from Madeira; *J. Blackwall.*—Nudibranchiate mollusks of Ceylon; *Kelaart.*—New genera and species of phytophagous insects; *Baly.*—Digestive power in the Actiniæ; *Holdsworth.*—New Entomostraca from Jerusalem; two plates; *Baird.*—N. American Fungi; *Berkeley* and *Curtis.*—A new antelope (Kobus), from Central Africa; *Gray.*—Nov. 1859.—Reproduction of Bark-lice, with a plate; *R. Leuckart.*—New Anthribidæ; *Pascoe.*—Nomenclature of the Foraminifera; *Parker* and *Jones.*—Coleoptera of Old Calabar; *Murray.*—On certain genera of plants; *Miers.*—On Hydroid Zoophytes; *Allman.*—Ceylon insects; *F. Walker.*—A new Catharus from Western Mexico; *Sclater.*

Proceedings of the Zoological Society of London.—The volume for 1858 contains the following papers of more or less interest to American zoologists:—p. 38, Genera of Olivæ; *J. E. Gray.*—p. 90, On Stavelia, a new genus of Mytilidæ, and on some Distorted Bivalves, with a plate; *J. E. Gray.*—p. 92, Nerita and its operculum; *J. E. Gray.*—p. 145, Rearrangement of the genera of British Actiniadæ; *W. Thompson.*—On Sponges, by Dr. *Gray*, pages 113, 114, 229, and 531, with plates.—p. 136, Separation of the Salamandridæ into two families, by the form of the skull, *J. E. Gray.*—p. 225, Description of new Pinnæ; *S. Hanley.* (The South Carolinian Pinnæ called by American Conchologists P. seminuda and P. muricata are P. carolinensis, Hanl. and P. squamosissima, Phil.)—p. 339, Systematic Arrangement of the Tail-less Batrachians; *Gunther.*—p. 360, On the genus Synapta; *Woodward & Barrett.*—p. 373, Geographical Distribution of Reptiles; *Gunther.*—p. 413, Monograph of the Cæcidæ; *P. P. Carpenter.*—Also many papers relating to Central American Ornithology, by *P. L. Sclater.*

W. S.

IV. ASTRONOMY AND METEOROLOGY.

1. *Discovery of the 57th planetoid* (*Mnemosyne*).—Another planet, appearing like a star of the 10th magnitude, supposed to be one of the group between Mars and Jupiter, was discovered by *M. Robert Luther*, Sept. 22, 1859, at the Observatory of Bilk. It is the 57th of the group, and has been named *Mnemosyne*.—(*Comptes Rendus*, Oct. 5, 1859.)

2. *Total Solar Eclipse of July* 18, 1860.—M. FAYE has called the attention of astronomers and of all lovers of astronomy to the rare opportunity for important observations presented by this eclipse, which will traverse the earth from California to the Red Sea. The total darkness will travel across North America about the 60th degree of North latitude, leaving it at Hudson's Straits, and leaping the Atlantic, pass across Spain, strike the Balearic Isles, pass through Algeria, and crossing the Nile north of Dongola, take leave in Ethiopia. He names seven stations as specially favorable for observation, viz., 1. In Oregon between the Pacific ocean and the Rocky Mountains. 2. In Labrador, in lat. 59° N. 3 and 4. In Spain on the Atlantic and on the Mediterranean coasts. 5. At Ivica in the Balearic isles. 6. At Kabylia in Algeria. 7. At Dongola on the Nile.

At the time of the eclipse, Venus, Mercury, Jupiter and Saturn, will be in the vicinity of the Sun, and form a sort of rhomboid about it. Such a spectacle will not be visible again for many ages.

The objects to be secured by these observations may be arranged under four heads. 1. The more exact determination of the errors of the lunar tables. 2. The determination of the longitudes of places too remote from each other to be connected by the electric telegraph. 3. The verification of the present data for the solar and lunar parallax and the flattening of the earth. 4. The solution of certain questions respecting the physical constitution of the sun, and of the space in its vicinity.

M. Faye proposes that at the two principal stations photographic methods should be substituted in place of direct observation. A telescope of large object glass and long focus should be used, and a large number of proofs should be taken between the first and last contact, taking care to keep horizontal the collodionized plate. During the total obscuration, the whole object glass should be uncovered, and the most sensitive plates employed in order to obtain proofs on a large scale of the aureola and solar flames, while observers provided with hand telescopes, with fresh eyes, should deliberately study all particulars which photography can not secure.

As to the meteorological phenomena, M. Faye proposes to add the sympiezometer as more quick to show the rapid fluctuations of the atmosphere; and instead of the common thermometer to use a self-registering Breguet's metallic thermometer carried into the air by a captive balloon. The variations of the magnet should also be observed, for if the earth's magnetism is affected by the spots which periodically obscure part of the sun's disk, may it not be affected by the more rapid obscuration of the same by the moon? Possibly the wires of the electric telegraph, arranged now with and now against the direction of the eclipse may show perturbations too fugitive to be detected by bar magnets.

The station at Ivica seems to combine all the advantages offered by the peak of Teneriffe. Here especial attention should be given to the form and prolongations of the aureola, the nature and intensity of its light, and also to the zodiacal light, which is now made to play so important a part in the solar system. Careful search should also be made for the small planets near the sun, suspected by M. LeVerrier. Perhaps, moreover, it may be possible to notice clearly the motion of the cone of the lunar shadow, the lower base of which should traverse the surface of the sea at the rate of 900 metres per second, while the upper terminus, if visible, will show by its distance from the zenith the height of the upper strata of our atmosphere.

3. *Notice of the Meteor of Nov.* 15, 1859; by Prof. E. Loomis.—On the morning of Nov. 15th, about 9½ o'clock, a remarkable meteor was witnessed by a large number of persons in New York and its vicinity. The meteor was so brilliant that although the sun was unclouded and had an elevation of about twenty degrees above the horizon, the flash attracted the attention of well nigh every person who happened at that time to be looking nearly toward that part of the heavens. The apparent diameter of its head was somewhat less than that of the sun, and it had an appendage like the tail of a comet several degrees in length. Its apparent path was nearly vertical, with a slight inclination towards the west; and the length of its visible path was variously estimated from 15° to 25°. The entire period of its visibility did not exceed one or two seconds. No sound was heard at New York which could reasonably be ascribed to the meteor. By taking the mean of the estimates of several observers, I have determined that the point of the horizon where the meteor vanished was about 21° west of south.

From the newspaper reports we learn that the same meteor was seen at Salem, Boston, and New Bedford, Mass., at Providence, R. I., at New Haven, Middletown, and Waterbury, Conn., at Albany and many other places in New York, at numerous places in New Jersey, at Baltimore, Md., at Washington and Georgetown, D. C., as also at Alexandria and Fredericsburg, Va. At all of those places the meteor appears to have been seen at the same instant of absolute time; and at all the stations north of New York the appearance was almost identical, and the direction of the meteor was somewhat west of south.

From a newspaper notice coming from Prof. Henry of the Smithsonian Institution, we learn that at Washington the apparent path of the meteor was nearly perpendicular to the horizon, and its point of disappearance was estimated to be four degrees north of east. Those lines of direction as observed at New York and Washington intersect at a point a little north of Cape May; and inasmuch as at each of those stations the apparent path was nearly vertical, the actual path must also have been nearly vertical, and the meteor undoubtedly struck the earth at some point not very remote from Cape May.

This conclusion is confirmed by the reports of the meteor from New Jersey. The meteor was generally observed throughout the southern part of that State, and was everywhere succeeded by a very remarkable explosion. At Beeseley's Point, situated on the Atlantic Ocean near lat. 39° 20′, the course of the meteor is said to have been from northeast to southwest. It was attended by a sudden flash of light, and left behind a curling track of a smoky or light cloudy appearance, which soon vanished. About a minute after the flash, there was heard a series of terrific explosions, which were compared to the discharge of a thousand cannon. These explosions continued for one or two minutes; they were very sharp and distinct, and shook the windows and doors of the houses. Similar noises have been reported from numerous stations in the southeastern part of New Jersey. These noises occasioned considerable alarm, and by some were thought to have been produced by an earthquake.

From the preceding facts it seems almost certain that the meteor must have struck the earth at some point a little north of Cape May; and as it was unquestionably a body of considerable size and of great density, if it struck on dry ground the meteor ought to have been discovered. As we have received no account of such a discovery there is reason to apprehend that the meteor may have descended into water, and probably into Delaware Bay. Analogy would lead us to conclude that this belonged to the class of iron meteors of which we have numerous specimens in our cabinets.

The velocity of this meteor was very extraordinary. It probably struck the earth at a distance of 110 miles from Washington, and is said to have been first seen at an elevation of 45°. This would make the length of its visible path 110 miles, and it is said to have described this path in two seconds, giving a velocity of 55 miles per second. A small portion of this velocity (7 miles per second) may be ascribed to the earth's attraction, and another portion was due to the motion of the earth in its orbit, for the earth was moving obliquely towards the meteor; but there still remains an independent velocity nearly double the velocity of the earth in its orbit. The path of the meteor in space could not therefore have been a circle with the sun for its center, as the above velocity is too great for any ellipse or even parabola; but such conclusions must be received with caution on account of the imperfection of the observations, for if we suppose the time of describing this path was three seconds, the independent velocity of the meteor would not have been much greater than that of the earth in its orbit.

4. *Meteoric Explosion, in West Tennessee, Sept. 1st,* 1859; by Prof. B. W. McDonnold, of Bethel College.—The first of September was made memorable by the great Aurora. Here, that day of the calendar had another *creta nota*—a meteoric explosion. This explosion was heard at Bethel College about 10 o'clock, A. M., and was at first thought to be the firing of cannon in honor of a political election.

The first report was double, like the almost simultaneous explosion of two great rockets, The reverberations were protracted, deep, distant. After the lapse of perhaps a half minute another explosion was heard, louder, deeper than the former, and the reverberations more protracted. The bearing of the sound was N. E.

I find that the report was heard forty miles north of us, where it was supposed to bear South East; it was heard twelve miles south and west, bearing same as here; but farther south and west it was not heard at all.

As yet, I have heard of no fragments of the meteor being found. I feel satisfied myself, however, of the meteoric origin of the explosion.

5. *Catalogue of the Meteorites in the Imperial Austrian Collection at Vienna;* by Prof. W. Haidinger.—Haidinger has communicated to the Austrian Academy of Sciences a complete list of the meteorites contained in the Imperial Collection at Vienna. It is an abstract from the complete catalogue made by the late Prof. Partsch and continued by Dr. Hoernes, the present director of the Imperial Cabinet.

In the list—which here follows—only the names of the localities and the time of falling (I) are given, or (II) in case of meteorites the time of falling of which is unknown they are classified according to the date when first described.

The letter *I* following the year indicates the specimens to be meteoric iron.

I. *Meteorites, with time of fall.*

	A. D.		
1.	1492,	Nov. 7,	*Ensisheim*, Alsace, Département du Haut-Rhin, France.
2.	1715,	April 11,	*Garz* (Schellin), near Stargard, Prussia.
3.	1751, *I.*	May 26,	*Agram* (Hraschina village), Croatia.
4.	1753,	July 3,	*Tabar* (Plan, Strkow), Bohemia.
5.	1753,	Sept. 7,	*Liponas*, near Pont de Verle and Bourg en Bresse, Dép. del'Ain, France.
6.	1768,	Sept. 13,	*Lucé* en Maine, Dép. de la Sarthe, France.
7.	1768,	Nov. 20,	*Mauerkirchen*, Inn, Lower Austria.
8.	1773,	Nov. 17,	*Sigena* (Sena village), Aragon, Spain.
9.	1785,	Feb. 19,	*Eichstaedt* (Wittens), Franconia, Bavaria.
10.	1787,	Oct. 13,	*Charkow* (Bobrik), Government Charkow, Russia.
11.	1790,	July 24,	*Barbotan* (Roquefort, Créon, Juillac, Mezin, Agen, &c.), Dép. des Landes, Dép. du Gers, Dép. du Lot et Garonne, formerly Gascony, France.
12.	1794,	June 16,	*Sienna*, Tuscany.
13.	1795,	Dec. 13,	*Wold Cottage*, Yorkshire, England.
14.	1798,	March 8–12,	*Salès*, near Villefranche, Dép. du Rhône, France.
15.	1798,	Dec. 13,	*Benares* (Krakhut village), Bengal, E. Indies.
16.	1803,	April 26,	*L'Aigle*, Normandy, Dép. de l'Orne, France.
17.	1803,	Oct. 8,	*Apt* (Saurette), Dép. de Vaucluse, France.
18.	1803.	Dec. 13,	*Maessing* (Dorf St. Nikolas), Eggenfeld, Bavaria.
19.	1804,	April 5,	*Glasgow* (High Possil), Scotland.
20.	1805,	March 25,	*Doroninsk*, Government Irkutsk, Siberia.
21.	1805,	June,	*Constantinople*, Turkey.
22.	1805,	Nov.,	*Asco*, Corsica.
23.	1806,	March 15,	*Alais*, St. Étienne de Solm and Valence, Dép. du Gard, France.
24.	1807,	March 13,	*Timochin*, (Timschino, according to Eichwald), Iuchnow, Gov. Smolensk, Russia.
25.	1807,	Dec. 14,	*Weston*, Connecticut.
26.	1808,	April 19,	*Parma* (Casignano, Borgo S. Donino).

27. 1808,	May 22,	*Stannern*, Iglau, Moravia.
28. 1808,	Sept. 3,	*Lissa*, Bunzlau, Bohemia.
29. 1809,	?	*Kikina*, Wiasemsk, Gov. Smolensk, Russia.
30. 1810,	Aug.	*Tipperary* (Mooresfort), Ireland.
31. 1810,	Nov. 22,	*Charsonville*, near Orleans, Dép. du Loiret, France.
32. 1811,	March 12,	*Kuleschowka*, Gov. Poltawa, Russia.
33. 1811,	July 8,	*Berlanguillas*, near Burgos, Spain.
34. 1812,	April 10,	*Toulouse*, Dép. de la Haute-Garonne, France.
35. 1812,	April 15,	*Erxleben*, between Magdeburg and Helmstaedt, Prussia.
36. 1812,	Aug. 5,	*Chantonnay*, between Nantes and La Rochelle, Dép. de la Vendée, France.
37. 1813,	Sept. 10,	*Limerick* (Adair, Scagh, Brasky, Faha), Limerick County, Ireland.
38. 1813,	Dec. 13,	*Lontalax* (Lontalaks), Gov. Wiborg, Finland.
39. 1814,	Feb. 15,	*Bachmut*, Gov. Iekaterinoslaw, Russia.
40. 1814,	Sept. 5,	*Agen*, Dép. du Lot et Garonne, France.
41. 1815,	Oct. 3,	*Chassigny*, near Langres, Dép. de Haute-Marne, France.
42. 1818,	April 10,	*Zaborzika* (Saboryzy, Saboritz on the Slutsch), Volhynia, Russia.
43. 1818,	June,	*Seres*, Macedonia, Turkey.
44. 1818,	Aug. 10,	*Slobodka*, Iuchnow, Gov. Smolensk, Russia.
45. 1819,	June 13,	*Jonzac* (Barbézieux), Dép. de la Charente, France.
46. 1819,	Oct. 13,	*Politz*, near Gera, Duchy of Reuss.
47. 1820,	July 12,	*Lixna* (Liksen), Lasdany, Gov. Witebsk, Russia.
48. 1821,	June 15,	*Juvenas*, near Libonnez, Dép. de l'Ardèche, France.
49. 1822,	Dec. 13,	*Épinal* (la Baffe), Dép. des Vosges, France.
50. 1823,	Aug. 7,	*Nobleborough*, Maine.
51. 1824,	Jan. 15,	*Renazzo*, in Ferrara, Papal States.
52. 1824,	Oct. 14,	*Zebrak* (Praskoles), near Horzowitz, Beraun, Bohemia.
53. 1825,	?	Government *Iekaterinoslaw*, Russia.
54. 1825,	Feb. 10,	*Nanjemoy*, Maryland.
55. 1825,	Sept. 14,	*Honolulu*, Sandwich Islands.
56. 1827,	May 9,	*Nashville* (Drake Creek), Tennessee.
57. 1827,	Oct. 5,	*Bialystok* (Kuasta or Kuasti village), Russian Poland.
58. 1828,	June 4,	*Richmond*, Virginia.
59. 1829,	May 8,	*Forsyth*, Monroe County, Georgia.
60. 1829,	Sept. 9,	*Krasnoi-Ugol*, Gov. Riesan, Russia.
61. 1831,	July 18,	*Vouillé*, near Poitiers, Dép. de la Vienne, France.
62. 1831,	Sept. 9,	*Wessely* (Dorf Znorow), Moravia.
63. 1833,	Nov. 25,	*Blansko*, Bruenn, Moravia.
64. 1833,	Dec. 27,	*Okniny* (Okaninah), Kremenetz District, Gov. Volhynien, Russia.

65. 1835, Nov. 13, *Simonod* (Samonot), Belmont, Dép. de l'Ain, France.
66. 1836, Nov. 11, *Macao*, Prov. Rio Grande de Norte, Brazil.
67. 1837, July 24, *Gross-Divina*, near Budetin, Hungary.
68. 1837, Aug. *Esnaude*, Dép. de la Charente, France.
69. 1838, June 6, *Chandakapoor*, Berar, E. Indies.
70. 1838, Oct. 13, *Capeland* (Bokkeveld, 15 miles from Tulpagh), South Africa.
71. 1839, Feb. 13, *Little Piney*, west of Potosi, Missouri, lat. 37° 55′ N., long. 92° 5′ W. from Greenwich.
72. 1840, July 17, *Cereseto*, near Offiglia, Casale, Piedmont.
73. 1841, March 22, *Grueneberg* (Heinrichsau), Prussian Silesia.
74. 1841, June 12, *Château-Renard*, S. E. of Montargis, Dép. du Loiret, France.
75. 1842, April 26, *Milena* (Milyan), Pusinsko Selo, 4½ miles S. of Milena, Croatia.
76. 1842, June 4, *Aumières*, Canton St. George, Dép. de la Lozère, France.
77. 1843, March, *Bishopville*, South Carolina.
78. 1843, June 2, *Utrecht*, Blaauw Kapel, Loewenhutye, Netherlands.
79. 1843, Sept. 16, *Klein-Wenden*, near Nordhausen, Prussia.
80. 1846, May 8, *Macerata*, Monte Milan village, Ancona, Papal States.
81. 1847, Feb. 25, *Iowa*, Linn County, Iowa.
82. 1847, *I.* July 14, *Braunau* (Hauptmannsdorf), Koeniggraetz, Bohemia.
83. 1849, Oct. 31, *Cabarras County*, North Carolina.
84. 1851, April 17, *Guetersloh*, Westphalia.
85. 1852, Sept. 4, *Mezö-Madaras* (and Fekete), Transylvania.
86. 1852, Oct. 13, *Borkut*, Marmaros, Hungary.
87. 1853, Feb. 10, *Girgenti*, Sicily.
88. 1855, May 13, *Bremervoerde*, Landdrostei Stade, Hanover.
89. 1857, Oct. 10, *Ohaba*, E. of Karlsburg, Transylvania.
90. 1857, Apr. 15, *Kaba*, S. W. of Debreczin, Nordbihar, Hungary.
91. 1858, May 19, *Kakova*, N. W. of Oravitza, Temesvar Banat.

II. *Meteorites, with time of discovery.*

92. 1751, *I.* *Steinbach*, between Eibenstock and Johann-Georgenstadt, Saxony (sometimes given as coming from Norway, Tabor, Senegal, &c.).
93. 1763, *I.* *Senegal*, Siratik in Bambuk, Africa.
94. 1776, *I.* *Krasnojarsk*, Gov. Ieniseisk, Siberia.
95. 1784, *I.* *Toluca*, Mexico.
96. 1788, *I.* *Tucuman* (Otumpa), Argentine Republic, S. America.
97. 1792, *I.* *Zacatecas*, Mexico.
98. 1801, *I.* *Cape of Good Hope*, Africa.
99. 1811, *I.* *Elbogen*, Bohemia.

100.	1811, *I.*	*Durango*, Mexico.
101.	1814, *I.*	*Bitburg*, Lower Rhine, Prussia.
102.	1814, *I.*	*Texas* (Red River).
103.	1815, *I.*	*Lénarto*, Scharosch, Hungary.
104.	1816, *I.*	*Bahia* (Bemdego), Brazil.
105.	1819, *I.*	*Baffins Bay*, Greenland.
106.	1822, *I.*	*Brahin*, Gov. Minsk, Russia.
107.	1823, *I.*	*Rasgata*, New Granada, S. America.
108.	1827, *I.*	*Atacama*, Bolivia.
109.	1828, *I.*	*Caille* (Grasse), Dép. du Var, France.
110.	1829, *I.*	*Bohumilitz*, Prachin, Bohemia.
111.	1830, *I.*	*Guilford*, North Carolina.
112.	1838, *I.*	*Claiborne*, Alabama.
113.	1839, *I.*	*Asherville*, Buncombe Co., North Carolina.
114.	1840, *I.*	*Smith County*, Coney Fork, Tennessee.
115.	1840, *I.*	*Cocke County*, Cosby-Creek, (also called Sevier iron), Tennessee.
116.	1841, *I.*	*Petropaulowsk*, Gov. Tomsk, Siberia.
117.	1843, *I.*	*Oaxaca*, Mexico.
118.	1844, *I.*	*Burlington*, Otsego Co., New York.
119.	1844, *I.*	*Arva* (Szlanicza), Hungary.
120.	1845, *I.*	*Lockport*, New York.
121.	1845, *I.*	*Green County* (Babbs-Mills), Greenville, Tennessee.
122.	1845, *I.*	*Government Simbirsk*, Russia.
123.	1845, *I.*	*Government Kursk*, Russia.
124.	1845, *I.*	*Government Poltawa* (according to Eichwald in the district of Kamensk), Russia.
125.	1847, *I.*	*Seeläsgen*, Neumark, Brandenburg, Prussia.
126.	1849, *I.*	*Chesterville*, South Carolina.
127.	1850, *I.*	*Schwetz*, Province of Prussia.
128.	1850, *I.*	*Ruff's Mountain*, Newberry, South Carolina.
129.	1850, *I.*	*Salt River*, Kentucky.
130.	1851, *I.*	*Seneca Falls*, Cayuga Co., New York.
131.	1852,	*Mayence*, Duchy of Hesse.
132.	1853, *I.*	*Union County*, Georgia.
133.	1853, *I.*	*Lion River*, Namaqua Land, South Africa.
134.	1854, *I.*	*Tazewell*, Claiborne Co., Tennessee.
135.	1854, *I.*	*Putnam County*, Georgia.
136.	1854, *I.*	*Canada*, Madoc, Canada West.
137.	1856,	*Hainholz*, S. W. of Paderborn, Minden, Westphalia.

V. MISCELLANEOUS SCIENTIFIC INTELLIGENCE.

1. *Inquiries into the Phenomena of Respiration;* by EDWARD SMITH, M.D., (L. E. and D. Phil. Mag., xvii, 439).—The author gives in this communication the result of numerous inquiries into the quantity of carbonic acid expired, and of air inspired, with the rate of pulsation and respiration,—1st, in the whole of the twenty-four hours, with and without exertion and food; 2nd, the variations from day to day, and from season to season; and 3rd, the influence of some kinds of exertion.

After a description of the apparatus employed by previous observers, he describes his own apparatus and method. This consists of a spirometer to measure the air inspired, capable of registering any number of cubic inches; and an analytical apparatus to abstract the carbonic acid and vapor from the expired air. The former is a small dry gas-meter, of improved manufacture, and the latter consists of—1st, a desiccator of sulphuric acid to absorb the vapor; 2nd, a gutta-percha box, with chambers and cells, containing caustic potash, and offering a superfices of 700 inches, over which the expired air is passed, and by which the carbonic acid is abstracted; and 3rd, a second desiccator to retain the vapor which the expired air had carried off from the potash box. A small mask is worn, so as to prevent any air entering the lungs without first passing through the spirometer, and the increase in the weight of this with the connecting tube and the first desiccator gives the amount of vapor exhaled, whilst the addition to the weight of the potash box and the second dessiccator gives the weight of the carbonic acid expired. The balances employed weigh to the $\frac{1}{100}$ of a grain, with 7 lbs. in the pan. By this apparatus the whole of the carbonic acid was abstracted during the act of expiration, and the experiment could be repeated every few minutes, or continued for any number of hours, and be made whilst sleeping and with certain kinds of exertion.

The amount of carbonic acid expired in the twenty-four hours was determined by several sets of experiments. Four of these, consisting of eight experiments, were made upon four gentlemen, on the author, Professor Frankland, F.R.S., Dr. Murie, and Mr. Moul, during the eighteen hours of the working day. In two of them, the whole of the carbonic acid was collected, and in two others the experiment was made during ten minutes at the commencement of each hour, and of each hour after the meals. The quantity of carbonic acid varied from an average of 24·274 oz. in the author to 16·43 oz. in Professor Frankland. The quantity evolved in light sleep was 4·88 and 4·99 grains per minute, and scarcely awake 5·7, 5·94, and 6·1 grains at different times of the night. The author estimates the amount in profound sleep at 4·5 grains per minute; and the whole evolved in the six hours of the night at 1950 grains. Hence the total quantity of carbon evolved in the twenty-four hours, at rest, was, in the author, 7·144 oz. The effect of walking at various speeds is then given, with an estimate of the amount of exertion made by different classes of the community, and of the carbon which would be evolved with that exertion.

The author then states the quantity of air inspired in the working day, which varied from 583 cub. in. per minute in himself to 365 cub. in. per minute in Professor Frankland; the rate of respiration, which varied in different seasons as well as in different persons; the depth of inspiration, from 30 cub. in. to 39·5 cub. in.; and the rate of pulsation. The respirations were to the pulsations as 1 to 4·63 in the youngest, and as 1 to 5·72 in the oldest. One-half of the product of the respirations into the pulsations gave nearly the number of cubic inches of air inspired in some of the persons, and the proportion of the carbonic acid to the air inspired varied from as 1 gr. to 54·7 cub. in. to as 1 gr. to 58 cub. in. The variations in the carbonic acid evolved in the working day gave an average maximum of 10·43, and minimum of 6·74 grains per minute. The quantity increased after a meal and decreased from each meal, so that the minima were nearly the same, and the maxima were the greatest after breakfast and tea.

The effect of a fast of forty hours, with only a breakfast meal, was to reduce the amount of carbonic acid to 75 per cent. of that which was found with food; to render the quantity nearly uniform throughout the day, with a little increase at the hours when food had usually been taken, and to cause the secretions to become alkaline.*

The variations from day to day were shown to be connected with the relation of waste and supply on the previous day and night, so that with good health, good night's rest, and sufficient food, the amount of respiration was considerable on the following morning, whilst the reverse occurred with the contrary conditions. Hence the quantities were usually large on the Monday. Temperature was an ever-acting cause of variation and caused a dimunition in the carbonic acid as the temperature rose.

The effect of season was to cause a diminution of all the respiratory phenomena as the hot season advanced. The maximum state was in spring, and the minimum at the end of summer, with periods of decrease in June and of increase in October. The diminution in the author was 30 per cent. in the quantity of air, 32 per cent. in the rate of respiration, and 17 per cent. in the carbonic acid. The influence of temperature was considered in relation to season, and it was shown that whilst sudden changes of temperature cause immediate variation in the quantity of carbonic acid, a medium degree of temperature, as of 60°, is accompanied by all the variations in the quantity of carbonic acid, and that there is no relation between any given temperature and quantity of carbonic acid at different seasons. Whatever was the degree of temperature, the quantity of carbonic acid, and all other phenomena of respiration, fell from the beginning of June to the beginning of September. The author then described the influence of atmospheric pressure, and stated that neither temperature nor atmospheric pressure accounts for the seasonal changes.

The kinds of exertion which had been investigated were walking and the treadwheel. Walking at two miles per hour induced an exhalation of 18·1 grs. of carbonic acid per minute, and at three miles per hour of 25·83 grs.; whilst the effect of the treadwheel at Coldbath Fields Prison was to increase the quantity to 48 grs. per minute. All these quantities vary with the season, and hence the author recommends the adoption of relative quantities, the comparison being with the state of the system at rest, and apart from the influence of food.

2. *Dr. Newberry's Explorations in New Mexico, Utah and Texas*, during the past season, are rewarded by many new and important discoveries, especially in structual geology and palæontology. His collection of fossils is very large, offering conclusive evidence of the geological structure of a very large area. Of the cretaceous deposits he was fortunate in obtaining a particularly satisfactory analysis. Contrary to all our previous notions, these beds turn out to be much more largely developed—that is, existing in much greater force, stratigraphically, West of

*** The quantity of air was reduced 30 per cent, that of vapor in the expired air 50 per cent, the rate of respiration was reduced 7 per cent, and of pulsation 6 per cent.**

the Rocky Mountains, than East of them. In Southern Utah, (just where Marcou claims there are no cretaceous rocks) he found beautiful exposures of 4000 feet thickness of strata of that age, with abundant fossils, both animal and vegetable. The bones of a huge Saurian are among Dr. Newberry's novelties.

We hope in our next Number to be able to give a more exact statement of Dr. Newberry's important discoveries.

Rumors reach us of other and even more startling geological discoveries in the extreme West and North, which we are not at liberty to name at present, but which ere long will be announced, from the proper authority.

3. *Discovery of Devonian rocks and fossils in Wisconsin.* (Private communication to the editors.)—At a late meeting of the Milwaukee Geological Club or Association, Mr. I. A. Lapham announced the discovery of rocks near Milwaukee, equivalent to the Devonian (Old Red Sandstone,) containing remains, which he exhibited, of characteristic fishes. These remains consist of fragments of bone, teeth, a paddle with portions of the tuberculated skin or osseous covering. The bed containing these remains overlies the Niagara group, and is the uppermost of the geological series yet observed in Wisconsin.

4. *Cretaceous Strata at Gay Head, Mass.*—Wm. Stimpson, Esq., accompanied by Messrs. Slack and Ordway, during an excursion in August to Martha's Vineyard, obtained at Gay Head many new fossils in addition to those mentioned by Hitchcock, an examination of which appears to authorize the conclusion that these well known beds are Cretaceous rather than Eocene. Among the fossils obtained are cretaceous bones, vertebræ and teeth of shark, (fragments of some teeth indicating a length of seven inches!) some brachyurous crustacea in a good state of preservation, twelve species of bivalve mollusca, and one univalve; also leaves, fragments and seeds of dicotyledonous plants, &c.

5. *The New Museum of Comparative Zoology, at Cambridge,* is making rapid progress. One wing is nearly ready to receive collections. During his summer trip in Europe, Agassiz made large and important acquisitions for the Museum, in addition to the vast stores already awaiting an occasion for display. Besides a superb suit of fossil Crustacea, Agassiz was so fortunate as to purchase at Heidleburg the collection of fossils from which Bronn's *Lethaea geognostica* was composed. This collection contains the original specimens of the first and most important writers on Palæontology.

Another important addition to the new museum has been made by a sea Captain, who has just brought from Penang and Singapore some three thousand specimens of fish, crustacea, and a most beautiful and choice collection of zoophytes.

6. William P. Blake, Esq., the geologist, has assumed the editorship of the Mining Magazine, a monthly heretofore published in New York. Under his direction this Journal will undoubtedly become a reliable exponent of the important interests it represents.

7. Prof. Wm. S. Chauvenet, lately of the U. S. Naval Academy, at Annapolis, has accepted the Chair of Mathematics in the University of Missouri, at St. Louis.

8. Professor DANA was, by our last dates (Nov. 26th) at Florence, on his way to Rome—designing to divide the winter and spring between Rome, Naples and Sicily, if the state of the country permits his visiting that Island. His health was improving.

NEW BOOKS.—

1. *Archaia; or Studies of the Cosmogony and Natural History of the Hebrew Scriptures;* by J. W. DAWSON, LL.D., F.G.S., author of "*Acadian Geology,*" Principal of McGill College. Montreal: B. Dawson & Son. London: Sampson, Low, Son & Co. 1860. 12mo, pp. 400.—The author of this interesting volume brings to his task the union of a varied scientific, literary and biblical acquirement with a hearty Christian faith. Like all devout and earnest men of science he has an unwavering confidence in the divine unity of truth, and does not for a moment doubt that the genesis of the rocks will confirm the genesis of Moses. He seizes boldly and with candor the real difficulties which every writer has found standing in his way when treating this interesting problem. After an eloquent introduction and a discussion of the object, character and authority of the Hebrew Cosmogony, of the general views of nature contained in the Hebrew Scriptures, &c., he thus sums up his chapter on the Days of Creation—the events of the first day. "At the beginning of the period, the earth, covered with a universal ocean and misty atmospheric mantle, was involved in perfect darkness. A luminous ether was called into existence, which spread a diffused light throughout the whole solar system. This luminous matter being gradually concentrated toward the centre of the system at length produced, in connection with the earth's rotation, the alternation of day and night. These changes were the work of a long period—an æon or day of the Creator."

Undoubtedly the most difficult points in the whole Mosaic Cosmogony, to explain in a rational manner consistent with the views of science, are the creation of plants before the appearance of the '*luminaries,*' and the separation of the two organic kingdoms by the introduction of this middle term. These difficult points are treated with much acuteness and learning, and with a full recognition of the various opinions put forth to meet them by various authors. If we cannot fully agree with our Author in his conclusions, we can truly say that no one has higher claims to a respectful hearing, and if the conclusions at which he arrives leave yet something to be desired, the want rests more in the imperfection of our knowledge than in Prof. Dawson's enunciation of it.

Prof. Dawson does not shrink from a fearless review of the much vexed question of the unity of the human race in a long and interesting chapter on the "Unity and Antiquity of Man." It is hardly necessary to add that he adopts the Mosaic view. We can do this volume little justice, in these few lines to which our last pages restrict us, but we can earnestly commend its spirit, and hope for its wide perusal by all who follow the course of the deep questions it involves.

2. *On the Origin of Species by means of Natural Selection: or, the Preservation of Favored Races in the Struggle for Life;* by CHARLES DARWIN. (Murray.)—[Waiting the arrival of our copy of this new volume from Mr. Darwin's pen, which at this present writing (Dec. 16) has not reached the United States, we copy some passages from a rather

timid and superficial notice of the work in the London Athenæum of Nov. 19th. It is no doubt destined to produce a great discussion on what may properly be called the most fundamental truth of natural history.—Eds.]

"Naturalists of the highest eminence are thoroughly satisfied that each species of animal—all that flies, and walks, and creeps, and wades—has been independently created; and the majority of naturalists have agreed with Linnæus in supposing that all the individuals propagated from one stock have certain distinguishing characters in common, which will never vary, and which have remained the same since the creation of each species. Mr. Darwin, on the contrary, believes that 'the innumerable species, genera, and families of organic beings with which this world is peopled, have all descended, each within its own class or group, from common parents, and have all been *modified in the course of descent.*' To his mind, 'it accords better with what we know of the laws impressed on matter by the Creator that the production and extinction of the past and present inhabitants of the world should have been due to secondary causes, like those determining the birth and death of the individual.' When he views 'all beings not as special creations, but as the lineal descendents of some few beings which lived long before the first bed of the Silurian system was deposited, they seem to him to become ennobled.' We confess some doubt and some uneasiness here. 'Judging from the past, we may safely infer that not one living species will transmit its unaltered likeness to a distant futurity. And of the species now living very few will transmit progeny of any kind to a far distant futurity; for the manner in which all organic beings are grouped shows that the greater number of species of each genus, and all the species of many genera, have left no descendants, but have become utterly extinct. We can so far take a prophetic glance into futurity as to foretell that it will be the common and widely-spread species, belonging to the larger and dominant groups, which will ultimately prevail and procreate new and dominant species.' We cannot say that this is easy doctrine.

"To support these bold views the volume is devoted. The world of animals is contemplated as engaged in one vast unceasing struggle for existence. All organic beings are exposed to severe competition. The face of Nature, it is true, is bright with gladness, and her garner-houses are stored with an abundance of food. Birds sing, insects hum, beasts prowl about in ease and take no thought for the morrow: but the morrow measured by seasons and years has not always a superabundance of food for them. The struggle for existence does not merely relate to self, but includes success in leaving healthy progeny. The high rate at which all organic beings tend to multiply approaches to the rapidity of geometrical increase. More individuals are produced than can by any possibility be supported. There must, then, in every case, be a severe struggle, either of one individual with another of the same species, or with individuals of distinct species, or with the physical conditions of life."

"Now, how does the struggle for existence operate with respect to Variation? Man can produce varieties in animals by the practice of selection. What he has already done by this means the menagerie, the poultry-yard, the field, and the garden display. Is there anything analogous to this in the course of Nature? The author contends that there is, and he names it *Natural Selection.* This principle, whatever others may think of it, and whether they admit its operations or not, in Mr. Darwin's book plays the prominent part. It may be plainly defined, and appears to be briefly this. Under domestication it may be truly said that the whole animal organization becomes in some degree plastic. As variations useful to man have undoubtedly occurred, is it not to be expected that other variations, useful in some way to each being in the great and complex battle of life, should sometimes occur in the course of thousands of generations? If such do occur, then, remembering the struggle for existence, individuals possessing any advantage over others would have the best chance of surviving and of procreating their kind, while injurious variations would be rigidly destroyed. Such a continual preservation of favorable, and rejection of injurious variations, is the principle of Natural Selection. It is illustrated, amplified and confirmed by abundant examples through many pages."

"Certainly there is something poetical in the conception of a succession of created beings, daily and hourly making the wisest election amidst all variations and divergencies; carefully rejecting what is bad, and preserving and accumulating all that is good; operating silently and insensibly, whenever and wherever opportunity offers, towards the improvement of every organized existence in relation to its organic and inorganic condition of life. There is, too, a certain simplicity in the theory of descent with modification through natural selection from a few vastly remote progenitors. 'I believe,' says Mr. Darwin, 'that animals have descended from at most only four or five progenitors, and plants from an equal or lesser number. Analogy would lead us one step further—namely, to the belief that all animals and plants have descended from some one prototype.' A cabbage may have been the parent plant, a fish the parent animal.

"A man of imaginative power might most attractively depict the grand yet simple and direct issues of such a theory. Here are a vast variety of forms of life, most wonderfully co-adapted, most closely connected, most richly adorned, yet they are all 'the lineal descendants of those which lived before the Silurian epoch; and one may feel certain that the ordinary succession by generation has never once been broken, and that no cataclysm has desolated the whole world. Hence, we may look with some confidence to a secure future of equally inappreciable length. And as Natural Selection works solely by and for the good of each being, all corporeal and mental endowments will tend to progress toward perfection.' Yes, an unbroken, sure, though slow, living progress towards animal perfectibility is a delightful vision; natural and gradual optimism is a welcome fancy. What need of distinct creation? If a monkey has become a man—what may not a man become?

"Let the past history of organic life speak. From the thirteen miles in thickness of British strata (exclusive of igneous rocks) comes there no testimony? Palæontology is summoned into court, and is closely interrogated by Mr. Darwin. This proves but a hesitating and reluctant witness; yet counsel for the new theory detects and exposes its imperfections where its testimony is not favorable. We might fairly expect to find in the fossiliferous rocks not a few proofs of the former existence of the numerous intermediate links between distinct specific forms if the proposed theory be true. We do not find them, many will allege, because they never existed. Not so, says our theorist,—but because they were never preserved. Palæontology, however, has not yet revealed any such finely graduated organic scale, and it is not logical to assume that it ever will. When a record is flatly against you, it is quite allowable for you to display its imperfection, but, that being proved, you have only established a negative, and have acquired no confirmation. Grant imperfection, enormous lapse of time, poverty of palæontological collections, and comparative restriction of research, and other such postulates, and then the theory stands just as it stood before, uncorroborated by geology.

"There is positively hostile testimony from the rocks to be confronted. Whole groups of species suddenly and abruptly appear in certain formations, and seem at once to contradict any theory of transmutation of species. Either that fact or the theory must be overturned. Of course, Mr. Darwin accepts the former alternative, and strives to show how liable we are to error in supposing that whole groups of species have been suddenly produced. But another and an allied objection may be started, derived from the manner in which numbers of species of the same group suddenly appear in the lowest known fossiliferous rocks. To meet this and uphold the new theory, it must be sustained by another, viz.,—that before the lowest Silurian stratum was deposited, immensely protracted periods elapsed, at least as long as any subsequent periods, and that during these vast extensions of time the world swarmed with living creatures. Several of the most eminent geologists, including Murchison, will refuse to admit this presumption. Mr. Darwin's geology is more singular than we had thought. 'For instance,' says he, 'I cannot doubt that all Silurian trilobites have descended from some one crustacean which must have lived long before the Silurian age, and which probably differed greatly from any known animal.' Extend and multiply such assumptions, and the theories may take any form you please."

"After all, this book is but an abstract. The larger work is nearly finished, but it will demand two or three more years for completion. Health, labor, and observations are wanting for awhile, but in due season we hope to see the work 'with references and authorities for the several statements.' We should offer remarks on some important topics but that our author says, 'A fair result can be obtained only by fully stating and balancing the facts and arguments on both sides of the question; and this cannot possibly be here done.'

"Meanwhile Mr. Darwin anticipates small favor from many of the older and more eminent naturalists; his hopes chiefly rest on the young, and, as he would say, the unshackled. 'A few naturalists,' he observes, 'endowed with much flexibility of mind, who have already begun to doubt on the immutability of species, may be influenced by this volume; but I look with confidence to the future, to young and rising naturalists who will be able to view both sides of the question with impartiality.' It is enough for us to add that neither book, author, nor subject is of merely ordinary character. The work deserves attention, and will, we have no doubt, meet with it. Scientific naturalists will take up the author upon his own peculiar ground; and there will we imagine be a severe struggle for at least theoretical existence."

3. *Elements of Somatology: A Treatise on the general properties of Matter;* by GEO. M. MACLEAN, M.D., Prof. Chemistry and Nat. Philosophy, in Alleghany City, Pa. New York: J. Wiley, 56 Walker St., 1859. 12mo, pp. 124.—This modest little volume, the author tells us in his preface, is the fruit of many hours of study during a period of ill-health—a sort of "Consolations of a Philosopher."

It is a simple exposition of accepted doctrines on the familiar subjects of extension, impenetrability, figure, divisibility, indestructibility, porosity, compressibility, dilatibility, mobility, inertia, contraction, repulsion, polarity, elasticity, and the constitution of matter. The subject of Attraction he considers under thirty-one subdivisions. His expositions of the phenomena of adhesion, capillarity and osmose are more full and satisfactory than it is usual to find in elementary works. On many points in his discussions, we might join issue with our Author, as when he states cohesive attraction to be only a modification of gravitation—(p. 57)—and when he adduces the phenomena of contraction in a soap-bubble, in illustration of the cause of the meniscus of capillarity (p. 69).

It would have added materially to the value of the Treatise, and its interest to the student and general reader, if the author had appended to passages marked as quotations a reference to the authority from which they are copied. Except a quotation accredited to Cavallo, we do not recall a single reference to any authority in the volume.

A brief statement of the accepted doctrines of physics on the subject of "*Molecular Forces*" would have relieved his chapters on attraction and repulsion of several obscure points.

The work bears marks of haste, or want of careful revision of the press. Among many examples of this we may name the sentence under capillary attraction, commencing "The tube having the form of a syphon," (foot of p. 69) which conveys so confused a notion of what the Author seeks to express, that after several readings we have been unable to comprehend it. Bodies are said to weigh *less* near the poles than at the equator, (p. 55), and numerous typographical blemishes evidence the disadvantage of printing a scientific book at a distance from the press. These minor faults are easily removed in a new edition—which will be very likely to be called for, as the book is one of convenient reference for all teachers. The author will, however, confer a great favor on all such readers in a new edition, by quoting his authorities.

4. *The Telegraphic Manual, a complete history and description of the Semaphoric, Electric and Magnetic Telegraphs of Europe, Asia, Africa, and America, ancient and modern, with 625 illustrations;* by T. P. SCHAFFNER, of Kentucky. N. York, 1859. 8vo, pp. 850.—The title of this volume is an index to its contents. That he may leave nothing behind him for future explorers, the author commences his labors with Adam and Eve, in Eden! Mr. Schaffner has, from his wide and long experience in telegraphic construction and management both in the United States and in Europe, remarkable qualifications for the work he has under-

taken. The result of his labors is satisfactory. His work, in fullness of detail, leaves little to desire, and he appears always solicitous to avoid the charge of partizanship in awarding to rival parties what he judges to be their respective shares of merit, in cases of contested claims. As a literary production, it is to be regretted that the author did not submit his manuscripts to the revision of some judicious literary friend—thus avoiding certain faults of style of too frequent occurrence. But these are minor faults and can be easily pardoned where there is so much to praise.

5. *Bail's Drawing System :* THE HUMAN HEAD, by LOUIS BAIL, (graduate of the Royal Academy of Fine Arts, in Munich). New Haven: Author, 1859. 8vo, 64 plates in outline.—Prof. Bail has here done a great service to both teachers and pupils in the Arts in the United States. The success which has followed the Author's use of his own system in many of our higher seminaries, as well as in public classes, is the best guaranty of the adaptation of its parts to the great ends of instruction, and no doubt will secure its general adoption.

6. *Memoir of John Griscom, LL.D., late Professor of Chemistry and Nat. Philosophy, &c.* ; by his son, JOHN H. GRISCOM, M.D. New York: Carter, 1859. 8vo, pp. 427.—Some among the older readers of this Journal will recall with pleasure the selections from foreign scientific literature, which for many years Prof. Griscom prepared for these pages. His active life was well filled with varied duty as an instructor and philanthropist. He was either largely or entirely instrumental in the establishment of the New York High School; the Society for the prevention of Pauperism; the House of Refuge; and other institutions of public charity, which amid all the complaints of profligacy in her public administration, have shed a peculiar honor on the active benevolence of the City of New York.

As early as 1818, he instituted and sustained for many years, independent courses of scientific lectures, in New York city, and in other places—illustrating his courses by numerous experiments, and a costly apparatus procured at his own expense. This was long before the era of *popular lectures*, and Dr. Griscom, with the senior Editor of this Journal, may claim the honor of inaugurating a system which has since become almost universal in the United States.

Dr. Griscom published two volumes of Travels in Europe, in 1818–19, remarkable for the spirit of candor and kindness which is seen on every page, and interesting to this day, for the characteristic personal sketches he gives of the distinguished men of science he met abroad.

Dr. Griscom was an eminently good man; a member of the Society of Friends; a devout Christain believer, and without bigotry. His mild and gentle nature delighted in the most catholic liberality, and many of his warmest friends were members of other Christian sects. We have recently been called upon to commemorate several eminent scientific friends and collaborators—Cleaveland, Hare, Redfield and Olmsted, now numbered with the dead. We now add the name of Griscom—a name cherished long and warmly by intimate social and scientific relations. Our early auxiliaries and friends in science are now few in number, and our duties are soon to pass into younger and we hope better hands—

but the pioneers will be remembered as the pilgrims of science, although its votaries are now a Legion. This Memoir is a fine example of its class, and does credit both to the filial piety and literary ability of its distinguished Author. B. S.

FLEURY: *Des races qui se partagent l'Europe.* 8vo, 132 pages. Hachette & Co.—This author has brought to his study of the races great learning and a deep knowledge of the facts. He considers modern European civilization as springing from the German race.

JAMIN: *Cour de Physique de l'Ecole polytechnique.* T. II. with 3 plates and 181 figures in the text.—This volume contains *Heat* and *Acoustics.* The plates are engraved and the mathematical formulæ are printed with the neatness and accuracy which distinguish at present the productions of the press of Bachelier above all others in France.

G. LAMÉ: *Leçons sur les coordennées, curvilignes et leurs diverses applications,* in 8vo. 1859. Mallet-Bachelier, Paris.—Under this title the distinguished Professor of the Polytechnic School introduces to us a new branch of methematical science. It is geometry considered from a physico-mathematical point of view.

OBITUARY.—

Professor WILLIAM W. TURNER, one of our most distinguished philologists, died at Washington, Nov. 29, 1859, in the 50th year of his age. Although an excellent linguist, he devoted himself less to the study of words than to that of the structure of languages, their origin and connections, upon which subject his views were eminently philosophic. He was born in London, but was brought in his fifth year to this country, where he has ever since resided. He early developed a taste for the study of oriental languages and was in 1842 appointed instructor in the Hebrew and cognate tongues in the Union Theological Seminary of New York. The last seven years of his life were spent in Washington, where his attainments and upright, amiable deportment secured to him a host of friends. He did a vast amount of work in the way of translation and grammatic compilation, little of which has, however, appeared in his name:—he is chiefly known for his contributions to the "Bibliotheca Sacra" and to the journals of the American Oriental and Ethnological societies. But his published works give no adequate idea of the extent of his labors; his stores of knowledge were always open to his friends and most freely imparted, thus contributing to the advancement of sciences other than his own. He had, during the past ten years, given much attention to the study of the Aborigines of North America and their languages, not only elaborating general principles from the vocabularies collected by travellers, but confirming these and adding new information by communication with the delegates from various tribes that visited Washington upon business with the central government. In this investigation he accumulated a large amount of materials which will, it may be hoped, be some time given to the world. W. S.

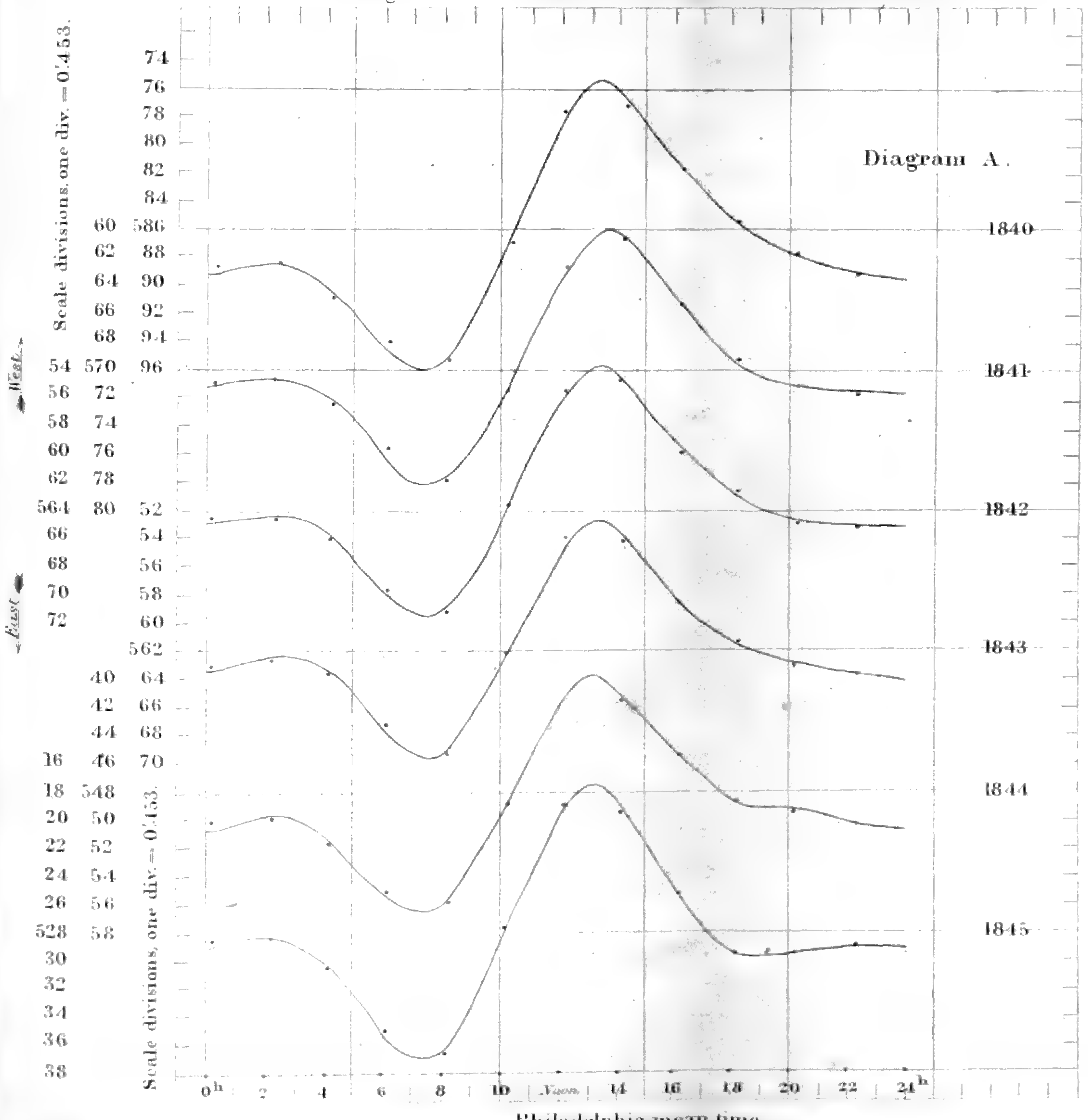
Dr. GEORGE WILSON, First Regius Professor of Technology in the University of Edinburgh, and Director of the Industrial Museum of that City, died near the end of November last, at the early age of 41. He was the biographer of Reid and Cavendish, and author of numerous researches, among which are the discovery of fluorine in blood and sea-water. Other of his published works are his "Researches on Color-blindness," an "Elementary Treatise on Chemistry," and "The Five Gate-ways of Knowledge." He is a great loss to his native city and the world.

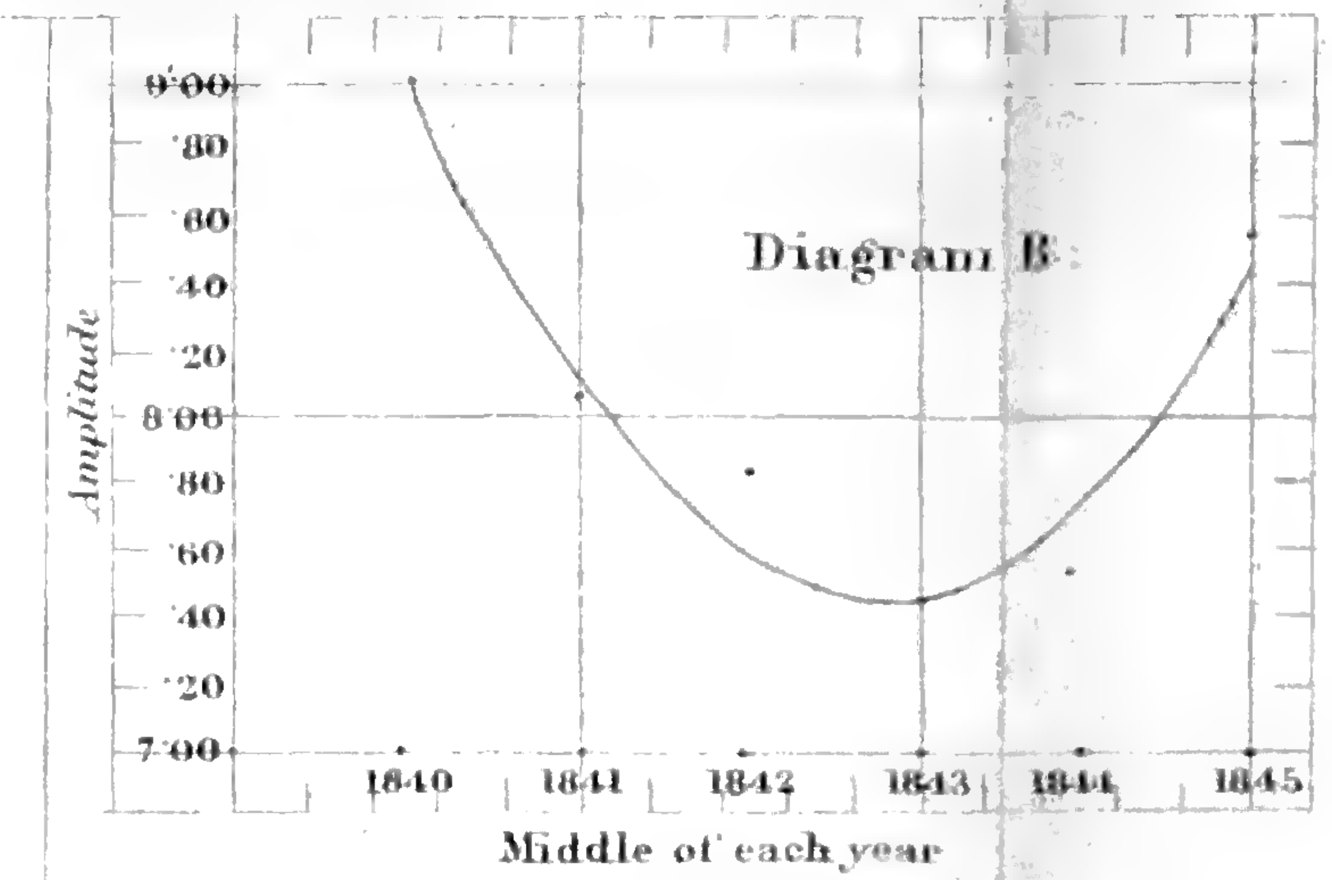
DISCUSSION OF THE MAGNETICAL AND METEOROLOGICAL OBSERVATIONS MADE AT THE GIRARD COLLEGE, PHILADELPHIA

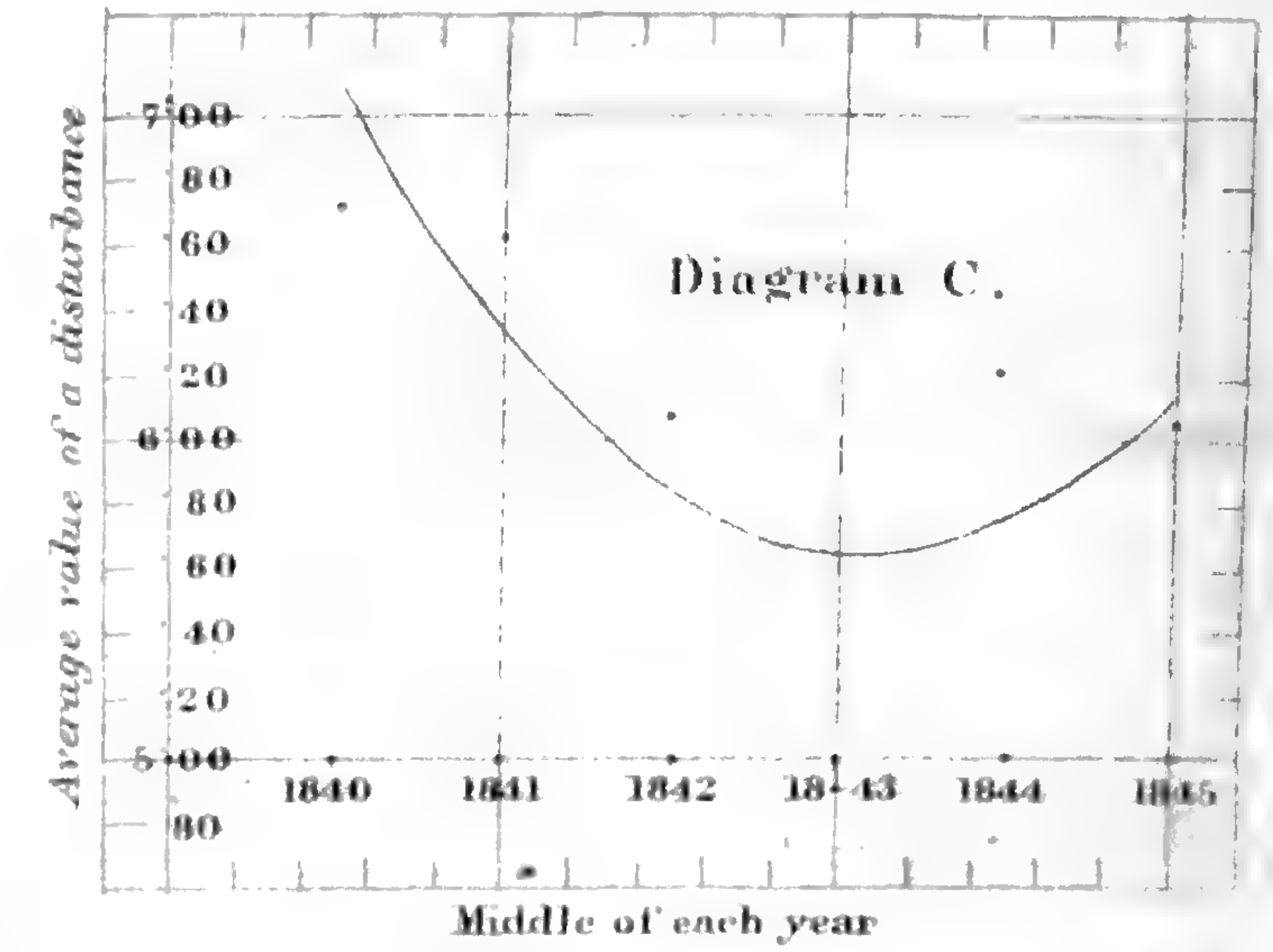
IN 1840, '41, '42, '43, '44 & '45 A.D. BACHE L.L.D. SUPERINTENDENT

DIAGRAMS TO PART I. 1859.

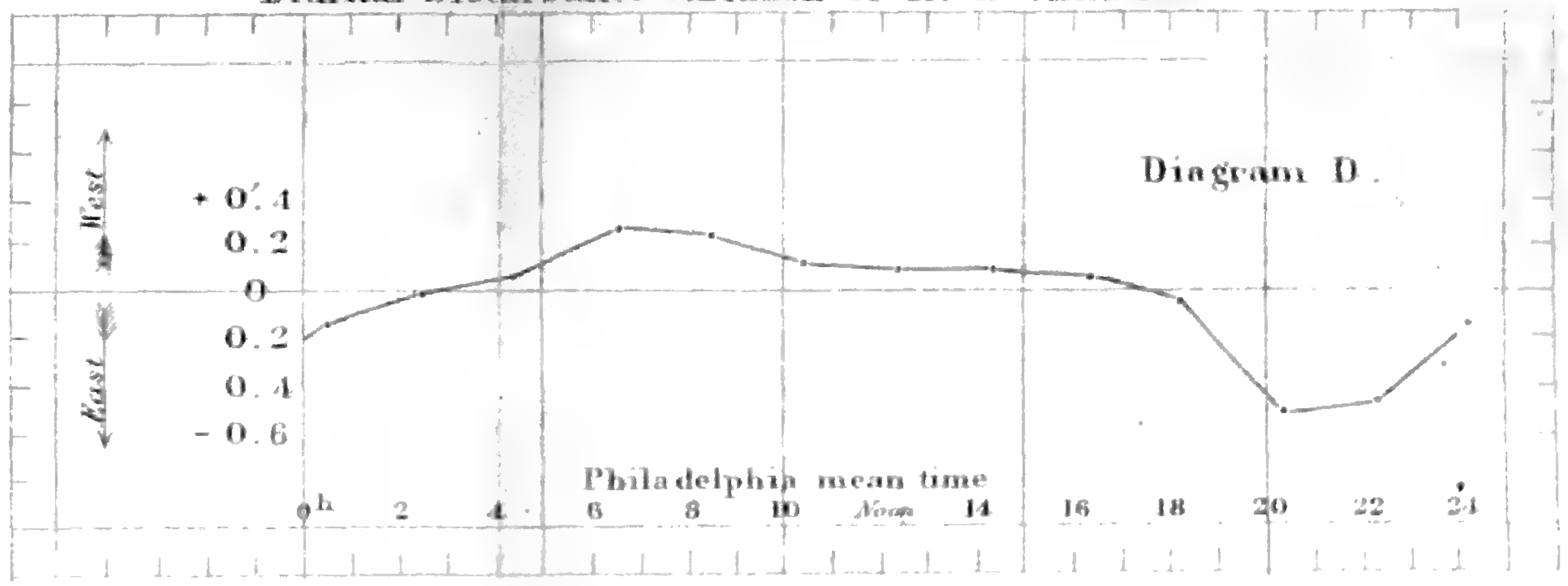
Regular Solar-diurnal Variation of the Declination

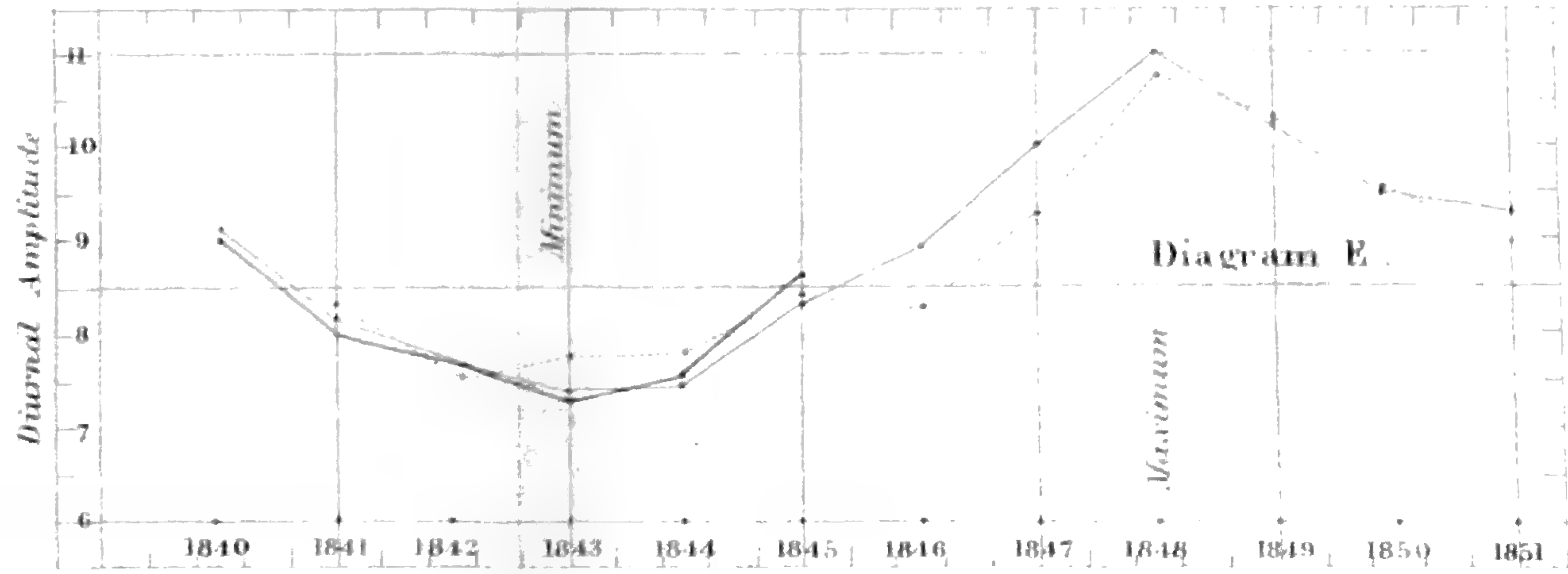






Diurnal Disturbance Variation of the Declination





J. H. LANE'S APPARATUS for a visual method of effecting a precise automatic comparison of time between distant Stations

[illegible] New Haven [illegible]

THE

AMERICAN

JOURNAL OF SCIENCE AND ARTS.

[SECOND SERIES.]

Art. XV.—*Review of Darwin's Theory on the Origin of Species by means of Natural Selection.**

Fully to understand the foregoing Essay of Dr. Hooker,† it should be read in the light of Mr. Darwin's book. The Essay is a trial of the Theory,—an attempt by one inclined in its favor to see how the theory will work, when applied to the flora of a large and most peculiar province of the world.

This book is already exciting much attention. Two American editions are announced, through which it will become familiar to many of our readers, before these pages are issued. An abstract of the argument,—for "the whole volume is one long argument," as the author states,—is unnecessary in such a case; and it would be difficult to give by detached extracts. For the volume itself is an abstract, a prodromus of a detailed work upon which the author has been laboring for twenty years, and which "will take two or three more years to complete." It is exceedingly compact; and although useful summaries are ap-

* *On the Origin of Species by means of Natural Selection, or the Preservation of Favored Races in the Struggle for Life*; by Charles Darwin, M.A., Fellow of the Royal, Geological, Linnæan, etc. Societies, Author of "Journal of Researches during H. M. S. Beagle's Voyage round the World." London: John Murray. 1859. pp. 502, post 8vo.

† This article was intended to follow the remaining part of the essay of Dr. Hooker, commenced in our January number; the continuation of which we are obliged to defer, for want of room.—Eds.

pended to the several chapters, and a general recapitulation contains the essence of the whole, yet much of the aroma escapes in the treble distillation, or is so concentrated that the flavor is lost to the general, or even to the scientific reader. The volume itself,—the proof spirit—is just condensed enough for its purpose. It will be far more widely read, and perhaps will make deeper impression than the elaborate work might have done, with its full details of the facts upon which the author's sweeping conclusions have been grounded. At least it is a more readable book: but all the facts that can be mustered in favor of the theory are still likely to be needed.

Who, upon a single perusal, shall pass judgment upon a work like this, to which twenty of the best years of the life of a most able naturalist have been devoted? And who among those naturalists who hold a position that entitles them to pronounce summarily upon the subject, can be expected to divest himself for the nonce of the influence or received and favorite systems? In fact, the controversy now opened is not likely to be settled in an off-hand way, nor it is desirable that it should be. A spirited conflict among opinions of every grade must ensue, which,—to borrow an illustration from the doctrine of the book before us—may be likened to the conflict in nature among races in the struggle for life, which Mr. Darwin describes; through which the views most favored by facts will be developed and tested by 'Natural Selection,' the weaker ones be destroyed in the process, and the strongest in the long run alone survive.

The duty of reviewing this volume in the American Journal of Science would naturally devolve upon the principal Editor, whose wide observation and profound knowledge of various departments of natural history, as well as of geology, particularly qualify him for the task. But he has been obliged to lay aside his pen, and to seek in distant lands the entire repose from scientific labor so essential to the restoration of his health,—a consummation devoutly to be wished, and confidently to be expected. Interested as Mr. Dana would be in this volume, he could not be expected to accept its doctrine. Views so idealistic as those upon which his "Thoughts upon Species"* are grounded, will not harmonize readily with a doctrine so thoroughly naturalistic as that of Mr. Darwin. Though it is just possible that one who regards the kinds of elementary matter, such as oxygen and hydrogen, and the definite compounds of these elementary matters, and their compounds again, in the mineral kingdom, as constituting species, in the same sense, fundamentally, as that of animal and vegetable species, might admit an evolution of one species from another in the latter as well as the former case.

* Article in this Journal, vol. xxiv, p. 305.

Between the doctrines of this volume and those of the other great Naturalist whose name adorns the title-page of this Journal, the widest divergence appears. It is interesting to contrast the two, and, indeed, is necessary to our purpose; for this contrast brings out most prominently, and sets in strongest light and shade the main features of the theory of the origination of species by means of Natural Selection.

The ordinary and generally received view assumes the independent, specific creation of each kind of plant and animal in a primitive stock, which reproduces its like from generation to generation, and so continues the species.* Taking the idea of species from this perennial succession of essentially similar individuals, the chain is logically traceable back to a local origin in a single stock, a single pair, or a single individual, from which all the individuals composing the species have proceeded by natural generation. Although the similarity of progeny to parent is fundamental in the conception of species, yet the likeness is by no means absolute: all species vary more or less, and some vary remarkably—partly from the influence of altered circumstances, and partly (and more really) from unknown constitutional causes which altered conditions favor rather than originate. But these variations are supposed to be mere oscillations from a normal state, and in Nature to be limited if not transitory; so that the primordial differences between species and species at their beginning have not been effaced, nor largely obscured, by blending through variation. Consequently, whenever two reputed species are found to blend in nature through a series of intermediate forms, community of origin is inferred, and all the forms, however diverse, are held to belong to one species. Moreover, since bisexuality is the rule in nature (which is practically carried out, in the long run, far more generally than has been suspected), and the heritable qualities of two distinct individuals are mingled in the offspring, it is supposed that the general sterility of hybrid progeny, interposes an effectual barrier against the blending of the original species by crossing.

From this generally accepted view the well-known theory of Agassiz and the recent one of Darwin diverge in exactly opposite directions.

That of Agassiz differs fundamentally from the ordinary view only in this, that it discards the idea of a common descent as the real bond of union among the individuals of a species, and also the idea of a local origin,—supposing, instead, that each species originated simultaneously, generally speaking over the whole geographical area it now occupies or has occupied, and

* "Species tot sunt, quot diversas formas ab initio produxit Infinitum Ens; quæ formæ, secundum generationis inditas leges, produxere plures, at sibi semper similes." —*Linn. Phil. Bot.*, 99, 157.

in perhaps as many individuals as it numbered at any subsequent period.

Mr. Darwin, on the other hand, holds the orthodox view of the descent of all the individuals of a species not only from a local birth-place, but from a single ancestor or pair; and that each species has extended and established itself, through natural agencies, wherever it could; so that the actual geographical distribution of any species is by no means a primordial arrangement, but a natural result. He goes farther, and this volume is a protracted argument intended to prove that the species we recognize have not been independently created, as such, but have descended, like varieties, from other species. Varieties, on this view, are incipient or possible species: species are varieties of a larger growth and a wider and earlier divergence from the parent stock: the difference is one of degree, not of kind.

The ordinary view—rendering unto Cæsar the things that are Cæsar's—looks to natural agencies for the actual distribution and perpetuation of species, to a supernatural for their origin.

The theory of Agassiz regards the origin of species and their present general distribution over the world as equally primordial, equally supernatural; that of Darwin, as equally derivative, equally natural.

The theory of Agassiz, referring as it does the phenomena both of origin and distribution directly to the Divine will,—thus removing the latter with the former out of the domain of inductive science (in which efficient cause is not the first, but the last word),—may be said to be theistic to excess. The contrasted theory is not open to this objection. Studying the facts and phenomena in reference to proximate causes, and endeavoring to trace back the series of cause and effect as far as possible, Darwin's aim and processes are strictly scientific, and his endeavor, whether successful or futile, must be regarded as a legitimate attempt to extend the domain of natural or physical science. For though it well may be that "organic forms have no physical or secondary cause," yet this can be proved only indirectly, by the failure of every attempt to refer the phenomena in question to causal laws. But, however originated, and whatever be thought of Mr. Darwin's arduous undertaking in this respect, it is certain that plants and animals are subject from their birth to physical influences, to which they have to accommodate themselves as they can. How literally they are "born to trouble," and how incessant and severe the struggle for life generally is, the present volume graphically describes. Few will deny that such influences must have gravely affected the range and the association of individuals and species on the earth's surface. Mr. Darwin thinks that, acting upon an inherent predisposition to vary, they have sufficed even to modify the species themselves and produce the present diversity. Mr. Agassiz believes that they have not even affected the geographical range and the actual association of species, still less their forms; but that every adaptation of species to climate and of species to species is as aboriginal, and therefore as inexplicable, as are the organic forms themselves.

Who shall decide between such extreme views so ably maintained on either hand, and say how much of truth there may be in each? The present reviewer has not the presumption to undertake such a task. Having no prepossession in favor of naturalistic theories, but struck with the eminent ability of Mr. Darwin's work, and charmed with its fairness, our humbler duty will be performed if, laying aside prejudice as much as we can, we shall succeed in giving a fair account of its method and argument, offering by the way a few suggestions, such as might occur to any naturalist of an inquiring mind. An editorial character for this article must in justice be disclaimed. The plural pronoun is employed not to give editorial weight, but to avoid even the appearance of egotism, and also the circumlocution which attends a rigorous adherence to the impersonal style.

We have contrasted these two extremely divergent theories, in their broad statements. It must not be inferred that they have no points nor ultimate results in common.

In the first place they practically agree in upsetting, each in its own way, the generally received definition of species, and in sweeping away the ground of their objective existence in Nature. The orthodox conception of species is that of lineal descent: all the descendants of a common parent, and no other, constitute a species; they have a certain identity because of their descent, by which they are supposed to be recognizable. So naturalists had a distinct idea of what they meant by the term species, and a practical rule, which was hardly the less useful because difficult to apply in many cases, and because its application was indirect,—that is, the community of origin had to be inferred from the likeness; that degree of similarity, and that only, being held to be conspecific which could be shown or reasonably inferred to be compatible with a common origin. And the usual concurrence of the whole body of naturalists (having the same data before them) as to what forms are species attests the value of the rule, and also indicates some real foundation for it in nature. But if species were created in numberless individuals over broad spaces of territory, these individuals are connected only in idea, and species differ from varieties on the one hand and from genera, tribes, &c. on the other only in degree; and no obvious natural reason remains for fixing upon this or that degree as specific, at least no natural standard, by which the opinions of different naturalists may be correlated.

Species upon this view are enduring, but subjective and ideal. Any three or more of the human races, for example, are species or not species, according to the bent of the naturalist's mind. Darwin's theory brings us the other way to the same result. In his view, not only all the individuals of a species are descendants of a common parent but of all the related species also. Affinity, relationship, all the terms which naturalists use figuratively to express an underived, unexplained resemblance among species, have a literal meaning upon Darwin's system, which they little suspected, namely, that of inheritance. Varieties are the latest offshoots of the genealogical tree in "an unlineal" order; species, those of an earlier date, but of no definite distinction; genera, more ancient species, and so on. The human races, upon this view likewise may or may not be species according to the notions of each naturalist as to what differences are specific: but, if not species already, those races that last long enough are sure to become so. It is only a question of time.

How well the simile of a genealogical tree illustrates the main ideas of Darwin's theory the following extract from the summary of the fourth chapter shows.

"It is a truly wonderful fact,—the wonder of which we are apt to overlook from familiarity—that all animals and all plants throughout all time and space should be related to each other in group subordinate to group, in the manner which we everywhere behold—namely, varieties of the same species most closely related together, species of the same genus less closely and unequally related together, forming sections and sub-genera, species of distinct genera much less closely related, and genera related in different degrees, forming sub-families, families, orders, sub-classes, and classes. The several subordinate groups in any class cannot be ranked in a single file, but seem rather to be clustered round points, and these round other points, and so on in almost endless cycles. On the view that each species has been independently created, I can see no explanation of this great fact in the classification of all organic beings; but, to the best of my judgment, it is explained through inheritance and the complex action of natural selection, entailing extinction and divergence of character, as we have seen illustrated in the diagram.

"The affinities of all the beings of the same class have sometimes been represented by a great tree. I believe this simile largely speaks the truth. The green and budding twigs may represent existing species; and those produced during each former year may represent the long succession of extinct species. At each period of growth all the growing twigs have tried to branch out on all sides, and overtop and kill the surrounding twigs and branches, in the same manner as species and groups of species have tried to overmaster other species in the great battle for life. The limbs divided into great branches, and these into lesser and lesser branches, were themselves once, when the tree was small, budding twigs; and this connexion of the former and present buds by ramifying branches may well represent the classification of all extinct and living species in groups subordinate to groups. Of the many twigs which flourished when the tree was a mere bush, only two or three, now grown into great branches, yet survive and bear all the other branches; so with the species which lived during long-past geological periods, very few now have living and modified descendants. From the first growth of the tree, many a limb and branch has decayed and dropped off; and these lost branches of various sizes may represent those whole orders, families, and genera which have now no living representatives, and which are known to us only from having been found in a fossil state. As we here and there see a thin straggling branch springing from a fork low down in a tree, and which by some chance has been favored and is still alive on its summit, so we occasionally see an animal like the Ornithorhynchus or Lepidosiren, which in some small degree connects by its affinities two large branches of life, and which has apparently been saved from fatal competition by having inhabited a protected station. As buds give rise by growth to fresh buds, and these, if vigorous, branch out and overtop on all sides many a feebler branch, so by generation I believe it has been with the great Tree of Life, which fills with its dead and broken branches the crust of the earth, and covers the surface with its ever branching and beautiful ramifications."

It may also be noted that there is a significant correspondence between the rival theories as to the main facts employed. Apparently every capital fact in the one view is a capital fact in the other. The difference is in the interpretation. To run the parallel ready made to our hands:*

"The simultaneous existence of the most diversified types under identical circumstances, ... the repetition of similar types under the most diversified circumstances, ... the unity of plan in otherwise highly diversified types of animals, ... the correspondence, now generally known as special homologies, in the details of structure otherwise entirely disconnected, down to the most minute peculiarities, ... the various degrees and different kinds of relationship among animals which [apparently] can have no genealogical connection, ... the simultaneous existence in the earliest geological periods....of representatives of all the great types of the animal kingdom, ... the gradation based upon complications of structure which may be traced among animals built upon the same plan; the distribution of some types over the most extensive range of surface of the globe, while others are limited to particular geographical areas, ... the identity of structures of these types, notwithstanding their wide geographical distribution, ... the community of structure in certain respects of animals otherwise entirely different, but living within the same geographical area, ... the connection by series of special structures observed in animals widely scattered over the surface of the globe, ... the definite relations in which animals stand to the surrounding world, ... the relations in which individuals of the same species stand to one another, ... the limitation of the range of changes which animals undergo during their growth, ... the return to a definite norm of animals which multiply in various ways, ... the order of succession of the different types of animals and plants characteristic of the

* Agassiz, Essay on Classification; Contrib. to Nat. Hist., i, p. 132, et seq.

different geological epochs, ... the localization of some types of animals upon the same points of the surface of the globe during several successive geological periods; .... the parallelism between the order of succession of animals and plants in geological times, and the gradation among their living representatives, ... the parallelism between the order of succession of animals in geological times and the changes their living representatives undergo during their embryological growth,* .... *the combination in many extinct types of characters which in later ages appear disconnected in different types*, ... the parallelism between the gradation among animals and the changes they undergo during their growth, ... the relations existing between these different series and the geographical distribution of animals, ... the connection of all the known features of nature into one system,—"

In a word, the whole relations of animals, &c. to surrounding nature and to each other, are regarded under the one view as ultimate facts, or in their ultimate aspect, and interpreted theologically;—under the other as complex facts, to be analyzed and interpreted scientifically. The one naturalist, perhaps too largely assuming the scientifically unexplained to be inexplicable, views the phenomena only in their supposed relation to the Divine mind. The other, naturally expecting many of these phenomena to be resolvable under investigation, views them in their relations to one another, and endeavors to explain them as far as he can (and perhaps farther) through natural causes.

But does the one really exclude the other? Does the investigation of physical causes stand opposed to the theological view and the study of the harmonies between mind and Nature? More than this, is it not most presumable that an intellectual conception realized in nature would be realized through natural agencies? Mr. Agassiz answers these questions affirmatively when he declares that "the task of science is to investigate what has been done, to enquire if possible *how it has been done*, rather than to ask what is possible for the Deity, since *we can know that only by what actually exists*;" and also when he extends the argument for the intervention in nature of a creative mind to its legitimate application in the inorganic world; which, he remarks, "considered in the same light, would not fail also to exhibit unexpected evidence of thought, in the character of the laws regulating the chemical combinations, the action of physi-

* As to this, Darwin remarks that he can only hope to see the law hereafter proved true (p. 449); and p. 338: "Agassiz insists that ancient animals resemble to a certain extent the embryos of recent animals of the same classes; or that the geological succession of extinct forms is in some degree parallel to the embryological development of recent forms. I must follow Pictet and Huxley in thinking that the truth of this doctrine is very far from proved. Yet I fully expect to see it hereafter confirmed, at least in regard to subordinate groups, which have branched off from each other within comparatively recent times. For this doctrine of Agassiz accords well with the theory of natural selection."

cal forces, etc., etc."* Mr. Agassiz, however, pronounces that "the connection between the facts is *only intellectual;*"—an opinion which the analogy of the inorganic world, just referred to, does not confirm, for there a material connection between the facts is justly held to be consistent with an intellectual,—and which the most analogous cases we can think of in the organic world do not favor; for there is a material connection between the grub, the pupa, and the butterfly, between the tadpole and the frog, or, still better, between those distinct animals which succeed each other in alternate and very dissimilar generations. So that mere analogy might rather suggest a natural connection than the contrary; and the contrary cannot be demonstrated until the possibilities of nature under the Deity are fathomed.

But the intellectual connection being undoubted, Mr. Agassiz properly refers the whole to "the agency of Intellect as its first cause." In doing so, however, he is not supposed to be offering a scientific explanation of the phenomena. Evidently he is considering only the ultimate *why*, not the proximate why or *how*.

Now the latter is just what Mr. Darwin is considering. He conceives of a physical connection between allied species: but we suppose he does not deny their intellectual connection, as related to a Supreme Intelligence. Certainly we see no reason why he should, and many reasons why he should not. Indeed, as we contemplate the actual direction of investigation and speculation in the physical and natural sciences, we dimly apprehend a probable synthesis of these divergent theories, and in it the ground for a strong stand against mere naturalism. Even if the doctrine of the origin of species through natural selection should prevail in our day, we shall not despair; being confident that the genius of an Agassiz will be found equal to the work of constructing, upon the mental and material foundations combined, a theory of nature as theistic and as scientific, as that which he has so eloquently expounded.

To conceive the possibility of "the descent of species from species by insensibly fine gradations" during a long course of time, and to demonstrate its compatibility with a strictly theistic view of the universe, is one thing: to substantiate the theory itself or show its likelihood is quite another thing. This brings us to consider what Darwin's theory actually is, and how he supports it.

That the existing kinds of animals and plants, or many of them, may be derived from other and earlier kinds, in the lapse

* Op. cit., p. 131.—One or two Bridgewater Treatises, and most modern works upon Natural Theology should have rendered the evidences of thought in inorganic nature not "unexpected."

of time, is by no means a novel proposition. Not to speak of ancient speculations of the sort, it is the well-known Lamarckian theory. The first difficulty which such theories meet with is that, in the present age, with all its own and its inherited prejudgments, the whole burden of proof is naturally, and indeed properly, laid upon the shoulders of the propounders; and thus far the burden has been more than they could bear. From the very nature of the case, substantive proof of specific creation is not attainable; but that of derivation or transmutation of species may be. He who affirms the latter view is bound to do one or both of two things. Either, 1, to assign real and adequate causes, the natural or necessary result of which must be to produce the present diversity of species and their actual relations; or, 2, to show the general conformity of the whole body of facts to such assumption, and also to adduce instances explicable by it and inexplicable by the received view,—so perhaps winning our assent to the doctrine, through its competency to harmonize all the facts, even though the cause of the assumed variation remain as occult as that of the transformation of tadpoles into frogs, or that of *Coryne* into *Sarzia*.

The first line of proof, successfully carried out, would establish derivation as a true physical theory; the second, as a sufficient hypothesis.

Lamarck mainly undertook the first line, in a theory which has been so assailed by ridicule that it rarely receives the credit for ability to which in its day it was entitled. But he assigned partly unreal, partly insufficient causes; and the attempt to account for a progressive change in species through the direct influence of physical agencies, and through the appetencies and habits of animals reacting upon their structure, thus causing the production and the successive modification of organs, is a conceded and total failure. The shadowy author of the Vestiges of the Natural History of Creation can hardly be said to have undertaken either line, in a scientific way. He would explain the whole progressive evolution of nature by virtue of an inherent tendency to development,—thus giving us an idea or a word in place of a natural cause, a restatement of the proposition instead of an explanation. Mr. Darwin attempts both lines of proof, and in a strictly scientific spirit; but the stress falls mainly upon the first; for, as he does assign real causes, he is bound to prove their adequacy.

It should be kept in mind that, while all direct proof of independent origination is unattainable from the nature of the case, the overthrow of particular schemes of derivation has not established the opposite proposition. The futility of each hypothesis thus far proposed to account for derivation may be made apparent, or unanswerable objections may be urged against it; and each victory of the kind may render derivation more improbable, and therefore specific creation more probable, without settling the question either way. New facts, or new arguments and a new mode of viewing the question may some day change the whole aspect of the case. It is with the latter that Mr. Darwin now reopens the discussion.

Having conceived the idea that varieties are incipient species, he is led to study variation in the field where it shows itself most strikingly and affords the greatest facilities to investigation. Thoughtful naturalists have had increasing grounds to suspect that a re-examination of the question of species in zoology and botany, commencing with those races which man knows most about, viz. the domesticated and cultivated races, would be likely somewhat to modify the received idea of the entire fixity of species. This field, rich with various but unsystematized stores of knowledge accumulated by cultivators and breeders, has been generally neglected by naturalists, because these races are not in a state of nature; whereas they deserve particular attention on this very account, as experiments, or the materials for experiments, ready to our hand. In domestication we vary some of the natural conditions of a species, and thus learn experimentally what changes are within the reach of varying conditions in nature. We separate and protect a favorite race against its foes or its competitors, and thus learn what it might become if nature ever afforded it equal opportunities. Even when, to subserve human uses, we modify a domesticated race to the detriment of its native vigor, or to the extent of practical monstrosity, although we secure forms which would not be originated and could not be perpetuated in free nature, yet we attain wider and juster views of the possible degree of variation. We perceive that some species are more variable than others, but that no species subjected to the experiment persistently refuses to vary; and that when it has once begun to vary, its varieties are not the less but the more subject to variation. "No case is on record of a variable being ceasing to be variable under cultivation." It is fair to conclude, from the observation of plants and animals in a wild as well as domesticated state, that the tendency to vary is general, and even universal. Mr. Darwin does "not believe that variability is an inherent and necessary contingency, under all circumstances, with all organic beings, as some authors have thought." No one supposes variation could occur under all circumstances; but the facts on the whole imply an universal tendency, ready to be manifested under favorable circumstances. In reply to the assumption that man has chosen for domestication animals and plants having an extraordinary inherent tendency to vary, and likewise to withstand diverse climates, it is asked:

"How could a savage possibly know, when he first tamed an animal, whether it would vary in succeeding generations, and whether it would endure other climates? Has the little variability of the ass or guinea-fowl, or the small power of endurance of warmth by the rein-deer, or of cold by the common camel, prevented their domestication? I cannot doubt that if other animals and plants, equal in number to our domesticated productions, and belonging to equally diverse classes and countries, were taken from a state of nature, and could be made to breed for an equal number of generations under domestication, they would vary on an average as largely as the parent species of our existing domesticated productions have varied."

As to amount of variation, there is the common remark of naturalists that the varieties of domesticated plants or animals often differ more widely than do the individuals of distinct species in a wild state: and even in nature the individuals of some species are known to vary to a degree sensibly wider than that which separates related species. In his instructive section on the breeds of the domestic pigeon, our author remarks that:—"at least a score of pigeons might be chosen, which if shown to an ornithologist, and he were told that they were wild birds, would certainly be ranked by him as well defined species. Moreover, I do not believe that any ornithologist would place the English carrier, the short-faced tumbler, the runt, the barb, pouter, and fantail in the same genus; more especially as in each of these breeds several truly inherited sub-breeds, or species as he might have called them, could be shown him." That this is not a case like that of dogs, in which probably the blood of more than one species is mingled, Mr. Darwin proceeds to show, adducing cogent reasons for the common opinion that all have descended from the wild rock-pigeon. Then follow some suggestive remarks:—

"I have discussed the probable origin of domestic pigeons at some, yet quite insufficient, length; because when I first kept pigeons and watched the several kinds, knowing well how true they bred, I felt fully as much difficulty in believing that they could ever have descended from a common parent, as any naturalist could in coming to a similar conclusion in regard to many species of finches, or other large groups of birds, in nature. One circumstance has struck me much; namely, that all the breeders of the various domestic animals and the cultivators of plants, with whom I have ever conversed, or whose treatises I have read, are firmly convinced that the several breeds to which each has attended, are descended from so many aboriginally distinct species. Ask, as I have asked, a celebrated raiser of Hereford cattle, whether his cattle might not have descended from long horns, and he will laugh you to scorn. I have never met a pigeon, or poultry, or duck, or rabbit fancier, who was not fully convinced that each main breed was descended from a distinct species. Van Mons, in his treatise on pears and apples, shows how utterly he disbelieves that the several sorts, for instance a Ribston-pippin or Codlin-apple, could ever have proceeded from the seeds of the same tree. Innumerable other examples could be given. The explanation, I think, is simple: from long-continued study they are strongly impressed with the differences between the several races; and though they well know that each race varies slightly, for they win their prizes by selecting such slight differences, yet they ignore all general arguments, and refuse to sum up in their minds slight differences accumulated during many successive generations. May not those naturalists who, knowing far less of the laws of inheritance than does the breeder, and knowing no more than he does of the intermediate links in the long lines of descent, yet admit that many of our domestic races have descended from the same parents—may they not learn a lesson of caution, when they deride the idea of species in a state of nature being lineal descendants of other species?"

The actual causes of variation are unknown. Mr. Darwin favors the opinion of the late Mr. Knight, the great philosopher of horticulture, that variability under domestication is somehow connected with excess of food. He also regards the unknown cause as acting chiefly upon the reproductive system of the parents, which system, judging from the effect of confinement or cultivation upon its functions, he concludes to be more susceptible than any other to the action of changed conditions of life. The tendency to vary certainly appears to be much stronger under domestication than in free nature. But we are not sure that the greater variableness of cultivated races is not mainly owing to the far greater opportunities for manifestation and accumulation—a view seemingly all the more favorable to Mr. Darwin's theory. The actual amount of certain changes, such as size or abundance of fruit, size of udder, stands of course in obvious relation to supply of food.

Really, we no more know the reason why the progeny occasionally deviates from the parent than we do why it usually resembles it. Though the laws and conditions governing variation are known to a certain extent, while those governing inheritance are apparently inscrutable. "Perhaps," Darwin remarks, "the correct way of viewing the whole subject would be, to look at the inheritance of every character whatever as the rule, and non-inheritance as the anomaly." This, from general and obvious considerations, we have long been accustomed to do. Now, as exceptional instances are expected to be capable of explanation, while ultimate laws are not, it is quite possible that variation may be accounted for, while the great primary law of inheritance remains a mysterious fact.

The common proposition is, that *species reproduce their like;* this is a sort of general inference, only a degree closer to fact than the statement that genera reproduce their like. The true proposition, the fact incapable of further analysis is, that *individ-*

uals reproduce their like,—that characteristics are inheritable. So varieties, or deviations once originated, are perpetuable, like species. Not so likely to be perpetuated, at the outset; for the new form tends to resemble a grand-parent and a long line of similar ancestors, as well as to resemble its immmediate progenitors. Two forces which coincide in the ordinary case, where the offspring resembles its parent, act in different directions when it does not, and it is uncertain which will prevail. If the remoter, but very potent ancestral influence predominates, the variation disappears with the life of the individual. If that of the immediate parent—feebler no doubt, but closer—the variety survives in the offspring; whose progeny now has a redoubled tendency to produce its own like; whose progeny again is almost sure to produce its like, since it is much the same whether it takes after its mother or its grandmother.

In this way races arise, which under favorable conditions may be as hereditary as species. In following these indications, watching opportunities, and breeding only from those individuals which vary most in a desirable direction, man leads the course of variation as he leads a streamlet,—apparently at will, but never against the force of gravitation,—to a long distance from its source, and makes it more subservient to his use or fancy. He unconsciously strengthens those variations which he prizes when he plants the seed of a favorite fruit, preserves a favorite domestic animal, drowns the uglier kittens of a litter, and allows only the handsomest or the best mousers to propagate. Still more, by methodical selection, in recent times almost marvellous results have been produced in new breeds of cattle, sheep, and poultry, and new varieties of fruit of greater and greater size or excellence.

It is said that all domestic varieties if left to run wild, would revert to their aboriginal stocks. Probably they would wherever various races of one species were left to commingle. At least the abnormal or exaggerated characteristics induced by high feeding, or high cultivation, and prolonged close breeding would promptly disappear, and the surviving stock would soon blend into a homogeneous result (in a way presently explained), which would naturally be taken for the original form; but we could seldom know if it were so. It is by no means certain that the result would be the same if the races ran wild each in a separate region. Dr. Hooker doubts if there is a true reversion in the case of plants. Mr. Darwin's observations rather favor it in the animal kingdom. With mingled races reversion seems well made out in the case of pigeons. The common opinion upon this subject therefore probably has some foundation. But even if we regard varieties as oscillations around a primitive centre or type, still it appears from the readiness with which such varieties originate, that a certain amount of disturbance would carry them beyond the influence of the primordial attraction, where they may become new centres of variation.

Some suppose that races cannot be perpetuated indefinitely even by keeping up the conditions under which they were fixed: but the high antiquity of several, and the actual fixity of many of them, negative this assumption. "To assert that we could not breed our cart and race horses, long and short-horned cattle, and poultry of various breeds, for almost an infinite number of generations would be opposed to all experience."

Why varieties develope so readily and deviate so widely under domestication, while they are apparently so rare or so transient in free nature, may easily be shown. In nature, even with hermaphrodite plants, there is a vast amount of cross fertilization among various individuals of the same species. The inevitable result of this (as was long ago explained in this Journal*) is to repress variation, to keep the mass of a species comparatively homogeneous over any area in which it abounds in individuals. Starting from a suggestion of the late Mr. Knight, now so familiar, that close interbreeding diminishes vigor and fertility†; and perceiving that bisexuality is ever aimed at in nature,—being attained physiologically in numerous cases where it is not structurally,—Mr. Darwin has worked out the subject in detail, and shown how general is the concurrence, either habitual or occasional, of two hermaphrodite individuals in the reproduction of their kind; and has drawn the philosophical inference that probably no organic being self-fertilizes indefinitely; but that a cross with another individual is occasionally—perhaps at very long intervals—indispensable. We refer the reader to the section on the intercrossing of individuals (p. 96–101), and also to an article in the Gardeners' Chronicle a year and a half ago, for the details of a very interesting contribution to science, irrespective of theory.

In domestication, this intercrossing may be prevented; and in this prevention lies the art of producing varieties. But "the art itself is Nature," since the whole art consists in allowing the most universal of all natural tendencies in organic things (inheritance) to operate uncontrolled by other and obviously incidental tendencies. No new power, no artificial force is brought into play either by separating the stock of a desirable variety so as to prevent mixture, or by selecting for breeders those indi-

* Vol. xvii, [2], 1854, p. 13.

† We suspect that this is not an ultimate fact, but a natural consequence of inheritance,—the inheritance of disease or of tendency to disease, which close interbreeding perpetuates and accumulates, but wide breeding may neutralize or eliminate.

viduals which most largely partake of the peculiarities for which the breed is valued.*

We see everywhere around us the remarkable results which Nature may be said to have brought about under artificial selection and separation. Could she accomplish similar results when left to herself? Variations might begin, we know they do begin, in a wild state. But would any of them be preserved and carried to an equal degree of deviation? Is there anything in nature which in the long run may answer to artificial selection? Mr. Darwin thinks that there is; and *Natural Selection* is the key-note of his discourse.

As a preliminary, he has a short chapter to show that there is variation in nature, and therefore something for natural selection to act upon. He readily shows that such mere variations as may be directly referred to physical conditions (like the depauperation of plants in a sterile soil, or their dwarfing as they approach an alpine summit, the thicker fur of an animal from far northward, &c.), and also those individual differences which we everywhere recognize but do not pretend to account for, are not separable by any assignable line from more strongly marked varieties; likewise that there is no clear demarcation between the latter and subspecies, or varieties of the highest grade (distinguished from species not by any known inconstancy, but by the supposed lower importance of their characteristics); nor between these and recognized species. "These differences blend into each other in an insensible series, and the series impresses the mind with an idea of an actual passage."

This gradation from species downward is well made out. To carry it one step farther upwards, our author presents in a strong light the differences which prevail among naturalists as to what forms should be admitted to the rank of species. Some genera (and these in some countries) give rise to far more discrepancy than others; and it is concluded that the large or dominant genera are usually the most variable. In a flora so small as the British, 182 plants generally reckoned as varieties, have been ranked by some botanists as species. Selecting the British genera which include the most polymorphous forms, it appears that Babington's Flora gives them 251 species, Bentham's only 112, a difference of 139 doubtful forms. These are nearly the extreme views; but they are the views of two most capable and most experienced judges, in respect to one of the best known floras of the world. The fact is suggestive, that the best known countries furnish the greatest number of such doubtful cases.

* The rules and processes of breeders of animals, and their results, are so familiar that they need not be particularized. Less is popularly known about the production of vegetable races. We refer our readers back to this Journal, xxvii, pp. 440-442 (May, 1859) for an abstract of the papers of M. Vilmorin upon this subject.

Illustrations of this kind may be multiplied to a great extent. They make it plain that, whether species in nature are aboriginal and definite or not, our practical conclusions about them, as embodied in systematic works, are not *facts* but *judgments*, and largely fallible judgments.

How much of the actual coincidence of authorities is owing to imperfect or restricted observation, and to one naturalist's adopting the conclusions of another without independent observation, this is not the place to consider. It is our impression that species of animals are more definitely marked than those of plants; this may arise from our somewhat extended acquaintance with the latter, and our ignorance of the former. But we are constrained by our experience to admit the strong likelihood, in botany, that varieties on the one hand and what are called closely related species on the other do not differ except in degree. Whenever the wider difference separating the latter can be spanned by intermediate forms, as it sometimes is, no botanist long resists the inevitable conclusion. Whenever, therefore, this wider difference can be shown to be compatible with community of origin, and explained through natural selection or in any other way, we are ready to adopt the *probable* conclusion; and we see beforehand how strikingly the actual geographical association of related species favors the broader view. Whether we should continue to regard the forms in question as distinct species, depends upon what meaning we shall finally attach to that term; and that depends upon how far the doctrine of derivation can be carried back and how well it can be supported.

In applying his principle of natural selection to the work in hand, Mr. Darwin assumes, as we have seen: 1, some variability of animals and plants in nature; 2, the absence of any definite distinction between slight variations, and varieties of the highest grade; 3, the fact that naturalists do not practically agree, and do not increasingly tend to agree, as to what forms are species and what are strong varieties, thus rendering it probable that there may be no essential and original difference, or no possibility of ascertaining it, at least in many cases; also, 4, that the most flourishing and dominant species of the larger genera on an average vary most (a proposition which can be substantiated only by extensive comparisons, the details of which are not given);—and, 5, that in large genera the species are apt to be closely but unequally allied together, forming little clusters round certain species,—just such clusters as would be formed if we suppose their members once to have been satellites or varieties of a central or parent species, but to have attained at length a wider divergence and a specific character. The fact of such association is undeniable; and the use which Mr. Darwin makes of it seems fair and natural.

The gist of Mr. Darwin's work is to show that such varieties are gradually diverged into species and genera through *natural selection;* that natural selection is the inevitable result of the *struggle for existence* which all living things are engaged in; and that this struggle is an unavoidable consequence of several natural causes, but mainly of the high rate at which all organic beings tend to increase.

Curiously enough, Mr. Darwin's theory is grounded upon the doctrine of Malthus and the doctrine of Hobbes. The elder DeCandolle had conceived the idea of the struggle for existence, and in a passage which would have delighted the cynical philosopher of Malmesbury, had declared that all nature is at war, one organism with another or with external nature; and Lyell and Herbert had made considerable use of it. But Hobbes in his theory of society and Darwin in his theory of natural history, alone have built their systems upon it. However moralists and political economists may regard these doctrines in their original application to human society and the relation of population to subsistence, their thorough applicability to the great society of the organic world in general is now undeniable. And to Mr. Darwin belongs the credit of making this extended application, and of working out the immensely diversified results with rare sagacity and untiring patience. He has brought to view *real causes* which have been largely operative in the establishment of the actual association and geographical distribution of plants and animals. In this he must be allowed to have made a very important contribution to an interesting department of science, even if his theory fails in the endeavor to explain the origin or diversity of species.

"Nothing is easier," says our author, "than to admit in words the truth of the universal struggle for life, or more difficult—at least I have found it so—than constantly to bear this conclusion in mind. Yet unless it be thoroughly engrained in the mind, I am convinced that the whole economy of nature, with every fact on distribution, rarity, abundance, extinction, and variation, will be dimly seen or quite misunderstood. We behold the face of nature bright with gladness, we often see superabundance of food; we do not see, or we forget, that the birds which are idly singing round us mostly live on insects or seeds, and are thus constantly destroying life; or we forget how largely these songsters, or their eggs, or their nestlings, are destroyed by birds and beasts of prey; we do not always bear in mind, that though food may be now superabundant, it is not so at all seasons of each recurring year."—p. 62.

"There is no exception to the rule that every organic being naturally increases at so high a rate, that if not destroyed, the earth would soon be covered by the progeny of a single pair. Even slow-breeding man has doubled in twenty-five years, and at this rate, in a few thousand years, there would literally not be standing room for his progeny. Linnæus has calculated that if an annual plant produced only two seeds—

and there is no plant so unproductive as this—and their seedlings next year produced two, and so on, then in twenty years there would be a million plants. The elephant is reckoned to be the slowest breeder of all known animals, and I have taken some pains to estimate its probable minimum rate of natural increase: it will be under the mark to assume that it breeds when thirty years old, and goes on breeding till ninety years old, bringing forth three pairs of young in this interval; if this be so, at the end of the fifth century there would be alive fifteen million elephants, descended from the first pair.

"But we have better evidence on this subject than mere theoretical calculations, namely, the numerous recorded cases of the astonishingly rapid increase of various animals in a state of nature, when circumstances have been favorable to them during two or three following seasons. Still more striking is the evidence from our domestic animals of many kinds which have run wild in several parts of the world; if the statements of the rate of increase of slow-breeding cattle and horses in South America, and latterly in Australia, had not been well authenticated, they would have been quite incredible. So it is with plants: cases could be given of introduced plants which have become common throughout whole islands in a period of less than ten years. Several of the plants now most numerous over the wide plains of La Plata, clothing square leagues of surface almost to the exclusion of all other plants, have been introduced from Europe; and there are plants which now range in India, as I hear from Dr. Falconer, from Cape Comorin to the Himalaya, which have been imported from America since its discovery. In such cases, and endless instances could be given, no one supposes that the fertility of these animals or plants has been suddenly and temporarily increased in any sensible degree. The obvious explanation is that the conditions of life have been very favorable, and that there has consequently been less destruction of the old and young, and that nearly all the young have been enabled to breed. In such cases the geometrical ratio of increase, the result of which never fails to be surprising, simply explains the extraordinarily rapid increase and wide diffusion of naturalized productions in their new homes."—pp. 64, 65.

"All plants and animals are tending to increase at a geometrical ratio; all would most rapidly stock any station in which they could anyhow exist; the increase must be checked by destruction at some period of life." —p. 65.

The difference between the most and the least prolific species is of no account.

"The condor lays a couple of eggs, and the ostrich a score; and yet in the same country the condor may be the more numerous of the two. The Fulmar petrel lays but one egg, yet it is believed to be the most numerous bird in the world."—p. 68.

"The amount of food gives the extreme limit to which each species can increase; but very frequently it is not the obtaining of food, but the serving as prey to other animals, which determines the average numbers of a species."—p. 68.

"Climate plays an important part in determining the average numbers of a species, and periodical seasons of extreme cold or drought, I believe to be the most effective of all checks. I estimated that the winter of 1854–55 destroyed four-fifths of the birds in my own grounds; and this is a tremendous destruction, when we remember that ten per cent is an extraordinarily severe mortality from epidemics with man. The action of climate seems at first sight to be quite independent of the struggle for existence; but in so far as climate chiefly acts in reducing food, it brings on the most severe struggle between the individuals, whether of the same or of distinct species, which subsist on the same kind of food. Even when climate, for instance extreme cold, acts directly, it will be the least vigorous, or those which have got least food through the advancing winter, which will suffer most. When we travel from south to north, or from a damp region to a dry, we invariably see some species gradually getting rarer and rarer, and finally disappearing; and the change of climate being conspicuous, we are tempted to attribute the whole effect to its direct action. But this is a very false view: we forget that each species, even where it most abounds, is constantly suffering enormous destruction at some period of its life, from enemies or from competitors for the same place and food; and if these enemies or competitors be in the least degree favored by any slight change of climate, they will increase in numbers, and, as each area is already stocked with inhabitants, the other species will decrease. When we travel southward and see a species decreasing in numbers, we may feel sure that the cause lies quite as much in other species being favored, as in this one being hurt. So it is when we travel northward, but in a somewhat lesser degree, for the number of species of all kinds, and therefore of competitors, decreases northwards; hence in going northward, or in ascending a mountain, we far oftener meet with stunted forms, due to the *directly* injurious action of climate, than we do in proceeding southwards or in descending a mountain. When we reach the Arctic regions, or snow-capped summits, or absolute deserts, the struggle for life is almost exclusively with the elements.

"That climate acts in main part indirectly by favoring other species, we may clearly see in the prodigious number of plants in our gardens which can perfectly well endure our climate, but which never become naturalized, for they cannot compete with our native plants, nor resist destruction by our native animals."—pp. 68, 69.

After an instructive instance in which "cattle absolutely determine the existence of the Scotch Fir," we are referred to cases in which insects determine the existence of cattle.

"Perhaps Paraguay offers the most curious instance of this; for here neither cattle nor horses nor dogs have ever run wild, though they swarm southward and northward in a feral state; and Azara and Rengger have shown that this is caused by the greater number in Paraguay of a certain fly, which lays its eggs in the navels of these animals when first born. The increase of these flies, numerous as they are, must be habitually checked by some means, probably by birds. Hence, if certain insectivorous birds (whose numbers are probably regulated by hawks or beasts of prey) were to increase in Paraguay, the flies would decrease—then cattle and horses would become feral, and this would certainly greatly alter (as indeed I have observed in parts of South America) the vegetation: this again would largely affect the insects; and this, as we just have seen in Staffordshire, the insectivorous birds, and so onwards in ever-increasing circles of complexity. We began this series by insectivorous birds, and we had ended with them. Not that in nature the relations can ever be as simple as this. Battle within battle must ever be recurring with varying success; and yet in the long run the forces are so nicely balanced, that the face of nature remains uniform for long periods of time, though assuredly the merest trifle would often give the victory to one organic being over another. Nevertheless so profound is our ignorance, and so high our presumption, that we marvel when we hear of the extinction of an organic being; and as we do not see the cause, we invoke cataclysms to desolate the world, or invent laws on the duration of the forms of life!"—pp. 72, 73.

"When we look at the plants and bushes clothing an entangled bank, we are tempted to attribute their proportional numbers and kinds to what we call chance. But how false a view is this! Every one has heard that when an American forest is cut down, a very different vegetation springs up; but it has been observed that the trees now growing on the ancient Indian mounds, in the Southern United States, display the same beautiful diversity and proportion of kinds as in the surrounding virgin forests. What a struggle between the several kinds of trees must here have gone on during long centuries, each annually scattering its seeds by the thousand; what war between insect and insect—between insects, snails, and other animals with birds and beasts of prey—all striving to increase, and all feeding on each other or on the trees or their seeds and seedlings, or on the other plants which first clothed the ground and thus checked the growth of the trees! Throw up a handful of feathers, and all must fall to the ground according to definite laws; but how simple is this problem compared to the action and reaction of the innumerable plants and animals which have determined, in the course of centuries, the proportional numbers and kinds of trees now growing on the old Indian ruins!"—pp. 74, 75.

For reasons obvious upon reflection the competition is often, if not generally, most severe between nearly related species when they are in contact, so that one drives the other before it, as the Hanoverian the old English rat, the small Asiatic cockroach in Russia, its greater congener, &c.: and this, when duly considered, explains many curious results;—such, for instance, as the considerable number of different genera of plants and animals which are generally found to inhabit any limited area.

"The truth of the principle, that the greatest amount of life can be supported by great diversification of structure, is seen under many natural circumstances. In an extremely small area, especially if freely open to immigration, and where the contest between individual and individual must be severe, we always find great diversity in its inhabitants. For instance, I found that a piece of turf, three feet by four in size, which had been exposed for many years to exactly the same conditions, supported twenty species of plants, and these belonged to eighteen genera and to eight orders, which showed how much these plants differed from each other. So it is with the plants and insects on small and uniform islets; and so in small ponds of fresh water. Farmers find that they can raise most food by a rotation of plants belonging to the most different orders; nature follows what may be called a simultaneous rotation. Most of the animals and plants which live close round any small piece of ground, could live on it (supposing it not to be in any way peculiar in its nature), and may be said to be striving to the utmost to live there; but, it is seen, that where they come into the closest competition with each other, the advantages of diversification of structure, with the accompanying differences of habit and constitution, determine that the inhabitants, which thus jostle each other most closely, shall as a general rule, belong to what we call different genera and orders."—p. 114.

The abundance of some forms, the rarity and final extinction of many others, and the consequent divergence of character or increase of difference among the surviving representatives are other consequences. As favored forms increase, the less favored must diminish in number, for there is not room for all; and the slightest advantage, at first probably inappreciable to human observation, must decide which shall prevail and which must perish, or be driven to another and for it more favorable locality.

We cannot do justice to the interesting chapter upon natural selection by separated extracts. The following must serve to show how the principle is supposed to work.

"If during the long course of ages and under varying conditions of life, organic beings vary at all in the several parts of their organization, and I think this cannot be disputed; if there be, owing to the high geometrical powers of increase of each species, at some age, season, or year, a severe struggle for life, and this certainly cannot be disputed; then, considering the infinite complexity of the relations of all organic beings to each other and to their conditions of existence, causing an infinite diversity in structure, constitution, and habits, to be advantageous to them, I think it would be a most extraordinary fact if no variation ever had occurred useful to each being's own welfare, in the same way as so many variations have occurred useful to man. But if variations useful to any organic being do occur, assuredly individuals thus characterized will have the best chance of being preserved in the struggle for life; and from the strong principle of inheritance they will tend to produce offspring similarly characterized. This principle of preservation, I have called, for the sake of brevity, Natural Selection."—pp. 126, 127.

"In order to make it clear how, as I believe, natural selection acts, I must beg permission to give one or two imaginary illustrations. Let us take the case of a wolf, which preys on various animals, securing some by craft, some by strength, and some by fleetness; and let us suppose that the fleetest prey, a deer for instance, had from any change in the country increased in numbers, or that other prey had decreased in numbers, during that season of the year when the wolf is hardest pressed for food. I can under such circumstances see no reason to doubt that the swiftest and slimmest wolves would have the best chance of surviving, and so be preserved or selected,—provided always that they retained strength to master their prey at this or at some other period of the year, when they might be compelled to prey on other animals. I can see no more reason to doubt this, than that man can improve the fleetness of his greyhounds by careful and methodical selection, or by that unconscious selection which results from each man trying to keep the best dogs without any thought of modifying the breed.

"Even without any change in the proportional numbers of the animals on which our wolf preyed, a cub might be born with an innate tendency to pursue certain kinds of prey. Nor can this be thought very improbable; for we often observe great differences in the natural tendencies of our domestic animals; one cat, for instance, taking to catch rats, another mice; one cat, according to Mr. St.John, bringing home winged game, another hares or rabbits, and another hunting on marshy ground and almost nightly catching woodcocks or snipes. The tendency to catch rats rather than mice is known to be inherited. Now, if any slight innate change of habit or of structure benefited an individual wolf, it would have the best chance of surviving and of leaving offspring. Some of its young would probably inherit the same habits or structure, and by the repetition of this process, a new variety might be formed which would either supplant or coexist with the parent-form of wolf. Or, again, the wolves inhabiting a mountainous district, and those frequenting the lowlands, would naturally be forced to hunt different prey; and from the continued preservation of the individuals best fitted for the two sites, two varieties might slowly be formed. These varieties would cross and blend where they met; but to this subject of intercrossing we shall soon have to return. I may add, that, according to Mr. Pierce, there are two varieties of the wolf inhabiting the Catskill Mountains in the United States, one with a light greyhound-like form, which pursues deer, and the other more bulky, with shorter legs, which more frequently attacks the shepherd's flocks."—pp. 90, 91.

We eke out the illustration here with a counterpart instance, viz., the remark of Dr. Bachman that "The deer that reside permanently in the swamps of Carolina are taller and longer-legged than those in the higher grounds."*

The limits allotted to this article are nearly reached, yet only four of the fourteen chapters of the volume have been touched. These, however, contain the fundamental principles of the theory and most of those applications of it which are capable of something like verification, relating as they do to phenomena now occurring. Some of our extracts also show how these principles are thought to have operated through the long lapse of the ages. The chapters from the sixth to the ninth inclusive are designed to obviate difficulties and objections, "some of them so grave that to this day," the author frankly says, he "can never reflect on them without being staggered." We do not wonder at it. After drawing what comfort he can from "the imperfection of

* Quadrupeds of America, ii, p. 239.

the geological record" (chap. 9), which we suspect is scarcely exaggerated, the author considers the geological succession of organic beings (chap. 10), to see whether they better accord with the common view of the immutability of species, or with that of their slow and gradual modification. Geologists must settle that question. Then follow two most interesting and able chapters on the geographical distribution of plants and animals, the summary of which we should be glad to cite; then a fitting chapter upon classification, morphology, embryology, &c., as viewed in the light of this theory, closes the argument; the fourteenth chapter being a recapitulation.

The interest for the general reader heightens as the author advances on his perilous way and grapples manfully with the most formidable difficulties.

To account, upon these principles, for the gradual elimination and segregation of nearly allied forms,—such as varieties, subspecies, and closely related or representative species,—also in a general way for their geographical association and present range, is comparatively easy, is apparently within the bounds of possibility, and even of probability. Could we stop here we should be fairly contented. But, to complete the system, to carry out the principles to their ultimate conclusion, and to explain by them many facts in geographical distribution which would still remain anomalous, Mr. Darwin is equally bound to account for the formation of genera, families, orders, and even classes, by natural selection. He does "not doubt that the theory of descent with modification embraces all the members of the same class," and he concedes that analogy would press the conclusion still farther; while he admits that "the more distinct the forms are, the more the arguments fall away in force." To command assent we naturally require decreasing probability to be overbalanced by an increased weight of evidence. An opponent might plausibly, and perhaps quite fairly, urge that the links in the chain of argument are weakest just where the greatest stress falls upon them.

To which Mr. Darwin's answer is, that the best parts of the testimony have been lost. He is confident that intermediate forms must have existed; that in the olden times when the genera, the families and the orders diverged from their parent stocks, gradations existed as fine as those which now connect closely related species with varieties. But they have passed and left no sign. The geological record, even if all displayed to view, is a book from which not only many pages, but even whole alternate chapters have been lost out, or rather which were never printed from the autographs of nature. The record was actually made in fossil lithography only at certain times and under certain conditions (i. e., at periods of slow subsidence and places of abundant sediment); and of these records all but the last volume is out of print; and of its pages only local glimpses have been obtained. Geologists, except Lyell, will object to this,—some of them moderately, others with vehemence. Mr. Darwin himself admits, with a candor rarely displayed on such occasions, that he should have expected more geological evidence of transition than he finds, and that all the most eminent palæontologists maintain the immutability of species.

The general fact, however, that the fossil fauna of each period as a whole is nearly intermediate in character between the preceding and the succeeding faunas, is much relied on. We are brought one step nearer to the desired inference by the similar "fact, insisted on by all palæontologists, that fossils from two consecutive formations are far more closely related to each other, than are the fossils of two remote formations. Pictet gives a well-known instance,—the general resemblance of the organic remains from the several stages of the chalk formation, though the species are distinct at each stage. This fact alone, from its generality seems to have shaken Professor Pictet in his firm belief in the immutability of species." (p. 335.) What Mr. Darwin now particularly wants to complete his inferential evidence is a proof that the same gradation may be traced in later periods, say in the tertiary, and between that period and the present; also that the later gradations are finer, so as to leave it doubtful whether the succession is one of species,—believed on the one theory to be independent, on the other, derivative,—or of varieties, which are confessedly derivative. The proof of the finer gradation appears to be forthcoming. Des Hayes and Lyell have concluded that many of the middle tertiary, and a large proportion of the later tertiary mollusca are specifically identical with living species; and this is still the almost universally prevalent view. But Mr. Agassiz states that, "in every instance where he had sufficient materials, he had found that the species of the two epochs supposed to be identical by Des Hayes and Lyell were in reality distinct, although closely allied species."* Moreover he is now satisfied, as we understand, that the same gradation is traceable not merely in each great division of the tertiary, but in particular deposits or successive beds, each answering to a great number of years; where what have passed unquestioned as members of one species, upon closer examination of numerous specimens exhibit differences which in his opinion entitle them to be distinguished into two, three, or more species. It is plain, therefore, that whatever conclusions can be fairly drawn from the present animal and vegetable kingdoms in favor of a gradation of varieties into species, or into what

* Proceedings of the American Academy of Arts and Sciences, iv, p. 178.

may be regarded as such, the same may be extended to the tertiary period. In both cases, what some call species others call varieties; and in the later tertiary shells this difference in judgment affects almost half of the species!

We pass to a second difficulty in the way of Mr. Darwin's theory; to a case where we are perhaps entitled to demand of him evidence of gradation like that which connects the present with the tertiary mollusca. Wide, very wide is the gap, anatomically and physiologically (we do not speak of the intellectual) between the highest quadrumana and man; and comparatively recent, if ever, must the line have bifurcated. But where is there the slightest evidence of a common progenitor? Perhaps Mr. Darwin would reply by another question: where are the fossil remains of the men who made the flint knives and arrow-heads of the Somme valley?

We have a third objection, one, fortunately, which has nothing to do with geology. We can only state it here, in brief terms. The chapter on hybridism is most ingenious, able, and instructive. If sterility of crosses is a special, original arrangement to prevent the confusion of species by mingling, as is generally assumed, then, since varieties cross readily and their offspring is fertile *inter se*, there is a fundamental distinction between varieties and species. Mr. Darwin therefore labors to show that it is not a special endowment, but an incidental acquirement. He does show that the sterility of crosses is of all degrees;—upon which we have only to say, *Natura non facit saltum*, here any more than elsewhere. But, upon his theory he is bound to show how sterility might be acquired, through natural selection or through something else. And the difficulty is, that, whereas individuals of the very same blood tend to be sterile, and somewhat remoter unions diminish this tendency, and when they have diverged into two varieties the cross-breeds between the two are more fertile than either pure stock,—yet when they have diverged only one degree more the whole tendency is reversed, and the mongrel is sterile, either absolutely or relatively. He who explains the genesis of species through purely natural agencies should assign a natural cause for this remarkable result; and this Mr. Darwin has not done. Whether original or derived, however, this arrangement to keep apart those forms which have, or have acquired (as the case may be) a certain moderate amount of difference, looks to us as much designed for the purpose, as does a ratchet to prevent reverse motion in a wheel. If species have originated by divergence, this keeps them apart.

Here let us suggest a possibly attainable test of the theory of derivation, a kind of instance which Mr. Darwin may be fairly asked to produce,—viz., an instance of two varieties, or what may be assumed as such, which have diverged enough to reverse the movement, to bring out some sterility in the crosses. The best marked human races might offer the most likely case. If mulattoes are sterile or tend to sterility, as some naturalists confidently assert, they afford Mr. Darwin a case in point. If, as others think, no such tendency is made out, the required evidence is wanting.

A fourth and the most formidable difficulty is that of the production and specialization of organs.

It is well said that all organic beings have been formed on two great laws; Unity of type, and Adaptation to the conditions of existence.* The special teleologists, such as Paley, occupy themselves with the latter only; they refer particular facts to special design, but leave an overwhelming array of the widest facts inexplicable. The morphologists build on unity of type, or that fundamental agreement in the structure of each great class of beings, which is quite independent of their habits or conditions of life; which requires each individual "to go through a certain formality," and to accept, at least for a time, certain organs, whether they are of any use to him or not. Philosophical minds form various conceptions for harmonizing the two views theoretically. Mr. Darwin harmonizes and explains them naturally. Adaptation to the conditions of existence is the result of Natural Selection; Unity of type, of unity of descent. Accordingly, as he puts his theory, he is bound to account for the origination of new organs, and for their diversity in each great type, for their specialization, and every adaptation of organ to function and of structure to condition, through natural agencies. Whenever he attempts this he reminds us of Lamarck, and shows us how little light the science of a century devoted to structural investigation has thrown upon the mystery of organization. Here purely natural explanations fail. The organs being given, natural selection may account for some improvement; if given of a variety of sorts or grades, natural selection might determine which should survive and where it should prevail.

On all this ground the only line for the theory to take is to make the most of gradation and adherence to type as suggestive of derivation, and unaccountable upon any other scientific view, —deferring all attempts to explain *how* such a metamorphosis was effected, until naturalists have explained *how* the tadpole is metamorphosed into a frog, or one sort of polyp into another. As to *why* it is so, the philosophy of efficient cause, and even the whole argument from design, would stand, upon the admission of such a theory of derivation, precisely where they stand without it. At least there is, or need be, no ground of differ-

* Owen adds a third, viz:—Vegetative Repetition; but this, in the vegetable kingdom is simply Unity of Type.

ence here between Darwin and Agassiz. The latter will admit, with Owen and every morphologist, that hopeless is the attempt to explain the similarity of pattern in members of the same class by utility or the doctrine of final causes. "On the ordinary view of the independent creation of each being, we can only say that so it is, that it has so pleased the Creator to construct each animal and plant." Mr. Darwin, in proposing a theory which suggests a *how* that harmonizes these facts into a system, we trust implies that all was done wisely, in the largest sense designedly, and by an Intelligent First Cause. The contemplation of the subject on the intellectual side, the amplest exposition of the Unity of Plan in Creation, considered irrespective of natural agencies, leads to no other conclusion.

We are thus, at last, brought to the question; what would happen if the derivation of species were to be substantiated, either as a true physical theory, or as a sufficient hypothesis? What would come of it? The enquiry is a pertinent one, just now. For, of those who agree with us in thinking that Darwin has not established his theory of derivation, many will admit with us that he has rendered a theory of derivation much less improbable than before; that such a theory chimes in with the established doctrines of physical science, and is not unlikely to be largely accepted long before it can be proved. Moreover, the various notions that prevail,—equally among the most and the least religious,—as to the relations between natural agencies or phenomena and Efficient Cause, are seemingly more crude, obscure, and discordant than they need be.

It is not surprising that the doctrine of the book should be denounced as atheistical. What does surprise and concern us is, that it should be so denounced by a scientific man, on the broad assumption that a material connection between the members of a series of organized beings is inconsistent with the idea of their being intellectually connected with one another through the Deity, i. e., as products of one mind, as indicating and realizing a preconceived plan. An assumption the rebound of which is somewhat fearful to contemplate, but fortunately one which every natural birth protests against.

It would be more correct to say, that the theory in itself is perfectly compatible with an atheistic view of the universe. That is true; but it is equally true of physical theories generally. Indeed, it is more true of the theory of gravitation, and of the nebular hypothesis, than of the hypothesis in question. The latter merely takes up *a particular, proximate cause*, or set of such causes, from which, it is argued, the present diversity of species has or may have *contingently* resulted. The author does not say *necessarily* resulted; that the actual results in mode and measure, and none other must have taken place. On the other hand the theory of gravitation, and its extension in the nebular hypothesis, assume a *universal and ultimate* physical cause, from which the effects in nature must *necessarily* have resulted. Now it is not thought, at least at the present day, that the establishment of the Newtonian theory was a step towards atheism or pantheism. Yet the great achievement of Newton consisted in proving that certain forces, (blind forces, so far as the theory is concerned,) acting upon matter in certain directions, must *necessarily* produce planetary orbits of the exact measure and form in which observation shows them to exist;—a view which is just as consistent with eternal necessity, either in the atheistic or the pantheistic form, as it is with theism.

Nor is the theory of derivation particularly exposed to the charge of the atheism of fortuity; since it undertakes to assign real causes for harmonious and systematic results. But of this, a word at the close.

The value of such objections to the theory of derivation may be tested by one or two analogous cases. The common scientific as well as popular belief is that of the original, independent creation of oxygen and hydrogen, iron, gold, and the like. Is the speculative opinion, now increasingly held, that some or all of the supposed elementary bodies are derivative or compound, developed from some preceding forms of matter, irreligious? Were the old alchemists atheists as well as dreamers in their attempts to transmute earth into gold? Or, to take an instance from force (power),—which stands one step nearer to efficient cause than form—was the attempt to prove that heat, light, electricity, magnetism, and even mechanical power are variations or transmutations of one force, atheistical in its tendency? The supposed establishment of this view is reckoned as one of the greatest scientific triumphs of this century.

Perhaps, however, the objection is brought, not so much against the speculation itself, as against the attempt to show how derivation might have been brought about. Then the same objection applies to a recent ingenious hypothesis made to account for the genesis of the chemical elements out of the etherial medium, and to explain their several atomic weights and some other characteristics by their successive complexity,—hydrogen consisting of so many atoms of etherial substance united in a particular order, and so on. The speculation interested the philosophers of the British Association, and was thought innocent, but unsupported by facts. Surely Mr. Darwin's theory is none the worse, morally, for having some foundation in fact.

In our opinion, then, it is far easier to vindicate a theistic character for the derivative theory, than to establish the theory itself upon adequate scientific evidence. Perhaps scarcely any philosophical objection can be urged against the former to which the nebular hypothesis is not equally exposed. Yet the nebular hypothesis finds general scientific acceptance, and is adopted as the basis of an extended and recondite illustration in Mr. Agassiz's great work.*

How the author of this book harmonizes his scientific theory with his philosophy and theology, he has not informed us. Paley, in his celebrated analogy with the watch, insists that if the time-piece were so constructed as to produce other similar watches, after the manner of generation in animals, the argument from design would be all the stronger. What is to hinder Mr. Darwin from giving Paley's argument a further *a-fortiori* extension to the supposed case of a watch which sometimes produces better watches, and contrivances adapted to successive conditions, and so at length turns out a chronometer, a town-clock, or a series of organisms of the same type? From certain incidental expressions at the close of the volume, taken in connection with the motto adopted from Whewell, we judge it probable that our author regards the whole system of nature as one which had received at its first formation the impress of the will of its Author, foreseeing the varied yet necessary laws of its action throughout the whole of its existence, ordaining when and how each particular of the stupendous plan should be realized in effect, and—with Him to whom to will is to do—in ordaining doing it. Whether profoundly philosophical or not, a view maintained by eminent philosophical physicists and theologians, such as Babbage on the one hand and Jowett on the other, will hardly be denounced as atheism. Perhaps Mr. Darwin would prefer to express his idea in a more general way, by adopting the thoughtful words of one of the most eminent naturalists of this or any age, substituting the word *action* for 'thought,' since it is the former (from which alone the latter can be inferred) that he has been considering. "Taking nature as exhibiting thought for my guide, it appears to me that while human thought is consecutive, Divine thought is simultaneous, embracing at the same time and forever, in the past, the present and the future, the most diversified relations among hundreds of thousands of organized beings, each of which may present complications again, which, to study and understand even imperfectly,—as for instance man himself—mankind has already spent thousands of years."† In thus conceiving of the Divine Power in act as cöetaneous with Divine Thought, and of both as far as may be apart from the human element of time, our author may regard the intervention of the Creator either as, humanly speaking, *done from all time*, or else as *doing through all time*. In the ultimate analysis we suppose that every philosophical theist must adopt one or the other conception.

* Contrib. Nat. Hist. Amer., i, p. 127–131.

† Op. cit., p. 130.

A perversion of the first view leads towards atheism, the notion of an eternal sequence of cause and effect, for which there is no first cause,—a view which few sane persons can long rest in. The danger which may threaten the second view is pantheism. We feel safe from either error, in our profound conviction that there is order in the universe; that order presupposes mind; design, will; and mind or will, personality. Thus guarded, we much prefer the second of the two conceptions of causation, as the more philosophical as well as Christian view,—a view which leaves us with the same difficulties and the same mysteries in Nature as in Providence, and no other. Natural law, upon this view, is the human conception of continued and orderly Divine action.

We do not suppose that less power, or other power, is required to sustain the universe and carry on its operations, than to bring it into being. So, while conceiving no improbability of "interventions of Creative mind in nature," if by such is meant the bringing to pass of new and fitting events at fitting times, we leave it for profounder minds to establish, if they can, a rational distinction in kind between His working in nature carrying on operations, and in initiating those operations.

We wished under the light of such views, to examine more critically the doctrine of this book, especially of some questionable parts;—for instance, its explanation of the natural development of organs, and its implication of a "necessary acquirement of mental power" in the ascending scale of gradation. But there is room only for the general declaration that we cannot think the Cosmos a series which began with chaos and ends with mind, or of which mind is a result: that if by the successive origination of species and organs through natural agencies, the author means a series of events which succeed each other irrespective of a continued directing intelligence,—events which mind does not order and shape to destined ends,—then he has not established that doctrine, nor advanced towards its establishment, but has accumulated improbabilities beyond all belief. Take the formation and the origination of the successive degrees of complexity of eyes as a specimen. The treatment of this subject (pp. 188, 189), upon one interpretation is open to all the objections referred to; but if, on the other hand, we may rightly compare the eye "to a telescope, perfected by the long continued efforts of the highest human intellects," we could carry out the analogy, and draw satisfactory illustrations and inferences from it. The essential, the directly intellectual thing is the making of the improvements in the telescope or the steam-engine. Whether the successive improvements, being small at each step, and consistent with the general type of the instrument, are applied to some of the individual machines, or entire new machines are constructed for each, is a minor matter. Though if machines could engender, the adaptive method would be most economical; and economy is said to be a paramount law in nature. The origination of the improvements, and the successive adaptations to meet new conditions or subserve other ends, are what answer to the supernatural, and therefore remain inexplicable. As to bringing them into use, though wisdom foresees the result, the circumstances and the natural competition will take care of that, in the long run. The old ones will go out of use fast enough, except where an old and simple machine remains still best adapted to a particular purpose or condition,—as, for instance, the old Newcomen engine for pumping out coal-pits. If there's a Divinity that shapes these ends, the whole is intelligible and reasonable; otherwise, not.

We regret that the necessity of discussing philosophical questions has prevented a fuller examination of the theory itself, and of the interesting scientific points which are brought to bear in its favor. One of its neatest points, certainly a very strong one for the local origination of species, and their gradual diffusion under natural agencies, we must reserve for some other convenient opportunity.

The work is a scientific one, rigidly restricted to its direct object; and by its science it must stand or fall. Its aim is, probably not to deny creative intervention in nature,—for the admission of the independent origination of certain types does away with all antecedent improbability of as much intervention as may be required,—but to maintain that Natural Selection in explaining the facts, explains also many classes of facts which thousand-fold repeated independent acts of creation do not explain, but leave more mysterious than ever. How far the author has succeeded, the scientific world will in due time be able to pronounce.

As these sheets are passing through the press a copy of the second edition has reached us. We notice with pleasure the insertion of an additional motto on the reverse of the title-page, directly claiming the theistic view which we have vindicated for the doctrine. Indeed these pertinent words of the eminently wise Bishop Butler, comprise, in their simplest expression, the whole substance of our latter pages:—

"The only distinct meaning of the word 'natural' is *stated*, *fixed*, or *settled;* since what is natural as much requires and presupposes an intelligent mind to render it so, i. e., to effect it continually or at stated times, as what is supernatural or miraculous does to effect it for once."

A. G.

Art. XVI.—*Forces;* by Theodore Lyman.

The first article in this Journal for November last* brings to mind the singular part which "force" now plays in science. The theory set forth in that article may be stated as follows: the world, and everything on it, may be considered as *matter;* this matter is not the same throughout, but consists of a certain number of ultimate species called *elements;* these elements are not always isolated, but are found joined to form, 1st, simple compounds, known sometimes as *minerals;* 2d, compounds of a nature higher, more complicated, and differently characterized, known as *vegetables;* 3d, compounds still higher and more complicated, and again differently characterized, known as *animals.* As the elements do not remain isolated, so also their compounds continually change their mutual relations; and the result of these changes is that continual falling down and building up which may be seen in the material world. To move these elements and their compounds there is a fund of *force*, constant in quantity and in quality; if ever it seems to be less in quantity, some of it is latent; if ever it seems different in quality, it is but changed in appearance, from being connected with some peculiar compound. Here is Cosmos at a glance!—there is the force, the mover, a; and these are the elements, the things moved, b, c, d, e, &c.—a may be a' (mechanical force), or a'' (chemical force), or a''' (vegetable force), &c., but still it remains a. a may act on b, c, d, e, and there may result such compounds as bc, ceb, dec, &c. When a joins b to c, a part of a becomes *latent*, and the result may be called $bc+a$; but, when this compound is decomposed by a *different form* of a (e. g. a'''' or light) then a latent is *set free*, and immediately takes b and joins it to d, e, making the *higher compound* bde, while c is set free as an element. To give an instance, if b is carbon, c hydrogen, and d oxygen, and a''' is vegetable force, then b, c, and d, joined by the action of a''', would be the compound bcd, and might be a turnip. This theory looks simple, but its very roundness is suspicious.

The human mind, craving something more than mere fact, has tried to get at the *reason.* The fact is the law, the reason is the cause. It is in the search for the latter that scientific men have fallen on that unfortunate word, that shadow of a shadow, that last resort of ignorance—*Force!* It is safe to say that no

* The instructive and ingenious essay, by Prof. Joseph LeConte, is quoted in no invidious spirit, but simply as a fair sample of a philosophy now very common. Prof. LeConte may contend that he uses the word "force" only as a convenient supposition, on which to build a theory; if this be the case, it should be remembered that a form of expression which is sure to mislead ninety-nine readers in every hundred, should not be used in scientific writing.

word in the English language has created so much ambiguity, so much meaningless discussion, and so much wrong-headed philosophy, as this ill begotten monosyllable. It is a gag for inquisitive people. What keeps the world from flying off in space? The force of attraction. What keeps it from rushing towards the sun? The centrifugal force. What makes oxyd of lead join acetic acid? The chemical force. What makes oxyd of lead *leave* acetic acid and go to sulphuric acid? The chemical force again. What makes a fish with a ventral fin? The vital force. What makes a fish *without* a ventral fin? The vital force again. Mark this! These forces are blind and they are the same always, yet they make different things, on the one hand, while on the other, they repeat the same thing over and over again. How is this? Can a round auger bore a square hole? Or, can it bore any hole at all without *intellect* to guide it? If several forces are not enough to account for the phenomena of the world, what can we expect of *one* force? Yet to this Prof. LeConte would reduce us. His "correlation of forces" is one force; and his "conservation of forces" is the amount of this force. The *amount of force*, acting on the *sum* of the *material elements*, produces all motion and all being. The force is spoken of as "latent," "set free," "liberated," and "developed," and as being "furnished" and "supplied," by decomposition.

Force is either something or nothing, if it be nothing, it is not worthy a place in science; if it be something it may be considered like any other fact of philosophy. What, then, is force and whence comes our idea of it? To answer these questions it is necessary to define, 1st, true causation; 2d, variable sequences; 3d, invariable sequences. The operations of my own will are the only instances of true causation of which I am conscious. The will, itself immaterial, works either on the material body, or on the immaterial mind. When the body is free and healthy the will has direct control over the voluntary muscles, but none over the involuntary. On the other hand, when the mind is free and healthy, the will has direct control over the whole of it. Whenever I will to do an act, mental or physical, I am *conscious* of using power. *Causation, then, is the act of x, using power to produce y.*

We may, in the outer world, see one thing happen immediately after another; yet, in a little while, we may see the first thing again, but this time *not* followed by the second. In such a case we say that the fact, that one thing followed another, was an accident. For instance, twelve mines of powder may explode, one after another, but probably this will not happen so again. *A variable sequence, then, is the fact that y has followed x, but will not, of necessity, do so again.*

We may, in the outer world, see a particular thing, which, as far as our experience goes, is always followed by another particular thing; for example, the twelve mines above mentioned are fired by electricity, and, as far as we know, a certain amount of electric heat in contact with gunpowder is followed by an explosion. *An invariable sequence, then, is the fact, that y always follows x.* To sum up; a man, by conscious exercise of will, discharged the battery (*efficient causation*); the electric spark was followed by an explosion of gunpowder (*invariable sequence*); certain other mines happened to explode, after this one, in quick succession (*variable sequence*).

The admission of universal, invariable sequence (i. e., every x is invariably followed by its peculiar y) is the ground-principle of the theory of necessity, which leads directly to pantheistic atheism, with its idealistic and materialistic branches. The sole object of science thus becomes, to find out all the xs in the universe and all the ys, and to give to each x its proper y. All these, when tabulated so as to show their fixed relations to each other, make up the course of nature, and human knowledge has then done its utmost and has nothing left to work on. This course of nature, if admitted as entirely true, may still be looked at in two ways: 1st, it may be said that this course is such as it is by reason of an *essence* which exists in matter; or, 2d, it may be stated, that the course is such as it is, and that no reason therefor can be given, or ought to be given. Such views as these might be allowable if all our knowledge were got from observations on the *outer* world, and without reference to our own consciousness of the structure of our minds; for we could not prove the existence of *power*, in the phenomena of the outer world; and, if we suspected it, we might still set it down as an inherent property of matter. But the moment *consciousness of self* is brought into the question, a new element is introduced, namely, *intelligence.* Consciousness is fundamental, and cannot be denied; consciousness of thought, that is intelligence, cannot be denied; consciousness of *power* has been denied, and with what truth? I am fully conscious, that I turn my mind to a given thought, and I sharply distinguish this act of *causation* from any *sequence*; here then is the immaterial will exercising *power* over the immaterial mind. If there is no such thing as power, whence comes the word, and whence the general idea of it? Again, I will to move some part of my body, and do move it, and I compare this power with that used in mental action, and I find them the same. There is this difference between the cases: in the first instance I move what is strictly myself, the unit, the immaterial; in the second instance I move what is only a frame, over which I have limited control; the compound, the material. And observe this; there is a difference between merely *wishing* and the act of *exerting power consciously.* I may wish that my legs would, all at once, walk off with my body;

but, if this should take place, I should be conscious that I was not exerting the power. Again, if, at my mere wish, a great rock should come to me, I should be conscious that something else moved it, and not myself. And, if my power of will was so extended, that I could thereby move rocks, I should be just as conscious of that power as I now am of the power to move my own body. A wish, followed by the thing wished for, is a sequence; a command, followed by the thing commanded, is power or causation. It is therefore wrong to say, that such sequences, if observed many times, would give me the idea that *I* exercised the power. When ignorant people attribute true causation to material bodies, (e. g., in the case of electricity,) they do so because they have the consciousness of power in themselves, and they attribute the same property to a body, which has in itself no power, but which shows signs of being acted upon by a power. If now it be said that matter has a certain essence, property, or what not, which makes it to do certain things at certain times, what is this essence or property but *power?* From our very idea of the word, anything that produces motion in another thing, either has or uses power. Still, though there may be power, is it *intelligent?* or do there exist a number of *un*intelligent powers, which so limit the scope of each other's action as to make a balance in the course of nature? (e. g., the power of centrifugal force balanced by the power of gravitation; the power of oxygen, produced by plants, balanced by the carbon, produced by animals; the power of insects, balanced by the birds that feed on them; the power of fruits, balanced by the parasitic fungi that grow on them.) To such a query, science is able to make this answer: "The phenomena observed in the outer world (*non-ego*) correspond to the phenomena of self (*ego*), which are produced by *intellect*."* This is a sound induction; if it be not good, then no induction is good; and, if induction cannot stand, science must fall. To sum up, if it be admitted that there is an essence, inherent in matter, which necessitates its actions, then, 1st, this essence is *power;* and 2d, this power is *intelligent;* for, to deny that the essence was power would be to stultify our understanding, and, to deny this power to be intelligent, would be to ignore induction and to destroy science. It may be well also to take notice of the fact, that most theories, savoring of materialism, speak of some ultimate essence (force) which is at the bottom of all motion and action, as if such essence were fundamental and satisfactory; but this, after all, does not help the theory, for, if this essence is true matter, it moves, first *itself*, and then other matter, and, if it is a property, then this property moves first *itself* and then matter.

* For a full consideration of intellect, as shown in nature, see Essay on Classification, by Louis Agassiz.

There still remains the second way of looking at the course of nature: it is such as it is, and no reason therefor can be given. What is the exact meaning of this statement? Simply that the upholder of the doctrine resolves, so far as concerns the gaining of truth, to put trust in the testimony of his eyes, ears, touch, and other physical senses, and in nothing else. He is a strict positivist; he says: "I see x, feel y, and hear z; to me, then, x, y, and z exist as things seen, felt and heard, respectively. To me there is no causation, for I do not directly perceive it. I believe in memory, it tells me what I have formerly seen; I believe in the axiom, *cogito ergo sum*, because the very denial of thought implies the action of thought. My duty is to clearly understand and to tabulate all the observations I have made; but there it stops; I may make many inductions and may use them for convenience, but am not sure of them. One induction tells me the sun will rise to-morrow: for my convenience I say it will; but of this I am not sure; perhaps it will *not*:—it is a thing of the future and the future I cannot see. I attempt to explain nothing:—*you* attempt to explain, and you come to no certain results:—I am more modest. By my individual observation I know certain things, and all other things are to me only as what may be. The earth may, or may not, turn again on its axis; causation may, or may not, exist; there may, or may not, be a God."

The holder of this theory, when compared with the believers in kindred opinions, is, in a certain sense, consistent; because he only makes statements, which are true as far as they go; and does not attempt explanations, which must in the end prove unsatisfactory. But his theory is incomplete and therefore one-sided and untrue. On the one hand, he acknowledges consciousness, and through consciousness believes in all the phenomena of the *outer* world (*non-ego*) as isolated facts; while, on the other, he ignores the properties of *self* (*ego*) which are also given by consciousness, and are as reliable as any of its data. Thus, he acknowledges the motions of his voluntary muscles, of which he is conscious, but refuses to acknowledge his own power to move them, or to leave them at rest, of which he is also conscious. We need go no further than the idea of *intelligent power*, to find the whole trouble in this, and in many other theories.*

What interest does a true conception of the ever-working Creative Intellect give to science! This correspondence of the human with the Divine mind! The astronomer works out, with pencil and paper, the possible answers to a certain problem of

* For able presentations of the doctrines of free-will and of necessity, consult the writings of Prof. Francis Bowen, and of J. S. Mill. The idea of causation has been carried to its last analysis by Sir William Hamilton.

motion; he looks at the heavens, and there sees these answers, illustrated in the orbits of celestial bodies. The zoölogist, marking the changes of the embryo, thinks of these changes as so many different animals; deep in the rocks he finds all stages of this embryo, each represented by a species, perfect in its kind!

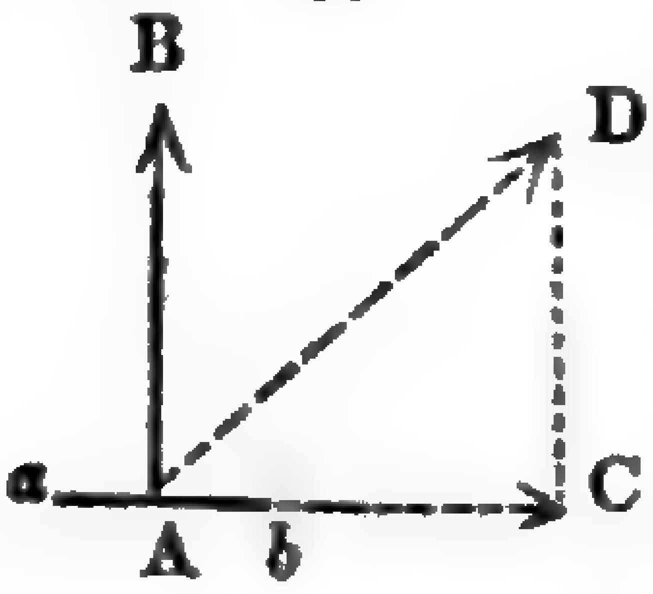
On the other hand, how dead the science, that puts "force" as its first cause! What is this force that makes the star-fish and the oyster, the medusa and the cuttle-fish, the crab and the whale, the tufted sea worm and the shark, each in its kind, and each telling its own story of manifold relations with animal creation, that is, that has been, and that is to come? Nature is no such simple thing that she should be dictated to by light, or heat, or electricity. These are her servants, not her masters!

Boston, Nov., 1859.

ART. XVII.—*On the causes of deviation in Elongated Projectiles;* by Maj. J. G. BARNARD, Corps of Engineers, U. S. A.

THE various and somewhat conflicting explanations given of the deviation of projectiles, both spherical and elongated, arising from their own rotary motions, leave room for a few additional words on this subject.

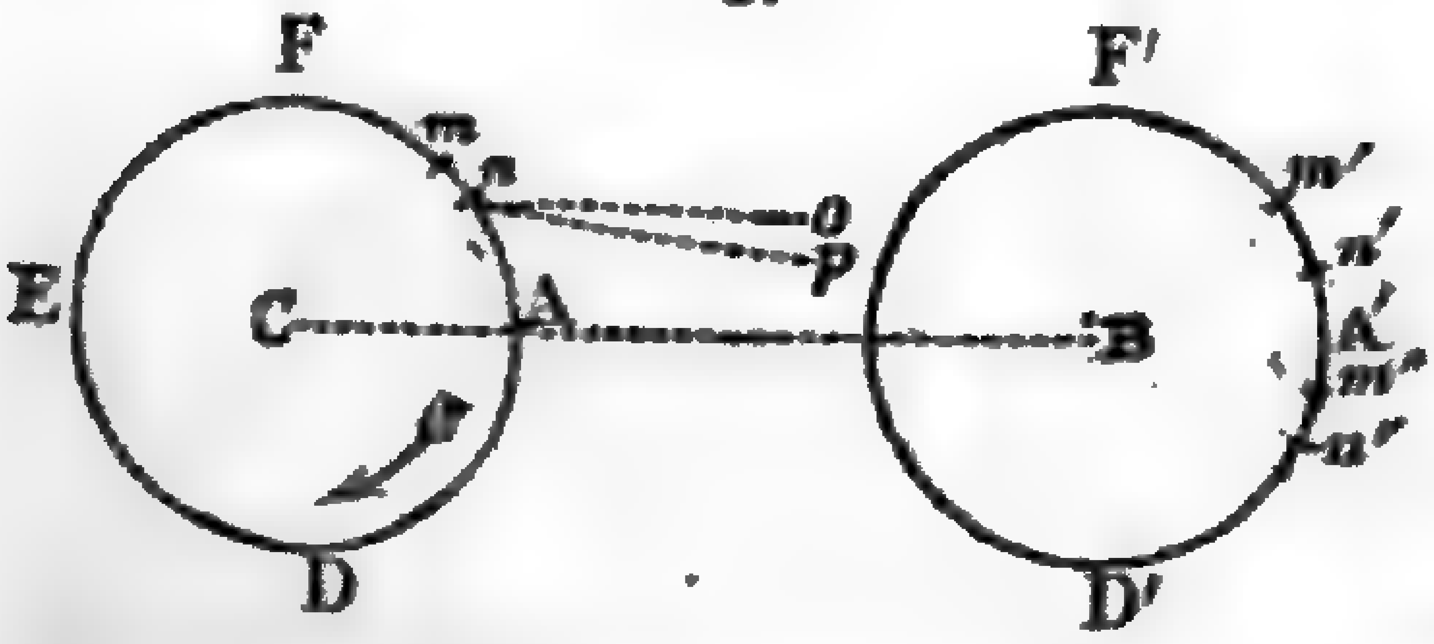
1.



If the plane surface ab moves, in an elastic medium in the direction of its normal, with a velocity A B, that medium will oppose a force to which we apply the term "resistance" and which is measured by a function of the velocity A B.

If, at the same time, the surface has a velocity A C, *in its own plane*, the result will be an actual velocity *of each point of the surface*, represented by the diagonal A D; but the velocity of impact of the surface with the air, is the same in both cases, being due only to the normal velocity A B. The motion A C in its own plane, would displace, in no degree, the atmospheric particles, (except through the agency of that action known as friction—not now considered,) and would therefore generate no component of "resistance."

2.



If the sphere, whose great circle is A D E F, move through the air in the direction C B, with a velocity V, a resistance will be opposed to its motion which will, in magnitude, be a function of the diameter, and of the velocity V. If we leave out of consideration the force of *friction*, the character and intensity of the impact of the sphere with the air will be identically the same, whether it possesses or not rotary motion: for in either case, the surface, considered *as a whole*, advances in identically the same manner—the displacement of atmospheric particles is the same, and the resulting resistance, the same.

Let the rotation be supposed about a horizontal axis, perpendicular to the line of flight, and in the direction A D. The velocity of the *individual points*, *m*, *n*—or if you choose—*elementary surface*, *m n*, will be the resultant of the rotary and translative velocities, and the little surface *m n*, instead of moving (at the instant) in the direction *n o*, will move in an oblique direction *n p*. But the *rotary* component of velocity lies in the plane of this elementary surface, and has, (as in the case of the *lateral* velocity A C of the plane, Fig. 1) no agency whatever in displacing the air, or in affecting the intensity or character of its impact.

These considerations will, perhaps, be rendered more clear by reflecting that the resistance of a fluid, is due and *due only* to the *displacement of its particles*—that when the centre of the sphere has advanced from C to B, the anterior surface has advanced from F A D to F′ A′ D′, and displaced the air in identically the same manner, whether the sphere revolves or not.

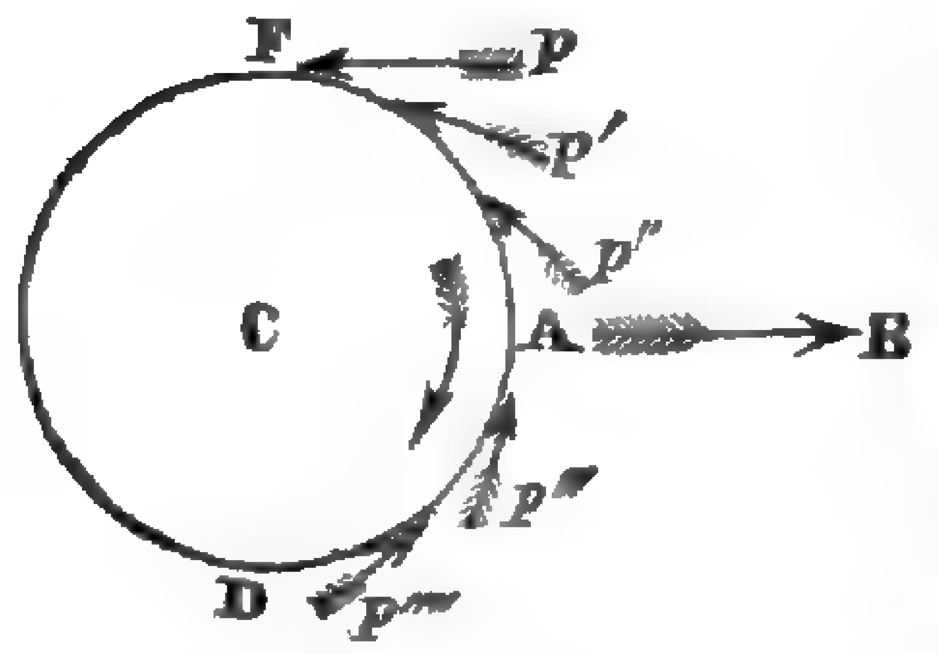
These considerations are so obvious that it seems superfluous to insist on them; yet few of the writers on this subject have exhibited a clear understanding of them; or rather it may be said that they exhibit the reverse.

Thiroux, rejecting friction entirely, or rather considering its effects inappreciable, bases his reasoning on the higher velocity with which the points of the surface on the side A F *impinge on the air*, over that belonging to points on the side A D; an idea, as just shown, entirely fallacious. Capt. Neumann (Prussian artillery), in a theory as pretentious as it is unmeaning (Delobel's *Révue de Technologie Militaire*, vol. i.), carries this absurdity to the extreme of considering each elementary surface *m n*, *separately*, with its combined motion of translation and rotation, and, applying to each the ordinary expression for resistance of a plane surface impinging obliquely upon an elastic medium, *integrates* through each half of the anterior surface, to obtain the total action on each side.

Not only are the conventional expressions for the resistance of isolated oblique plane surfaces found most inaccurate in practice, but they lose *all* applicability when they cease to be *isolated*, and form part of another larger surface (not plane); and this problem, of which the knot is so expertly cut by Capt. Neumann, who proceeds to apply his results to the criticism or *test* of Magnus' and other theories, is the very "pièce de résistance" which has defied the analysis of d'Alembert, Poisson and Poncelet—perhaps I might add of Newton and Laplace; one of those problems of mechanics to which the term *difficult* would be misapplied, for analysis has never yet been able to grasp it at all.

I have said that without the consideration of *friction*, the action upon the air of a rotating and non-rotating ball are identically the same. But *friction* materially alters the character of this action. Whatever may be the immediate cause of this force—whether simply a collision of the inequalities of the surface with the particles of the fluid—or whether it is due to *adhesion*, the effect is that the moving surface puts in motion *with it*, the adjacent fluid particles, and in so doing, developes forces tangential and opposed to its own motion.

3.

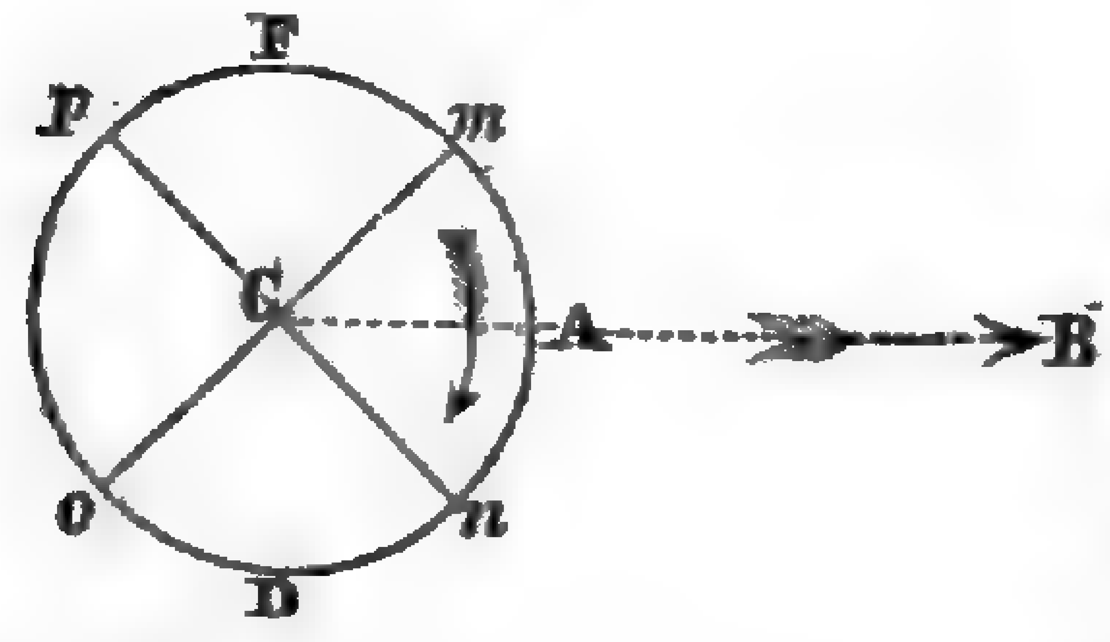


Thus the anterior surface of the sphere C, revolving from F to D, and advancing from A to B, creates, at each point, forces, p, p', p'' &c., tangential and opposed to its rotary motion, the resultant of which is a force acting from D towards F and tending to deflect the flight of the ball in that direction. This is the point of view, *and the sole one*, in which Poisson has considered the effects of friction.

But there is *another* effect which proves to be very powerful. Force cannot be applied to an elastic fluid, neither can motion be imparted or destroyed, without effecting, at the same time, its density and *pressure*. To retard a flowing current is to increase its pressure; to accelerate it is to diminish the same.

Applying this to the ball, the air, displaced and compressed in front, escapes along the surfaces A F and A D. Near its surface, the action of friction is to retard the escaping currents on the side A F, and to accelerate them on the side A D, and in consequence, an increase of pressure ensues on the side A F, and a diminution on the side A D; and therefore, a resulting pressure tending to deflect the ball from F towards D. If we divide the great circle A D F into four quadrants by the lines $m\,o$ and $n\,p$, drawn at angles of 45° with the direction of translation A B, we may better analyze the effects of friction, in the two forms in which I have presented them.

4.



The posterior quadrant $o\,p$ is in air so highly rarified that its action is insensible or nearly so.* On the side quadrant $m\,p$ the resultant of the forces of friction (the forces p, p', &c., of Fig. 3) are parallel (or nearly so) and *opposed* to the motion of translation. They have no effect (or but trifling) to *deflect* the ball from its course, but acting upon the air, in direct opposition to

* The *high velocities* of translation of military projectiles is supposed.

the escaping currents, their whole force is expended in destroying velocity and generating *pressure.* On the anterior quadrant $m\,n$ the resultant of the forces p, p', &c., is from n towards m, and tends, almost entirely, to deflect the ball in that direction.

On the quadrant $n\,o$ the resultant of the forces p, p', is parallel to the motion of translation, and co-incident in direction with the escaping current whose motion it accelerates and whose *pressure* it diminishes. Thus, taking the four quadrants, in *one*, $o\,p$, the forces of friction are absent; in *two*, $m\,p$ and $n\,o$, they are expended in producing an *inequality of pressure* on the two sides of the ball, tending to deflect the ball towards the side D (right); while in the anterior quadrant $m\,n$, they act to deflect the ball in the opposite direction F (left).

It would have been difficult to decide *a priori*, which of these forces would prevail, though, while the force of friction is nugatory in *one* quadrant, in two ($m\,p$ and $o\,n$) it expends itself in developing forces tending to deflect to the right, and in only *one*, $m\,n$, does its direct action tend to deflect to the left; yet it must be remarked that in this quadrant the air is most dense, friction the greatest, and that it acts *directly* upon the projectile. In the two lateral quadrants the air is less dense, and it is only through pressures *developed in the air* that it produces its effect; a loss of effect ensuing in the medium through which it acts.

Experience has shown, however, that the forces developed in the two lateral quadrants prevail, and the projectile is deflected to the right; and the experiments of Dr. Magnus give the same result when, instead of a projectile moving through the air, a *current* of air is directed upon a revolving cylinder.*

The deviation of elongated projectiles, having rotary motion about their axis of figure, though many authors, Thiroux, Panôt, Tamissier, &c., have attempted to refer it to the same causes which produce the deviation in spherical balls, is evidently governed by other causes.

Not only do such writers have to make, as to the direction which the axis maintains, assumptions which conflict with each

* Of course the division into quadrants which I have made is arbitrary, and only used as a simple means of illustrating how the conflicting effects are produced from one and the same *cause.* I comprehend under the term *friction*, all the forces by which a solid *surface* acts upon a fluid flowing along it, whether by adhesion or simple mechanical collision of particles; and this is the usual meaning of the word in this connection. Poisson has considered friction only in its direct action, i. e., as the resultant of the forces p, p', &c., of Fig. 3, and, deducing therefrom a deviation to the left, he arrives at the conclusion that the magnitude of the force was not sufficient to account for the *amount* of observed deviation (irrespective, I presume, of *direction*). If this is true, it is difficult to conceive that the force (overlooked by him) arising from developed *pressures* (and which I show to have its origin in friction), is sufficient to annihilate the *direct* effects of friction itself, and to produce the deviations beside. Nevertheless it is all we can account for, and the experiments of Magnus seem to indicate its adequacy.

other,* but the causes they assign are inadequate, and, moreover, as the deviation of an elongated ball depends more on the direction in which its axis happens to be deflected than upon the direction of the disturbing forces, no such uniformity in the deviations as is observed, would be produced, were friction on the inequalities of pressure the governing causes.†

There is another cause which *necessarily* operates in this case; a cause which long before I knew of the experiments of Dr. Magnus, I conceived to be the predominating one.

The gyroscope shows, to the apprehension of every one, that a rotating solid of revolution supported by a point in its axis, about which it is free to move, solicited by a force tending to turn it in any direction, turns, *not* in the direction of the force which solicits it, but, with uniform and slow motion, normally to it; that the axis of figure will describe a *cone* about a line passing through the point of support, parallel to the direction of the soliciting force.

The elongated projectile is discharged from the piece with its axis coinciding with its trajectory, but, through the action of gravity, the trajectory deflects from its original direction and from that of the axis.‡ In consequence of this, the resistance of the air acts obliquely to the axis, and, with the ordinary forms of elongated projectiles, without grooves, its resultant passes in front and *above* the centre of inertia, tending to *raise* the point,

* Thiroux's theory requires that in the descending branch of the trajectory, the point of the ball shall be depressed *below* the trajectory; Panot's, that it shall continue parallel to its original direction, and hence elevated *above* the trajectory. Dr. Magnus supposes the axis to keep pretty nearly coincident with the trajectory, and says that experiments made with balls fired at low velocities confirm this assumption.

† To illustrate my meaning, take the friction theory of Panôt which supposes the axis elevated above the trajectory and that the friction on the under side (where the air is *densest*) carries it to the *right;* yet were this ball a hollow one (as nearly all are) the tail of the ball would be so much lighter than the point that the forces of friction (equal all along the cylindrical surface) would affect the tail more than the point and thus deflect the axis to the *left*, and produce a deviation in that direction.

5.

‡ *Which preserves its original direction through the stability it derives from its axial rotation*, is the unmeaning and false expression (or something equivalent) commonly used in this connection. Properly *there is no such thing* as stability of the axis. Take away the possibility of this *conical* motion (in precession) just described, *confine the axis to one plane*, and it becomes as movable as if no rotation existed. This is clearly established in the Analysis of the Gyroscope (this Journal, [2], xxiv, 49), where the following language is used, "the popular idea that a rotating body offers any direct resistance to a change of plane, is unfounded. It requires as little exertion of force (in the direction of motion) to move it from one plane to another, as if no rotation existed." The "surprising phenomena" of "complete mobility" when one of the rings of the gyroscope is so held that the axis is confined to one plane of motion, which Dr. Magnus arrives at *experimentally* (and all the modifications of his experiments) flow directly from my analysis. *If, then*, stability of axial direction is attributed to axial rotation in projectiles, that which is the *inseparable accompaniment* of this *apparent* stability (i. e. the conical motion described in the text), and without which there is nothing even *resembling* stability—must be accepted with it, and the *deviation* of the projectile, which will *certainly result therefrom*.

and from this results the conical motion of the axis to the *right*, if the rotation is to the right—to the *left* in the contrary case.*

Before attempting to apply the foregoing to the real case, it will help us to consider a more simple one.

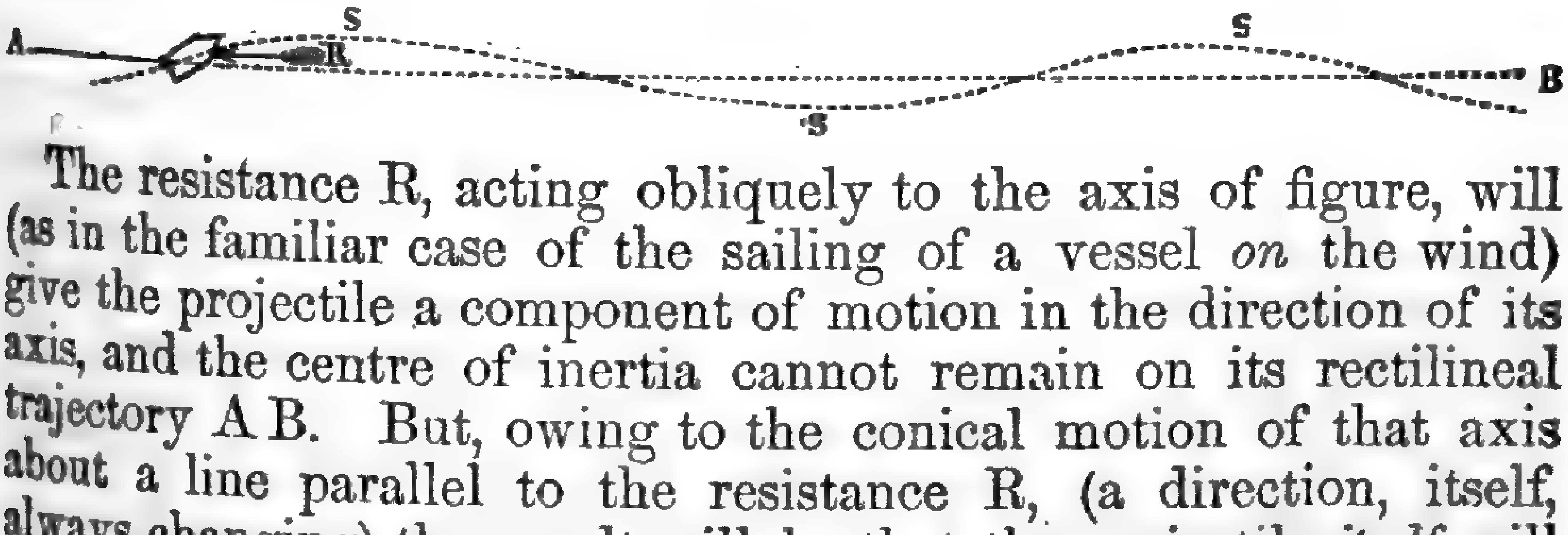
Suppose the rotating projectile with its axis inclined to its trajectory to be propelled through the air, and that the centre of inertia is *confined* to a rectilinear path (as if it slid along an extended wire, A B, for example), and that the force of gravity does not act.

6.

The resistance of the air, R, acting *immediately* to increase the angle of inclination, produces *instead* the resulting conical motion to the right, which, combined with the motion of translation, would cause the *point* to describe a helix *s s s* about the rectilinear path of the centre of inertia.

Let now all the circumstances be as above *except* the confinement of the centre of inertia.

7.



The resistance R, acting obliquely to the axis of figure, will (as in the familiar case of the sailing of a vessel *on* the wind) give the projectile a component of motion in the direction of its axis, and the centre of inertia cannot remain on its rectilineal trajectory A B. But, owing to the conical motion of that axis about a line parallel to the resistance R, (a direction, itself, always changing) the result will be that the projectile *itself* will describe a helix, *s s s*, about the line of original direction A B.†

* These results have been exhibited *experimentally* by Prof. Magnus; they flow directly from my analysis made *without* experiment of any kind.

† The *period* of these conical, or rather helical revolutions, could be computed if we knew the exact intensity of the force R; the distance C *m*, (or γ) from the centre of inertia, at which it cuts the axis of figure; the value *k*, of its radius of gyration about its axis of figure; and the velocity, *n*, of rotation.

8.

The period, T, (as deduced from my treatise on the Gyroscope) would be expressed in seconds thus—

$$T = 2\pi\,\frac{k^2 n}{\gamma r}$$

In which $r = \frac{R}{M} =$ *total* resistance of the air $\div$ mass of the projectile. The value of r for the ordinary musket ball would be about 100. Assuming it at 30 for the elongated ball with more mass and *less* resistance—and assuming γ but $\frac{1}{10}$ inch, putting $n = 2\pi \times 150$, and $k^2 = \cdot 15$ inch, we should get $T = \frac{3}{4}''$ or three-fourths of a second. If we suppose there to be an actual centre of *resistance*, *m*, (a supposition not true probably, but like that of the meta-centre, in hydraulics, admissible in narrow limits) this period would be *the same*, whatever be the inclination of the projectile. The period would increase as the resistance of the air diminished by loss of initial velocity.

In the foregoing case there is no *permanent* departure, or *deviation* from the original direction of projection; nor would there be much *apparent* departure in the range of the modern rifled arms since the periods of these helical revolutions would probably be but a fraction of a second, while the time of flight is from 6 to 8 seconds.

The matter is much more complicated when, instead of the imaginary case above presented, the force of gravity is introduced and the actual flight of a projectile is considered. The ball is projected, having its axis coincident with the trajectory, but from the very outset of the flight a severance commences in the directions of the axis and trajectory, owing to the action of gravity. The resistance of the air becomes gradually more and more oblique to the axis, pressing (as in Fig. 7) the point upwards, and producing a *precession* (or conical motion) to the right.

The entire character of the motion becomes altered by the constantly shifting direction in which this resistance is acting, owing to the constant *fall* produced by gravity.

It is very difficult, *a priori*, to describe the exact character of the motion. It depends upon unknown facts, and doubtless varies much with the form of the projectile—its velocity of translation and rotation.

It would be going into reasoning probably too difficult to express intelligibly, to attempt to explain why, in all cases, the period of these helical revolutions would be very greatly prolonged, and why I think that in *most* cases (if not all) *no complete* revolution takes place at all, but that the whole motion is confined to the first quadrant. In the latter case the flight of the projectile would exhibit one continuous and constantly increasing deviation to the right. In the case of a considerable number of helical revolutions actually performed, there would still be, with each revolution, an increased deviation to the right, owing to the descending branch of the vibration being so much larger than the ascending* one.

It would, perhaps, be unprofitable—in the absence of any sufficient data—in the absence even of experiments made with any knowledge of the *real* causes of deviation, and therefore nearly useless as reference for data, to pursue this subject any further.

That the *precessory* motion is an inseparable attendant to that (so called) *stability* of axis which is the *very object* of giving rotation, I have shown; that it is an *adequate* cause of deviation I think will be admitted. Hence, while *other* causes *may* contribute to the effect, or may oppose it (according to the shape of the ball), this peculiar effect must be looked upon as the controlling cause of deviation of elongated balls discharged from rifled arms.

* In this case the helical path would probably be so *prononcé* as to be easily detected by firing through screens.

I will but add a few very general conclusions I have arrived at from the foregoing.

First, the elongated ball possesses, from *its very shape*, a tendency to pursue the *direction* of *its axis*. This tendency should be made available, so far as practicable, to sustaining the flight, and flattening the trajectory.

Second. In the modern improvements in the art of throwing projectiles from rifled fire-arms, a decided step has been made *backwards*, in losing that most essential element to range and accuracy, *initial velocity*. It is desirable, and, I should think, not impracticable to restore it.

The initial velocity of the old rifle ball (weighing but about 180 grains) was 1750 feet per second. To the modern projectile, weighing about 500 grains, is given but from 900 to 1000 feet initial velocity.

It would doubtless be difficult to *give* the high velocity to so heavy a ball, and if given, the recoil would be inadmissible. But why throw so heavy a ball?—and why adhere to such calibres? The long range of such balls is not due to their weight, but to their *model* and to the *low ratio of their cross section to that weight*.

Both these advantages can be attained with a light ball as well as with a heavy one.

An increase of weight over that of the old musket ball is, *in itself*, objectionable, by increasing the soldier's load.

I can see no reason why, to a ball of the weight of the old musket ball (340 grains), may not be given all the properties of the heavier ones, with the additional great advantage of a *high* initial velocity, approximating to that of the old rifle balls.

To accomplish this, of course, the calibre must be greatly reduced—the ball elongated, and a *comparatively* large charge used (less, however, than that used in the old musket—viz. 110 grains).

This highly elongated ball, *balanced* (if I may use the term) by grooves around the after end, so arranged that the resultant of the air's resistance shall pass as nearly as practicable through the centre of inertia,* will fulfill likewise the *first* condition I

* According to my theory of the matter, these grooves, instead of acting like an arrow's feathers, to keep the axis coincident with the tangent to the trajectory, *have just the contrary object*, to keep it (if properly adjusted) parallel to that original direction, by so balancing the forces of resistance as to cause their resultant to pass through the centre of inertia. The theory of "arrows' feathers" supposes, on the contrary, that, by the grooves, this resultant is thrown *behind* that centre, and tends to *tilt* the axis; an operation which must inevitably, *likewise*, produce *deviation*. Prof. Magnus' observation on balls with low initial velocities (so as to be visible to the eye) confirm my position, though I attach little value to those observations.

Experience seems to show that this balancing of the forces of resistance may be produced *without* grooves (vide Wilcox, p. 160), for the Enfield ball and the Whitworth projectile seem to be without them, as likewise Lancaster's. I have seen no statements, however, as to the *deviation* of these projectiles.

have laid down. For the highly elongated ball possesses in a proportional degree the tendency to pursue the direction in which its axis points: and the causes of deflection of the axis' direction being eliminated by a proper balancing about the centre of inertia of the forces of resistance, the tendency is to pursue the *original* line of direction, in opposition to the downward curvature due to gravity, and thus to *flatten* the trajectory and increase the range.

These conclusions are those of theory alone, but, if I mistake not, all the most recent advances in rifled arms have been in the direction which they indicate. The Swiss (Federal) rifle, one of the most perfect in Europe (see Lieut. Wilcox's "Rifles and Rifle Practice," p. 187) has a calibre of but 0·41 (inch), and its ball, 2½ calibres in length, weighs but 257 grains, thus combining with the small calibre, the highly elongated form, and even then weighing *less* than our old spherical musket ball. Though its initial velocity is not given, yet as the greatest *proportional* charge of powder is used with it, doubtless it also receives the *highest initial velocity*,* of any of its class of projectiles now known in any service.

The *hexagonal projectile* of Whitworth is another instance in confirmation of the principles I advance.

Of small calibre and highly elongated,† he throws this projectile with such accuracy as to hit with certainty, at 500 yards, a disk *not more than two inches in diameter;* and "asserts that he will not rest satisfied till he has fired a ball from one of his guns into the barrel of another, at a distance of 500 yards." (Edinburgh Review, Ap. 1859.)

The "Armstrong" projectile is another characteristic instance. So decidedly is the *elongation of the ball* characteristic of the most recent and successful efforts in obtaining range and accuracy, that the English writer just quoted applies the term "bolts" to the Whitworth and Armstrong projectiles.

In fact, the *two* springs from which have risen the modern improvements in projectile weapons are 1st, the application of the *rifled principle* to *all arms;* 2d, the *elongation* of the projectile. *Either one alone* may produce, *to a certain extent*, the results desired; it is only by the best possible combination of the two that the best results can be educed. The *increase of range* is due

* Initial velocity is the *very first* element in procuring range and accuracy. The greater it is, other things being equal, the *flatter* will be the trajectory (one of the principal elements of accuracy), and the flatter the trajectory the less the lateral deviation laterally. The extreme curvature of trajectory (though it may have its advantages in some peculiar circumstances), is the great difficulty as to *accuracy of fire*, in modern rifled weapons.

A *necessary evil* with the heavy balls used; the pretence that it is, *in itself*, an advantage, is too absurd for controversy.

† According to Wilcox the mean calibre is about 0·46, and its length 2½ calibres.

almost entirely to the latter principle, and it is only by applying it to the utmost *practicable* extent (as in the case of the Whitworth and Armstrong projectiles), that the greatest range and most perfect accuracy can be obtained.

The *calibres* in use have been a positive *bar* to the successful use of this principle in small arms. Borrowed from the old smooth-bored weapons, the adherence to them has caused an unnecessary *increase* of weight,* and made a loss of *initial velocity* inevitable, with all the attendant evils of a highly curved trajectory, and large deviation.

"Notwithstanding" (says Wilcox) "the long time that has elapsed since the discovery of the rifle, its principle is not yet so well understood as to have led to the general adoption of any particular form of this arm as the best."

The above conclusion of the author of "Rifles and Rifle Practice," will justify me, I hope, in venturing to make the foregoing suggestions on the subject.

New York, Dec. 15, 1859.

ART. XVIII.—*Gulf Stream Explorations—Third Memoir. Distribution of Temperature in the Water of the Florida Channel and Straits*; by A. D. BACHE, Sup't. U. S. Coast Survey.—With Diagrams.

(Communicated by authority of the Treasury Department to the American Association for the Advancement of Science.)

The results of the explorations of the Gulf Stream in the survey of the Coast, have been communicated to the Association from time to time, as phenomena of peculiar interest have been developed.

The original plan of these explorations having been carefully studied, and having proved successful, has steadily been adhered to. The more recent observations have been directed to that part of the stream between Havana and Cape Florida, known as the channel and strait of Florida.

I have now to present four sections showing the depth and temperature in this most important region of the Gulf Stream. These results are from the observations of Commander B. F. Sands and Lieut. Commanding T. A. Craven, U. S. Navy, Assistants in the Coast Survey, whose names have already been mentioned before the Association in connection with explora-

* The old spherical musket ball of ·69 calibre weighed 340 grains. The new "rifle-musket" ball of ·58 calibre weighs 500 grains, while still heavier balls are in use in Europe. Thus we see in use balls surpassing in weight the musket ball, and having *twice*, and even *three times* the weight of the Swiss ball described in the text, admitted to be, practically, the best in Europe.

tions of the Gulf Stream, and furnish a sufficient guaranty that the results have all the reliability which care, experience and zealous labor could give them.

Section No. 1, from Cape Florida to Bemini was run by Lieut. Commanding Craven, in May, 1855; Section No. 4, by Commander Sands, in May, 1858; and Sections 2 and 3 by Lieut. Commanding Craven, in April and May of the present year, (1859). Sections 2, 3 and 4 are perpendicular to the direction of the Stream at distances of about fifty, one hundred, and two hundred miles from Cape Florida. The Florida strait is funnel-shaped, being about ninety miles wide at Havana and about forty-five miles wide at Cape Florida, the narrowest part.

Form of bottom.—The area of the water way and the form of the bottom are represented on diagrams 7, 8, 9 and 10. The Arabic numerals at the top represent distances from the Florida coast (the Keys) in miles, and the Roman numerals, the positions at which observations are made. The numbers at the left hand represent the depth in fathoms.

Commencing at the Cape Florida section, it will be seen that there is a rapid descent of the bottom to the Havana section, from three hundred and fifty fathoms to eight hundred fathoms, or twenty-seven hundred feet in a distance of two hundred miles. The most shallow as well as the narrowest part of the Stream is therefore at Cape Florida. The deepest water follows the coast of Cuba and the Grand Banks, the depth being eight hundred fathoms at a distance of only five miles from Havana, nearly four hundred fathoms within five miles of Salt Key Bank, and three hundred fathoms close to the island of Bemini. The descent from the Florida side is for the most part gradual, but from the opposite side abrupt. This effect seems to have been produced by the action of the sub-current in wearing a deeper channel upon the concave side of the Stream. At Havana there is an abrupt descent of nearly a mile within five miles of the shore, while on the side of the Tortugas and Key West the water is comparatively shallow, and the descent gradual. This fact goes to confirm the conclusion that the stronger current of the Gulf Stream makes the circuit of the Gulf of Mexico, since, if it impinged directly upon the island of Key west and the Tortugas we should find its effects in the wearing of a deeper channel on that side.

TEMPERATURES.

Change of temperature with depth.—In a former communication the law of change of temperature with depth was discussed, and types of the curves representing the law were given for different parts of the Stream. The curves were all merely modifications of a more general form. Thus, the cold water between the Gulf Stream and the Coast gave one form; the axis of the Stream another; and the water beyond the axis a third form, while in the strait of Florida a fourth was developed. It would be natural to expect in the course of many years' explorations by different individuals with different instruments not even of the same class, that general phenomena of this character should present some contradictions and some inexplicable results. Experience however has confirmed the first conclusions and the constancy of the phenomena. It is not difficult, having the curve representing the temperatures at any position from the surface to the depth of several hundred fathoms to determine from the temperatures alone, in what part of the Stream they were taken.

Temperature in a direction perpendicular to the Stream.—Diagrams 2, 3, 4 and 5, show the changes of temperature for the same depth in each of the sections, and diagrams 7, 8, 9, and 10, the depth for the same temperature.

Bands of warm and cool water.—In the section from Cape Florida to Bemini, the division of the Stream into bands is plainly exhibited, though more faintly than in the northern sections, and the form of the bottom in this section shows the elevations and depressions corresponding to the divisions. In the sections south of Cape Florida, all traces of the bands seem to disappear as well as the ridges of the bottom. The bands therefore seem to have their origin near Cape Florida, and the conclusion stated some years ago, as the probable one, is strengthened, that they are caused by the ridges and valleys of the bottom parallel to the general course of the Stream, and along which the Stream and counter stream have their course.

The Cold Wall.—The cold wall, as an exception to the remark made above in reference to the bands, is traced as far as the Tortugas, and is plainly shown in all the sections with more or less distinctness. In the Sombrero Key section (No. 3) it is strongly marked at depths ranging from seventy to a hundred fathoms, while in all the sections the warm water at the surface overflows the cold wall and reaches quite to the shore.

Diagram No. 6 represents the comparative curves of the cold wall in different sections of the Gulf Stream, including those in the Straits of Florida. The figures at the top show the distances of the cold wall from the shore in the different sections, and the numbers on the left the degrees of temperature. The curves are drawn for different depths in the several sections, as shown in the notes at the bottom of the diagram. The curves *g*, *h*, *i*, *k*, represent the cold wall in the four sections under consideration.

Longitudinal Sections.—It has been found very difficult to deduce any satisfactory law for the decrease of surface temperature along the axis of the Stream owing to the variability of the temperature of the water of the regions from whence the Gulf Stream is supplied. Two modes of investigating the subject have been pursued, one by following the stream from the Gulf of Mexico, and making hourly observations of the temperature of the water, and the other by comparing the mean temperatures of the various sections with each other, and with the temperature of the Gulf of Mexico. In the first method, the vessel must be allowed to drift with the current of the stream, a difficult condition except in the best weather, even for a day, and to float along thus, for hundreds of miles would rarely be practicable. Any motion communicated by sails or by steam must carry the vessel beyond the water in which she commenced her voyage, and the lateral overflow carries the water constantly from the axis toward the edges of the Stream.

In the comparison of mean temperatures of the different sections, the fact has been established, that the temperature of the water of the Stream at any point may be higher than at a point nearer the source, and hence vessels in running along the Stream may, and generally do, pass through water not of a constantly diminishing temperature, but from cool to warm, and the reverse. This is to be explained mainly though not entirely, by the variability of temperature at the source.

By taking the mean temperature of any one section, and going back to the date of the departure of the waters from the Gulf of Mexico, as determined by the velocity of the stream, and comparing the temperatures observed with the temperature of the Gulf waters, it was supposed that a solution of the question might be obtained. The temperatures were taken from the most authentic meteorological records of the Gulf for a series of years, and those periods sought which corresponded to the dates desired. The uncertainty of the temperatures of the waters of the Gulf of Mexico, as obtained from air temperatures taken here and there along its shores rendered the results unsatisfactory. Enough seems to have been determined, however, to show that the surface temperature of the Gulf Stream along its course is variable; that a vessel sailing along the axis at a more rapid rate than the motion of the stream, will pass through water of higher and lower temperature, depending generally upon two conditions, viz: the distance from the Gulf of Mexico, and the temperature of the Gulf at the time the water entered the straits of Florida; and further, that the latter cause is the predominating one in the parts of the Gulf Stream adjacent to the Atlantic coast where the current is rapid.

The influence of the form of the bottom in forcing the cold counter current of the bottom upward, has been adverted to, and the fact appears to be well established in the cross sections where the ridges and valleys parallel to the direction of the stream separate it into bands of warmer and cooler water, and this conclusion, as has just been stated, is strengthened by the fact that the bands and ridges simultaneously disappear south of Cape Florida. The phenomenon is moreover strikingly exhibited in the longitudinal section of the bottom, in connection with the lower temperatures.

The shallowness of the Stream in the strait of Florida, connected with the fact that the bottom falls off rapidly to the north and south afforded an excellent opportunity for testing the question. If the cold water of the under polar current follows the bottom, it should appear in the shallow part of the strait, and here the warm water of the surface, and the cold water of the bottom, would approach each other. Diagram No. 1 shows the curves of 40°, 45°, and 50° (bottom temperatures) along the deepest part of the stream, commencing at Sandy Hook, and running as far as the Tortugas. All these curves rise with the bottom and pass over the ridge which divides the bed of the Atlantic from that of the Gulf of Mexico, and again fall with the slope of the bottom towards the Gulf. In the narrowest part of the strait where the depth is three hundred and fifty fathoms, the temperature from the surface to the bottom, ranges between 80° and 40°.

On the effects of pressure on Saxton's deep sea thermometer.—In the exploration of the Gulf Stream, the temperatures below one hundred fathoms have mostly been determined by Saxton's metallic thermometer, and although the results have been consistent amongst themselves, and have agreed well with the indications of other thermometers, yet it was thought advisable to determine the effect of pressure by direct experiment.

Saxton's thermometer consists essentially of a compound ribbon of silver and platinum fused and pressed together by rollers. This ribbon is wound in a spiral form, one end of the spiral being firmly fastened to an interior solid axis and the other left free. Upon the free end is placed an index arm which moves over a circular graduated scale carrying with it a friction hand or indicator which is left at the extreme point of the arc reached by the true index. The instrument is enclosed in a case to which the water is freely admitted. A variation of temperature is immediately noticed, as the effect is to give a rotary motion to the index.

The experiments to determine the effect of pressure were made at my request by Mr. J. M. Batchelder with means devised by Mr. Thomas Davison at the Novelty Iron Works. The following description of the apparatus employed, is given by the last named gentleman.

"The gauge consists of a brass cylinder H, about eight inches long into which a steel plunger is fitted, the upper part of the plunger at A being ·70 of an inch in diameter, and the lower at B about ·786, so that the difference in area of the ends is equal to one tenth of a square inch. The cylinder is bored out a little larger than the plunger except for about a fourth of an inch near each end at C and D where both are accurately fitted. To the branch E a pipe connects, communicating with the hydraulic cylinder and leading the water into the centre of the gauge which it reaches after passing through the chamber F filled with sponge to prevent any impurities in the water from reaching the plunger. The upper end of the plunger connects by a wire W, to a spring as shown in the sketch at G, so constructed as to indicate pressure from 0 to 450 ℔s., the spring being so strong that 450 ℔s. produce a movement of the plunger equal to three-eighths of an inch. It is evident that as the difference in area of the ends of the plunger is one-tenth of an inch, one hundred pounds pressure from the water on this surface, as indicated by the balance, would equal a pressure of water of 1000 ℔s. per inch, or a pressure ten times as great as that indicated by the balance throughout its scale. The only difficulty in the use of the gauge is that of fitting the plunger to the cylinder so that while it is perfectly free to move it is also perfectly water tight. This difficulty however has been overcome, and much advantage was also derived from Mr. Batchelder's suggestion for supplying the wear of the plunger and cylinder by depositing brass on the plunger through the galvanic process."

Connected with this gauge by a pipe is a strong wrought iron cylinder, sixteen inches long by four inches in diameter, in which the thermometer was placed, the opening being firmly closed by a screw plug. This second cylinder was immersed in a tub of water for the purpose of regulating the temperature. The thermometer once placed in the cylinder, is not again removed, the index being read by means of a mirror until the observations are completed. By the use of this apparatus, the effect of pressure up to 4000 ℔s. per square inch was observed upon two thermometers, and the results are given below. The observations were made to indicate the effects of 500, 1000, 1500, 2000, 2500 ℔s. pressure, etc. Seven series of experiments were made with thermometer No. 5, and five series with No. 10. The mean results show that a pressure of 1000 ℔s. per square inch has no effect upon the thermometer; at 1500 ℔s. the effect is less than one degree; and from 1500 to 4000 ℔s. per square inch, the effect is to diminish the readings, the maximum effect being seven degrees.

The diagram exhibits the law of diminution by increase of pressure, and the depth corresponding to different pressures. The correction to be applied varies with the depth. For thermometer No. 5 it is only four tenths of a degree Fahrenheit at the depth of 600 fathoms. For thermometer No. 10, it is one degree at the same depth.

At 1500 fathoms the corrections are respectively five and a half and seven degrees.

Nearly all the temperatures observed in the Gulf Stream have been taken at depths less than six hundred fathoms.

Table showing differences of readings of Saxton's Thermometer under pressure and free from pressure.

Thermometer No. 5.						
No. of Series.	Pressure in Pounds.					
	1500.	2000.	2500.	3000.	3500.	4000.
	°	°	°	°	°	°
1	1·6	0·	3·75	0·	0·	0·
2	0·	1·00	2·	2·8	4·5	5·5
3	0·	1·0	2·25	3·75	4·75	5·7
4	0·	0·5	0·50	2·0	3·6	5·5
5	0·	1·75	2·25	3·5	5·0	6·5
6	0·	1·25	2·25	3·75	6·0	6·5
7	0·5				3·5	4·25
Means	0·3	1·1	2·1	3·2	4·5	5·6

Thermometer No. 10.						
No. of Series.	Pressure in Pounds.					
	1500.	2000.	2500.	3000.	3500.	4000.
	°	°	°	°	°	°
1	0·	2·0	3·25	4·5	6·25	8·25
2	2·0	1·0	3·5	4·5	6·0	7·25
3	0·75	2·0	3·0	3·25	5·5	6·5
4	1·75	2·0	3·5	4·75	5·5	7·25
5	·75	1·75	1·75	3·75	5·0	6·75
Means	1·00	1·75	3·0	4·25	5·6	7·25

Art. XIX.—*On the Chemical Composition of Pectolite;* by J. D. Whitney.

A few years since I made some examination of specimens of a radiated fibrous mineral from Isle Royale, Lake Superior, which proved on analysis to be pectolite. A mineral, closely resembling pectolite, from Bergen Hill, New Jersey, which had been analyzed by L. C. Beck, and considered by him as identical with the stellite of Thomson, was examined at the same time and found to agree in composition with pectolite, as had been previously suggested by J. D. Dana. Both the stellite and Wollastonite of Thomson were referred by me, at that time, to pectolite,* a reference the correctness of which has since been shown by Messrs. Heddle and Greg, in a paper on the composition of the English varieties of this mineral.†

Notwithstanding so many analyses of pectolite have been made by different chemists, there has not been a sufficient accordance in the results obtained to justify a positive decision as to the real formula of the mineral, although that of Von Kobell has been generally adopted. It will be sufficient to refer to the various published analyses, to see that there is but an unsatisfactory degree of uniformity in their results, whether of specimens from American or European localities. Thus, for instance, in

* Journal of Boston Nat. History Soc., vi, 40.

† Philos. Mag., [4], ix, 238; also in Erdmann and Marchand's Journal, lxvi, 144.

Von Kobell's analysis of the Monte Baldo pectolite, the silica is given at 51·3 per cent, while other analyses of Scotch and American varieties give as much, in some instances as 54 and 55 per cent of that substance. In the like manner, the amount of lime, as stated by different analysts, varies from 29·8 to 35·2 per cent, while there is even less agreement in the water, which is given at from 0·41 to 3·39 per cent.

The difficulty of procuring, in a perfectly pure state, a mineral which only occurs in a finely-fibrous condition is undoubtedly one of the principal causes of these discrepancies in the analyses; but it is also possible that the unusual care required for the correct determination of the silica in the very soluble class of minerals to which pectolite belongs may not, in all cases, have been appreciated. The great abundance and purity of the specimens of this mineral which have been obtained from the tunnel of the Erie railroad, recently excavated through Bergen Hill, seemed likely to obviate the first difficulty mentioned above. The results of three analyses indicated that this material was really of almost absolute purity, while no pains were spared to effect a complete and accurate separation of the various ingredients, and especially of the silica.

The pectolite dissolves more or less completely in chlorohydric acid, according to the strength and quantity of the latter. By using a considerable excess of rather dilute acid, all, or nearly all, the pulverized material may be dissolved into a clear liquid. As the attack is usually performed, a portion of the silica remains in solution and the remainder separates as a flocky precipitate.

The following experiments show the difficulty of estimating the silica correctly in this class of highly soluble silicates, and the necessity of unusual precautions in its determination.

On digesting the *ignited* mineral with acid until a perfect attack seemed to have taken place, the solution gelatinized on evaporation, and there was no perceptible gritty feeling when it was stirred with a glass rod or the spatula; on separating the silica, however, after evaporating to dryness, moistening with acid, and adding water, in the usual way, its amount was found to be equal to 62·10 per cent of the substance taken.

Another portion of the *unignited* mineral was attacked by acid, and the silica separated without evaporating to entire dryness; its amount equalled only 35·6 per cent. To procure the whole amount of this substance present in the mineral, and uncontaminated by any traces of the bases, it was found necessary to use the unignited substance for the attack with acid, carefully to evaporate to entire dryness over the water-bath, then to moisten the dried mass with strong acid and allow it to stand for some time before adding water and filtering. These precautions will give a perfectly pure silica, but not all of it, as two or three per cent will still remain in the solution, a part of which will go down with the ammonia precipitate, and the remainder be found after driving off the ammoniacal salts, (the lime having been previously separated,) and igniting the residuum.

The following are the results of three analyses of as many different specimens of the pectolite from the Bergen Hill tunnel.

	I.	II.	III.
Silica,	54·32	54·76	54·27
Lime,	33·12	32·88	32·83
Protoxyd of manganese,	·66	1·16	1·24
Protoxyd of iron,	·26		
Soda,	8·78	9·17	8·94
Water, by loss,	2·86	2·03	2·72
	100·00	100·00	100·00

The direct determination of the water on the substance dried at 80° C. gave, for II, 3·03, and for III, 2·75 per cent. Specimen III, from the Wheatley Collection, Union College, was apparently the purest; it was a fragment of a mass, the fibres of which were several inches in length, slightly divergent from a common centre, and being nearly transparent and evidently quite free from any admixture with quartz or any other foreign substance.

In analysis III the oxygen is as follows:

Silica,	28·942
Lime,	9·379
Soda,	2·306
Protoxyd of manganese,	·290
Water,	2·44

This gives as the ratio:*

	$\dot{H}$	:	$\dot{Na}$	:	$\dot{Ca}$	:	$\ddot{Si}$
	1	:	1·05	:	3·83	:	11·84
or nearly	1	:	1	:	4	:	12

If we attempt to express this ratio by a formula, we have

$$\dot{Na}^3\,\ddot{Si}^4 + 4\dot{Ca}^3\,\ddot{Si}^2 + 3\dot{H}.$$

The percentage demanded by this formula is given below, by the side of that required by Von Kobell's.

	Whitney.	Von Kobell.
Silica,	54·22	52·05
Lime,	33·73	35·33
Soda,	9·33	9·77
Water,	2·74	2·83

It is evident, from a comparison of the figures given above, that the formula now suggested agrees more nearly with the re-

* The atomic weights used are as follows:
Na=23 : Ca=20 : Si=21, H being =1.

sults of the analyses of the Bergen Hill pectolite than any yet proposed. As written here, its relations to that of spodumene are to be noticed, as also to those of Wollastonite and pyroxene. The latter connection will be made plainer by writing the formula of pectolite thus:

$$\dot{R}^3 \, \overline{Si}^2,$$

$$\dot{R} \text{ being } (\dot{C}a\tfrac{4}{6} + \dot{N}a\tfrac{1}{6} + \dot{H}\tfrac{1}{6}).$$

Northampton, Mass., Feb. 1859.

ART. XX.—*Notes on the Ancient Vegetation of North America*; by Dr. J. S. NEWBERRY. In a letter to Prof. DANA, dated Santa Fe, New Mexico, Oct. 15th, 1859.

Dear Sir:—I have just returned to Santa Fe after an absence of three months, spent in an examination of the geological structure of the country bordering the San Juan and Upper Colorado rivers, in Utah and New Mexico, connected with the War Department topographical survey under Capt. Macomb, Topog. Engrs.

The region visited proved interesting in many respects—beautifully picturesque and unexpectedly productive—covered with ruins, and once densely populated by a race that has now entirely abandoned it.

I would cheerfully give you a sketch of its remarkable physical and geological structure, but the results of the expedition will doubtless be published in detail by the War Department, and it is not proper that any part of them should now be given to the public. I will say however, in general, that our field of exploration includes an immense labyrinth of great cañons, scarcely less abysmal than those of the Lower Colorado in which we were last year involved—some of which are over a mile in depth—and even more varied and wonderful in character. The sections exposed in their walls permitted me to measure and examine all the strata between the base of the Carboniferous and the summit of the Cretaceous series; the latter formation attaining a thickness of 4000 feet, and occupying an immense area west of the main divide of the Rocky Mountains.

Our work this season connected on the south with that of the party with which I was associated last year under Lieut. Ives, T. E.—and combining the results of both expeditions, I have now a complete and detailed section of all the rocks composing the great central plateau of the continent, from the base of the palæozoic series to the summit of the Cretaceous. These strata are conformable throughout, and over 10,000 feet in thickness.

The collection of fossils made, both animal and vegetable, is quite large, and, with considerable new matter, includes what is much better, many well known species of which the geographical range will be seen to be much greater than has been heretofore supposed.

I was agreeably surprised to find here on my arrival, the May, July and Sept. numbers of the Journal, published during my absence, by which I have been in a degree placed *au courant des affaires scientifiques.* In the articles in the July number from the pen of my friend Mr. Lesquereux, I have been much interested, as they refer to matters which have engaged much of my attention for some years. In the letter of Prof. Heer there is however a passage which seems to me to require notice; although, situated as I am, without specimens or books for reference, I am scarcely prepared to take up the discussion of the questions involved in it. This is the less necessary now, as what I have to say in reference to them will be found *in extenso* in the reports on the geology of the country west of the Mississippi which I have made or have in preparation for the general government.

The paragraph to which I allude is as follows:—

"Your views of the gradation of the flora of North America agree perfectly with what we find in Europe. This led me to believe that the plants of Nebraska belong to the tertiary and not to the cretaceous formation. It is true that I have seen only some drawings which were sent to me by Messrs. Hayden and Meek; but they are all tertiary types. The supposed *Credneria* is very like *Populus Leuce*, Ung., of the lower Miocene, and the *Ettinghausiana* seems hardly rightly determined. Besides it is a genus badly founded, and which has as yet no value. All the other plants mentioned by Dr. Newberry belong to genera that are represented in the Tertiary and not in the Cretaceous. And it is very improbable that in America the cretaceous flora has had the characteristic plants of the tertiary; and this would be the case if these plants did belong to the Cretaceous."

It will be seen that Prof. Heer in this paragraph makes several distinct statements, which for the sake of brevity, I will notice in the order in which they occur. They are,

1st. That the fossil plant I supposed to be a *Credneria* is very like *Populus leuce*, Ung.

2d. The *Ettingshausenia* (called erroneously *Ettinghausiana*) is wrongly determined.

3d. That the genus *Ettingshausenia* is badly founded and has no value.

4th. That all the other plants enumerated by me are represented in the Tertiary and not in the Cretaceous.

5th. That it is improbable that in America the Cretaceous flora had the characteristic plants of the Tertiary, as would be the case if the plants of which outline sketches were sent to Prof. Heer by Mr. Meek, were Cretaceous.

To which I reply,

1st. The plant I considered a *Credneria* is not *Populus leuce* Unger, which, according to his descriptions in the *Foss. Flor. v. Sotzka* and *Genera et Species Plant. Foss.* has a toothed margin, while the leaf in question is entire.

I have recently obtained more and better specimens of this fossil, of which I have apparently three species, but as yet have not had opportunity to study them carefully.

They strongly resemble some of the species of that portion of the old genus *Credneria* to which Stiehler left the name (*C. integerrima*, &c.) when he established his genus *Ettingshausenia*. It may prove a new genus. Further study will alone determine this.

2d. That I was wrong in considering some of Dr. Hayden's fossils generically identical with those Stiehler designated by the name *Ettingshausenia*, I am by no means prepared to admit.

Prof. Heer has had but a single outline sketch of the plant, and can hardly speak decisively on the subject. When at Washington I had before me all the figures and descriptions of Stiehler, Zenker, Dunker, Bronn, and Unger, of the genus *Credneria*, and a large number of specimens in good preservation, for comparison. To me, and to Mr. Meek, who examined the subject with me, there seemed to be a marked correspondence in general form, texture and nervation, between our specimens and some lobate *Crednerias* (*Ettingshausenias* Stieh.). With these I regarded our fossils as generically identical, and shall continue so to regard them until the question—if question there be—can be definitely settled by a comparison of specimen with specimen—old world and new.

3d. Whether Stiehler was in error in establishing the genus *Ettingshausenia* upon a group of species of *Credneria*, I will not pretend to say, for I have nothing like the ample material possessed by him when he made the division; indeed this question has nothing to do with that now before us.

It is true that at the Smithsonian Institution I had access to nearly everything that has been published upon the genus *Credneria*, and it seemed to me the most natural thing in the world that Stiehler should give generic value to the cuneate or lobate form, and strongly marked yet finely reticulated nervation, which characterize the group of species of *Credneria* of which *C. cuneifolia* may be taken as a type, while he left the broadly rounded, entire, or merely toothed leaves, with a sparse and rectangular nervation, such as *C. integerrima*—leaves not very unlike those of *Coccoloba*—to which they have been compared—to retain the name of *Credneria*.

The vindication of his accuracy may, doubtless, be safely left to Stiehler. At least it would be nothing short of arrogance for any one who had not before him a suite of the specimens compared by Stiehler, to review his work and pronounce it either erroneous or correct.

4th. The statement that aside from the so-called *Credneria* and *Ettingshausenia*, *all* the genera enumerated in my letter to Messrs. Meek and Hayden, are represented in the Tertiary and not in the Cretaceous, is at least surprising. I am almost inclined to infer from it that Prof. Heer, though confessedly the highest authority in reference to the Tertiary flora of Europe, has neglected to acquaint himself fully with that of the Cretaceous formation. He makes the statement doubtless in good faith, but he can hardly have seen Stiehler's paper on the Cretaceous plants of Blankenburg, and if he has not seen that he is certainly not yet prepared to discuss intelligently the claims of *Ettingshausenia* to be recognized as a good genus; nor indeed the Cretaceous flora in any of its aspects.

Whoever will take the trouble to examine Stiehler's paper (*Palæontographica*, 1857) will see in the enumeration of plants found in the Lower Cretaceous strata (Quader sandstein) *Populus*, *Salix*, *Acer*, and several other genera which Prof. Heer says are represented in the Tertiary but not in the Cretaceous.

The fossil flora of Blankenburg is indeed strikingly like that of our Lower Cretaceous formation, from which the plants that have given rise to this discussion have been derived, except that ours is more varied, and we have as yet found no palms or *Cycadaceæ*.

5th. In regard to the probability or otherwise that the Cretaceous rocks of America should contain a flora similar to that of the Tertiary, it may be said, that it is not now a question of probabilities but of fact, the evidence of the case being now before us, and in abundance.

In what has heretofore been written in reference to these fossil plants two great questions have been raised, 1st, as to their botanical affinities, 2d, as to their geological position.

As to their botanical relations—outline sketches of a few of the plants have been examined by Prof. Heer. By him they were decided to contain representatives of the genera *Liriodendron*, *Populus*, *Laurus*, *Sapotacites*, *Phyllites*, *Leguminosites*, &c., and were pronounced Lower Miocene.

The entire collection was placed in my hands for examination and description before I knew that Prof. Heer had been written to on the subject. I supposed I found among them *Liriodendron*, *Salix*, *Alnus*, *Populus*, *Platanus*, *Pyrus*, &c., with the Cretaceous genera *Credneria* and *Ettingshausenia* and considered them Cretaceous. That you may see on what evidence that opinion was based, I enclose a copy of my letter to Messrs. Meek and Hayden, which I chance to have with me.

Washington, D. C., Nov, 12th, 1858.

Messrs. MEEK and HAYDEN,

Gents: The fossil plants which you requested me to examine, I have looked over with great pleasure, and, in answer to your question as to the age of the strata from which they were derived, concur with you in the opinion that they belong to the Cretaceous epoch. They include, however, so many highly organized plants, that were there not among them genera exclusively Cretaceous, I should be disposed to refer them to a more recent era.

A single glance is sufficient to satisfy any one that they are not Triassic. Up to the present time no angiosperm dicotyledonous plants have been found in rocks older than the chalk, while of the eighteen species which compose your collection sixteen are of this character.

What was the general aspect of the flora of our Cretaceous continent we can only conjecture, as the specimens of it which we have, represent only its ruder and coarser elements,—the leaves of some of its deciduous trees, which, perhaps by an annual frost, were, as now in autumn, scattered on the surface of stream, lake, or sea, and, sinking, mingled with the sediment accumulating at the bottom.

In such an herbarium we could expect to find little else than the relics of some of the ligneous plants, and a very imperfect picture of the flora of the period.

The evidence furnished by your specimens is, however, good as far as it goes, and we are warranted in inferring from them the existence of a more highly organized flora during the Cretaceous period than has usually been attributed to it.

A flora so highly organized, embracing so many angiosperm dicotyledonous plants, should lead us to expect the discovery of what have not yet been found, plants of this rank in the Jurassic and Triassic rocks. Such a flora as is indicated by your specimens, could hardly have at once burst into being, but was doubtless preceded in the older formations by more or less highly organized plants, the prophetic types of those which followed them.

From the enumeration of the genera represented in your collections it will be seen that the flora of the Cretaceous epoch was not very unlike that of the temperate portions of our continent at the present time. The same thing may be said of the Miocene Tertiary flora of the Upper Missouri so fully illustrated in the collections of Dr. Hayden. In both the tropical and sub-tropical forms so common in the floras of the same period in Europe, are apparently wanting; indicating a greater relative uniformity of climate during the later geological epochs, and carrying the aspects of nature of the present, far back into the past. Thus it may be said of our plants as of our fishes, that many of them are "old-fashioned" types.

An interesting fact in this connection, to which I can only allude, is that the later extinct floras of Europe are more like the existing flora of North America, than is that now growing over the rocks which contain them.*

* Including as they do *Liquidambar, Liriodendron*, &c., now exclusively American.

The species of your fossil plants are probably all new, though generally closely allied to the Cretaceous species of the Old World. From the limited study I have given them, I have referred them to the following genera:

Sphenopteris,	Cornus,	Salix,
Abietites,	Liriodendron,	Magnolia?
Acer,	Pyrus?	Credneria,
Fagus,	Alnus,	Ettingshausenia.
Populus,		

Of these the last two are exclusively Cretaceous and highly characteristic of that formation in Europe.

For comparison with the preceding list of genera, I subjoin a catalogue of the Cretaceous genera found at Blankenburg in the duchy of Brunswick, given by Stiehler in the *Palæontographica*, Sept. 1857.

	Credneria,	Pterophyllum,	Comptonites,
Algæ	Chondrites,	Flabellaria,	Populus,
Algæ	Halymenites,	Pinites,	Alnites,
Algæ	Delessertites,	Geinitzia,	Acer,
	Equisetum,	Araucarites,	Quercites,
	Pecopteris,	Salicites,	Juglandites.

I may say, in confirmation of the assertion that your fossil plants are Cretaceous, that I found near the base of the Yellow Sandstone series in New Mexico—called Jurassic by Marcou,—a very similar flora to that represented by your specimens, one species at least being identical with yours—associated with *Inoceramus*, *Gryphæa*, and *Ammonites*, of Lower Cretaceous species.

Yours, &c.,

J. S. Newberry.

Since that letter was written, I have added largely to my material illustrative of the American Cretaceous fauna and flora, having been for some months engaged in studying that formation over a large area, and where it exhibits an unequalled development.

Of the geological age of the deposits which contain the fossil leaves of which sketches were sent Prof. Heer, there cannot now be the slightest doubt. I have in my hands over sixty species of dicotyledonous plants obtained from the Cretaceous formation. At least half of these are derived from near the base of that system in New Jersey, Nebraska, Eastern, Middle and Western Kansas, New Mexico and Utah, collected by Prof. Cook, Mr. Meek, Dr. Hayden and myself. Some of the species are common to nearly all the exposures of the Lower Cretaceous sandstones, which I have examined, and everywhere serve for the accurate identification of these strata. Overlying the rocks containing all this flora, in the same continuous section, where the strata are conformable and undisturbed, both Dr. Hayden and myself have, in repeated instances, found many of the most characteristic fossils of the chalk, such as *Gryphæa Pitcheri*, *Inoceramus problem-*

aticus, *Ostrea congesta*, *Baculites ovatus*, *Ammonites placenta*, *Scaphites Conradi*, *Ptychodus Whipplei*, &c.

The botanical character of this group of plants is, in all essential respects, just what I represented it to be in my letter to Meek and Hayden. Among them are certainly *Populus*, *Salix*, *Alnus*, *Platanus*, *Liriodendron*, *Fagus*, *Quercus*, &c., the most common genera in our present forests.

The plant regarded by Prof. Heer as identical with Unger's *Laurus primigenia* is not a *Laurus*, but a *Salix*, as Prof. Heer would have seen if the specimen had been sent him, instead of an outline sketch. As I have before said, his *Populus leuce?* is not that species. The plants which he calls *Sapotacites* and *Leguminosites* are of doubtful affinity, but certainly not referable to these genera. The latter has a nervation closely allied to that of some of the *Rhamnaceæ*. *Phyllites* is not, as Prof. Herr is made to say in Marcou's pamphlet on "American Geology," "peculiar to the Lower Miocene," but is a general receptacle for fossil leaves of all ages of which the botanical affinities are doubtful, just as *Carpolithes* is a general name for fossil fruits.

It is greatly to be regretted that Prof. Heer could not have applied his great knowledge to the specimens themselves rather than to outline sketches; or, at least, that he should not have been permitted to exercise his excellent judgment unbiased by erroneous oral testimony.

The remarks of Prof. Heer on the fossil plants from the Pacific coast described by Mr. Lesquereux, are exceedingly interesting as forming a new page in the botanical history of American geology, and yet the quite different flora which has come under my observation from the Miocene strata of another part of the continent proves that what he has predicated of the flora, and hence the climate of the continent, though doubtless true of the region where Dr. Evans' fossils were found, is not of universal application.

The study of the floras of the different geological formations has always seemed to me to promise much toward giving us a just idea of the physical geography of our continent, during the different geological epochs. Acting on this conviction in such parts of the continent as I have visited, the fossil plants found, and the nature of the sediments containing them—generally the direct debris of the ancient land—have been to me objects of special interest and attention.

The general results of these observations on the extinct floras of North America may be very briefly stated as follows:

1st. The flora of the Devonian and Carboniferous epochs in America was, in all its general aspects, similar to that of the Old World, which has been so fully described; most of the genera, and a larger number of species than at any subsequent period having been common to the two sides of the Atlantic. The relative number of identical species has, however, it seems to me, been somewhat overrated. In many of the species regarded as the same in Europe and America, the American plants present prevalent or constant characters which may serve to distinguish them. These differences, though frequently remarked by writers, have not been thought to have a specific value; yet it is quite certain that they are as tangible and important as those which now separate many American and European species of recent plants and recent or fossil animals. I have a conviction that the progress of science will considerably diminish the proportion of identical species; a closer scrutiny and more extensive comparison of specimens resulting in the discovery of constant, though inconspicuous characters which shall be ultimately conceded to be specific.

It is true also that in molluscous palæontology, recent geology and botany, the number of species common to the two continents has been considerably reduced of late years; a large number of American representatives of European species at first considered identical from their striking and obvious coincidences, having, on closer study, afforded constant, though less conspicuous differences.

2d. The Permian, Triassic and Jurassic rocks have hitherto furnished us but few species for comparison, but the material is increasing, and I have now on hand quite a collection which has not yet been studied. Enough is already known to show that the great revolution which took place in Europe at the close of the Permian epoch was matched by a parallel though less sudden change in the flora of America.

Here as there the Lepidodendroid trees, the *Sigillariæ*, the *Nœggerathiæ*, the *Asterophyllitæ*, and the great variety of ferns that gave character to the Carboniferous vegetation, were superseded by *Voltzia*, *Tæniopteris*, *Camptopteris* and a varied and beautiful Cycadaceous flora, in which were many species of *Zamites*, *Pterophyllum*, *Nilssonia*, etc., the representatives of those of the "Age of Gymnosperms," which culminated in the Jurassic epoch of Europe.

During this great interval the generic correspondence between the floras of Europe and America was perhaps as plainly marked as during the Carboniferous age, but the relative number of identical species was apparently smaller.

3d. At the commencement of the Cretaceous epoch the flora of the continent was again revolutionized and the vegetation of its temperate portions given the general aspect that it now presents.

This statement will surprise many, for the flora generally ascribed to the Chalk period is greatly different from that of the present. Unger has thus represented it, and Brongniart calls it a transition from the great Cycadaceous flora, of the Jurassic period, to the Angiospermous flora of the Tertiary. In Europe the Cretaceous flora was, apparently, more like that of the Lias and Oolite than in this country, for while the genera *Salix*, *Acer*, *Populus*, *Alnus*, *Quercus*, &c., were then introduced there as here, its general aspect was modified by the presence of numbers of *Cycadaceæ*, and its sub-tropical character attested by fan-palms.

We may find hereafter, in other parts of the continent than those in which I have examined the Cretaceous strata, fossils which shall assimilate our flora of that period more closely to that of Europe, but as far as at present, known, our plants of this age present an *ensemble* quite different. I have now some sixty to seventy species of Cretaceous plants collected in New Jersey, and in various parts of the great Cretaceous area of the interior of the continent, all of which indicate a flora very similar to that now occupying the same region; many, perhaps most, of the genera being now represented in our forests—such as *Liriodendron*, *Platanus*, *Acer*, *Populus*, *Salix*, *Alnus*, *Fagus*, &c. These specimens have been collected in localities included between the 36th and 41st parallels of latitude, but range from the 74th to the 110th of longitude. Nowhere within this area have I yet detected any traces of palms or any indications of a tropical climate. At the base of the Yellow sandstone series of New Mexico, (Lower Cretaceous,) I have found a varied and interesting flora, containing *Pterophyllum*, *Nilssonia*, *Camptopteris*, &c., with a few Angiosperm dicotyledonous leaves. This is evidently the point of junction between the Cycadaceous flora of the Jurassic age and that of the Chalk; for in the entire overlying Cretaceous strata, 4000 ft. in thickness, though Angiospermous leaves are abundant, those of Gymnospermous plants were nowhere discovered, nor any traces of palms, either leaves or stems. The sandstones of the Cretaceous series contain immense numbers of silicified trunks but they are for the most part coniferous.

4th. For the glimpses I have obtained of the Tertiary flora of North America I am mainly indebted to the kindness of Dr. Hayden, who has spent several years in most successfully exploring the geology, botany and zoology of the country bordering the Upper Missouri. Among his rich collections are fifty or more species of beautifully preserved fossil plants from the Miocene, which have been put in my hands for examination, and of which descriptions will be published immediately after my return to Washington.

Not having the specimens, or my notes on them, with me, I can speak only generally of the flora they represent. I remember, however, that they include species of *Platanus*,—one of which closely resembles Unger's great *P. Hercules* and is perhaps as large; *Populus, Acer, Castanea, Sapindus, Carpinus, Ulmus, Diospyros, Quercus, Salix, Taxodium*, and others which indicate a flora in all its general aspects similar to that now occupying the Valley of the Mississippi. A few plants in the collection would seem to have required a somewhat warmer climate than that which the localities where they are found enjoy at present; but there are no palms among them, nor any of the tropical genera *Cinamomum, Sterculia, Dombeyopsis*, &c., so common in the Tertiary strata of Europe.

In the enumeration of the Miocene plants of the Pacific coast, given by Mr. Lesquereux in the May number of this Journal,* I find also evidence of a marked and interesting difference of temperature during the Tertiary epoch, in different parts of the North American continent, under the same parallels of latitude. Mr. Lesquereux finds in Dr. Evans's collection Palms *Salisburia, Cinamomum*, &c., which indicate, at least a sub-tropical climate; a flora quite unlike that from the Miocene of the Upper Missouri, although, as he remarks, similar to that of the Miocene of Europe.

I am tempted to dwell for a moment on the interesting glimpses of the physical geography of our continent in geological times, which these facts and others that have come under my observation afford; for, to you, who have done so much toward the elucidation of its geological history, this cannot, I am sure, be a matter of indifference, but my letter has already grown to an unreasonable length. Let me then close with a few generalizations, referring you to my reports for all details of fact sustaining them.

1st. A large continental area occupied the place of the interior of North America from the earliest Palæozoic ages.

2d. During the Carboniferous epoch this land sustained a vegetation similar to that of the Coal period of Europe and Eastern America, though far less varied.

3. Through the Triassic and Jurassic ages the sediments from the land were strikingly like in mineral character to those of the same age in the Old World: and the flora was characterized by a preponderance of Cycadaceous plants, analogous to those of the Jurassic of Europe.

4th. In the Cretaceous age, the central nucleus of the continent was sufficiently extensive to furnish from its ruins arenaceous sediments that now cover more than half a million square miles. These sediments contain vast deposits of carbonaceous matter, mainly derived from the land plants which covered the continent. As far south as lat. 35° these plants were for the most part Coniferous or Angiospermous, and included many genera now characteristic of temperate climates.

Through the Tertiary epoch our continent had nearly the form and area it now has, the Tertiary deposits merely skirting its

* xvvii, [2], 361.

borders. The Marine Tertiaries are nearly limited to the shores of the present oceans, while the patches of strata of that age found nearer the centre of the continent are all, so far as I have observed or heard, of fresh water or estuary origin. Between the western base of the Sierra Nevada and the Mississippi there are, I believe no Tertiary beds not of this character, and the larger part of the great central plateau has never been covered with Tertiary or Drift sediments, but has, since the close of the Cretaceous epoch, been as now, dry land.

The facts which I have enumerated seem to indicate that over this ancient land the isothermal lines were curved much as now, and that during the Tertiary ages there was, perhaps, as great a difference between the climate of the Pacific and Atlantic water sheds as exists at present.

ART. XXI.—*Abstract of a Meteorological Journal, kept at Marietta, Ohio:* lat. 39°·25 N. and lon. 4°·28 W. of Washington City; by S. P. HILDRETH, M.D.—For 1859.—[Thirty-third annual report.]

1859. MONTHS.	Thermometer. Mean temperature.	Maximum.	Minimum.	Fair days.	Cloudy days.	Inches of rain and melted snow.	Prevailing Winds.	Barometer. Maximum.	Minimum.	Range.
January,	33·00	63	4	17	14	3·10	W., S.W.	29·85	28·95	0·90
February,	37·77	62	10	10	18	7·20	S.W., & E.	29·60	28·85	0·75
March,	48·48	77	25	12	19	5·08	S.E., & S.W.	29·70	28·80	0·90
April,	52·03	83	28	14	16	6·46	N., & N.W.	29·60	28·85	0·75
May,	67·20	90	46	21	10	1·56	S.W., & N.	29·50	29·10	0·50
June,	67·23	93	33	17	13	4·62	S., W., & E.	29·70	29·20	0·50
July,	74·23	102	49	23	8	1·08	E., S.E., & N.	29·73	29·20	0·53
August,	72·10	95	46	15	16	4·46	S., & N.	29·53	29·30	0·23
September,	63·51	86	46	12	18	4·95	S., & W.	29·65	28·85	0·80
October,	49·12	79	26	17	14	2·79	S.W., & N.W.	29·85	29·00	0·85
November,	45·50	72	20	20	10	2·08	S.S.W., & E.	29·75	28·80	0·95
December,	30·48	71	5	12	19	5·17	N. & W.,			
Mean for year,	53·38					Rain 48·55 inches.				

Remarks on the seasons.—The mean temperature for the year 1859 is 53·38, which is somewhat above the average for this locality.

The amount of rain and melted snow is 48·55 inches. The average in a series of years, being forty-two inches, falling occasionally to thirty-two inches, and again rising, as in 1858, to near sixty-two inches, so that our climate is quite variable in this respect. The number of cloudy days bear testimony to the humidity of the year.

Winter.—The mean of the winter months is 37°·14, which may be considered very mild for this latitude. The thermometer was at no time as low as zero, so that it did not make ice sufficiently thick for filling ice houses, only two or three inches being the extent of the best, and the main supply for summer use was brought from the heads of the Muskingum river. Steam boats were laid up only a few days, by the floating ice, during the winter. In the eastern states the cold was excessively severe. On the tenth of January, after a great snow storm on the eighth of that month, extending from the borders of Pennsylvania to Maine, the mercury sunk in Salem, Mass., to 23° below zero, in the city of New York to 11° below, and at Ogdensburgh, N. Y., to −38°. The extreme cold on the Atlantic coast was said to be greater than at any time during the last seventy years.

Spring.—The mean for the spring months is 55°·90, an unusual high range, being nearly four degrees above 1858, and more than ten above 1857, that being only 45°·89, so that there is a wide range in the temperature of our springs, which is most strikingly apparent in the blossoming of trees, especially that of the peach, there being a variation of not less than forty days in the opening of the fruit buds of this highly prized tree. The later the bloom is retarded the greater the chance for a crop, but so variable is the climate of southern Ohio, that only one season in three can be counted on for the production of this delicious fruit. The apple crop is rather more certain, and yet nearly every other year is a failure from the blighting effects of late spring frosts. But for this drawback it would be one of the most productive countries in the world in fruit, and the valley of the Ohio as celebrated pomologically as it now is for the growth of Indian corn. The unusual heat of the spring is chiefly attributable to the month of May, which was 67°·20, or six degrees above the average temperature, which is sixty-one degrees. The heat was nearly that of June, and rarely experienced, as there is commonly a difference of ten or twelve degrees in these two months. The peach was in blossom this year on the 28th of March, and the apple on the 12th of April. In 1857 the peach opened on the 2nd of May, and the apple on the 9th.

Summer.—The mean of the summer months is 71°·19, which is not much below the average, notwithstanding the uncommonly low temperature of June. The unprecedented occurrence of a severe and destructive frost as late as the fifth of June overwhelmed the country with fear and astonishment; at a period in the growth of wheat usually considered as past all danger of this kind, a sudden change of temperature in one night spread destruction and ruin to a large portion of the fields of this important cereal over all the central portions of the valley of the

Ohio, and extending from Iowa to northern New York. Indian corn shared largely in this calamity. The warmth of May had hastened the growth of this plant in many fields to the height of twelve or eighteen inches. In all such cases the fields had to be replanted, but where it was only a few inches above the surface it recovered from the injury, and produced a fair crop. The wheat being in full head, and much of the grain in the milk, was entirely ruined. Potatoes were badly frosted, but in a good measure regained a healthy state. Peaches and apples, which in most orchards had attained the size of almonds, were so much damaged as to fall from the trees in a few days, and only certain favored localities ripened any fruit. So serious an injury from untimely frost has not been experienced since the first settlement of the state in 1788. In the year 1834, severe frosts visited Ohio as late as the middle of May, but the wheat crop was not so far advanced, being only in blossom, and by throwing up new stalks from the uninjured roots, produced finally an abundant harvest. Providentially the autumnal frosts of 1859 were retarded until near the close of October, and the late planted fields of corn were fully matured, to the great delight and wonder of the husbandman, for the failure of this grain would be a more serious calamity than that of wheat, as both man and beast largely depend on it for sustenance.

Autumn.—The mean for the autumnal months is 52°·71, which is rather below the average, but was sufficient to ripen all the late crops. Sweet potatoes were uncommonly good in quality, and abundant in quantity. Buckwheat was largely cultivated, partly in place of the common grain, and produced a great yield. The Catawba and other grapes ripened well, and abounded in saccharine principle, so necessary in making good wine. The smaller fruits were plentiful, so that on the whole we have more cause to be thankful, rather than to complain of the dealings of Providence in the past year.

Floral Calendar, &c.—February 25th, Bluebird heard; 27th, Yellow garden crocus in bloom; March 4th, Many birds of passage seen and heard; 9th, White crocus; 12th, Hepatica triloba; 14th, Golden bell or Forsythia viridis, Acer rubrum, (Red maple), Ulmus Americana; 16th, Hepatica acutifolia; 21st, Grass quite green in pastures; 22d, Magnolia conspicua, Claytonia Virg.; 26th, Red cherry, Balm of Gilead, and Sugar maple; 27th, Crown Imperial; 28th, Peach tree, Red Pyrus Japonica, Hyacinths; 29th, Sanguinaria Canadensis; 31st, Gooseberry.—April 1st, June berry; 2d, Dandelion, Pink colored Pyrus-Japonica, Cherry and Plum, Primroses; 4th, Flowering almond, Anemone nemorosa; 5th, Phlox divaricata, Dielytra cucul.; 7th, Annona triloba, Papaw; 11th, Burgundy pear, Trillium grandiflorum; 12th, Double flow.-Peach, Siberian crab, Spiræa prunifolia, Cercis Canadensis, or Red bud; 13th, Apple tree;

14th, Chickasaw plum, Strawberry, Sedum ternatum; 17th, White ash tree; 22d, Cornus Florida; 24th, Dodecatheon Amer.; 27th, Harebell; 28th, Vernal snow drop; 30th, Tulips.—May 2d, Tree peony, var. papaw; 4th, Haw tree, Dicentra spectabilis; 5th, Mountain ash, Aquilegia Canad.; 6th, Magnolia tripetala, Viburnum, (Snow ball); 8th, European Horse chestnut; 10th, Rose colored peony, Yellow Harrison rose; 12th, Blackberry and Robinia Pseudacacia; 15th, Rose Acacia and Annual roses; 16th, Iris Persica, Crimson peony; 17th, Purple peony; 18th, Moss rose; 19th, White peony, also several new peonies from seed planted five years ago, bloom first time; 21st, Peas on table, planted in January; 22d, Syringa Philadelphica, Strawberry ripe; 24th, Bulbous Iris; 26th, Foliage of trees unusually rich and fine; 27th, Fragrant peony, and large rose colored; 28th, Linocera flexuosa; 30th, Erigeron annuum.—June 1, Star of Bethlehem; 2d, Common cherry, ripe; 5th, Severe frost, killing wheat, corn and fruit, made ice in a bowl of water, half an inch thick, a few miles west of Marietta; 12th, Canterbury bell in bloom; 19th, Red raspberry ripe; 23d, Pennsylvania lily in bloom; 27th, Wheat harvest begins in fields that escaped the frost, on high hills, or where protected by the fog from the rivers; 28th, June apple ripe.—July 5th, Chestnut tree in bloom; 8th, Sweet bough apple and Gravenstein ripe; 12th, Gladiolus flori; 13th, Blackberry ripe, but a large portion destroyed by frost, American broom in bloom.—August 8th, Catherine pear ripe; 13th, Watermelon ripe.—September 1, St. Michael pear; 15th, Seckel pear fully ripe.

In every month during the past year there has been more or less frost, as in the year 1816.

ART. XXII.—*Geographical Notices;* by DANIEL C. GILMAN. No. XI.

BIOGRAPHICAL SKETCH OF DR. KARL RITTER.—The death of Dr. Karl Ritter, the father of the modern science of Physical Geography, and one of the most eminent and beloved of the scientific men of Germany, has already been announced in this Journal. We present herewith a sketch of his life, translated and condensed from a highly interesting tribute to his memory which is attributed to the pen of Dr. Kramer of Halle, in the Berlin *Zeitschr. für allgemeine Erdkunde.*

CHARLES RITTER was born at Quedlinburg the 7th day of August, 1779. His father, a man of noble character, fine feelings and a pious mind, was physician to the Abbess of the Convent there, and was much esteemed for his skill. However, in consequence of the slanders of an envious individual, he lost the largest part of his practice, and, although his good name was restored after the lapse of two years, and his clients returned, grief and sorrow had so heavily weighed upon him during this time, that in the full strength of manhood, he succumbed to a typhous fever. He left an almost destitute widow with five little children, of whom the eldest, a boy, was ten years of age, the fourth, Charles, only five years old. This situation of the poor widow, a noble and highly educated woman, excited the utmost sympathy of her neighbors. All endeavored, either by words, or in a more substantial way, to make good the wrong which had been done to her husband. She found many sympathizers, away from home. Thus, the Prince of the adjoining Bernburg took care of the education of the eldest boy. Salzmann, the celebrated educator, a former associate of Basedow, had bought Schnepfenthal, and was about to open an educational institution there. He had made it a point to take a boy as his first pupil, gratis. A notice in a journal of the death of Dr. Ritter at Quedlinburg, who had left a widow with five little children, first attracted his attention. Soon after he sent two of his friends there, to make the acquaintance of the children and to see whether there was a boy amongst them, that would conform to his wishes. They decided in favor of little Charles. The mother, though with a sorry heart, assented, and, at the invitation of Salzmann, brought the child herself to Schnepfenthal. She was accompanied by one of her elder sons and Gutsmuths, then a candidate of theology and instructor of the children, who had not left them, although the mother had declared that she was no longer able to pay him his salary. A residence of a few days in Salzmann's house cemented the ties of mutual friendship and esteem, so that Salzmann, shortly before their departure, expressed a wish to keep the older boy also. To Gutsmuths he proposed to remain in Schnepfenthal as a teacher. This had been a secret wish of the mother, but she did not think it possible. Ritter accordingly came to Schnepfenthal, the first pupil of the new institution, and remained there for eleven years, until he went to the university. This lovely spot, which Ritter always considered his true home, was situated at one end of the 'Thueringer Wald' and was surrounded by a most charming landscape, having in one direction a view of a far extending fertile plain, richly adorned with cities and villages; in the other, there rose well timbered mountains of various shapes, intersected by fine valleys. All around was activity and life. Here he received from his early youth the most vivid impression of the glory of God's creation, of the variety of formations on the surface of the earth, and their special relations to the life upon them. Here Ritter grew up under the guidance of excellent men and skillful teachers. Those that exerted the greatest influence upon him, were Salzmann himself, Bechstein and Gutsmuths, the latter of whom continued here also, to take special care of young Charles, and probably implanted in him a love for geographical knowledge.

The method of instruction was that suggested by Basedow, and tried first in the Philanthropin at Dessau, but it was freed from those vain and needless peculiarities that adhered to it there. Classical languages were less studied, but the most attention was paid to all those sciences and accomplishments, which stand in direct relation to life, and among these the modern languages occupied a more prominent place, than anywhere else. To this an unusual impetus was given by scholars from different countries, who thronged, soon after the opening of the institution, to Schnepfenthal. By physical training, and by strengthening the character and intellect, a general and equal development of body and mind were especially aimed at, and, although practical rationalism pervaded the whole institution, darkening a little the deepest sources of true blessings, there still reigned piety, love, and the purity of high moral sense. Under these influences all those noble qualities of Ritter's heart and mind were developed, that distinguished him so much in after days. The future lay dark before him, and he had not decided upon his course in life, but he felt a strong desire to study, of which however there was as yet no prospect. His mother, though married again several years after the death of her first husband to the celebrated pedagogue Zerrenner, was not able to provide for him. But Providence interposed here also. A rich merchant from Frankfurt on the Main, associated with the large firm of Bethmann, Mr. Hollweg, visited the institution at Schnepfenthal and became very much interested in the young Ritter. After hearing of his circumstances, he declared himself willing, on the recommendation of Salzmann, to furnish the necessary means of study, upon the condition however, that Ritter, after the completion of his studies, should enter Hollweg's house as instructor of his children.

So Ritter went, at the age of 17 years, to the University of Halle, and was matriculated November 2, 1796, as *studiosus cameralium* under the prorectorate of Curt Sprengel. Here he remained for two years. Halle was then the centre of great scientific activity. F. A. Wolf especially was then in the height of his renown. Ritter did not pursue a specific course of studies, as his previous education had not been directed towards that channel, which however he sometimes regretted in later life. He often mentions A. H. Niemeyer, to whose circles he had access, and in whose house he lived, and who exercised upon him an important influence in improving his mind and inciting him to farther study. Niemeyer occupied then a prominent place in the pedagogic world, and that work by which he became most known, "Grundzuege der Erziehung und des Unterrichts" (or, Principles of education and instruction), first appeared in 1796, and must have been of special interest to Ritter, as he was himself preparing for the calling of an instructor.

In 1798 Ritter left Halle and entered Hollweg's house as instructor of his four children, especially of the two boys, one six, the other three years old. It was a great change for the young man of 19 years, to step out from the quiet circles in which he formerly had moved, into the midst of a world quite unknown to him, and to move among the aristocracy of a mercantile city. He had to struggle with many difficulties. But he went to his task in all earnestness, and with the ardor of a true and powerful mind, conquering all impediments so completely, that he gained results such as but few instructors can boast of. This was especially true in the case of his younger pupil, the other having died in the bloom of his youth. Ritter conducted the education of the former until he went to the University, and this pupil is the present minister of clerical, educational and medicinal affairs in Prussia, von Bethmann-Hollweg. Equally successful was he in the education of a son of the celebrated S. Th. Soemmering, and out of this relation of teacher and pupil grew the most intimate friendship and love, which lasted for life. During his stay in Hollweg's house Ritter came in contact, and even into nearer relations, with many eminent men, and by the intercourse with them his ideas expanded and became freer and more independent. Amongst those that exerted the greatest influence in this respect, S. Th. Soemmering must be mentioned above all, a man of great genius and deep scientific knowledge. Ritter thus speaks of him in the introduction to the second edition of his 'Erdkunde:' 'If in the explanation of the laws of the geographical relationship of all animated nature there should be prominent some interesting opinion and view, then the author is indebted for this whole tendency of investigation to the long, instructive, and I say it with pride, familiar intercourse with a noble man, S. Th. Soemmering, a man who is an honor to his century and nation; for his spirit filled others also with the premonition of the depth of nature, which his own genius had penetrated into its most hidden mysteries.' Ritter was also befriended at Frankfort by J. G. Ebel, the author of the classical work on Switzerland. This was not only of the highest importance for him during his repeated travels to Switzerland in relation to the knowledge of this country, but it also impelled him to farther study. Ritter, speaking of Ebel, says in the above-mentioned introduction: 'What the present work may have of vivacity and warmth, it owes to an intercourse of many years with this excellent man at the time I commenced it.'

Ritter's mind moreover was much aroused by the daily intercourse with men of equal aim, and inspired with the same ardor for the education of youth, such as E. Mieg and J. B. Engelmann. Besides he sometimes came into transient but important intercourse with men of eminence who travelled through Frankfort. He met (to mention only a few names) Alexander von Humboldt and Leopold von Buch in Hollweg's house. But even life itself in this old interesting city, uniting so many instructive elements, showing so many different relations of a most various character, and being situated in the midst of the district of the largest river in Germany, always invited to new observations, wanderings and study.

Ritter used with the greatest ardor all these opportunities to acquire information. The time of his sojourn at Frankfort was a time of the most various studies. So he applied himself with much zeal to the classics, and read with the assistance of his friends, F. C. Matthiae and J. F. Grotefend, then at the head of the gymnasium at Frankfort, the most important works of the Greeks and Romans, but the tendency of his mind towards geography always appeared with marked prominence. In order to become entirely at home in this department, he not only studied thoroughly the most important works on the subject, but he also made observations of his own in frequent excursions to different parts of the country. The ability to draw with the greatest ease those objects in a landscape which were important to him, and so to fix them forever, was of much service to him. He always brought a number of characteristic sketches home from his journeys, which served both for himself and others as illustrations of his observations. This tendency towards geography was manifested in his first contributions to the 'Neuen Kinderfreund,' edited from 1803–1806 by Engelmann in connection with his pedagogic friends, but it became yet more apparent, when in 1806 he published his six charts of Europe, and not long after, in 1811, when his geography of Europe (2 vols.) appeared. In both works the peculiarity of his geographical perception is thus early indicated. They are the groping essays, the *incunabula* of what was lying in his mind. But, before his ideas could come to maturity and light, other preparations were to be made. As such, in different respects, must be considered the journeys which he, from the year 1807, repeatedly undertook with his pupils to Switzerland and Italy, and the last of which, commenced in 1811, comprised several years. These travels must indeed have been a rich source of instruction to his observing and powerful mind, that was so well prepared and matured by assiduous study and labor. Just these countries are the most expressive representations of the most important and most various geographical types which Europe has to show.

Switzerland, the most important parts of which he crossed in different directions, impressed him deeply with the grandeur and glory of a majestic and infinitely rich nature, which invited irresistibly to the study of this gigantic structure. Italy, on the other hand, which he passed through down to its southern point towards Sicily, furnished him important information in reference to volcanic activity, exhibited to him the relation of land to sea, showed the effects of climatic differences and the close connection of the nature of the country with the development of its people. Those treasures of art, which Italy possesses so abundantly above all other countries, must have added their proper share also to his store of knowledge laid up in his naturally fine and carefully educated mind.

During his travels in Switzerland he met many men of eminence and note; amongst them Pestalozzi, von Tuerk, Niederer and many others of that district. He spent many happy and instructive hours in their society, and between him and many of them a mutual friendship was established. He always remembered Pestalozzi, whom he often visited at Ifertên, with reverence and gratitude, and had a picture of him in full length in his study.

The most important point in Switzerland for him was Geneva, where he remained from the middle of the year 1811 for more than twelve months. This city was then particularly noted for the active part it took in the cultivation of science, especially natural science, and was much distinguished by the fine tone of its society. Saussure, the first man of the city and the state, had shortly before died, and his pupils, men of European fame, such as M. A. Pictet, de Candolle and others, were considered the centres of the higher circles. With the first, Ritter became very intimate and to him he owes many valuable hints. St. Gervais, close at the foot of Mont Blanc, was another very interesting point for Ritter, as it offered to him an opportunity of observing the nature of high mountains in all their details. From this spot he made that tour around the Mont Blanc, of which he gives such an interesting and instructive description, in explanation of the bas-relief executed by Kummer.

During his travels in Italy, Rome most especially attracted his attention. It was not only the centre of numerous monuments of history and art, but he also met here men like Thorwaldsen, Overbeck, Cornelius, and others, who by their genius and love for the arts, had raised them again to a most flourishing state. By intercourse with such men, Ritter's insight into the nature of Art was much enlarged.

Thus variously enriched, he returned home, and soon commenced that work, which was the chief production of his whole life, and which will make his memory immortal. In order to prepare it, he went in 1814 to Goettingen with his two pupils, who at that time were beginning their academical studies. Here he used all the means of learning within his reach, he searched out the rich treasures of the library, was in active intercourse with the masters of science, (Hausmann especially was dear to him,) and did not disdain to enter again as a student the auditoriums of the professors and to hear lectures on the most various subjects. After a residence of two years he went to Berlin (1816) and there gave his work its last finish, after which it was put to press. Next year he went again to Goettingen to superintend the publication of his work, of which the first part in 1817 with the title, 'Erdkunde im Verhaeltniss zur Natur und Geschichte des Menschen oder allgemeine vergleichende Geographie als sichere Grundlage des Studiums und des Unterrichts in physikalischen und historischen Wissenschaften,' or, Geography in relation to the nature and history of man, or general comparative geography as a safe foundation in studying and teaching physical and historical sciences. In this work geography had been entirely remodelled and changed. It had indeed been raised to the rank of a true science, constituting the link between the natural sciences and history. The first part contained Africa and part of Asia; a year after, the second part appeared, which completed Asia.

We abstain from giving a detailed description of this work, as it is well known, and as the method, in which geographical matter is treated here, has been adopted throughout the whole scientific world. Ritter's aim is briefly indicated by its title; a more detailed account of its leading ideas however is given by himself in his introduction to the second edition of the first volume, which appeared in 1822. Ritter's intention was, to give with the greatest accuracy a vivid image of the formation of the superficies of the earth in its horizontal and vertical dimensions by means of a conscientious and careful use of all existing sources, and to represent and explain the characteristic qualities of its parts and their relation to each other and to the whole earth, but at the same time to make it serve as a substratum of all animated nature, and as a foundation and condition of the development of the different nations and the whole human species in their manifold mutual relations to one another.

This was a stupendous task, but Ritter performed it marvellously well. Its execution required a combination of great and varied talents, such as rarely ever have been or will be found, cultivated by deep and assiduous study. In it we see powerful and truly ingenious displays of general geographical intuition and combination, we perceive a care, that indefatigably penetrates into the deepest recesses and most minute details, we find evinced an extensive knowledge of the natural sciences and a perfect command of extensive historical materials, and lastly, a truthfulness and thoroughness of learned inquiry combined with the rare gift of a rich, fresh, vivid and expressive representation. Truth and knowledge of the living God were the springs that actuated his mind and after which he aspired. Hence his humility, hence his close and perfect application, his concentration upon the subject before him. No difficulty ever deterred him in his investigations, although the matter before him was continually and vastly accumulating. His work was to him, as he wrote in his diary, when, after a long interruption, he again commenced his labor, 'his song of praise to the Lord.'

When Ritter had completed the first two volumes of his geography (Erdkunde), in connection with a work that was the direct result of his Asiatic studies, 'Vorhalle Europaeischer Voelkergeschichten vor Herodotus um den Kaukasus und an den Gestaden des Pontus,' or, 'Vestibule of the history of European nations before Herodotus around the Caucasus and at the shores of the Pontus,' he received in the year 1819 a call to be Professor of history at the Gymnasium of Frankfort. In the fall of the same year he married. In September 1820 he accepted a call as Professor extraordinarius of history in the Military School and the University of Berlin.

At Berlin the second half of his life commences, and here the richest fruits of his former labors and preparations matured. Berlin was indeed the most favorable city for such an object as he had in view. Nowhere were the means for study and instruction so abundantly and generally supplied. Both the University and the Military School displayed great scientific activity, which was kept alive by eminent scientific men, into whose circle Ritter soon entered as a highly esteemed member. His lectures were soon well attended at both institutions. Besides he kept up an active intercourse with other scientific celebrities, among whom Leopold von Buch, but especially Alexander von Humboldt may be mentioned, whom he highly esteemed, and with whom he lived on terms of the most intimate friendship. Moreover, Ritter lived here in the midst of his nearest relations; one brother lived in Berlin, another near the city, and among his dearest friends, was his former pupil, Hollweg, then also professor at the University. All this encouraged him much, and instigated him in various ways to still farther study and advancement. Ritter's activity was now, besides the duties of his office, chiefly directed towards preparing a second edition of his Erdkunde, of which the first volume appeared in the year 1822. It was much enlarged and in every respect more complete than in the first edition, although now only comprising Africa. The continuation of this work however suffered a long interruption and the chief reason for it was, that his official duties claimed his attention more and more. Accordingly he entered, though only for a short time, as a member, the scientific examining commission for history and geography, and soon after the death of his friend Woltmann he also undertook the historical lectures at the Military School.

In 1825 he was appointed Director of Studies to the corps of cadets. Besides, he instructed Prince Albrecht of Prussia in history for many years, and received, especially during the winter months, frequent invitations from the Crown-Prince to lecture on history and geography before him and some of his nearest relatives and friends. Similar invitations came also from other sides, and he very often complied with them. Thereby a very considerable part of his time and strength was claimed, as he was always wont to apply the utmost care and scrupulousness to all his subjects. Nevertheless by his extraordinary diligence, which was a necessity to him, assisted by a robust healthy body and by his collectedness and freshness of mind, he still found leisure to work and to promote and advance his scientific labors, which were never lost sight of. The external fruits of these labors were however confined in that period, to his reports in the Academy of Science, of which he was a member after 1822, and to brief essays, like the one on India in the Berlin Almanac for 1824. Many of the results of his studies were communicated to the geographical society, which he, in 1828, had founded in connection with several friends, and of which he was the chief supporter. Those travels which he regularly undertook during the long fall vacations were of the highest importance for him in every respect. They not only served him for bodily and mental recreation, but were also very useful for the advancement of his geographical studies, whether he was occupied in the observation of nature itself, or by investigations in important geographical centres, as Vienna, Paris, London, and other places. These journeys extended in very different directions over the countries of Central Europe and sometimes occupied the larger part of summer. The most extensive and important were: a tour to Greece, Constantinople, through Bulgaria, Wallachia, Sieben-Burgen and Hungary; repeated travels to Paris, through the southern, and at another time, western part of France, and the Pyrenees; through Belgium and Holland; through Denmark, Sweden and Norway; to London, and through a part of England. He often visited and explored, but always in different directions and with different objects, the middle and southern part of Germany, the system of the Alps in its different parts, Switzerland, and the northern part of Italy. The results of these journeys were a great variety of impressions and observations (mostly recorded and narrated in his detailed and highly interesting letters to his family), oral communications of a most varied character, and the establishment of manifold personal relations and connections.

In the year 1831, Ritter withdrew from all business and labor foreign to his geographical studies. He felt that if he would advance geographical science, which he considered the task of his life, he must concentrate his powers. Thus, when he again had leisure, the fruits of his studies became more apparent. There now appeared, from the year 1832, in quick succession, that series of volumes on Asia, of which he concluded the nineteenth a few weeks before his death. This work will be a lasting monument of his genius, and a standard work for all ages, however great the progress of geographical science may be hereafter.

The author's name grew in proportion with the progress of the work, his acquaintances increased in all civilized countries of the world, and his influence upon the course of geographical investigation and science was greatly augmented. He became one of the most important personal centres for the science, since he possessed an immense store of knowledge, and a sound judgment, and took a most active interest in all questions relating to this subject. He entered into everything, even if trifling or troublesome, with an amiability and urbanity that never tired.

He received marks of acknowledgment and distinction of all kinds. Most of the learned societies, in and out of Europe, made him a member, many Orders were given to him, and his Sovereign gave him frequent proofs of his personal favor during the many years of his residence at Berlin. Ritter occupied undoubtedly, as savant and author, one of the most exalted positions among his contemporaries. But he was not less great as a teacher also. There were few lecturers who exercised such an invariable power of attraction as he did. When in 1820 he first announced his lectures on general geography, no hearers came forward, and in the course of the term only a few presented themselves. Ritter began his lectures, but even in the next semester hearers were yet scarce, and this must not be wondered at, as but a few of the students had heard anything of Ritter, and the great majority of them considered geography as something hardly worth the hearing. This state however soon changed, and already in 1823 Ritter wrote in his diary: "Full auditory, I must take a larger one." The numbers of his hearers increased from year to year, so that sometimes even the largest auditory scarcely could hold them. Ritter was now looked upon as the one whose lectures had to be attended by every student of a high scientific aim. Which of his numerous hearers does not remember with gratitude the pleasant and instructive hours of his lectures? Ritter showed a most perfect tact, acquired by many years' experience; he knew how and what to select from the immense store of matter over which he had a perfect command; he knew what was best adapted to a verbal exposition and from what the greatest benefit would be derived. His delivery showed that he had deeply and thoroughly mastered his subject, which he always elucidated by drawings, placed on the black-board with great ease. Every one of his hearers felt the importance of the subject, perceived Ritter's deep scientific researches, and was delighted at the results, which were made so accessible, and at the improvement gained by means of them. His lectures were always instructive and always excited to farther study. His delivery was dignified, but everywhere and under all circumstances unassuming. His purity of mind, his modesty and amiability shone forth everywhere, and exercised a peculiar charm especially on those that came in closer contact with him. None ever approached him without meeting a most friendly and hearty reception. No effort in science, however imperfect, was made, that he did not acknowledge and encourage by his counsel and assistance. Egotism was entirely foreign to him; he was the truest and most affectionate friend; in his family most tender-hearted and loving, and his greatest pleasure was to see those around him happy. He himself was without children, but he was a father to many that were comparatively strangers to him. His mildness of temper exercised a most soothing influence upon all; his peace of mind, pervading his whole nature, could not easily be shaken, even by severe losses, such as the death of a dear sister and of his beloved wife in her full strength, both occurring in the course of a few days. These noble qualities of mind were the precious fruits of a strong and living faith. Ritter was a Christian in the full meaning of the word, although never saying very much on the subject and never raising himself up as judge of the religious belief of others. The holy word of God accompanied him everywhere, and to confirm its truth by the results of his investigations was his highest joy. His own words found after his death, are the best testimony: 'Although at present, while preparing for a journey to the western part of France and the Pyrenees, I am healthy and well, life nevertheless lies in the hand of God, whose mercy and grace has guided my fate so wonderfully and gloriously, that I cannot but sing to him, the Allgood, praise and glory with all my power, in all my thoughts and actions. Should it not please him to let me return to my beloved family and to my calling, but should he assign to me another place in his heavenly kingdom, that I may obtain happiness, a happiness which already here sometimes has moved me to tears of joy, then I ask my friends not to grieve over my going home, for all that the Lord does is done well.

My eternal fate my Savior in his great mercy will decide. In deep acknowledgment of my infirmities and sins, I am still full of trust and confidence, since I know that my Redeemer lives, who will make his people partakers of the mercy of the Eternal and Just one.'

Ritter's health was generally good. His constitution was strong and was hardened by exercise. His numerous travels, often on foot, renewed his strength, when weakened by close application to his fatiguing scientific labors. In his last years, however, many infirmities were felt. The Teplitz medicinal springs had relieved him several times, and Ritter tried them again in July 1859, but this time without relief. Great heat and hot baths seem to have weakened him more, and this weakness was still more increased by frequent hemorrhages from the bladder; he lost his appetite, and his strength begun rapidly to fail, even when his appetite partially returned. He died September 28, 1859, at 10 o'clock A. M., and was interred October 1, in the Marienkirchhof by the side of his beloved wife, who died in 1840.

Mr. Lentz's Report of his Explorations in Persia and Afghanistan.—We have received through the Smithsonian Institution, Washington, reports of the meetings of the Imperial Geographical Society of St. Petersburg, Oct. 7, and Nov. 4, 1859. From the former of these we translate the following account of the Russian expedition under Messrs. Khanikoff and Lentz into Afghanistan and Persia.

Mr. R. Lentz, who took part in the expedition to Khorassan, presented some interesting information in regard to the scientific results of his travels during the sixteen months which he passed in Persia and Afghanistan.

The main object of Mr. Lentz in his travels was to determine the geographical position and elevation of the places, which he visited; to investigate the three elements of terrestrial magnetism (declination, inclination and force of tension); to ascertain the heights of the mountains; and finally to make meteorological observations.

The expedition arrived at Astrabad, in the province of the same name, early in April 1858. As Mr. Khanikoff, to whom the direction of this expedition was confided, had gone to Teheran, Mr. Lentz employed his time, most profitably, during the absence of the former, in observing the movement of the chronometers and in ascertaining the absolute longitude of the city of Astrabad. He also determined at this place, in the same manner as at Ziared, the three magnetic elements, ascertained the height of some of the summits in the Albourz chain, and made meteorological observations.

Towards the middle of May, the expedition left Astrabad, and, having passed Albourz, stopped at Schakhroud. Mr. Lentz observed here, as everywhere in his travels, the movement of the chronometers, and determined again the three magnetic elements and the latitude of a great number of peaks in the Albourz chain.

On the first of June, when Mr. Khanikoff had returned, the expedition directed its course towards Meschel, the actual capital of Khorassan. During the whole route Mr. Lentz took great pains to determine the greatest possible number of geographical points and ascertain the elements of terrestrial magnetism. The five weeks of his sojourn at Mesched were spent in works of this kind, and he also made an excursion in the neighborhood in order to effect astronomical and barometrical observations.

The expedition left Mesched in the first part of August and reached Herat during the first days of September; having already determined a considerable number of geographical points, the position of which had been previously unknown.

In Khorassan, Mr. Lentz was chiefly occupied in determining with the greatest possible accuracy the absolute longitude of the most eastern point touched by the expedition. From Herat Mr. Lentz advanced to Tebbes, which is situated at the eastern limit of the salt desert of Khorassan, and then to Birdia-Sand, and succeeded in collecting many valuable additions to science.

In the midst of February 1859, the expedition left Khorassan and took its course towards Lasch, a fortified city and the capital of a little state of the same name. The observations made in this country are particularly interesting. They show that the terrestrial surface rises gradually from Herat to the passage Senhighé-Sia, near the city of Sabzor (Kingdom Hérat), where this elevation reaches a height of 5000 English feet; from this point the country gradually descends to a lake, the waters of which, however, have still an elevation of 1200 feet.

The expedition stopped, in its course, on the eastern shores of lake Zaré, near the place, where the river Kharoud or Adraskan empties into it. Mr. Lentz determined the absolute height and the geographical position of this point. The appearance and the dimensions of the lake change continually, sometimes the northern portion of it is dried up, and only its southern part is seen; sometimes the reverse takes place, according to the quantity of water, furnished by the three principal rivers, which empty into it, viz.: the Kharoud and the Ferraroud in the north, and the Khilmend, which flows not far from Kandahar, in the south. Sometimes it happens, that the waters of the lake divide into two parts, one towards the north, and the other towards the south, and are kept separated by a band of completely dry land as was the case at the time of the expedition. This at least is the account given by some Persian travellers, who had recently left the eastern shore of the lake and declared that they had crossed it by following this tongue of land.

Beyond the village of Nekh, the expedition came to a desert of 250 versts in extent. This they crossed in its narrowest part between the villages of Serri Tschakh and Dekhi-Seïf, at a place where in a length of 200 versts no trace of water was found. After a journey of four days they reached Kirman.

This place might be considered as entirely unknown up to this time. It appears from barometrical observations made there, that from lake Zaré (1200 feet high) the surface of the earth rises again up to the villages of Nekh and Serri-Tschakh (4000 and 5000 feet high), when the country gradually descends to its lowest point (900 feet high) in the desert at a place called Schakhri-Lut, but rises again as far as Khubbis, which is situated at the foot of the mountains, at an elevation of 1500 feet, and reaches its maximum (8000 to 9000 feet) at the top of these mountains, then it falls again towards Kirman, which however is still found to be 5500 feet high.

Over the whole area, which extends from Esd to Ispahan, (the point toward which the expedition directed its course,) the countries which border south and east upon the great salt desert, are 3000 to 4000 feet above the level of the sea; the same is the case with those countries which separate Schakroud from Mesched along the northern side of the desert; only in two places more considerable heights are found.

From Ispahan the expedition passed through Zerghendé, 10 versts from Tehran, went beyond the village of Firouska, reached the provinces of Mazanderan and Astrabad, and followed the course of the river Talar, which empties into the Caspian Sea.

Mr. Lentz determined, during his travels in Persia and Afghanistan, about two hundred geographical points; at twenty-eight points he could determine the three magnetic elements (intensity, declination and inclination); at twenty-nine other points, however, he observed only two of them. He ascertained with the aid of the trigonometrical calculus, the heights of about two hundred mountain summits, and about four hundred others were measured by him, and his travelling companions, with the aid of the barometer. Thus Mr. Lentz traced a profile of the whole country, which the expedition visited. Mr. Lentz gives also some interesting results of his meteorological observations, which he never neglected during the whole voyage. The barometrical observations show, with an incredible regularity, the slightest variations of the atmospheric pressure; the variations of temperature are also very regular, and it is interesting to state, that the temperature reached constantly its maximum about four hours after noon, and not two hours, as it is generally the case in our climate.

Throughout Khorassan the air is usually very little charged with vapors and its average hygrometrical state varies between 20 and 30 per cent. At Schakhroud, Mr. Lentz found only 17 per cent of moisture in the atmosphere, and in the desert near Kirman, only 14 per cent."

SCHLAGINTWEIT'S ETHNOGRAPHICAL COLLECTIONS.—Mr. Joh. Ambr. Barth, of Leipsic, has offered for sale a large collection of plaster-casts taken from the heads, hands and feet of individuals in the different castes and tribes of India, and has published a carefully prepared catalogue of the series. We make the following extract from his announcement:

"Messrs. Hermann, Adolphe and Robert de Schlagintweit, the enterprising travelers in India and High-Asia, having, since the year 1854, had charge of a scientific mission from the India House, have been enabled during their travels, in addition to their researches in physical geography and geology, to devote much of their time to ethnology.

"The various countries through which they passed, some of which have hitherto been but little explored, and others never reached by Europeans, afforded peculiarly advantageous opportunities for pursuing their ethnological researches.

"Besides measurements and photographs, they, in collecting their materials, made also casts of the features of living persons taken in plaster of Paris; 275 casts of faces were thus made, and 37 of hands and feet.

"The moulds have been reproduced by galvanoplastic deposits of copper, which gives without the least contraction the most minute irregularities of the skin with great perfection. This first series however was found not sufficiently strong, and the attempts which have been made to produce the heads in as great perfection as possible, have led to a different method, consisting in making strong metallic casts of zinc the basis, coated with a galvanoplastic deposit of copper, varied in color according to the different degrees of color of the native tribes. To exclude as perfectly as possible the change of the tints by the gradual oxydation of the copper, a thin stratum of colorless varnish has been put most carefully over the heads.

"Together with these casts, measurements of the various proportions of the skull and the body have been taken, which will be spoken of in detail in the work, which Messrs. de Schlagintweit are about to publish under the auspices of the President and Council, now at the head of Indian affairs, who having the same interest in science as the Court which preceded them, have found this collection an object of particular importance.

"This work, 'Results of a scientific Mission to India and High Asia,' is independent of this collection: it is in progress of being printed and is published by F. A. Brockhaus, publisher at Leipzig.

"The ethnographical part of Messrs. de Schlagintweits' work will chiefly treat of characteristic features obtained by well defined measurements, constantly referring to the casts made. Many other individuals, together nearly 400 persons, have been carefully measured, and the zealous labors of other distinguished, particularly Indian, ethnologists, amongst whom we name Buist, Carus, Cunningham, Davis, Walter Elliot, Falconer, Hodgson, Hooker, Humboldt, Morton, Latham, Owen, Rawlinson, the Stracheys, Sykes, &c., will be found to have been carefully studied for the purposes of scientific generalization.

"We scarcely need add how important objects these facial casts will be for all those who take an interest in such researches, while the interesting nature of the objects themselves, as well as the careful and novel mode of their execution, will render them a most beautiful and important addition to public and private museums.

"This collection has met with great approbation; we mention as particularly important the well known personal interest of the late Baron v. Humboldt."

Adolphe Schlagintweit's Death in Turkistan.—Our readers are already acquainted with the fact that one of the bold brothers, whose expedition to the Himalayas has attracted the attention of the whole scientific world, fell a victim to his energy. From the surviving brothers, Hermann and Robert, we have received a printed document in which are given all the official reports which have yet reached them in respect to the fate of Adolphe. The conclusions at which they have arrived are thus concisely stated.

"The information from India and Russia, collected from natives by European officers of the adjoining districts, concur but too accurately in establishing the fact, that Adolphe Schlagintweit was killed at Káshgar in Turkistán (Central Asia) in August, 1857, falling a victim to his scientific mission.

"He was recognized as a European after having passed the Karakorúm and Küenlüen, in disguise, where before us no European had ever travelled; he had taken a route more westerly than ours, and had succeeded in penetrating far into Central Asia.

"The reports which have reached us are so various, that they do not of course all agree, as to the immediate cause and particulars of his death; yet it is evident from all of them, that the political condition of these countries, and the circumstance of the deceased being recognized as an officer of the Indian Government, notwithstanding every precaution, essentially contributed to his tragic end. Even with the lively sympathy ever so energetically evinced by England, in the fate of scientific travelers, it will scarcely be possible to succeed in bringing the murderers of our brother to account.

"According to some reports he perished in consequence of having taken up the cause of some captive Bhot-Rajpúts, British subjects, interceding for them, that they might not be executed or sold as slaves. Other accounts state the immediate cause of his death was, his having been recognized as a European, and fallen by the hand of fanatic Mussălmáns.

"Notwithstanding our most zealous exertions for some months past, in endeavoring to obtain his manuscripts, drawings, etc., we have not yet been successful in learning anything definite about them: still, however, many very important geographical communications have been made to us by his followers, and we are not without hope that from the active sympathy which the Indian Government has always displayed in our scientific mission to India and High-Asia, nothing will remain untried that can tend to the rescue of his last papers."

LETTER FROM DR. LIVINGSTONE.—At the January meeting of the American Geographical and Statistical Society of New York, the following letter from Dr. Livingstone, the celebrated explorer in South Africa, was read by D. W. Fiske, Esq., Librarian of the Society:

Tette Zambesie, Feb. 22, 1859.

"*My Dear Sir*—Having been elected a member of your Society, I take the liberty to send you a short account of our attempt to open the interior of Africa, in the hope that, though it may not appear interesting to your members, it may, at least show my good will and desire to perform a corresponding member's duty.

We entered the delta of the Zambese in May, 1858, taking the most southerly branch we could find, but after ascending about seventy miles we found it impossible to enter the Zambese by that, as the points of junction were filled up with reeds and other aquatic plants. You may have a clearer idea of the region if you bear in mind the fact that the Zambese has in the course of ages formed a delta, which juts out into the ocean, and forms the most prominent part of the coast. The prevailing winds of these quarters beat, almost constantly, against the head of the promontory. These, aided by the oceanic currents, have helped to dam up the main stream, but the pent-up waters have escaped sideways. The main stream called Gualeo enters at the point of the promontory most exposed. To it we went after leaving the southern branch, but saw no possibility of entrance during three days, though her Majesty's ship Lynx has since found a channel in it, after a search of ten days. We then proceeded to examine the side branches and the very first we came to called Kongone, was all we required. There are other good ports, but all in the side branches. There are also communicating branches between these, and those within the influence of the tides are generally deep.

Having got into the main stream, we found that we had, in going to it and spending a month there, allowed the water to fall considerably. It was, also, so very much lower than usual that the Portuguese prophesied that we could not ascend ten miles. It was said, also, that war was raging, and no one would be allowed to go up, even if he could. Our ship drew nine feet seven inches, and she was under engagement to go to Ceylon. We, therefore, to avoid detention in the river, sent her off, and went up to the seat of war in a small steamer, drawing two feet six inches. We had no difficulty with the "rebels," as they were called—indeed, we got pilots from them, and continued ever after on the best of terms with the Portuguese. They were called "rebels," as they had all been runaway slaves, and bore the marks, in brands on their chests, of their former servitude. Slaveholders here must be civil, for it is so easy to run away, that if slaves go to the Landius, who are of the Zulu family, they never deliver them up. I have never heard of but one case to the contrary, and the owner—a great favorite of theirs—was obliged to give them his full value. This is a digression, but I may finish off by saying that the Portuguese governor attacked the rebels, and they retired before him, there being plenty of iron for all parties.

We continue carrying on luggage up the river till November, when it reaches its lowest point; and with care a flat-bottomed boat would do business even then. We know it now at its very worst, and, as it spreads out to from one to three miles in breadth, it is in many of the crossings not more than two and a half or three feet. Just now the water stands twelve feet above low water mark in November, and we are all quite sure that during at least eight months in each year a steamer of four or five feet could trade without embarrassment. The reason why so little has been known about the Zambese river, has been the branching in the stormy promontory by which it was hidden from navigators. And their easy chair geographers, dreaming over the geography of Ptolemy, actually put down the Zambese as flowing into the sea at Quilimane, which, in his days, it probably did, though not a drop of Zambese water, in ordinary circumstances, reaches that part. Had some branch of the Anglo-American family planted their footsteps on its banks, we are such a babbling newspaper set, the world would have known all about it long ago; and no one would have ventured to play with this river as has been done, making it lose itself and flow under the Kalobaro desert. You may form a better idea of its size if I tell you of one of the branches. We ascended the Shire lately, fully a hundred miles from the confluence, and found it with a two fathom channel all the way up. It varied from 80 to 150 yards in width, and contains no sand banks. It flows in a beautiful fertile valley, about twenty miles high, and fringed with mountains of great beauty, well wooded to the top. Mora M. Vala we ascended, and found it 400 feet high. (This, by mistake, is placed on the wrong side of the Shire, in my map.) It was well cultivated on the top, and had several fine little fountains, the waters of which were slightly chalybeate; they have a hot sulphurous fountain at the base, (temperature 174° Fahrenheit). The people had many sweet potatoes, holcus sorghum, and other grains, and pine apples, lemon and orange trees. They were very hospitable, and independent. The vegetation is very different from the plains, and so is the climate; yet with all these disadvantages, no use has been made of it as a sanitorium by the Portuguese, and as far as we can ascertain, this river has never been explored by Europeans before. One part of the luxuriant valley of the Shire is marshy, and abounded in lagoons, in which grow great quantities of the lotus plant. The people were busy collecting the tubers, which, when boiled or roasted, resemble chestnuts. They are thus Lotophagi, such as are mentioned by Herodotus. Another part of the valley abounds in elephants. My companions estimated the numbers we saw at eight hundred. Herd upon herd appeared as far as the eye could reach; and noble animals they were. We sometimes chased them in our little steamer, for the shore branches off occasionally and forms islands. The upper part of the valley is well peopled, and many of the hills are cultivated high up. But never having seen Europeans before, they looked on us with great suspicion. They watched us constantly, well armed with bows and poisoned arrows, ready to repel any attack, but no incivility was offered when we landed, nor were our wooding party molested. We obtained what may be considered reliable information that the Shire actually does flow out of Lake Nyanga. We were brought up by a cataract, but five days beyond this point the water is smooth again, and Arabs come down in canoes from Nyanga thither. Seeing the suspicions we had aroused, we deemed it unsafe to leave the vessel and go overland. But no collision took place. The greatest coward fires first, so, thinking we had as much pluck as they, we did not lift a gun, though we were ready to fire, or rather shoot. We did nothing to make us ashamed to return, and mean to do so next month; and if we have their confidence we may go farther. They had abundance of provisions, and sold them at a cheap rate. Also cotton of two kinds—one indigenous, short in the staple but very strong, and woolly to the feeling; the other very fine, and long in the staple. We brought a number of specimens of their spindles and yarn, and it was quite equal to American uplands; did not offer them any American seed. The cotton plant is met with everywhere, and though burned down annually springs up again as fresh and strong as ever. They grow sugar cane too, bananas, &c. The men are said by the Portuguese to be very intelligent, but very mild. The women wear the lip ornament, round one of which I put my pen. The slit is made in the upper lip, at first, by a ring in childhood. The ends are gradually pressed closer together, and cause absorption till a hole is made. This is enlarged by bits of reed, until in a lady of fashion a ring, either hollow or cup shaped, is inserted, and the edge of the lip protrudes beyond the perpendicular of the nose at least an inch. I am thus particular in case our own ladies, who show a noble perseverance when fashion dictates, may wish to adopt lip ornaments.

Above this we have a rapid, called Kebra, or rather Kaorabasa. When the water is low it shows a deep grove, with perpendicular sides. When steaming up this the man at the lead kept calling "no bottom" at ten fathoms, and the top of the walls of the grove towered from 50 to 80 feet above our deck. It is from 60 to 80 yards wide, but at this season is comparatively smooth. There were some cataracts in it which high water obliterates. This steamer is too weak to ascend. She being only ten horse power, and her plates $1\frac{1}{16}$th of an inch thick, we dare not try her in the rapids. We shall work down here some time yet. I long to lead back my faithful Mackalolo, who are still at Telle, though thirty of them died of the small pox, and six were killed by a neighboring chief.

I shall refer to one point more before concluding. We were warned by the fate of the Niger expedition not to delay among the mangrove swamps of the Delta—the very hot beds of the fever. We accordingly made all haste to get away, and we took daily a quantity of quinine. The period of the year I selected, though not the most favorable for navigation, was the most so for health; and, thank God, our precautions were successful.

The Kroomen from Sierra Leone have had more of it than we, until a short time ago, when, it being the most unhealthy season of the year, and even to the natives, three of us have had touches of the complaint, but are all now quite well. I have never had a day's illness since my return. We find, too, that so far from Europeans being unable to work in a hot climate, it is the want of work that kills them. The Portuguese all know that so long as they are moving about they enjoy good health, but let them settle down and smoke all day, and drink brandy, then—not a word about brandy in the fever that follows—the blame is all put on the climate. I am, &c. DAVID LIVINGSTONE.

KRAPF'S RESIDENCE AND TRAVELS IN EASTERN AFRICA.—Messrs. Trübner & Co. of London announce as nearly ready for publication a work which is likely to rival in interest the recent volume of Dr. Livingstone. We refer to the narrative of a Missionary Residence in Abyssinia by Dr. J. L. Krapf—one of the agents of the Church Missionary Society of London. It will be recalled by our readers that it was by him and his comrade Rebmann that intelligence was first given to the civilized world of the possible existence of snow-covered mountains near the equator—the famous Kilimandjaro. The observations of these missionaries have given rise to many warm discussions, and the correctness of their opinion has been earnestly disputed. In the forthcoming volume we may anticipate that this controverted point will be examined with thoroughness and detail. Aside from this discussion, the work of Dr. Krapf will abound in interesting comments upon his missionary life. His land journeys, which were mostly upon foot, extended 9000 miles. We quote the following from the prospectus of this work.

"Two things may be said of Dr. Krapf which can be affirmed of no other modern African traveler. He has traversed Abyssinia from north to south and from east to west; and further, he has explored the whole coast of Eastern Africa, from Suez to the 10th degree of south latitude, and inspected every place of importance to be found on it. Such journeys and voyages would alone bestow a high value on a volume like the present one, which communicates their most important results. But more than this, the large and interesting country which stretches from the Equator to the 5th degree of south latitude was, from the eastern coast inwards, all but a *terra incognita*, until it was traversed, on foot, by Dr. Krapf, and by his colleague and fellow-worker, the Missionary Rebmann, whose experiences are also included in this work. From the Mission-station at Rabbia Mpia, on the coast, these brave and fearless men prosecuted journeys for at least three hundred miles into the interior, exposed to every possible peril and privation. These journeys were repeated by different routes—the dangers incurred on one seeming only to stimulate to self-exposure to greater dangers on another. Rebmann's three journeys to Dschagga, Krapf's two journeys to Usambara, and two more to Ukambani, in the course of which they explored regions and visited—Bible in hand and Gospel on lip—populations never before seen by European, have rarely been exceeded in interest—religious, adventurous, and geographical. The story of Dr. Krapf's abandonment and wanderings in the wilderness, during his second journey to Ukambani, carries the reader back to the old days of adventurous travel. Scarcely in the whole annals of modern missionary effort has there been anything equal to the spectacle displayed in this section of the volume, of two individuals, each isolated, pursuing again and again, on foot, without external encouragement of any kind, and in the face of every possible obstacle, journeys among ignorant and savage heathen, far away from help, or the hope of help, and confiding solely in the guidance and support of Providence. The splendid geographical and ethnological results which were among the rewards of these daring pilgrimages will be found fully chronicled for the first time in the present volume."

Speke's Explorations in Eastern Africa.—At a recent meeting of the Royal Geographical Society of London, Capt. Burton and Capt. Speke both gave a narrative of their explorations in Eastern Africa, which are of particular importance, as our readers are well aware, in connection with the long disputed problem of the sources of the Nile. So much interest has been manifested everywhere in this expedition that we regret that our limits will not permit us to reprint entire the discussion to which these two papers gave rise in that learned association. Sir R. I. Murchison, Col. Sykes, Mr. Macqueen, Mr. Galton, and other well known gentlemen presented their views upon this important topic, a report of which will be found in the Society's Proceedings, vol. iii, No. 6. From the same source we extract the following statement of the remarks of Capt. Speke.

"The region traversed by Captain Burton and myself is divisible into five bands. They all run parallel to the coast, and each of them is characterised by special geographical features. The first is low land between the coast range and the sea. Its breadth is about 120 miles, and its average slope not more than 2 feet per mile. Forests of gigantic trees, and tall grasses, cover its surface. The second band is the coast range of mountains. These are hills in lines and in masses, intersected by valleys, through which the rivers of the east coast find their way. This range is easily crossed, and nowhere exceeded 6000 feet, adjacent to the line of road taken by our travelers. It is capable of cultivation, though neglected, because the slaving forays to which it is subjected drive away the inhabitants. The third band reaches to Unyanyembe. It is a dry plateau, with a slight inclination toward the interior, and ranging in height between 3000 and 4400 feet. Tributary streams, running southwards to the Ruaha, intersect it. The fourth zone is a continuation of the above, but it is better watered, and is studded with granite hills. Here is the water-parting between the streams that run eastward to the Indian Ocean, and westward to the Tanganyika Lake. The Nyanza Lake is situated in this band. The fifth band is a remarkable slope, that inclines to the shores of the Tanganyika. It sinks no less than 1800 feet in 45 miles; it is exceedingly fertile, but harrassed by marauders of the Watuta tribe.

On arriving at Ujija, the party found that the only boats to be had were wretched canoes; while the troubled state of the country rendered it unsafe to explore the lake unaccompanied by a large escort. There was, however, a small sailing craft belonging to an Arab, on the other side of the lake, which would be large enough to contain the entire party; and Captain Speke started to hire her, with seventeen savages, as a crew, and four of his own men. He first coasted to Kabogo, a bold promontory usually selected as the starting point, when the lake has to be crossed, and reached it in five days. He describes the shore as wild and beautiful, affording many convenient harbors, and requiring but a little art to make it quite a fairy abode. There were no inhabitants, but an abundance of game,—hippopotami, buffaloes, elephants, antelopes, and crocodiles. The passage across the lake, a distance of 26 miles, was made rapidly and safely, and Captain Speke was cordially welcomed by the Sultan of the country on the opposite side. The owner of the sailing boat was there also, and was ready to afford every assistance; but he himself was on the point of starting on an ivory expedition 100 miles into the interior, and the crew of his sailing boat were, at the same time, his armed escort: he could not therefore spare them. What made the disappointment doubly vexatious, was that this Arab desired Captain Speke's companionship in his intended journey, and he promised the boat on his return. Had Captain Speke been unfettered by time, this would have been an excellent opportunity of farther travel. As it was, he was obliged to go back to Ujiji without the sailing boat, and proceeded with Captain Burton to a more extended exploration of the Tanganyika Lake, which lasted a whole month. The mapping of its southern portion depends on information given by this Arab.

On returning to Unyanyembe, Captain Burton's continued illness again made it necessary for Captain Speke to proceed alone to the northward to explore the Lake Nyanza. He went with thirty-three men, through a line of populous country, less visited by strangers than that which he had hitherto traveled on. There were numerous petty sovereigns who were hospitable enough but very troublesome. The view of Lake Nyanza, with its numerous islands, reminded Captain Speke of the Greek archipelago. The islands were precisely like the tops of the same hills that studded the plains he had just traveled over. In fact, the lake had the features of a flooded country rather than those of a sheet of permanent water, with well marked banks. Its water is sweet and good: those who live near it drink no other.

Captain Speke's explorations did not extend beyond its southern shores. The more northern part of his map is based on native information, especially on that of a very intelligent Arab, whom he has previously met with in Unyanyembe, and whose data, so far as the shores of the lake, were found by Captain Speke to be remarkably correct. This Arab had traveled far along its western shores. In thirty-five long marches he reached the Kitangura river, and in twenty more marches, Kibuga, the capital of a native despot. Between these two places he crossed about 180 rivers, of which the Kitangura and the Katanga were the largest. The former is crossed in large canoes; the latter, though much larger and broader, is crossed during the dry season by walking over lily leaves; but in the wet season it spreads out to an enormous size, and is quite unmanageable. The rainy season is very severe in these parts. No merchants have gone farther than Kibuga; but, at that place, they hear reports of a large and distant river, the Kivira, upon the banks of which the Bari people live. This river is believed by Captain Speke to be the White Nile."

Sir R. I. Murchison in reviewing the labors of the two explorers, remarked that "they have, by means of astronomical observations, fixed the position, the longitude and latitude of these two great lakes, and have shown you that whilst one is like other lakes, of which we had previously heard, situated on a great plateau, the other is situated at such an elevation that, as Captain Speke has explained to you, it may very possibly be found to feed the chief sources of the Nile. I will not now argue that difficult question, because I am quite sure there is one gentleman here, if not others, who may dispute that inference. I will, therefore, first call attention generally to the great importance of these discoveries. My friends here have not only traversed the district and furnished us with a good picture of the manners and customs of the inhabitants, but have also brought home rock specimens which enlighten us as to the fundamental features of this country; and to these rocks I will for a moment advert. Captain Burton placed before me this morning certain specimens which show me that at an elevation of upwards of 3000 feet above the sea and towards the interior there are fossilized land shells, showing that from very ancient periods the lands have maintained their present configuration. These deposits, whether purely terrestrial or lacustrine, have been consolidated into stone, and show that the existing internal condition of Africa is that of ages long gone by, as I took the liberty of pointing out to the society some years ago, when treating of Livingstone's first explorations. Another striking feature in connection with this great zone of country is this. You will observe that our friends spoke of remarkable herds of oxen on the banks of the lake Tanganyika, and tribes of people between that vast lake and the coast range, who are a thriving, peaceful, agricultural population, whilst the adjacent districts in the north and south are frequently disturbed by wars for slave-hunting purposes. This is a great fact as indicating a broad line of route by which we may hope hereafter to establish intercourse with the interior country. There is another important fact, though I do not think Captain Speke alluded to it, namely, the absence of that great scourge of parts of southern Africa, the *Tsetse* fly.

With regard to the physical geography of the country, it is remarkable that all the adjacent rivers fall into the great Tanganyika lake, which was formerly supposed, on the contrary, to afford the sources of the Zambesi river. All theory, therefore, on this subject is now set at rest. Lastly, we come to the subject which is likely, as I said, to give rise to much discussion, and that is the theory upon which I think my friend Captain Speke may rest his claim to our most decided approbation. On my own part I am disposed to think that he has indicated the true southernmost source of the Nile. Now, in saying this I do not mean to deny that the great mountains flanking the lake on the east, of which a point or two only is marked on the map before us, do not afford the streams which flow into this great lake. That must probably be the case on the east, just as Captain Speke ascertained from the Arabs that the so-called "Mountains of the Moon" feed the same lake from the west by other streams. You must here recollect that the same Arab sheik who gave him the information which turned out to be correct concerning the existence of the lake Tanganyika also told him of the existence of the Nyanza, which lake was found to be exactly in the position indicated. As Captain Speke has determined that this great lake Nyanza is nearly 4000 feet above the sea, it may well, indeed, be the main source of the White Nile. Everything (as far as theory goes) being in its favor, this view is farther supported when we reflect on the fact that the tropical rains cause these upland lakes and rivers to swell and burst their banks at a period which tallies very well with the rise of the Nile at Cairo. These, then, are grounds which I think must go to strengthen the belief of Captain Speke, and I may, therefore, repeat what I stated at the anniversary, that highly worthy as Captain Burton was to receive a gold medal, not only on account of this great expedition which he led, but also for his former gallant and distinguished expeditions, Captain Speke, who now sits at your Lordship's left hand, is also entitled to a gold medal of the Royal Geographical Society."

H. Schlagintweit on the Salt Lakes of the Himalayas.—At a recent meeting of the Royal Geographical Society of London, Mr. H. Schlagintweit exhibited some chromo-lithographic sketches of the Himalayan Mts., and in commenting upon the remarkable erosion which takes place upon that range, he spoke as follows of the salt-lakes which form a peculiarity of that region:

"Another consequence of the erosion is the gradual drainage of fresh water lakes, or their conversion into salt water lakes. It is very characteristic for the Himalayas, and in this respect they differ essentially from most other mountain systems in the world, that hardly any fresh-water lakes now occur. The only few lakes of any considerable extent which have been made known by Captain Strachey, Captain Speke, and Major Cunningham, as well as those we visited besides, are all salt water. But the explanation we think we must give of this phenomenon is different from the explanation formerly given. Some have thought that a raising of the country might have caused a general drainage. We think that supposition rather improbable, from the recent strata round these salt lakes being all horizontal, and the outlets of these salt lakes being in a different direction in reference to the horizon. If any raising of the country had effected the drainage of the salt lakes, the effect would have been a perfectly different one, according to the position the outlet of these lakes had in reference to the points of the horizon, a modification which is nowhere met with.

"The Tso mo Ri ri and the Tso mo Gnalari, the two great salt lakes of Rupchu and Pankong, of which drawings are presented, happen to be a good example of two large lakes, being about equally salt, with differently directed former outlets, and with quite horizontal banks of detritus and of watermarks along their circumferences. The gradual progress of the erosion of the valleys seems to us to be also the chief cause of the gradual transformation of freshwater lakes into saltwater lakes in Tibet.

"By this progressive excavation thousands of square miles, still marked as former lakes by the form of the surface, have been emptied, and the consequence is that the local evaporation could no more keep the equilibrium with the precipitation; in consequence the lakes, of which parts remained undrained on account of their greater depth, now gradually became more and more salt."

Journal of the Royal Geographical Society of London.—We have just received the twenty-eighth volume of the Journal of the Royal Geographical Society of London. Like the previous parts of this series it is full of important contributions to our knowledge of the physical geography of every country where British enterprize is manifested. We have heretofore quoted from the anniversary address of the President, Sir R. I. Murchison. To many of the other articles we shall have occasion to allude. The following is a statement of the contents of the volume:

Articles—1. Journal of the North Australian Exploring Expedition; under the command of Augustus C. Gregory, Esq. (Gold Medallist, R.G.S.); with Report by Mr. Elsey on the health of the party.—2. Notes on the Physical Geography of Northwest Australia; by Mr. James S. Wilson, Geologist to the North Australian Expedition.—3. Journey from Colesberg to Steinkopf in 1854–5; by Robert Moffat, Esq., F.R.G.S., Government Surveyor at the Cape.—4. Journey from Little Namaqualand eastward, along the Orange River, the Northern Frontier of the Colony, &c.,

&c., in August 1856; by Robert Moffat, Esq., F.R.G.S., Government Surveyor at the Cape.—5. A Coasting Voyage from Mombasa to the Pangani River: Visit to Sultan Kimwere: and Progress of the Expedition into the Interior; by Captains Richard F. Burton, commanding the East African Expedition, and J. H. Speke, F.R.G.S.—6. Explorations in the Desert East of the Haurán, and in the Ancient Land of Bashan; by Cyril C. Graham, Esq., F.R.G.S., &c.—7. Contributions to the Knowledge of New Guinea; by Dr. Salomon Müller,—8. On the supposed discovery, by Dr. E. K. Kane, U.S.N., of the North Coast of Greenland, and of an Open Polar Sea, &c., as described in the 'Arctic Explorations in the years 1853, 1354, 1855;' by Dr. Henry Rink, M.D., Inspector in Greenland for the Danish Government.—9. The Yang-tse-Keang, and the Hwang-Ho or Yellow River; by William Lockhart, Esq., F.G.R.S.—10. Extracts from a Journal kept during a Reconnaissance Survey of the Southern Districts of the Provinces of Otago, New Zealand; by J. Turnbull Thomson, F.R.G.S., Chief Surveyor.—11. Observations relative to the Geographical Position of the West Coast of South America; by Carlos Moesta, Director of the National Observatory, Santiago de Chile, May 29, 1856.—12. Excursion made from Quito to the River Napo, January to May, 1857; by Dr. William Jameson.—13. Description of the State of San Salvador, Central America; communicated by John Power, Esq., F.R.G.S., of Panama.—14. On the Latitude and Longitude of some of the principal places in the Republic of Guatemala; by A. van de Gehuchte.—15. On the Fine Regions of the Trade Winds; by Thomas Hopkins, M.B.M.S., Vice-President of the Manchester Literary and Philosophical Society.—16. Remarks upon the Amount of Light experienced in high Northern Latitudes during the absence of the Sun; by Captain Sherard Osborn, R.N., C.B., F.R.G.S., Officier Légion d'Honneur, &c.—17. Notes on the River Amúr and the adjacent Districts; by MM. Peschurof, Permikin, Shenurin, Vasilief, Radde, Usoltzof, Pargachefshi, &c.

Illustrations.—1 and 2. Map to illustrate the Route of the North Australian Expedition, and Mr. Wilson's Paper on the Physical Geography of N.W. Australia.—3 and 4. Map to illustrate Mr. Moffat's Journey from Colesberg to Steinkopf; and from Little Namaqualand Eastward, along the Orange River.—5. Map to illustrate the Progress of the East Africa Expedition.—6. Map to illustrate Mr. Cyril Graham's Explorations East of the Haurán, &c.—7. Map to illustrate Dr. Rink's Paper on Dr. Kane's Arctic Explorations.—8. Map to illustrate Mr. Thomson's Survey of Otago.—9. Map to illustrate Capt. Sherard Osborn's Paper on Light in the Arctic Regions.—10. Map to illustrate Notes on the River Amúr.

Yale College Library, March, 1860.

ART. XXIII.—*On the Species of Calceola found in Tennessee: Calceola Americana;* by Prof. J. M. SAFFORD.

FOR many years it has been known that a species of *Calceola* occurs in the marly and glade-forming limestones of Western Tennessee. This species has been considered to be identical with the European *C. sandalina* of Lamarck, an error (for such I hold it to be) which has contributed much to the confusion that has existed with reference to the age of the limestones mentioned. Individuals of the species are frequently found upon marly glades of Decatur, Perry, Wayne, and Hardin counties. The identity of the species with *C. sandalina* (and a few other determinations of the same kind) once taken for granted, it was an easy matter to designate the rocks of these glades "Devonian."

Since my attention has been called particularly to this species, I have regarded it as distinct, and now propose for it the name *Calceola Americana.*

In the first place, its different geological position would, at least, indicate a distinct species. It is without doubt an Upper Silurian fossil, and moreover belongs exclusively, so far as my observations have extended, to the Niagara Period.* The grey marly limestones of *the glades*, although much alike lithologically, are generally easily separable, by their fossils, into two beds, the lower one representing, in part, the Niagara Period, and the other the Lower Helderberg. It is to the former of these that our *Calceola* belongs. Among its associates are *Orthis elegantula, Platyostoma Niagarensis, Caryocrinus ornatus, Eucalyptocrinus decorus,* &c. *Halysites escharoides* and *Cladopora reticulata* have been observed in a local coralline limestone resting upon the bed containing the *Calceola.*

In the second place, the characters which separate it from *C. sandalina* are well marked. In general form, it is much like the European species, but differs in the following particulars:†

1. In *C. sandalina* the central cardinal process or tooth of the large valve is divided longitudinally by a shallow linear groove, making the tooth apparently double; in all my large valves of *C. Americana* this tooth is not grooved, but, on the other hand, is rounded and smooth along its summit; it is moreover longer and larger than in the European species.

* The *Niagara Period*, as here used, is equivalent to the New York rocks from the Oneida Conglomerate to the Niagara Group inclusive.

† In making these comparisons I have before me forty specimens of the American species and seven of the European. Of the first, one is an excellent specimen with both valves united, two are good specimens of the small valve, and the remainder are large valves. The small valves are seldom found. I have seen altogether four of them. Of my European specimens, one is entire with both valves, another is an excellent small valve, and the rest are large valves all in good condition.

2. In my specimens of *C. Americana* the rows of punctures, so conspicuous on the internal surfaces of *C. sandalina*, are not seen.

3. Within the largest valve of our species, in the older individuals, there is adjoining the hinge line, and on each side of the cardinal process, (but separated from the latter by a deep groove,) a prominent callosity. In very old specimens, these callosities nearly fill up the back portion of the cavity of the shell, and, at the same time, nearly obliterate the striæ or ridges which run forward from the hinge line. Most of the inner surface of the large valve has an irregular wavy appearance, indicating a vesicular structure, which, in fact, the mass of the valve has.

4. The small valve (the dorsal), so far as I have seen, has externally no proper cardinal area; its *apex* is not immediately over the hinge line, but is removed about one-fourth of the length of the valve towards the front, the cardinal edge being bevelled off from the apex to the hinge line. The lines of growth are prominent along this bevelled edge; so they are too on the cardinal area of the large valve.

5. The external surface of *C. Americana* is obscurely marked longitudinally in front by striæ, which, so far as they have been seen, are coarser and less numerous than in *C. sandalina*.

There are other points of difference which appear to be constant, but those given are sufficient to characterize the species. Upon a future occasion the fossil will be illustrated by the proper figures.

Lebanon, Tenn., Feb. 1, 1860.

Art. XXIV.—*The Great Auroral Exhibition of August 28th to September 4th*, 1859.—3d Article.

In the two preceding numbers of this Journal* we have given observations of the Aurora of Aug. 28th to Sept. 4th, from numerous places in North America. We now continue our record of the phenomena, and intend in a subsequent number to present a summary of the observations made in other parts of the world. We are indebted to Mr. Benj. V. Marsh, of Philadelphia, for a considerable number of the following notices.

1. *Observations at Montreal* (lat. 45° 31′), *by* Dr. Archibald Hall.

August 28th about 8^h 20^m P. M. the sky was about seven tenths obscured by massive cumuli, when in the interval between them I observed streamers of a ruddy tint passing from the south towards the zenith. The wind was N.N.W. and blowing

* Vols. xxviii, p. 385; xxix, p. 92.

rather stiffly. About 10 P. M. the streamers seemed to converge towards the zenith in all directions, and to possess a deep ruddy tint. There was a large cumulus cloud in the W.S.W. and from a clear space beneath it a streamer shot upwards and distinctly traversed the cloud, illuminating it vividly. The same phenomenon was witnessed by another observer at the other end of the city.

At $2^h\ 10^m$ A. M. Sept. 2d, a brilliant aurora was seen in the vacant space between masses of huge cumuli and lasted until $3^h\ 30^m$ A. M. The sky was at first of a bright coppery red tint, and the light emitted so great that it was possible to read moderately large print by it. This space became interspersed with streamers of a rich roseate hue stretching to the zenith. The manifestation was chiefly observed in the W.S.W.

Sept. 2, at $9^h\ 40^m$ P. M. we had another auroral display. The streamers were mostly white, springing from three well-defined arches, stretching between the N.E. and N.W. They flickered magnificently about $10^h\ 20^m$ P. M. in the zenith, where they formed a huge corona having a tent-like appearance. These displays have been the finest seen here for many years, and it is to be regretted that on the two first occasions, clouds should so far have concealed them from our view.

2. *Observations at Montreal* (lat. 45° 31′), *by* Prof. CHARLES SMALLWOOD, LL.D.

Aug. 28th at 9 P. M. we had a splendid aurora extending over nearly the whole horizon with the exception of a small space in the south and S.W., varying in color from a pale yellow to deep orange and violet or crimson, and nearly as light as when the moon is at its full. The aurora was first noticed between $8^h\ 30^m$ and 9^h P. M., and this appearance lasted, with modifications, till nearly sunrise.

On the following night, Aug. 29th, there was also a fine display, but not to be compared in brilliancy to that of the previous evening. The sky was on this occasion cloudless, and a few streamers were occasionally seen tinted with a pale violet color.

The most remarkable incident was the unusual amount of atmospheric electricity present. At 9 P. M., Aug. 28th, the electrometers indicated a maximum of 250 degrees in terms of Volta's electrometer No. 1, of a positive character (but almost constantly varying in intensity); an amount equalled only during the thunder storms of summer, and the heavy snow storms of winter. The amount during the following day and night indicated a maximum of ten degrees, which is however somewhat above the usual average.

The appearances would lead to the opinion that the clouds might have been the medium of conducting the atmospheric electricity to the earth, for the indications of the electrometers were such as are observed during the passage of clouds charged with electricity, and this phenomenon seems to have extended to the wires of the electro-magnetic telegraph.

The following day and night indicated a small increase on the usual amount of electricity, which may be owing to the continued presence of the aurora, or in some measure to the decrease in temperature.

Similar indications of the electrical state of the atmosphere during the aurora were never observed here, although its effect on the magnetic telegraph has been before witnessed.

3. *Observations at St. Paschal* (lat. 47° 40′ N., long. 67° 40′ W.), *communicated by* Prof. C. SMALLWOOD.

It was about 10 P. M. Aug. 28th, that the aurora was first noticed here. It was a magnificent display which threw out streamers from the zenith all around the horizon, and the light was nearly that of the day. I believe it was visible at Lake St. John on the Saguenay, lat. 48° 8′, long. 71° 9′.

4. *Observations at Halifax* (lat. 44° 39′), *by* Lieut. N. HOME, *of the Royal Engineers.*

Aug. 28th at 5 P. M., I remarked a long narrow belt of cloud from E. to W. having a peculiar orange-white appearance.

At 8 P. M. I observed this cloud (which in the interim appeared to be stationary) suddenly to become luminous, particularly at its eastern extremity. This cloud was about 10° wide, and appeared to extend from horizon to horizon; no other clouds were visible.

Soon after 8 P. M. two arcs of light N. and S. appeared, that to the south being the brightest. Under both these arcs the heavens were dark; but observers were uncertain as to whether the darkness was cloud or not. No stars were seen below the arcs, although quite visible above them.

At 9¼ P. M. the appearance was as if these two arcs were a small circle of the sphere, dipping to the south at an angle (measured by sextant) of 15° to the horizon, and 12° above it. The corona being formed at a point (by sextant) 10° south of zenith. There was only one band or arc of light, and that was continuous around the whole heavens. There were two remarkable patches, one due west, at an elevation of about 36°, having a red color; and the other east by north, at an elevation of 25°, having an orange color. These points were brightest during the whole display.

Two distinct sets of streamers appeared to be formed; one set from the arc of light, the other from the corona, which seemed to be constant or nearly so; as during the five hours I watched the aurora, there appeared to be always light in or near the zenith, and always in the arc. The streamers were the variables, and appeared to work from W. by N. to south. I think they worked along from E. to W., but another observer said from W. to E. To the south they were so vivid and rapid it was not easy to tell.

A volume of light, as if a quantity of burning spirit was poured over the heavens, appeared to stream across from north to south quite independent of the streamers. The corona sent down rays, but it seemed to be only half way; the streamers from the arc meeting them and toothing in, appearing to alternate, short and long ones.

5. *Observations at Grafton, Canada West* (lat. 44° 3′ N., long. 78° 5′), *by* JAMES HUBBERT.

Aug. 28th at 8^h 30^m P. M. my attention was attracted by the peculiar appearance of the southwestern sky. Streamers and flashes of light of a pale yellow and red color were rising, sailing towards a point 8° south of the zenith, and meeting others from the N.W. and north. By 8^h 53^m the whole northern and eastern sky was a blaze of lurid light, which seemed most dense in a band seven degrees wide, extending from N.W. to S.E., along which there was a constant succession of streamers and nebulous patches, exhibiting every shade of white, yellow and red. Columns were now darting up from all parts of the horizon. The aurora hung along the south, in a line at a maximum height of 17°. This from 8^h 50^m to 9^h was very perfect; while a similar arch but much less regular was formed in the north, reaching to the east. The latter had an altitude of 27°, and like the other seemed to rest on a dark bank. The first corona that I observed was formed at 9^h, at an altitude of 70°. It was imperfect and vanished almost instantly; but was soon replaced by another in nearly the same spot. This in turn gave place to another still more complete. From 9^h 15^m to 10^h 15^m the drapery was gorgeous in the highest degree. A diffused light made surrounding objects very distinct. Cocks crew, and the animal world seemed to think that day was dawning.

I noted constant changes which were little more than a repetition of the above till 3^h in the morning. The corona was distinct from 12^h 37^m to 1^h 5^m. The color was white, merging into every shade of yellow, crimson, scarlet, purple, and sometimes tinged with green. I listened with great earnestness, and once or twice thought I heard a rustling noise, but I think it must have been the wind. When the wind was hushed, as it was at intervals in the latter part of the night, not a sound could be heard. Just at 10^h the aurora, after nearly disappearing, became intensely brilliant, equalling the light of the moon at the last quarter. The aurora continued till daylight, when it gradually faded away.

The evening of Aug. 29th was clear; and at $8^h 45^m$ the aurora was again visible, but very much less extended and brilliant than on the preceding night. There were thin, misty clouds of a nebulous appearance, with occasional streamers of a pale white light, sometimes merging into red.

On the night of Aug. 30th I observed no unusual appearance.

Aug. 31st the sky was covered with a dense mass of clouds; but the existence of the aurora was evident from the clearness of the night. After midnight the clouds disappeared, and the display was magnificent. All the characteristics of the night of the 28th were repeated; but the arch was rather lower along the southern horizon. A fiery bank was formed in the south, from which rays were constantly darting upward, and the whole sky was a gorgeous canopy of crimson and gold. This was most vivid from $1^h 15^m$ to $1^h 45^m$, but was continued till almost daylight.

Sept. 1st was cloudy, and I saw no indications of the aurora.

Sept. 2d there were dense clouds, yet the aurora might be occasionally seen. It was confined to the N. and N.E., and was particularly bright from $9^h 51^m$ to 11^h.

Sept. 3d was clear. At $8^h 50^m$ the aurora appeared in the N.N.E. and W. The light was yellow and white, with traces of crimson and green. At 10^h an imperfect corona was formed, but almost instantly disappeared. Others followed, but none of them were complete.

Sept. 4. The same phenomena were observed, but much diminished in brilliancy.

Sept. 5. No trace of the aurora was visible.

6. *Observations at Rochester, N.Y.* (lat. 43° 8′), *by* Prof. C. Dewey.

The aurora of Aug. 28th was exceedingly splendid both before and after midnight, with the corona a little south of the zenith; and exhibited many colors, with red or crimson predominant.

Sept. 1st. The aurora began late in the evening, and exhibited the usual appearances.

Sept. 2d at 1 A. M. it was cloudy, but very bright and red in the N.E.; the light increased rapidly and extended. At 2 A. M. there was a magnificent glow of red over the southeast, south and southwest; yellowish green, green and crimson, forming a gorgeous display quite down to the south horizon as seen from the housetops. A splendid corona was formed just south and east of the zenith, with splendid coruscations from towards the horizon up to the zenith. The streamers shot upward towards where the corona was formed, but none went to it.

Sept. 3d. The aurora was considerable at 9 P. M., and over the north, streamers were shooting upwards. At 10 P. M. there was a bright red space in the N.W. or W.N.W. with white and greenish-white bands on each side. The flashing of light upwards soon began, and the streams or clouds of aurora were splendid. At 10½ P. M. the corona began a little S.E. of the zenith, and was very splendid, towards which the streaming upwards was on all sides but less from the south. At 11 P. M. it nearly disappeared. This aurora was equal to that of November, 1837.

7. *Observations at Newburyport, Mass.* (lat. 42° 48′), *by* Dr. HENRY C. PERKINS.

The aurora of Aug. 28th was the most splendid ever witnessed at Newburyport by the present generation. About 7½ P. M. the eastern sky seemed to outvie the western, but with reversed colors, the pink of the morning taking the place of the golden hues of the setting sun. In a few moments these hues were repeated in the northeast and the west, and the yellowish-white luminous arch had passed the zenith and was fast covering the southern sky, and at 7¾ P. M. had enveloped Antares. At 7^h 52^m the star Tau Scorpii was at the southwestern edge of the luminous fringe. At 9 P. M. Lambda Scorpii marked its southern border. At 9¾ P. M. the northern border of a luminous arch passing from the east to the west, was marked by Nu in the right foot of the Swan, while the whole southern and southwestern heavens were glowing with streamers rushing to the pole of the Dipping-needle, the whole northern heavens being entirely destitute of the auroral light. At this juncture, in an instant as it were, the merry dancers sprang up from the northern heavens, and at 10 P. M. the whole celestial vault was glowing with streamers, crimson, yellow, and white, gathered into waving brilliant folds, a little to the south and east of the zenith, affording a canopy of the richest tints and most magnificent texture. The light was examined by the polariscope, and found not polarized. The stars were so lost amid the effulgence as to render it somewhat difficult to make out the constellations. Print might be read by the aid of a small lens, and the time ascertained from the watch by the simple light of the aurora.

During the evening of Sept. 1st the aurora was quite bright, and about a quarter to one (Sept. 2) it spread very rapidly, and soon enveloped the whole heavens. At about one the spectacle was magnificent, a perfect dome of alternate red and green streamers being formed, and the light being so great that ordinary print could be read as easily as in the day-time. It continued till morning.

8. *Observations at Lunenburg, Mass.* (lat. 42° 35′), *by* Prof. WILLIAM B. ROGERS.

The aurora of Aug. 28th has rarely been equalled in this latitude, and the meteor was repeated with more or less splendor for the eight following nights. The displays of Sept. 1st and 2d were scarcely inferior in beauty to that of the 28th, while that of Sept. 2d, in some of its features, was the most interesting of them all.

On the evening of Aug. 28th, throughout most of the northern half of the sky, the stars were dimmed by what seemed to be a luminous haze, which in some places quite eclipsed their light, and which itself glowed changefully with a golden and crimson coloring. In the earlier stage, the obscure space on the northern horizon had not assumed the usual arched form, and was sufficiently translucent to show a few flaky clouds, floating within its confines. At 8^h 20^m this dark space had become more opaque, and had moulded itself into a symmetrical arch, bounded by a broad luminous band.

At 9^h 30^m the display attained its highest magnificence. The dome of the heavens was hung around with white and golden and rose-tinted streamers converging from all quarters towards the magnetic pole. Over the glowing stripes of this marvellous pavilion there came broad flushes of the richest crimson light, until it suffused all the upper part of the sky, and the whole southern quarter except a narrow space next the horizon.

At 10^h 30^m nothing remained of this wonderful spectacle but a faint auroral arch low down in the north, accompanied by a few dim streamers.

The aurora recurred in great splendor between 1 and 2 A. M., Aug. 29th, when the crimson color was particularly remarkable. At 3^h 30^m A. M. there was a fine auroral arch in the north, with a long array of streamers rising from it.

Sept. 2d, a clear sunset was followed by a peculiar greenish and purplish light extending round the horizon, even beyond the north. Over the northeast quarter, the air to the height of 30° had a dark opacity, which had the effect of arresting the light coming from beyond.

At 7^h 30^m P. M. an irregular obscure space began to form along the northern horizon. At 7^h 50^m a faint arch of white light made its appearance, resting on the horizon a little north of the E. and W. points, and culminating some distance below the pole star. This continued to rise until 8 P. M., when its apex was within a few degrees of the pole.

At 9^h 20^m a low luminous segment showed itself on the horizon beneath the arch. The latter now resolved itself into an array of bright streamers, with equidistant shadowy spaces between them.

At 9^h 30^m the streamers had extended and grown brighter, while the low luminous segment, diffusing itself upward, had merged into the outer arch, which now reached nearly to the pole star. At this moment the arch began to send off successive waves of light, rapidly following one another towards and beyond the zenith. In a few seconds this wave movement gave place to more rapid and seemingly broken pulsations, flitting upwards in close succession through the northern, eastern and western quarters of the sky, and visible, though less distinctly, in the south. This wonderful appearance exhibited everywhere a convergency of the lines of motion towards a point considerably south of the zenith.

When these luminous phenomena were at their height, every spot to which the eye was directed, except the southern quarter near the horizon, was traversed by quickly successive flashes of white, greenish, and pale roseate light, all seemingly moving upwards.

At 10^h 30^m the pulsating movement again extended over all the northern and part of the southern half of the sky. Innumerable waves of white, yellowish and purplish light chased each other from every quarter towards the magnetic pole, while the crimson flush spread wider and higher from the west.

The various phases of this aurora recurred according to a somewhat uniform order of succession. First, the dark segment on the northern horizon took a regular arched form, and as it rose, became bounded above by a broad luminous curve, at the same time developing one or more bright concentric arches within. The streamers now shot forth from all parts of the luminous zone; and as these increased the upper arch faded away, as if it had expended itself in producing them. And now the lower arch took its place, to be obliterated in its turn by a like seeming process of exhaustion. At length, one of the grander effusions of light coming on, the whole arch was broken up, and the dark segment below was reduced to a shapeless mass. Then there occurred a comparative pause in the phenomena, until the dark segment again took form, with its one or more luminous bands, and a like cycle of development was repeated.

9. *Observations at Steubenville, Ohio,* (lat. 40° 25′), *from the Steubenville Daily Journal.*

The magnificent auroral display of Aug. 28th was unusually interesting. 1st. It covered a much larger space of the heavens than any we ever saw before, at least since 1835. 2d. It lasted from dark until daylight, appearing with the first approach of darkness and only disappearing as daylight gradually overpowered it. 3d. Instead of an arch, shooting up rapid and various colored rays, its first appearance was that of a luminous mist, with barely perceptible rays along its southern border, and moving with the rolling motion of clouds, rather than the straight darting motion usually seen in auroras. 4th. It varied in intensity more than any we have ever seen before, twice fading nearly out, and remaining so for nearly half an hour or more, and then kindling up with greater brilliancy than before.

About 7½ P. M. it was a barely perceptible light in the northeast. As the darkness deepened, this luminous spot grew brighter, and moved to the south, till a little before 8 P. M. when the light spreading from it met that coming from the west, and formed an arch about half way between the zenith and southern horizon, and there its advance ended, and it began instantly fading out. It retreated just as it had advanced, only more rapidly, and at $8^h\ 10^m$ there were left only the two centres in the northeast and northwest with a fitful gleam between them. During this retreat, portions of the luminous cloud broke off and floated for some minutes far away from the main body, surrounded by deep darkness, like islands. One of them, and the most beautiful, was a long bright bar in the south, which extended more than half way across the sky from west to east, with a wide sea of darkness between it and the parent cloud, which gradually melted away and disappeared to the westward.

At 9 P. M. the light advanced again, this time with a blood-red tinge in the eastern and western portions, and passed clear to the south as before, but shooting up many and variously colored rays, sometimes from the east, sometimes from the west, sometimes from the north, and from all parts of an irregular luminous arch that bent over the northern horizon about twenty degrees above it. This display faded away in an hour, and at 10½ P. M. there was no light that would attract attention, more than is frequently seen in the north.

About 3 A. M. it blazed up with redoubled brilliancy, shooting up white rays far above the zenith, and making the earth as light as a full moon behind a mist could have done. This time the rays seemed to dart up in broad masses, giving the sky the appearance of being covered with slabs of light, which were tinged with red in the zenith, and rested on a broken irregular arch in the north that in some places fell to the horizon, and in others rose in angular openings to thirty degrees above. During this last display, the pulsations of the aurora were beautifully marked, the rays shooting up in a sort of volley, many hundreds together; while broken and separate masses of luminous cloud were seen in various parts of the sky.

10. *Observations at Burlington, N. J.,* (lat. 40° 5′), *by* BENJAMIN V. MARSH.

Aug. 28th an arch of light rose in the north, passed the zenith and descended to within about 20° of the south horizon by 8h 30m P. M. Soon after this, the whole space overhead was occupied by a dense unbroken cloud of milky whiteness. There was however up to this time a considerable number of small black clouds moving southward, which soon afterward disappeared entirely. These clouds were very thin, and we were for a while in doubt whether they were not patches of clear sky; but by watching their effect upon the stars, we satisfied ourselves that they were clouds.

Still later, about 20° above the south horizon, there was a dense whitish arch a few degrees in width, its lower margin being regular and well-defined. About 30° or 35° above the north horizon was the top of another arch, wider than the first, but not so regular or well-defined. Between these two arches were numerous streams and fragments of white auroral cloud.

Between 9½ and 9¾ P. M. there was a perfect corona; the streamers on the south side were short, and mostly white, and moved pretty rapidly westward. Their number at one time was probably five or six. At one time the central space was perfectly clear; but afterwards the streamers ran through it to its centre.

11. *Observations at Crawfordsville, Indiana,* (lat. 40° 3′), *by* Prof. JOHN L. CAMPBELL.

Aug. 28th, the aurora began about 7h 30m P. M. with an unusual white light in the form of an arch in the north. At 8h 45m P. M. the white light appeared in two brilliant spots about 60° on each side of the magnetic pole.

At 9 P. M. streamers of white, red and pink light in circular currents about the magnetic pole (variation 5° 45′ east) extending beyond the zenith.

At 9¼ P. M. the streamers were concentrated into brilliant ones passing nearly along the magnetic prime vertical.

At 10 P. M. streams of white light were formed in the east, and rapidly passed westward, a little south of the zenith. These streams or clouds were entirely separate from each other, and the more northern band, and possessed a real motion. The time occupied in passing was about one second. Not less than twenty flashes passed over. They were formed about 30° above the eastern horizon, and disappeared about 60° above the western. After 10 o'clock the white light in the north became more brilliant, and tinged with red, extended very far towards the south. We traced the red tinge on the east to within 40° of the south point, and on the west to within 50° of the same. At 11 P. M. the aurora was still bright in the north.

Aug. 29th, 2 A. M. Very brilliant streams of white and red light filled the northern hemisphere. These streams were perpendicular to the horizon in the north, and were inclined at regular decreasing angles towards the east and west to about 70°. Brilliant flashes passed across the heavens, originating in the northeast, and passing in a southerly direction vanished in the southeast.

At 2^h 45^m A. M. Heavy bank of red light ten degrees north of east. Patches of white light in north with occasional streams.

At 3 A. M. the whole northern hemisphere was filled with streams of white light with the same inclination as at 2 A. M. At 3^h 15^m A. M. the auroral storm was at its height. Flashes of red and white light each instant flew across the northern hemisphere. At 3^h 30^m A. M., a bright band of white light covered the hemisphere except low down in the north; and the incessant flashes in the northeast and east still continued. Long streams of light flashed across the entire hemisphere. The lower part of the band passed through the heavens at an elevation of 40°. In the zenith was displayed a brilliant red bank. In the east, red and white flashes were very brilliant—better defined but not so rapid in transition as at 3^h 15^m A. M.

At 3^h 45^m A. M. Magnificent corona in the zenith. Central portion spiral, red and white, changing instantly to a beautiful rose color, with spiral streams shooting forth into all parts of the heavens; the most brilliant streams flowing east and west. The heavens were completely covered with these streams of light.

At 4 A. M. the white light in the north was still very bright, but the dawn obscured the eastern bank.

12. *Observations at Philadelphia,* (lat. 39° 57′), *by* CHARLES J. ALLEN.

Soon after half past 8, Aug. 28th, the southern margin of the luminous auroral curtain was well defined, and its position between two fixed objects carefully noted. It was afterwards ascertained by actual measurement that this gave an elevation of about 22½ degrees above the southern horizon.

13. *Observations at Sandy Spring, Md.,* (lat. 39° 9′), *by* Prof. BENJAMIN HALLOWELL.

The elevation of the southern margin of the luminous auroral curtain above the southern horizon, Aug. 28th, about 9 P. M., I thought, and mentioned to those with me, was about the meridian altitude of the equator, say 51 degrees.

14. *Observations at Stockton, California,* (lat. 38° 10′), *from the San Joaquin Republican.*

The aurora of Aug. 28th first appeared about 9 P. M., when a faint white light commenced about north and extended to about east by north. About 9¼ P. M. great streams of red and blue shot up all along the northeastern horizon, but they appeared to shoot highest about mid-way of the light. These streams would faint and brighten in such a wonderful manner, that we imagined some painter in the skies drew his great brush from the horizon up to 40 or 50 degrees, dipped with vermillion, then with sky blue, and then with white and flesh color.

15. *Observations at Sacramento, California,* (lat. 38° 34′), *by* THOMAS M. LOGAN, M.D.

I have observed the aurora only at five different times during a residence of nine years at Sacramento, viz., Dec. 16, 1857, Oct. 27, 1858, Aug. 28, 1859, Sept. 1, 1859, and Oct. 18, 1859. I know of but three other well authenticated instances of the phenomenon having been witnessed in California; one by Geo. H. Goddard at Sonora, Jan. 19th, 1852; and two by Henry Gibbons, M.D., at San Francisco, Jan. 19th, 1852, and Feb. 19th, 1852. This shows the infrequency of its appearance in this State.

The aurora of Aug. 28th, 1859, commenced at 9 P. M. and ended about 3 A. M. next morning. The appearances exhibited during this extended period were so various as to render it impossible to note the particular hours of the different changes. In its perpetual movements and fantastic changes were recognized all of the characteristic features that mark this phenomenon, from its close resemblance to the aspect of the sky before sunrise, to the formation of the luminous arc, darting forth palpitating rays towards the zenith, of white, pale red, and deep blood color. This last mentioned feature was seen in its greatest glory at about midnight; and lambent streamers about this time were noticed to shift gradually from west to east, and vice versa. The summit of the arc was not more than six or eight degrees above the horizon, and appeared to coincide with the magnetic meridian. The lower segment of the arc was not as obscure as in the other auroras observed by us. The most remarkable feature during the whole display, was the long continued gleaming of a dark rose or carmine illumination, particularly at the western extremity of the arc; this rosy light passing occasionally along the belt with a fluctuating movement towards the opposite end. The whole northern sky at one time seemed to be a cupola on fire, supported by columns of divers colors, relieved and intensified by dark shadows or rather streaks. The sky remained almost entirely clear the whole night. This aurora was observed in all parts of the State, and very generally throughout the whole north Pacific region.

The aurora of Sept. 1, 1859, was first observed about 10 P. M. There was seen first a warm glow in the northwest, and two white silvery clouds in the north. Soon the light extended in all directions, until the entire firmament was suffused with a ruddy light so bright at times that the hour could be distinguished on the dial of a watch. At midnight a splendid glowing corona was seen extending from the eastern to the western horizon, and the whole southern hemisphere appeared to be in one continuous blaze. These ever changing phenomena continued to manifest themselves until lost in the dawning day.

16. *Observations at St. Louis, Mo.* (lat. 38° 37′), *from a St. Louis Journal.*

On the night of Sept. 1st we had a most beautiful exhibition of the aurora. The view did not approach its highest grandeur until after 11 o'clock. At first there was a hazy appearance, embellished here and there by faint streaks and tremulous touches of light. Then the wavy pencillings grew stronger and broader, and the light spread until it had crept up to the zenith, when half of the world seemed enveloped in a sheet of mellow flame.

17. *Observations at Louisville, Ky.* (lat. 38° 3′), *from the Louisville Journal.*

One of the most magnificent auroras ever witnessed in this latitude was seen about 9 o'clock, Aug. 28th. The whole heavens, from the northern horizon to the zenith, were brilliantly illuminated with a rose-colored light, and the flashes were very vivid. The northwestern sky was the portion most brilliantly illuminated, but in the northeast, the rosy flush was exquisitely beautiful.

18. *Observations at Charleston, S. C.* (lat. 32° 46′), *from the Charleston Mercury.*

Aug. 28. The northern heavens were brilliantly lighted until about 2 o'clock Monday morning, when the aurora faded entirely away. On the morning of Sept. 2d the auroral exhibition far surpassed any former instance observed in this city, for the general extent and diffusion of the lights.

19. *Observations at Bermuda* (lat. 32° 34′), *from the Bermuda Royal Gazette.*

The aurora of Aug. 28th appeared to ascend from a few degrees above the northern horizon a great distance upwards towards the zenith, assuming a variety of shades and beautiful colors; sometimes in tremulous sheets of pale yellow, changing gradually into a deep crimson, or shooting upwards in streams of light resembling those frequently observed from the setting sun. It covered at times the entire space between N.W. and N.E., leaving the sky from the horizon to its apparent base, perfectly clear. Towards 10 o'clock its brilliancy gradually died away, but it continued more or less visible till the dawn of day.

Sept. 2d, between 2 and 3 A. M., the aurora displayed itself in greater splendor than it did on the 28th. Many persons were awakened from their slumbers by the intense light which entered their chambers.

20. *Observations at Savannah, Ga.* (lat. 32° 5′), *from the Savannah Republican.*

On the evening of Aug. 28th we had a brilliant display of the aurora borealis. The northern sky, for an extent of some forty-five degrees, was luminous with a mass of red light, from whence shot up towards the zenith the usual streaks, at times vivid and beautiful.

Sept. 2d, about 1 A. M. the aurora again appeared and was of a very intense and beautiful color, being a mixture of pink, gold and purple. After it had reached an elevation of about 45°, it seemed to dissolve in the centre, and spread out both east and west. About 2 o'clock it formed a complete arch overhead, from N.E. to S.W. About 3 A. M. it gathered in the zenith, and sent out bright fiery flashes in every direction. It was far more magnificent than the aurora of Aug. 28th.

21. *Observations at Mobile, Ala.* (lat. 30° 41′), *from the Mobile Daily Register.*

The aurora showed itself a little east of north about 7¼ P. M. Aug. 28th, and kept up the exhibition until about 9½ P. M., when its paling light died out. It was of a reddish hue, inclining to yellow, and its flickering light assumed a kind of pyramidal form, shooting up into the heavens, nearly to the zenith. Then the centre seemed to grow dim, and a division took place, its right wing moving to the extreme north, where the left wing after a short time joined it.

Between 12 and 3 o'clock on the morning of Sept. 2d, the aurora was repeated upon a scale of beauty and grandeur never before witnessed in the south. A bright pink colored light shot up from the northern horizon, and darted off into beautiful rays, flickering and brightening until they reached the zenith, and soon encircled the hemisphere like a belt from east to west. After about three-quarters of an hour, during which time the aurora occasionally furnished light enough to read by, the bright and beautiful light suddenly clothed the entire firmament.

22. *Observations at New Orleans, La.* (lat. 29° 57′), *from the New Orleans Daily Delta.*

A grand auroral display appeared between 8 and 9 o'clock, Aug. 28th, in the northern horizon. A column of light first shot up into the sky, which soon spread up towards the zenith, and around the horizon, and made one of the most magnificent appearances that the sky has ever exhibited.

About 11 P. M., Sept. 1st, the aurora reappeared and continued until 3 or 4 o'clock in the morning. Nearly the whole visible northern hemisphere was covered with a fiery, blood-reddish, though transparent vapor. The deepest color was on the east and west, a space around the polar centre seeming to be the only non-illumined portion of the northern heavens. Over this crimson ground, spears and pencils of pale flickering light shot up at intervals from the horizon, converging at a point near the zenith. The whole sky along those lines was at once luminous and tremulous. In a moment vast segments of arches would rise, and then suddenly disappear.

23. *Observations at Galveston, Texas* (lat. 29° 17′), *by* Prof. C. G. Forshey.

Aug. 28th, as early as twilight closed, the northern sky was slightly lurid, and at times lighter than other portions of the heavens. At $7^h\ 30^m$ a few streamers showed themselves. Soon the whole sky, from Ursa Major to the zodiac in the east, was occupied by the streams or spiral columns that rose from the horizon. Spread over the same extent, was an exquisite roseate tint which faded and returned. Stately columns of light reaching up about 45° from the horizon, moved westward about one degree for every ninety seconds of time. There were frequent flashes of lightning, apparently from distant clouds, along the whole extent of the aurora; but no clouds were visible, except a single streak near the horizon. At 9 P. M. the whole of the streaking had faded, leaving only a sort of twilight over the northern sky, and we ceased our observations.

At 3 A. M. Aug. 29th, I awoke and perceiving that it was very light outside, rose, and found the whole northern heavens again on fire. Such a display I have never seen equalled since the aurora of Sept. 1, 1839. The whole distance before-named was tinted with the roseate hue; darker, nearly crimsoned at the two flanks. In the centre, near the meridian, stood a stupendous pyramid of white light with its apex near the zenith. On either side at some twenty degrees, stood a pyramid of rosy light, each about sixty degrees in height and in exactly symmetrical positions. Scarcely had I sketched the outline of this noble spectacle when the columns drifted westward and faded.

Another fine display of the aurora commenced about 10½ P. M. Sept. 1st, and lasted until near daylight the next morning. A dusky red, like the reflection of an immense conflagration, overspread almost the entire heavens, beyond the zenith, far down towards the southern horizon.

24. *Observations at Sea* (lat. 28° 30′, long. 79° 30′), *Barque Pride of the Sea.*

Sept. 2d, at 12^h 35^m A. M., a bright spot or cloud appeared in the N.W. which shot up rays resembling the aurora, and in thirty or forty minutes formed an arch across the horizon from N.W. to N.E., which became lighter as it arose, and at 1^h 15^m A. M. it was light enough to read the smallest print without a light. At the time the horizon was cloudy, but overhead was clear, the larger stars being just seen. At 2^h 15^m the arch passed over to the southward, when it became dark again.

25. *Observations at Key West* (lat. 24° 32′), *from a Journal.*

A brilliant exhibition of the aurora was witnessed at this place Aug. 28th, and a still more brilliant one on the morning of Sept. 2d. The whole northern half of the heavens was tinged with crimson, red as blood. Occasional flashes of blue and white light shot up towards the zenith and then slowly melted away.

26. *Observations at Havanna, Cuba* (lat. 23° 9′), *by* M. ANDREAS POEY.

In his former communication (Am. Jour., vol. xxviii, p. 406) Mr. Poey stated that during the auroras of Aug. 28th and Sept. 1st he was unable to obtain any indications of atmospheric electricity. In a later communication he states that neither at the time of these auroras, nor on the preceding or following days, was there the smallest interruption or disturbance experienced on the electro-magnetic telegraph lines of Cuba.

27. *Observations at Inagua, Bahama Islands* (lat. 21° 18′), *from the New York Journal of Commerce.*

The aurora of Aug. 28th was distinctly seen from this place, and was supposed to have been a large fire in the neighborhood. It was remarkably brilliant, but was not attended by that flashing appearance which is sometimes noticed in higher latitudes.

28. *Observations at Cohe, Cuba* (lat. 20°), *by* GEORGE F. ALLEN.

On the night of Sept. 1st a Spanish mechanic who worked for me called me out of bed to see the great light in the northern sky. He was much struck with it, and said the people in St Jago de Cuba would think the end of the world was at hand.

I found a display which would have been considered more than ordinary even in the latitude of New York. It resembled the auroral displays occasionally seen in New York when more than usually brilliant. The same rosy light, on a darker horizon, fading off into yellower and whiter as it spread upwards, variegated occasionally with white streamers. It extended horizontally, according to my rough estimate, about one-third or two-fifths of the horizon, and upwards about two-fifths of the arch of the visible heavens. It was a very brilliant display, and surprised me much by its brilliancy in that latitude.

29. *Observations at Kingston, Jamaica* (lat. 17° 58′), *from the New York Herald.*

An extraordinary light appeared in the north on the night of Sept. 1st and the morning of Sept. 2d. It appeared as if there was a colossal fire on earth which reflected its flames on the heavens. The whole island was illuminated. The light was seen at Montego Bay (lat. 18° 21′) at 10 P. M., but it was not observed at Kingston until 1 A. M. Sept. 2. It continued until 5 A. M., when it gradually disappeared. It looked as if Cuba was on fire, and many believe that a portion of this island had been destroyed by a conflagration. Other persons were of opinion that the light was that of an aurora, but the aurora has never before been seen in this latitude. A similar fire was observed on the north side of Jamaica Aug. 28th.

30. *Observations at Guadeloupe, West Indies* (lat. 16° 12′), *from L'Institut.*

On the 2d of September, from $1\frac{1}{2}^{h}$ till daylight, an Aurora Borealis was seen at Guadeloupe to the great astonishment of the population. Its ruddy light was noticeable in the interior of the houses. At the centre of this vast conflagration were noticed two rays of whitish light which rose parallel to each other, passing a little to the left of the pole star. The aurora attained its maximum of brightness at 3 A. M.

31. *Observations at La Union, San Salvador* (lat. 13° 18′), *from the Gaceta del Estado.*

On the night of Sept. 2d, a most extraordinary phenomenon was witnessed. About 10 o'clock, a red light illuminated all the space from north to west, to an elevation of about 30° above the horizon. The light was equal to that of day-break, but was not sufficient to eclipse the light of the stars. The sea reflected the color, and appeared as if of blood. This lasted until three in the morning, when a dense black cloud arose in the east, and commenced to spread over the colored portion of the heavens, presenting a most curious spectacle; for in the parts where the cloud was not dense enough, the red light shone through, and formed a thousand fantastic figures, as if painted with fire on a black ground.

In the city of Salvador (lat. 13° 44′) the same phenomenon was visible, occupying the same space in the heavens, and the red light was so vivid that the roofs of the houses and the leaves of the trees appeared as if covered with blood.

February, 1860.

Art. XXV.—*Correspondence of Mr. Jerome Nicklès, dated Nancy, November 10th, 1859.*

Biography.—Cagniard-Latour.—We have already given some notice of this physicist, so lately lost to science; the following details are taken from an autobiography, which gives a very interesting account of the circumstances which led him to some of his discoveries. His researches may be classed under four heads; acoustics, mechanics, chemistry, and general physics having successively occupied his attention. His first invention (1809) was a pneumatic Archimidean screw, which is now in common use for conveying gases under liquids, and has received the name of the *Cagniardelle.* The ingenious inventor simply inverted the action of the ordinary screw of Archimedes, making it revolve from right to left. As Arago remarked in the chamber of deputies in 1844, during a discussion of the law of patents, although the *Cagniardelle* is nothing more than Archimedes' screw reversed, it is not less true that 2000 years had passed before any one conceived the idea of making this simple change and rendering it available in mechanics as a pneumatic machine.

The *Siren* (1819) as is well known, is an instrument for measuring the vibrations of the air which constitute sound. If, as had been supposed by physicists, the sounds produced by musical instruments are due to the regular succession of impulses given to the air by their vibrations, it was evident that a mechanism which would enable us to strike the air with the same rapidity and regularity should in like manner produce sounds. Reasoning in this manner, Latour was led to the invention of this well-known and beautiful instrument.

In 1822 he published his experiments on the combined action of heat and pressure upon certain liquids, such as water, alcohol, ether and naphtha. He imagined that the dilation of a volatile liquid must have a limit beyond which, notwithstanding the compression, it would pass to the state of vapor, provided that the capacity of the vessel permitted the liquid to expand beyond its maximum of dilation. The remarkable results to which he was led by this reasoning are well known.

In 1837 he published with Mr. Demonferrand the description of an acoustic pyrometer, by which the authors proposed to render the measurement of all temperatures appreciable through the medium of sound. In the same year he examined the pressure to which the air in the trachea is exposed during the act of sounding. He had previously been employed in investigating the pressure to which the air in the lungs is exposed when employed in sounding certain reed instruments, and had found it for the clarionette equal to the pressure of the atmosphere, *plus* a column of water of 30 centimeters. In order to extend these inquiries to the human larynx, it was necessary to find a person having an opening in the trachea, and yet able to produce vocal sounds at will. After long search, Cagniard-Latour found such a man, who was for his purpose as precious as the subject with the permanent gastric fistula became for the well-known experiments of Dr. Beaumont. In the same year he made known his chronometric balance, designed to measure the dynamic effects of machines in motion.

Next appeared a memoir on the alcoholic fermentation, of which these were the principal results:

1st. The yeast of beer is made up of little globular bodies, apparently vegetable, and capable of reproduction in two different manners. These bodies seem to act upon a solution of sugar only when in a state of life, and he hence conceived it probable that it is by a vital process that they transform the sugar into carbonic acid and alcohol. This investigation was suggested by a question long before proposed as the subject of a prize by the French Academy of Sciences, in the seventh year of the Republic, viz. What are the characters which distinguish among animal and vegetable matters, those which serve as ferments and those which are subject to fermentation?

Cagniard-Latour now resumed his researches upon vibrating bodies, and succeeded in producing a sound by causing a glass rod to oscillate easily between two metallic columns. The peculiarity in the sound thus obtained was that the number of vibrations indicated by it corresponded only to one-half the synchronous number of simple oscillations of the rod, although the apparatus was arranged in such a manner that each movement backwards and forwards should produce two strokes of equal intensity, by the alternate blows upon the two columns. The experiments made with this instrument enabled him to give the theory of the production of sounds by vibrating cords. During the same year (1840) he studied the production of grave tones, like those of the human voice, and made various researches to discover the mechanism of the human voice.

In 1851 he laid before the Academy a memoir upon the *moulinet à battemens*, demonstrating some new acoustic phenomena. In two previous papers published in 1830 and 1831, upon the sounds produced by solid bodies turning with great velocity, he had shown certain facts relative to the musical tones produced by the friction of the axle of a wheel against its supports. Subsequently he conceived the idea that a solid of revolution, a cylinder for example, arranged so as to turn vertically around its axis upon two center-holes, might give rise to pulsating sounds (beatings), although moving with a feeble velocity, provided it received in the lower center-hole the friction of the revolving axis of a winch turning in an opposite direction to the cylinder. It was to the instruments constructed according to these ideas that he gave the name of the *moulinet à battemens*.

We have also from Cagniard-Latour an investigation on the action of heat on different kinds of wood enclosed in hermetically sealed glass tubes. Sir James Hall, in his experiments upon the saw-dust of pine wood and of horn in sealed gun-barrels, had observed that the mixture underwent fusion and was cemented into a sort of coal. Similar results were obtained by Latour with thick glass tubes.

These are not the only researches which we owe to this lamented physicist; in our previous letter we mentioned that while connected with the Government powder works in 1814, he made some useful improvements in that department, especially in the glazing of powder. We also spoke of his very light, portable, and efficient flour-mill, which consisted of a steel rasp moving vertically with an alternating motion between two fixed rasps, also of steel. During the severe winter of 1816, the streams being all frozen and the mills stopped, Cagniard-Latour was directed to have an immense number of these hand-mills constructed, and thus in a few days the public were saved from the fears of a famine. We have also seen the part which he took in the establishment of gas-lighting in Paris. He was besides the constructer of an aqueduct, a model of its kind, suspended between two rocks, and formed of a single span 200 metres in length. When we consider these varied achievements, we learn with surprise that it was only in 1851 that he became a member of the Academy of Sciences.

The aurora borealis and its theory.—The late brilliant auroras have called attention to De la Rive's theory, of which we have formerly spoken, and which is explained at length in his *Traite d'Electricité.* Great perturbations were observed along the telegraphic lines over the European continent, similar to those remarked some years since by Matteucci in Tuscany, and Highton in England. The most remarkable fact in these electrical disturbances is that they were produced by a continuous current, while those of a thunder storm are instantaneous, and only mark points upon the paper in Morse's apparatus; the aurora of the 29th August traced continuous lines of greater or less length. These effects lasted for several days after the aurora.*

It is fortunate that the aurora of the 29th was carefully studied by a man so competent as Coulvier-Gravier. This observer, who has studied the heavens for nearly 60 years, and has so much advanced the sciences of Cosmography and Meteorology, was found that night as usual at his post in the observatory which the government prepared for him twenty years since at the Luxembourg palace. The phenomenon was in all its splendor at 2^h 45^m A. M.; its extent included more than 100°, and M. Coulvier-Gravier declares that he had never seen it more beautiful during the long period of his observations.

The observations during the late auroras support the theory of De la Rive, which he has thus defined. The vapors constantly rising from the sea, and especially from the equatorial regions, carry with them to the higher regions of the air a great quantity of positive electricity, to which they serve as the vehicle, leaving the surface of the globe negatively electric. Borne to the poles by the currents which always prevail in the higher regions of the atmosphere, these vapors carry with them their electricity, and thus give to the whole atmosphere a positive electric condition, which diminishes from above downwards. This positive electricity tends unceasingly to combine with the negative electricity of the earth,

* See p. 92, this volume.

directly through the stratum of air, but more especially at the two poles, where the currents of vapor carried by winds converge and are condensed.

According to Mr. De la Rive the aurora of the 29th August was a natural consequence of the great drought which had prevailed over the continent. The dryness of the air had prevented the positive electricity from neutralizing itself directly with the negative of the earth. From this accumulation of electricity there finally took place towards the polar regions a discharge much more intense and much more rapid than usual, which constituted the brilliant aurora in question. A fact which tends to show that the aurora is an electrical and not a magnetical phenomenon, is furnished by the ozonometrical observations made by Mr. Perigny, at Versailles. From the 28th of August to the 2d of September, he found the air to contain a quantity of ozone, relatively large, and more abundant by night than by day.

Human Remains in the Drift.—For the last twenty years it has been known that axes of flint, evidently wrought by human skill, were found in beds of the drift at Amiens, associated with the bones of extinct species of animals. This discovery, made by a learned antiquary, Mr. Boucher de Perther, had been regarded as doubtful, and it was supposed that sufficient precaution had not been observed in conducting the explorations. Recent discoveries made in a cavern at Brixham, near Torquay, in England, however, recalled attention to the observations of Mr. Perther, and Mr. Prestwich, with several other geologists, accordingly visited Amiens in order to make the excavations necessary to decide this important question. Every precaution was of course taken to prevent errors or deception, and Mr. Prestwich could find nothing at Abbeville, but at Amiens he was more fortunate; one of his companions, Mr. Flower, in examining a bed of gravel at six meters from the surface, and evidently undisturbed, extracted with his own hands a fine axe, more than five inches long. After this Mr. Prestwich having been informed that a similar discovery had been made in 1737, at Haxne, in Suffolk, visited the place, and learned that some years since wrought flints were still found in abundance, although rare at present. He however succeeded in finding two axes, similar to those of Amiens, but of less perfect finish. An analogous fact has just been verified by French geologists, who have found these axes in Picardy, associated with remains of *Elephas primogenius*, *Rhinoceros tichorhinus*, *Equus fossilus*, and an extinct species of *Bos*. The conclusion from all these facts is that man was cotemporaneous with these several species of large animals now lost, and known to us only by their fossil remains.

Curare in the treatment of Tetanus.—This important question has elicited much discussion at the Academy, and many contradictions and differences of opinion. In 1850, Mr. Claude Bernard showed that the curare, or arrow poison, acts by paralyzing the system of motor nerves; following up this observation, an Italian physician, Villa, of Turin, made in 1854 a series of experiments, in the course of which he showed that curare exerts upon the nervous system an action so completely antagonistic to that of strychnine, that the two poisons may be neutralized by each other. Dr. Villa having been attached to the French hospitals during the late war, was induced to apply the curare in the treatment of three cases of traumatic tetanus one of which recovered. It was the case of a soldier wounded by a ball in the right foot. The curare dissolved in water was applied to the wound, with the effect of diminishing the pain and suspending temporarily the tetanic spasms, which, however, returned. After fifteen days of this treatment the patient left the hospital completely cured. The experiments of Mr. Villa have been repeated in the hospitals of Paris; but as yet only a single case of cure has been reported out of several failures; the experiments, however, continue. According to a letter from Sir Benj. Brodie, of London, to the Academy of Sciences, the application of curare as a specific against tetanus, was unsuccessfully made upon horses at London in 1815, by Dr. Sewell, professor at the Veterinary College. Such are the principal facts in the question as far as yet made known.*

The new alloys of Platinum.—We recall the interesting researches of Messrs. Deville and Debray on this subject only to mention their industrial applications. Hitherto it had been supposed that the presence of iridium impaired the quality of platinum, but the labors of Deville and Debray have shown that on the contrary alloys of these two metals may be prepared which are greatly superior to pure platinum, presenting greater strength and rigidity, and resisting better both heat and acids. Thus the alloy containing 21·3 of iridium is highly malleable and scarcely attacked by aqua regia. As the quantity of iridium is less, the alloy becomes softer, and one containing 10 or 15 per cent. is peculiarly fitted for chemical vessels. These alloys are now largely wrought in Paris; retorts for the manufacturers of sulphuric acid have been made, having the strength and rigidity of rolled iron.

Messrs. Deville and Debray are at present making some trials at the French mint, for the Russian government, to determine the fitness of the new alloys for coinage. They have found that those containing 20, 10, 7½ and 4 per cent of iridium, take the impression of the dies with great perfection. The same is true of the natural alloy, which is obtained by directly fusing crude platinum, and retains only the iridium and rhodium in combination with the platinum, the other metals having been removed by volatilization or oxydation. The platinum workers of Paris are now manufacturing and selling the new alloys, and, contrary to the wishes of the discoverers, are exacting higher prices than for pure platinum.

Rifled Cannon.—The invention of these guns appears to be due to a former captain of artillery, Mr. Tamisier, who was in 1842 charged with the course of instruction in musketry at Vincennes, where he applied himself with great assiduity to the study of various questions connected with his profession, with results which have contributed very much to the improvement of the system of musketry instruction in the army and in the arms of precision. After studying the effects of elongated projectiles in rifles and muskets, Mr. Tamisier was led to construct a rifled mortar, with cylindro-conical shells. The duke of Montpensier, then colonel of artillery, at once saw the importance of this new project, and after causing many experiments to be made, ordered at his own expense, in 1847, the construction by Capt. Tamisier of elongated balls and shells. These however were not tried until 1850, from which time up to 1853, Mr.

* See experiments on two new varieties of South American arrow poison by Drs. Hammond and Mitchell, Am. Jour. Med. Sci., July, 1859, (this Jour. [2], xxviii, 303).

Tamisier continued his experiments at Vincennes and at La Fère. The first experiments were made at Vincennes, on the 15th of July, 1850, when it was shown that rifled six-pounders with elongated projectiles, carried much farther and with greater exactness than ordinary guns of smooth bore. On the 14th August, 1851, Capt. Tamisier repeated his experiments at the polygon at Vincennes, before the President and the Minister of War. A six-pounder rifled with three grooves projected balls of five kilogrammes to a distance of 1500 meters with a charge of only 700 grammes of powder. The government then ordered further experiments to be made, which were conducted at La Fère, a fortified place in the Department of the Aisne, where greater secrecy could be secured than near Paris. The trials were there made by Col. Trenille and by Col. Virlet, now directors of the School of Artillery at Metz, and led to a complete solution of the problem, so that the army of Italy was able to bring into the field more than 200 rifled guns of a calibre of 84 millimeters, requiring for service and transport only two-thirds the men and horses hitherto necessary, and carrying balls of four kilogrammes 3500 meters with such precision that at this distance they would all fall in a rectangle of 80 meters in length by 40 meters in breadth.

Acclimatation.—In our last letter we spoke of a number of Arabian camels, which the Society of Acclimatation had procured in Algeria, to be introduced into Brazil. We learn that they were safely landed there after a voyage of 28 days.

Photo-Chemical Researches: Persistent Activity of Light.—In this Journal for March, 1859, p. 257, we have described the beautiful experiments of Niepce de St. Victor, upon the persistence of the effects of insolation. According to Mr. Laborde, the curious results obtained by Mr. Niepce are due not to radiation, but to a veritable emanation from the card-board, which has been impregnated with tartaric acid, and exposed to the sun-light. The principle subsequently evolved from this card-board, which acts upon sensitive paper prepared with a silver salt, is, according to Mr. Laborde, no other than formic acid. This is well known to be a product of the oxydation of tartaric acid, especially when in contact with peroxyd of lead. He found that a concentrated solution of tartaric acid mixed with peroxyd of lead, and placed in a dark place, evolved vapors which reddened litmus and discolored prepared paper after a very short time. There is, then, in the case of insolated tartaric acid, an emanation and not a radiation; but Mr. Niepce obtained similar results with nitrate of uranium spread upon porcelain and insolated, in which case it is not easy to conceive of an emanation of vapor. However this may be, the facts announced by Mr. Niepce remain, and the only difference of opinion is as to their interpretation.

Maritime Canals.—Besides the canals proposed at Panama and Suez, the question of one between the Caspian and the Black Sea is now being discussed in Russia. The construction of such a canal was ordered by Peter the Great, after having been commenced a century before under the Sultan Selim II, upon the suggestion of his Grand Vizier, Mohammed Sokolli, who was for Turkey what Colbert was for France. The canal was commenced in 1569, and during two years there were employed upon it 5000 janissaries and 20,000 prisoners.

The canal now projected will unite the Don and the Volga, the first of these falling into the sea of Azof, and the second into the Caspian, near Astrachan, at a point designated on the maps as Tzaritsin. The two rivers are separated by a distance of only 55 kilometers. The importance of a canal which will connect the Caspian with the Mediterranean will readily be seen, and it must at the same time be confessed that it will be much more easily executed than the tunnel under the channel proposed by Mr. Thornè de Gamond, since the canal, being exclusively on Russian soil, will be a question for engineers, with which politicians will have no concern.

For the last thirty years, it has been proposed to make a sea-port of Paris by a ship-canal leading from the Channel; the introduction of railways caused the project to be for a time abandoned, but it is now again discussed. Among the plans proposed is one which appears in the *Ann. des Sciences* of the 8th October, 1859. It is intended to excavate at the mouth of the Seine, near Havre, a harbor 1000 meters long and 200 wide, with a depth of 12 meters. The canal, 150 kilometers in length, will follow the course of the Seine, and will have the same depth as the harbor, which will be furnished with gates to preserve the water at a proper height. A double line of railway will accompany the canal, one line serving for passengers and freight, and the other for towing vessels, which will thus make the voyage in four hours.

The position and direction of the canal will be such that the west winds, which are the most frequent at Paris, will help vessels in coming up, while the water of the Seine, to be let in by sluices, will aid them, by their current, in descending. The great difficulty in this enterprise will arise from the tunnels required, the total length of which will be from twenty to twenty-five kilometers. These will be vaulted, and with a height of 30 meters by at least 50 meters in breadth. Mr. Piorry estimates that the expense of this work may amount to a billion (1,000,000,000) of francs.

Engineers are also occupied with a plan for joining the English Channel with the Mediterranean, by taking advantage of the river Rhone, the Saône, the Yonne, and the Seine. The details of its execution having only a local interest, we spare our readers their recital.

SCIENTIFIC INTELLIGENCE.

I. CHEMISTRY AND PHYSICS.

1. *On two new series of Organic Acids.*—Hentz has studied the action of methylate of soda and similar substances upon chloracetic acid, and has obtained interesting new acids in which hydrogen may be considered as replaced by the deutoxyds of methyl, ethyl, etc. The acid resulting from the action of methylate of soda on chloracetic acid has the formula $C_6H_6O_6$, which is that of lactic and paralactic acids, but is not identical with either of these. To this acid the author gives the name of methoxacetic acid; it is monobasic, and gives beautifully crystallized salts. The acid itself is easily prepared by decomposing the zinc salt with sulphu-

retted hydrogen, and distilling the liquid after separating the sulphid of zinc. The boiling point rises gradually till it becomes constant at 198° C., when the acid passes over as a colorless liquid, with a sour smell, resembling that of acetic acid.

When monochloracetate of soda is boiled continuously, an acid liquid passes over, which, when saturated with baryta, yields on evaporation a crystallizable salt having the formula of glycolate of baryta. The author proposes for the acid contained in this salt the name of oxacetic acid; it is isomeric, but not identical with the glycolic acid. Its formula is $C_4H_4O_6$.

Ethylate of soda acts violently on monochloracetic acid; the products of the reaction are chlorid of sodium, and the soda salt of a new organic acid, homologous with the two last described, and which the author terms ethoxacetic acid. This acid is volatile without decomposition and appears to boil at a lower temperature than the corresponding methyl compound. Its formula is $C_8H_8O_6$.

Amylate of soda acts in a similar manner on monochloracetic acid. The new acid formed in the reaction is an oily liquid which has the formula $C_{14}H_{14}O_6$.

Phenylate of soda, under similar circumstances, yields phenoxacetic acid, as an oily liquid which crystallizes at a low temperature, and distils over without decomposition. The analyses appeared, however, to show that the phenyl alcohol employed contained benzalcohol, and that the product examined was therefore a mixture of the homologous acids $C_{16}H_8O_6$ and $C_{18}H_{10}O_6$.

When the soda salts of organic acids are heated with monochloracetic acid, chlorid of sodium is formed, and new organic bodies which the author proposes to study. It is easy to see that all the acids belonging to the formic series will yield similar new acids with the peroxyds of the alcohol radicals homologous with hydrogen, and that in this manner a very great number of new compounds may be obtained.—*Journal für prakt. Chemie*, 78, *p.* 174.

Note.—According to Kolbe's view, lactic acid is to be regarded as propionic acid, in which one equivalent of hydrogen is replaced by one of HO_2, so that its rational formula is $\left.\begin{matrix}C_6H_4(HO_2)O_2\\H\end{matrix}\right\}O_2$. Upon this view the oxacetic acid of Heintz must be identical with glycolic acid, which appears not to be the case. The question may doubtless be decided directly by examining the products of the action of water upon chloropropionic acid, which ought thus to yield lactic acid directly, since we should have the reaction

$$C_6H_4ClO_3HO+HO_2H=C_6H_4(HO_2)O_3HO+HCl.$$

Heintz's acids may be more simply regarded as derived from the formic series by simple replacement of hydrogen by the peroxyds HO_2,$C_2H_3O_2$, $C_4H_5O_2$, &c. Dichloracetic and trichloracetic acids ought to yield analogous products in which two or three equivalents of hydrogen are replaced by two or three equivalents of peroxyds which need not be of the same radical. The number of possible acids would thus almost or quite equal that of the ammonias or ammoniums. W. G.

2. *On the chemical constitution of Isethionic acid and Taurin.*—By the distillation of isethionate of potash with perchlorid of phosphorus, Kolbe has obtained a new acid which has the empirical formula $C_4H_5ClS_2O_6$, and which he terms chlorethyl-sulphuric acid. This acid yields taurin by the substitution of NH_2 for Cl, and ethyl-sulphuric acid $C_4H_6S_2O_6$ when the chlorine is simply replaced by hydrogen. Kolbe draws a parallel between the derivatives of carbonic and sulphuric acids, which is best illustrated by the following tabular view:

$\underbrace{C_2O_2 . O_2}$ Carbonic acid.	$\underbrace{S_2O_4 . O_2}$ Sulphuric acid.
$HO . \underbrace{(C_4H_5)(C_2O_2)} . O$ Propionic acid.	$HO . \underbrace{(C_4H_5)(S_2O_4)}, O$ Ethyl-sulphuric acid.
$HO . \underbrace{(C_4H_4Cl)(C_2O_2)}, O$ Chloropropionic acid.	$HO . \underbrace{(C_4H_4Cl)(S_2O_4)}, O$ Chlorethyl-sulphuric acid.
$\underbrace{(C_4H_4Cl)(C_2O_2)}, Cl$ Chlorethyl-carbon-chlorid.	$\underbrace{(C_4H_4Cl)(S_2O_4)}, Cl$ Chlorethyl-sulfochlorid.
$HO . \underbrace{(C_4H_4NH_2)(C_2O_2)}, O$ Amidoethyl-carbonic acid. (Alanin)	$HO . \underbrace{C_4H_4NH_2)(S_2O_4)}, O$ Amido-ethyl-sulphuric acid. (Taurin).
$HO . \underbrace{(C_4H_4(HO_2)),(C_2O_2)}, O$ Oxethyl-carbonic acid Lactic acid.	$HO . \underbrace{(C_4H_4(HO_2))S_2O_4}, O$ Oxethyl-sulphuric acid. Isethionic acid.

The author promises a more detailed account of the compounds and reactions referred to in the above brief preliminary notice.—*Ann. der Chemie und Pharm.*, cxii, 241.

3. *Researches on the atomic weight of Graphite.*—Brodie has communicated an exceedingly interesting and suggestive memoir on the atomic weight of graphite, considered as an allotropic form of carbon, the fundamental idea being that the different modifications of the same substance may exhibit a difference in equivalents, as well as in their ordinary chemical and physical properties. The author finds that graphite, when heated with nitric acid and chlorate of potash, increases in weight, and ultimately yields a light yellow crystalline substance. The details of the process are as follows: a portion of graphite is intimately mixed with three times its weight of chlorate of potash, and the mixture placed in a retort. A sufficient quantity of the strongest fuming nitric acid is added to render the whole fluid. The retort is placed in a water-bath, and kept for three or four days at a temperature of 60° C. until yellow vapors cease to be evolved. The substance is then thrown into a large quantity of water and washed by decantation nearly free from acid and salts. It is then dried in a water-bath, and the oxydizing operation repeated with the same proportion of nitric acid and chlorate of potash, until no farther change is observed. This is usually after the fourth time of oxydation. The substance is then to be dried, first in vacuo, and then at 100°. By placing the mixture in a flask exposed to sunlight, the change takes place more rapidly and without the application of heat.

The formula of the body thus obtained is $C_{22}H_4O_{10}$, or, as the author writes it, $C_{11}H_4O_5$.

Its crystals belong either to the right or oblique prismatic system. It is insoluble in water, containing acids or salts, and very slightly soluble in pure water. It unites with alkalies, and the crystals have an acid reaction: ammonia converts it into a transparent jelly, but the substance is not dissolved. Acids separate it from this combination, as a gelatinous mass resembling silica. Treated with deoxydizing agents, it is readily decomposed. When a solution of sulphate of ammonium or of potassium is poured upon the dry substance, a crackling sound is heard, and a body is formed resembling graphite.

The crystals are decomposed with ignition on the application of heat, gases being evolved, and a black residue left, which resembles finely divided carbon. This substance the author proposes to term graphic acid.

When graphic acid is heated in naphtha to about 270°, water and carbonic acid are given off, while the naphtha takes a deep red color. The residual substance resembles graphite and has the formula $C_{44}H_2O_8$, or with the author's equivalents, $C_{22}H_2O_4$. When this substance is heated in a current of nitrogen to a temperature of 250°, water is given off with a little carbonic acid; the substance remaining is found to have the formula $C_{132}H_4O_{22}$ or $C_{66}H_4O_{11}$. This body may be exposed for several hours to a red heat in a current of nitrogen without losing all its oxygen and hydrogen.

The author compares graphic acid with a remarkable compound of silicon discovered by Buff and Wöhler, which has the formula $Si_4H_4O_5$, and which was obtained from the graphitoid form of that element. The properties of the two substances agree very closely, whence it may be inferred that the graphite compound is the same term in the system of carbon as the silicon compound in the system of silicon. The total weight of graphite which in the compound is combined with atoms of hydrogen and of oxygen is 132. If we assume that this weight is like the corresponding weight, 84· of silicon, to be divided into four parts, we arrive at the number 33 as the atomic weight of graphite. Representing this weight by the letters Gr, the formulas of the substances $C_{11}H_4O_5$, $C_{22}H_2O_4$, and $C_{66}H_4O_{11}$ become $Gr_4H_4O_5$, $Gr_8H_2O_4$ and $Gr_{24}H_4O_{11}$, where $O=16$.

According to the law of Dulong and Petit, the specific heats of the elements are inversely as their equivalents. The elements are divided into two classes, one in which the product of the specific heat into the equivalent is about 3·3—the other in which this product is 6·6. The specific heat of carbon in the form of graphite—0·20187—presents a remarkable exception to the law, if we take its equivalent as 6· or 12, but if we assume the atomic weight of graphite as 33, we have for the product of the specific heat into the atomic weight, the number 6·6 which is according to the law of Dulong and Petit.

The relation which exists between the atomic weights of boron, silicon and zircon, and that form of carbon for which a place may be claimed as a distinct element, *graphon*, is precisely the kind of numerical relation which is found to exist between the weights of analogous elements. We have

Boron	11
Silicon	21
Graphon	33
Zircon	66

These considerations lead to the inference that graphite functions as a distinct element, forming distinct combinations with a distinct equivalent, viz: 33. How far this inference may be extended to the allotropic forms of other elements, experiment alone can decide.—*Quart. Journal of Chem. Soc.*, vol. xii, p. 261.

[*Note.*—With respect to the numerical relations between the eqs. of boron, silicon, graphon and zirconium which Brodie points out, it may be remarked that boron—at least with the equivalent 11—is triatomic, as shown by the density of the vapor of BCl_3 and other considerations. It cannot, therefore, with this equivalent, belong to the same natural group with silicon and zircon, which are diatomic, as shown by recent investigations. Marignac has established the isomorphism of SnF_2+KF with SiF_2+RF, while Troost and Deville have shown from the vapor-density of chlorid of zirconium that its true formula is $ZCl_2=2$ vols. or Z_2Cl_4 if we assume that all compounds correspond to 4 vols. in a gaseous state. The vapor density of $SiCl_2$ also agrees with the supposition that silicon is diatomic, supposing it to represent 2 vols. The true equivalents of silicon and zirconium become therefore respectively 14 and 44 or 28 and 88, if we admit the 4-volume theory. The equivalents of carbon, silicon and zirconium are then to each other as 6, 14 and 44, or as 12, 28 and 88, the common difference being 8 or 16 nearly. The formula $Si_4H_4O_{10}$ was deduced by Buff and Wöhler upon the supposition that the equivalent of silicon is 21, the element being triatomic as assumed by Berzelius. But if we take 14 as the true equivalent, the formula for the same compound becomes $Si_6H_4O_{10}$, and comparing with this the formula $C_{22}H_4O_{10}$ we have 132 parts by weight of carbon, representing 6 eqs. of graphon instead of 4, as assumed by Brodie. This gives 22 as the equivalent of graphon, instead of 33. If now we multiply the spec. heat of graphite as found by Regnault, namely 0·201, by 22 we have 4·4 so that the spec. heat of an atom of graphon does not obey the laws of Dulong and Petit, as the product should be either 3·3 or 6·6. It may, however, be remarked that the spec. heat of graphitoid silicon has not yet been determined, and that there may be other classes of elements whose atoms have the intermediate spec. heats 4·4 and 5·5. The formulas of Brodie's compounds become, if we take the equivalent of graphon as 22—$Gr_6H_4O_{10}$, $Gr_{12}H_2O_8$, $Gr_{18}H_4O_{22}$, (taking $O=8$ and not with Brodie as 16). No probable relation can be pointed out between the numerical values of the equivalents of graphon and of other elements, until we know to what natural group graphon belongs, since it is not certain or even very probable that the allotropic modifications of the same element belong

w. G.

to the same group.—w. G.]

4. *On the Cause of Color and the Theory of Light*; by Mr. JOHN SMITH, M.A. (Read by his brother, Dr. R. A. Smith).—The author, in attempting to explain certain natural phenomena, could not satisfy himself by applying the principles of either theory of light, and said that many natural phenomena indicated beats or vibrations in the luminous ether very

different from what science taught. That is, that there were greater intervals between them than Newton had demonstrated and scientific men believed. He therefore endeavored to contrive experiments by which he would be able to make as many revolutions or beats in a second as he considered the effective vibrations of light were repeated in a second of time, and argued that by certain contrivances to produce light and shade in alternate vibrations he should produce color. A series of experiments was subsequently undertaken, which led to the conclusion that varieties of color are produced by pulsations of light and intervals of shadow in definite proportions for each shade of color. That is, supposing white light to consist of the motion of an ether, blackness to consist of an entire absence of motion, then a certain color, blue, red, or yellow, will be produced by the alternate action of the light and the shadow. The author used shadow in the positive sense as the sensation was positive.

On pursuing the inquiry, he first caused a small parallelogram cut in card board to revolve over a black surface with a rapidity which he considered equal to the vibration of light. By this motion he obtained a distinct blue, while at another time in different weather he obtained a purple. He then made a disc with several concentric rings, which he painted respectively $\frac{1}{3}$, $\frac{2}{3}$, $\frac{3}{4}$, and $\frac{1}{2}$ black, leaving the remainder white, and on making this disc revolve the rings became completely colored. There was no appearance of any black or white. In a bright day with white clouds in the sky, the rings were colored respectively a light yellowish green, two different shades of purple, and a pink. By using discs of a great variety of shapes and different proportions of white and black, the author said that he produced successively or together all the colors of the rainbow, although he had not yet arrived at the exact arithmetical determination of the amount of light and shade needful for each color.

These experiments were made before the Society by the light of a paraffin oil lamp with a reflector. The author said that they were much more brilliant by sunlight.

There was another set of experiments which the author considered as very effective, and especially as being easily made and described, but requiring strong sunshine to show them. These were made by casting a shadow of a particular figure on a white wall or on a sheet of paper, so as to produce alternate beats of light and shadow when put in revolution. The figure became colored of different shades, and because these could be seen on the wall, like the spectrum from the prism, he called them spectra by reflection.

He mentioned also that the colors may be produced by making a black disc, with figures cut out of it, revolve before a white cloud or white screen.

There were many others which he had no time to enumerate, much less to describe, but he described some of the figures which produce the phenomena which are perceived when looking through transparent solids.

The author considered that his theory gave an entirely new and simple explanation of the phenomena of refraction through the prism, and summed up as follows :—

The experiments prove the homogeneity of the ether.

They prove the undulatory hypothesis, but oppose the undulatory theory.

They enable us to dispense with the different refrangibilities of the rays of light, as taught by Newton.

They help to explain many of the phenomena of what is called the polarization of light.

They give a new explanation of prismatic refraction, and explain in a plain and simple manner many very interesting natural phenomena.

Startling, he said, as these conclusions are to those who are conversant with the subject of light, he thought he was perfectly warranted in drawing them from his experiments. The general process of reasoning could not, however, be given in a short abstract.—*Ordinary Meeting, Oct. 4th, 1859, Manchester Literary and Phil. Society.*

TECHNICAL CHEMISTRY.

1. *Vegetable Parchment.—Papyrine.*—The interesting substance obtained in 1846 by Poumarède and L. Figuier (Comptes Rendus, xxiii, 918; see also this Journal xxviii, 431,) by immersing bibulous paper in partially diluted sulphuric acid—called *papyrine** by its discoverers—which with the exception of a few comparatively unimportant applications in France, where it was used for the shelves on which silk-worms are reared, &c., had excited scarcely any interest other than that naturally attaching to it as a chemical curiosity, until patented (Dec. 6, 1853) in England, by Gaine, (see *Rep. of Pat. Inv.* [E. S.] xxiv, 151) and manufactured by the well known house of De LaRue & Co., of London, has recently been investigated by Prof. A. W. Hofmann, (Ann. Ch. u. Pharm., Nov. 1859, cxii, 243; from a report to Messrs. Thos. De LaRue & Co.) In its prominent properties it resembles ordinary parchment very closely: indeed the two can hardly be distinguished from each other except on close inspection. Both exhibit the same peculiar pale, yellowish tint, the same degree of translucency, the same half fibrous, horn-like texture. Like animal parchment, the artificial product is not easily torn: it may be repeatedly bent or folded without exhibiting any special appearance of breaking in the creases formed. Like ordinary parchment it is extremely hygroscopic, and becomes more pliable by absorbing moisture. When wet with water it comports itself like untanned skins, swelling up to a slippery mass through which water cannot pass except by endosmose: the coherence of the substance is not at all impaired by thus soaking.

Vegetable parchment is best prepared by immersing unsized paper during a few seconds in oil-of-vitriol which has been diluted with half its volume of water, and immediately afterwards washing it in a dilute solution of ammonia; a thorough washing with pure water completing the process. Hofmann has ascertained by direct experiment that not less than one-fourth volume, or more than one-half volume, of water must be used with one volume of monohydrated sulphuric acid, in preparing the acid bath. The paper must not be immersed too long, nor should the temperature of the bath be higher than about 15° (C.)=[59° F.] A considerable amount of practice is moreover requisite before one can obtain a perfectly satisfactory product. When paper is transformed into vegetable parchment it undergoes no appreciable increase in weight. The action

* Should not this term, which has an undoubted right of priority, be preserved as the scientific name of the substance?—[F. H. S.]

of the sulphuric acid is purely molecular, the ultimate chemical composition of the paper—*cellulose*—remaining unchanged. [As already stated by Poumarède and Figuier *loc. cit.* and by J. Barlow, Proc. of the Royal Inst. 1857, ii. 411]. The result of the momentary action of sulphuric acid in this instance is comparable with that which a longer action of this acid upon woody fibre produces, viz.: formation of dextrine, a substance well known to be isomeric with cellulose. Indeed, the vegetable parchment may be regarded as a middle term between dextrine and cellulose.

The samples of parchment-paper examined by Hofmann [and by Barlow] contained no trace of free sulphuric acid; small portions of sulphate of lime and of sulphate of ammonia being the only soluble impurities present.

There is no apparent reason why the parchment-paper should not endure for an indefinite length of time. It is evident that if its destruction were dependent in any way upon the chemicals used in preparing it, decomposition would set in at once. Nothing of the kind occurs, however. Specimens of the factitious parchment which have been in Hofmann's possession during four years being undistinguishable from those recently prepared.

From experiments made in order to ascertain the strength of parchment-paper, as compared with that of true parchment and of unsized paper, it appeared that while strips of unsized paper broke when subjected to a weight of 15 or 16 pounds, similar strips of vegetable parchment supported 74 lbs., and those of ordinary parchment 75 lbs., before breaking. The cohesive force of unsized paper is thus increased five-fold by the treatment with sulphuric acid. It was also proved by experiment that for equal *weights* of the two substances parchment-paper exhibited about three-fourths the cohesive power of animal parchment. It also appeared that while the strength of strips of parchment-paper taken from different sheets was nearly constant, that of strips of animal parchment, even when cut from a single piece, was extremely variable, owing to the differences in thickness to which it is liable.

Parchment-paper although not quite so strong as ordinary parchment, is nevertheless more capable than the latter of withstanding the action of chemical agents, and especially of resisting the action of water; it may be left in this liquid for days, or even boiled in it, without undergoing any change, other than the increase of volume already alluded to, its original cohesion, and indeed all its properties being regained on drying. As is well known, animal parchment is soon converted into glue when boiled with water.

Since the parchment-paper contains no nitrogen, it is much less liable than ordinary parchment to putrefy when exposed to moisture, and will probably be less subject to the attacks of insects. Not only may the new parchment be substituted for that ordinarily employed for legal documents, &c.; but from its cheapness it will probably soon be used for ledgers and other important records—possibly for bank-notes—instead of the more perishable paper now employed. ["It will take the place of ordinary paper in school books, and other books exposed to constant wear." "It also promises to be of value for photographic purposes, and for artistic uses, in consequence of the manner in which it bears both oil and water-color."—*Barlow.*] Its strength and power of resisting the action of moisture seem also specially to adapt it for the use of architects and engineers—particularly for working-plans liable to receive rough usage; also for the envelopes of letters and for cartridges. In thin leaves it affords an admirable tracing paper. As a material for binding books it will without doubt be extensively used. The ease with which it receives both printers' and ordinary writing ink is remarkable. For chemical laboratories it affords a most convenient material for fitting together retorts, condensers, and the like; while its power of resisting the fluids used in galvanic batteries suggests that it may be useful for diaphragms, &c. It is already used by tons, instead of bladder, as a covering for jars containing preserves, marmelades, etc.

Parchment-paper has been successfully manufactured on the great scale for a year or more by the firm of De LaRue, the numerous difficulties which presented themselves having been fully overcome by the perseverance of one of its members—the distinguished chemist Warren De La Rue. [Specimen sheets of the parchment-paper accompany Hofmann's memoir.—F. H. S.].

2. *Weighing of Moist Precipitates;* by FERDINAND F. MAYER.—Mr. Ch. Mène, of Creusot,* gives a mode of weighing which does away to a great extent with the tediousness and difficulties attending the drying of many precipitates, especially in volumetric analysis. He washes the precipitate thoroughly by decantation, and then introduces it carefully into a bottle, the exact weight of which, when filled with distilled water at a certain temperature, is known. Since the precipitate is heavier than water, the bottle when filled again will weigh more than without the precipitate, and the difference between the two weights furnishes the means of calculating the weight of the precipitate.

In case the precipitate settles but slowly it may be collected on a filter, and together with the filter, after washing, be introduced into the bottle, in which case the weight of the filter and its specific gravity, supposing any difference should exist between its own and that of water, is to be taken in account. Precipitates soluble in or affected by water may be weighed in some other liquid.

This method, of which the above are the outlines, is spoken of in the Jahresbericht der Chemie for 1858† in rather disparaging terms, and I consider it not more than justice to the method, if not also to Mr. Mène, to prove its correctness, the more so as I have applied the principle on a large scale as far back as 1855.

I engaged in that year in the manufacture of carbonate of lead from refuse sulphate of lead, by treating the latter in a pulpy condition with carbonate of soda. The sulphate of lead I used contained very varying proportions of water and soluble impurities, from which latter it had first to be freed by washing. It was then in the state of a thin pulp, and the difficulty was to find the amount of dry sulphate of lead, as it was a matter of importance to use as little carbonate of soda, and to obtain as pure a carbonate of lead and sulphate of soda as possible. This could only be done by weighing it as a whole, or in portions; but as the

* Journal de Pharmacie et de Chemie, Oct. 1858.
† Jahresbericht der Chemie, by Will and Kopp, for 1858, p. 6.

drying of a tubful of sulphate of lead (from 500 to 1200 pounds) was impracticable, and sampling not less so, since the upper strata contained a much larger proportion of water than the lead at the bottom: I contrived the following method, which enabled me to leave the management of the process in the hands of a workman.

I took a strong oaken pail, weighing eight pounds when empty, and caused a black mark to be burnt in horizontally around the inside of the pail, two inches below the rim, up to which mark it held twenty pounds of water. I reasoned as follows: The specific gravity of sulphate of lead being 6·3, the pail if filled up to the mark would hold 126 pounds of pure sulphate of lead. The specific gravity of water being 5·3 less than that of sulphate of lead, it followed that if there was one pound of water in the pailfull of moist sulphate, the pail would weigh 5·3 pounds less than 126 (+8, the tare of the pail) =120·7 (+8); if there were two pounds of water present, the weight would be 115·4 (+8), and so on. This enabled me to calculate a table, giving in one column the actual weight of the pail when filled with moist sulphate, and opposite in a second column, the amount of dry sulphate corresponding to the gross weight. The weight of dry sulphate was thus found as accurately as could be desired, although the amounts varied in practice from 30 to 105 pounds.

It is nothing but an application of the Archimedean theorem, that, when a solid body is immersed in a liquid it loses a portion of its weight, equal to the weight of the fluid which it displaces, or to the weight of its own bulk of the liquid.

This, as I suppose, is precisely the principle applied by Mr. Mène. *The precipitate he obtains by a certain chemical manipulation is a substance of known composition and specific gravity.* Supposing it to be sulphate of lead, and the bottle, when filled with water at the normal temperature, to weigh 70 grammes =50 grammes of water, and 20 for tare. After introducing the precipitate and filling again with water it weighed 71·06 grammes. Now, as the specific gravity of sulphate of lead is 6·3, or as the weight of a cubic measure of sulphate of lead is 6·3 times that of a cubic measure of water, and as the space of one part by weight of water is taken up by 6·3 parts by weight of sulphate of lead, it follows that the quantity of sulphate of lead in the bottle, which has taken up the space of one part by weight of water, increases the original weight of the bottle (filled with pure water) by 5·3. To find the amount of water displaced it is only necessary to divide the overweight (1·06 grammes) by 5·3=0·2, which, added to the overweight 1·06+0·2 gives 1·26 grammes as the weight of the precipitate.

Hence the rule, which is of great convenience in volumetric analysis, that *to find the weight of a moist precipitate, which is a compound of known specific gravity, weigh it in a specific gravity bottle or some other vessel of known weight when filled with water, or any other liquid, at the normal temperature, again fill it with the water or other liquid, divide the excess of the new weight by the specific gravity of the substance, less that of the water or other liquid (that of water being =1) and add the quotient to the overweight, which gives the weight of the precipitate.*

The editor of the Jahresbericht appears to have overlooked the fact that the precipitates weighed in this manner are definite compounds, the specific gravity of which is well ascertained.

The principle I have exemplified above may not be novel; but as I have never met with it, chemists, as well as manufacturers (especially of colors), will probably also find it of interest, and certainly highly practicable and easy of execution.

36 Beekman street, New York, Feb. 3d, 1860.

3. *New Chemical Journal.*—The Chemical News (with which is incorporated the Chemical Gazette), edited by WILLIAM CROOKES. London: Weekly. Price 3*d.*, stamped 4*d.* 8vo. 12 p. each number. This new Journal commenced on the 10th of December last, and eight numbers have already reached us. The contents are divided under Scientific and Analytical Chemistry, Technical Chemistry, Pharmacy, Toxicology, &c., Proceedings of Societies, Notices of Patents, Correspondence, Scientific Notes and Queries, Laboratory Memoranda, Miscellanies, and Answers to Correspondents. Mr. Crookes is favorably known by several valuable researches, and thus far has shown good judgment and spirit as an editor. His verbatim reports of the late lectures of Dr. Faraday (the Holiday lectures) at the Royal Institution, attest his appreciation of the true sources of vitality for such a journal.

4. *American Druggists' Circular and Chemical Gazette;* N. Y. Feb. 1860. 4to.—Although chiefly special and wholly technical in its objects, this Journal (which has now reached its 4th volume, whole number 38) is conducted by Mr. Mayer and others, in a manner to entitle it to rank as a valuable coadjutor in technical chemistry.

II. GEOLOGY.

1. *On some of the Igneous Rocks of Canada;* by T. STERRY HUNT, F.R.S. (In a letter to one of the editors, dated Jan. 1860.)—There occurs in the district of Montreal a series of isolated hills running nearly east and west for a distance of ninety miles along the line of an undulation which has disturbed the lower Silurian strata. These hills, which often cover considerable areas, consist of igneous rocks which have apparently been solidified under a considerable pressure, and have subsequently been exposed by the denuding action which has removed from around them the soft and unaltered palæozoic strata. The names of these mountains counting from the west are Rigaud, Mount Royal, Montarville, Belœil, Rougemont, Yamaska, Shefford and Brome, to which we may add Monnoir a similar mass lying somewhat to the south of Belœil.

I am now engaged in the study of the various rocks composing these mountains, which offer great diversities in lithological character and composition. Prominent among them we may mention the trachytes, which in their various types of compact, granular, porphyritic and granitoid are abundant. The mountains of Brome and Shefford appear to be made up entirely of a granitoid trachyte, which consists of crystalline orthoclase, without quartz, and with small portions of hornblende or mica, sphene and magnetite. The orthoclase in a great number of these rocks which I have analyzed contains like sanidin a large proportion of soda. The other varieties of trachyte which occur in veins and dykes often contain a portion of carbonates amounting to from 6·0 to 18·0 p. c. and consisting chiefly of carbonate of lime with some magnesia and iron. Some of these rocks pass into phonolites through the admixture of a silicate which gelatinizes with acids and has the composition of natrolite. This mineral in one case amounted to more than 40·0 p. c. of the rock, the remainder being orthoclase with a small amount of carbonates.

A large part of the mountain of Yamaska consists of a coarsely crystalline diorite the feldspar of which approaches anorthite in composition, while an apparently similar diorite, which makes up the mass of Monnoir, contains oligoclase in large crystals. Other diorites from this series contain labradorite; mica and sphene, in small quantities, are often present.

Dolerites are also abundant, and sometimes pass, owing to a scarcity of feldspar, into an augite rock, generally with ilmenite and magnetite. A fine-grained dolerite from Rougemont contains abundance of crystallized olivine, and a large part of Montarvilles consists of a remarkable granitoid rock, made up of a crystalline feldspar, in some parts at least labradorite, with sparsely disseminated crystals of black augite, a little brown mica, and a great abundance of crystals of honey-yellow olivine, which amount to more than 45·0 p. c. of the mass. The composition of this olivine I have found to be silica 37·17, magnesia 39·68, protoxyd of iron 22·54 = 99·39.

Many of these diorites and dolerites, except in their lithological structure, closely resemble the stratified rocks made up of anorthic feldspar, with hornblende and pyroxene, and containing magnetite and ilmenite, which are so abundant in the Laurentian system, suggesting the notion that the intrusive masses may be nothing more than these stratified rocks displaced and injected among the Palæozoic strata. Durocher has already pointed out a similar resemblance between the intrusive rocks of some parts of Scandinavia, and the subjacent gneiss.—(*Bul. Soc. Geol. France*, [2] vi, 33.)

The granitoid trachytes as well as the dolerites, diorites and peridotite (olivenite rock) make up mountain masses, while the earthy and porphyritic trachytes and the phonolites are generally found cutting the above, and the adjacent strata. The absence of quartz or of any excess of silica from all these rocks is a remarkable feature. Farther to the east, however, intrusive granites are very abundant; these penetrate the Devonian strata but are older than the carboniferous. Quartziferous plutonic rocks are also abundant in the county of Grenville, where they penetrate the Laurentian series. These plutonic rocks consist of dolerites, syenites and eurites, which are in their turn cut by dykes of very beautiful porphyries. The base of these is jasper-like, black, red or green in color, and encloses crystals of red orthoclase and occasional grains of quartz. The analysis of the base shows it to consist of the elements of orthoclase with an excess of silica and a little oxyd of iron. The syenites are cut by large veins of chert, and in the vicinity of these have been changed into a sort of kaolin from a decomposition of the feldspar, which may have been the source of the silicious accumulations. This group of igneous rocks, which is overlaid by the Potsdam sandstone is very unlike those which we find penetrating the palæozoic strata.

The details of my investigations on these rocks, so far as completed, will be found in the Report of the Geological survey of Canada, for the last year, now in press; a first portion has already appeared in the Report for 1853–56, p. 485. It is by the systematic study of different series of igneous rocks that we may hope to arrive at just notions as to their origin, their mode of formation and their relations to metamorphic sedimentary rocks.

2. *Notes on the Dolomites of the Paris Basin, etc.*; by T. STERRY HUNT, F.R.S. (In a letter to one of the Editors, dated Montreal, Feb. 3, 1860.)—The gypsums of the Paris basin are evidently not of epigenic origin but regularly stratified and alternating with marls and limestones. In September, 1855, I visited with Elie de Beaumont and several members of the Geological Society of France, the gypsum quarries at the hill of Chaumont, and there insisted upon the views which I have since urged in this Journal (vol. xxviii, p. 365) upon the different origins of gypsum. For a report of my remarks on that occasion see the *Bulletin de la Société Geologique de France*, [2], xii, 1306.

I have subsequently shown in the memoir just cited in the last volume of this Journal that the formation of these stratified gypsums by the double decomposition of bicarbonate of lime and sulphate of magnesia involves the production of carbonate of magnesia, which unless carried away or decomposed by an irruption of sea water will be found to overlie the gypsums forming the dolomite which is their common associate. The presence of carbonate of magnesia in the gypsiferous series of the Paris basin has hitherto been unnoticed, and having at the time above mentioned collected specimens from the quarries at Chaumont I was recently induced to examine the so-called *white marls* which overlie the gypsiferous series, and find them to be magnesian. The analyses of two specimens, one penetrated by seams of gypsum, gave each about 60·0 per cent of dolomite mingled with clay. The Paris gypsums then offer no exception to the general rule.

Beneath the gypsiferous series and in the lacustrine group known as the lower travertine, or St. Owen limestone, occur beds of a whitish, very fissile, shaly matter, enclosing concretions of menilite (opal), and consisting of a hydrated silicate of magnesia, identical with meerschaum or quincite in composition, intermingled with small portions of earthy carbonates. I have examined a specimen of this mineral which I collected near Paris and find it to be the same with that described by Dufrénoy and Berthier as occurring in similar positions in various other localities. This appearance of beds of a silicate of magnesia approaching talc in composition, in the midst of unaltered deposits, is interesting inasmuch as it seems to show that such silicates may be formed in basins at the earth's surface by the reactions between magnesian solutions and dissolved silica. I have many years since described the existence of similar silicates among the deposits during the artificial evaporations of natural alkaline waters, and farther inquiries in this direction may show us to what extent certain rocks consisting of calcareous and magnesian silicates may be directly formed in the moist way.

I propose to send you very soon a supplement to my paper on Gypsums and Magnesian rocks, describing some recent experiments which confirm what I have already announced that no dolomite is formed in the experiment of Von Morlot and Haidinger, recently resuscitated by Charles Deville, and appealed to by Prof. Phillips in his last Annual Address to the Geological Society of London, as resolving the problem of the origin of dolomite. This double salt is however readily formed when a mixture of the moist amorphous carbonates (such as is obtained by precipitating in the cold by an excess of carbonate of soda a solution of the chlorids of calcium and magnesium in equivalent proportions), is gradually heated under pressure.

3. *New Palæozic Fossils;* by J. H. McCHESNEY. Chicago, 1859. 8vo. pp. 64. In this publication the following species from the Carboniferous rocks of the Western States are noticed as newly described:

CRINOIDEA.—*Platycrinus ornogranulus, P. inornatus, Scaphiocrinus longidactylus, Zeacrinus bifurcatus, Z. mucrospinus, Actinocrinus asterius, A. tenuisculptus, A. subæqualis, A. Fosteri, A. subventricosus, A. urnæformis, A. Hardianus, A. æquibrachiatus, A. Andrewsianus, A. Hageri, Forbesiocrinus Pratteni.*

BRACHIOPODA.—*Orthis, Kaskaskiensis, O. Lasallensis, O. Pratteni, O. Richmonda, Productus asperus, P. symmetricus, P. Wilberanus, P. tubulospinus, P. fasciculatus, P. inflatus, P. pileiformis, Ambocelia gemmula, Spirifer transversa, S. subelliptica, S. perplexea, S. subventricosa, Retzia subglobosa, Athyris spiriferoides, A. orbicularis, A. differentius, Terebratula inornata, Rhynchonella Eatoniæformis, R. explanata, R. carbonaria, R. Algeri, Trematospira Mathewsoni, Discina caputiformia.*

LAMELLIBRANCHIATA.—*Leda Oweni, L. gibbosa, L. polita, Nucula parva, N. cylindricus, N. rectangula, Asturtella varica, Edmondia concentrica, Allorisma clavata, A. sinuata, Myalina Swallovi, Nuculites Vaseyana, Pinna Adamsi, Syringopora multattenuata, Cyathoxona prolifera.*

GASTEROPODA.—*Bellerophon ellipticus, B. vittatus, B. Blanyana, B. Stevensiana, Pleurotomaria Beckwithana, P. nodomarginata, Natica Shumardi, Platyceras crytolites, Platyostoma Peoriensis, Bucania Chicagoensis.*

CEPHALOPODA.—*Nautilus Forbesianus, N. Illinoiensis, N. quadrangulus, N. nodocarinatus, Gonitites Hathawana, Cyrtoceras (Lituites?) giganteum, Trochoceras Desplaniensis, Orthoceras Rushensis, O. Knoxensis.*

The typography of this brochure is very good, and the descriptions of the species show that the author has a wide acquaintance with the Carboniferous fauna of the West. We think, however, that the science would present to the student a much less formidable array of difficulties, if Palæontologists would on all occasions give accurate measurements of the species they describe. Throughout this book, for instance, the size of the individual is recognized as a specific character, and yet as no dimensions are given it must be impossible for even the most experienced practical naturalist to decide whether *O. Lasallensis, O. Richmonda* and *O. Pratteni* are small or large forms; whether two lines or two inches wide. The first mentioned of these species is also said to have the "*surface marked by sharp rugose radiating striæ increased by implantation,*" but so have a great many other *Orthides*, and until some standard for comparison is furnished, all the students of this book, except those who may have access to the original specimens, must remain in doubt upon the question as to whether there are ten or twenty striæ in the width of one line. We find it stated further that in *O. Richmonda* the striæ are "finer and less rugose" than they are in *O. Lasallensis* while in *O. Pratteni* they "are not so distant and not so rough," as in either of the other two, but as we have no means of ascertaining their size in the first named species, which is here made the standard, all that relates to the surface characters of the second and third might have been left out without detracting anything from the value of the description. No doubt all this appeared to be sufficiently clear to the author while he was engaged with the specimens before him, but we think upon a little reflection he will agree with us, that without some clue to the size and surface characters of these species, they cannot be identified. Where a shell is said to be smaller or larger than some other species which has been described in some other book, the difficulty would be somewhat less, provided that access could be had to that book, and even then, such questions as "how much smaller?" or "how much larger?" cannot be decided. By adding two or three lines to their descriptions, palæontologists may add a vast deal to the value of their labors, and save others engaged in the same pursuits much perplexing, and too often fruitless, intellectual toil. It is not always possible to give figures of new species, but it is easy to furnish measurements. The absence of these is the great defect in the work before us, and it is a defect that may be observed in books of much greater pretensions. We notice a new genus, (AMBOCELIA) among the Brachiopoda, which is said to have been "recently established by Prof. James Hall," in the "Regents' Report of the State of New York for the present year," (meaning 1859). Has this report been pnblished? If not, then the genus is not established. No author can establish a genus before he has published his description, and, even then, he may not succeed in shewing that it is new.

The practice of antedating genera and species should be discountenanced, as it cannot be beneficial to science. Where specific names are derived from names of persons or places, the initial should be a capital letter, a rule which has been of late much disregarded.

4. *Explorations in Nebraska.*—Dr. F. V. HAYDEN, in a letter to Prof. DANA, dated Deer Creek, Nebraska, Dec. 1, 1859, states, after speaking of the interest of the region, that as soon as the grass is sufficiently high this coming Spring, and the swollen streams permit the passage of pack trains (which cannot be sooner than the first of June), Capt. Reynolds proposes to divide his party into two divisions—one party in charge of Lieutenant Maynadier, Asst., will pass up the Wind River Valley along the western side of the Big Horn Mountains. The other division under Capt. Reynolds will proceed up the Platte to the South Pass, exploring the Wind River Mountains—the two parties to spend the 4th of July at Clark's Pass. Here Lt. M.'s party will proceed down the Yellowstone to Fort Union, and Capt. R.'s party will reach the same point by crossing at the head waters of the Missouri and descending that stream, and thence to the States as soon as possible. Such is the programme of explorations; it may be slightly varied, but in any case will traverse a country very interesting for geology.

Dr. Hayden, in addition to his geological labors, has since 1852 interested himself in ethnographical studies, and will have by the end of this trip material enough for a large volume on this subject.

5. *Geological Surveys of South Carolina and Kentucky.—The geological survey of South Carolina* has unfortunately been stopped by the unwise action of the legislature of that State at its last session. Dr. O. M. Lieber, late the State Geologist, is occupied in preparing his final report, which will embrace Anderson and Abbeville district, and a part of Edgerfield. Dorn's gold mine is described in it.

The geological survey of Kentucky is continued with unabated zeal.* We have received the Synoptical Report for the past year—a brochure of 50 pages—by Dr. D. D. Owen, principal, aided by S. S. Lyon and Jos. Lesley, Jr., Topographical Assistants, Leo Lesquereux, Palæontological Assistant, and Dr. Robert Peter, Chemist. Dr. Owen here expresses the opinion that "the report of Mr. Lesquereux of the last season's work (now completed), is by far the most practically useful geological report on this subject (coal), which has ever appeared, not only in the United States but in any part of Europe."

In his synopsis of this forthcoming report, Mr. Lesquereux says, "The third section of the report contains a short comparison of the distribution, geologically and geographically, of the coal strata in Kentucky, Ohio and Pennsylvania. This comparison is of high scientific interest, as it fixes the general distribution of the coal strata in the whole extent of the coal basins of the United States, and cannot but give to the geological reports of Kentucky a great value as containing the key of the general distribution of the coal. Henceforth all the reports treating of the distribution of coal strata will naturally take their guide and standard of comparison from the section in the Kentucky coal-fields."

6. *First report of Progress of the Geological and Agricultural Survey of Texas;* by B. F. SHUMARD, M.D., State Geologist. Austin, Texas, 1859. pp. 17.—We learn from this brochure that satisfactory progress has been made in the preliminary reconnaissance of the vast territory (237,500 square miles) included within the State of Texas. Dr. Shumard states that a more complete series of the geological formations exists in Texas than in any State of the Union, ranging from the Potsdam sandstone to the latest Tertiary. He has made no less than *seven* lines of section extending in various directions a collective distance of 1220 miles, determining his levels by the barometer. He has found time also for minute and final surveys of eleven counties and partial surveys of several others. The coal measures cover an area not less than four thousand or five thousand square miles, with a thickness of eight or nine feet of coal in about 300 feet of coal rocks. The coal is good in quality. Extensive beds of brown coal also occur in the Tertiary rocks in the eastern and middle portions of the State.

The fossil forms of the various strata in Texas are very abundant, and Dr. Shumard informs us that his collection is already very rich.

From the head waters of the Brazos river he has been fortunate enough to obtain a fine mass of meteoric iron weighing about 320 pounds, and a smaller mass of the same kind from Denton county, specimens of which have reached us.

* Notwithstanding the cramped appropriation of money for the two years past—two-thirds of the whole sum being consumed by expenses of publication, which is an absurdity unworthy of the spirited state of Kentucky.

7. *Post-pleiocene Fossils of South Carolina;* by FRANCIS S. HOLMES, A.M., &c. Nos. 6 to 10 inclusive, containing plates 11 to 20 inclusive. Quarto. Charleston, S. C., 1859; Russell & Ivens.—This beautiful monograph, previously noticed,* is continued in the same excellent manner in the numbers now received. The description and figures of the mollusca end with the part containing Nos. 6 and 7, and with Nos. 8, 9, and 10 commences a "Description of Vertebrate Fossils, by Prof. Jos. LEIDY." The student will refer to this memoir with particular interest at the present moment, when so much attention is being given to the occurrence of human reliquiæ with the remains of animals heretofore judged to be extinct before the human epoch. The Eocene and Post-pleiocene beds on the Ashley River are exposed to the wash of the water, and "the fossils washed from them form part of the shingle on the shore, and here become mingled with the remains of recent indigenous and domestic animals, together with objects of human art." Of those vertebrate remains actually obtained in excavations of the Post-pleiocene and Eocene formations, more confidence is felt in determining the actual age to which they belong. Both the collections submitted to Dr. Leidy by Prof. Holmes and Capt. Bowman, contain remains of the horse, ox, sheep, hog and dog, which we feel strongly persuaded, with the exception of many of those of the first mentioned genus, are of recent date, and have become intermingled with the true fossils of the Post-pleiocene and Eocene periods on the Ashley River and its tributaries. In regard to the remains of the horse from the facts related (in this memoir) we think it must be conceded that several species of this animal inhabited the country of the United States during the Post-pleiocene period, contemporaneously with the mastodon, the giant sloth, and the great broad-footed bison."—pp. 99–100.

8. *Assiniboine and Saskatchewan Exploring Expedition;* by HENRY YOULE HIND, M.A., Prof. of Chemistry and Geology in Univ. of Trinity Coll., Toronto. Toronto, 1859. 4to, pp. 202, with many maps, sections and plates.—This valuable Report comes to hand at too late an hour to enable us to do more than give its title, deferring to our next an analysis of its contents.

9. *Geology for Teachers, Classes, and Private Students;* by SANBORN TENNEY, A.M., Lecturer on Physical Geography and Natural History in the Mass. Teachers' Institutes. 12mo, pp. 311. Philadelphia, 1860.—As the scope of this little work comprises the whole range of geological phenomena, it gives necessarily a very concise account of the various departments included. The matter however seems in most cases well selected, and shows the author to be acquainted with his subject. The book is illustrated by some 200 good wood-cuts, and has one excellent feature;—that the illustrations are taken, whenever possible, from *American* objects, which cannot be said of most geological text-books previously published in this country, the authors of which have generally gone to Europe for their examples. American geology should be the main subject of American text-books, and it is pleasant to see a step taken in the right direction. Our author however is not quite up to date in some instances, as in ignoring our Permian and western Jurassic beds; in including the Lias in the Oölitic system, etc. W. S.

* This Journal, xxvii. 156.

III. ZOOLOGY.

1. *On Botanical and Zoological Nomenclature;* by Wm. Stimpson.—A more careful attention to the subject of nomenclature is urgently demanded of the followers of all branches of Natural History. It is a subject to which too little attention has been paid in an abstract or general sense, and too much perhaps in particular cases. A comprehensive code of rules, recognised by the authority of the greater lights of science, has been always needed. This was attempted during the last century by Linnæus and Illiger, and in 1842 "Rules of Nomenclature" were drawn up by the British Association, and ratified by the American Association in 1845. These are excellent as far as they go, but need much extension and many additions, as any one may observe who attempts to decide by them all questions which occur in his experience.

On the other hand, in particular cases of species and genera, the discussion of questions of nomenclature has reached such a pitch that it is no uncommon thing to see the greater part of a new zoological work devoted to synonymy. One author, after six pages of historical and synonymical matter, evincing great critical acumen and much bibliographical research, will arrive at what appears to him to be a certain and final conclusion that the true *Orthonymus aliquis* is such and such a species. The next writer who succeeds him in the same field will triumphantly prove in *ten* pages that it is not that species at all, but the *O. neminis.* And so on to the end of the chapter, if it ever will have an end, which is doubtful unless some decided action is soon taken by naturalists for the purging of their favorite science from this opprobrium. After all the pages which have been written upon some of these cases we seem no nearer to a settlement than at first. The difficulty increases rather than diminishes,—each succeeding author putting forth views differing from those of his predecessors. All this discussion, let us bear in mind, is merely preliminary, and for the purpose of indicating with certainty an object about which the author has perhaps not a dozen words to say.

Now it may appear at the first glance that the application of the law of priority is exceedingly simple. The name given by the first describer of a genus or species is to be respected, and applied to that genus or species throughout all time. But as soon as we come to apply this rule, we find cases without number in which complications occur, rendering limitations of the law necessary. Genera are to be subdivided, and are subdivided with different limits by different authors; the species of one are found by another to include two or three distinct forms, and so on. Some of the limitations of the law of priority have been laid down in the "Rules" of the British Association, but not enough to enable us to decide half the cases which may arise,—leaving the remainder subject to the whims or dependent upon the extent of the knowledge of the author who would follow them.

In applying the great law, the most difficult question of all immediately arises,—What constitutes a description? or, When has an author so designated his species that his name for it should hold? On this subject we have every variety of opinion, from that of the German ornithologists, who consider that a simple published name, referring to a specimen in a museum, is sufficient, to that of the lamented Edward Forbes, who once insisted that no name proposed should be accepted unless accompanied by a *Latin* description or an illustrative figure. The first opinion we believe to be scouted by nine-tenths of living naturalists;—the second appears to be too stringent, as an author can of course write better in his own language than in any other, though we doubt if a description appearing in Chinese would gain the least notice from modern naturalists.

The question, "What constitutes a description," can never be decidedly answered. No rule can be proposed which is universally applicable. With regard to its *length;*—we may say that two words are not sufficient, an hundred are; but where shall we draw the line? The two sentences of one author may be better than the two pages of another. One writer will describe an object well except in one point, in which from defective observation, a character is represented in exact opposition to the true state of the case. Some descriptions are sufficient to enable the naturalists of one country, from their collateral knowledge, to determine a species, while those of another country or continent would be left entirely in the dark. An author may publish descriptions in a work for private distribution, which will be inaccessible to the great body of naturalists. We might fill many pages with such cases as these, and yet, were rules made out applicable to each, there would still be cases constantly arising which could be decided by none of them. How then can the matter be settled in these latter instances? We will suggest a method further on.

It will be observed that it is among the more common and earliest-described species that the synonymic heap is greatest. This is exceedingly embarrassing to the student, who in general has occasion to use these very species, being those most easily accessible, in the course of his studies. He may find in a dozen different books the characters, anatomical or otherwise, of what appear to him a dozen different objects, since the names used may be different, and elementary works cannot be expected to go into synonymical details. At the present day, thanks to the advance of knowledge and precision, and the international exchange of scientific works, the name of an entirely new genus or species may escape the burden to which that of older species is subjected. It is with those published in the last century that the greatest trouble occurs. Investigators among antique and forgotten books are constantly finding some obscure work or paper, perhaps scarcely known out of its immediate vicinity even at the time it was published, in which names occur which must be adopted, in the opinion of some, to the exclusion of the familiar titles which have been used for half a century. The disinterment of Klein's name *Cyclas* is an instance of this. How strange it must seem to a conchologist of the present day to be obliged to designate the common marine *Lucina* by a name which has been in use seventy years for a freshwater bivalve, while this freshwater bivalve becomes *Sphærium;* and to use *Cyclostoma* for *Delphinula*, *Terebellum* for *Turritella*, etc. The restoration by G. R. Gray of Boddaert's names in ornithology is another instance. By the discovery of a meagre pamphlet of the eighteenth century, only two or three copies of which now exist, we find ourselves forced to change the generic names of common birds, familiar as they are by long and constant usage.

In the discussion of these questions all personal considerations should be entirely rejected. The smallest interest or convenience to the science in general, followed as it is by a republic of thousands, is of more importance than any compliment to the feelings of a living, or the memory of a deceased naturalist. In fact our mere recognition of an author's names is not of such vast importance to his reputation. His fame must rest upon a securer foundation than this. For the custom of placing the name of an author after a species described by him is not (or should not be) done for that author's personal advantage, but simply to assist us in the recognition of that species. It is a short method of referring to the place where the description of the species may be found, or enables us to distinguish it from some other to which the same name has been by mistake applied; as, *Pleurotoma violacea*, Hinds, *non* Mighels. In this view, how ludicrous it appears, to hear, as we often do, naturalists complain that if the custom of placing after a species the name of that author who first placed it in its proper genus is adhered to, more than one-half of Linné's species will be wrested from him. Does the fame of the great Linnæus depend upon the number of species he described?

We will now mention a few points concerning which great difference of opinion exists in the minds of naturalists, and which for the good of science should be immediately settled in one way or the other. The first is: shall the same generic name be allowed to occur in different departments of zoology or botany, or even in both these, or, we may add, in other sciences. Many are of the opinion that they may be used, and should not be changed, if so occurring;—in view of the great difficulty now experienced in selecting a name which is not preoccupied, and shall be at the same time descriptive or suggestive of the object intended. But what is the object of a name? Surely, the *main* object is to enable us to distinguish one thing from another, and from all others, that when it is used we may know what is intended, and not be forced to decide by other aids. Is it not of vastly more importance that a name should serve this purpose, than that it should remotely indicate (which is the most generally possible) some character of the object, which it may after all hold in common with an hundred others? Greek compounds are by no means exhausted yet, and if they were, we might fall back upon euphonic names, which serve the purpose however barbarous they may appear in the eyes of some. The custom of using the same name for many diverse objects is productive of serious inconveniences. If we have stars, countries, minerals, plants, vertebrates, articulates, mollusks and radiates, all named alike, some singular anomalies might occur, since we can of course reduplicate *specific* appellations as often as we please in different genera. For instance, suppose a travelling naturalist "making his researches in Arizona, observed specimens of the *Arizona pætula* (hermit-crab) inhabiting the shell of *Arizona pætula* (univalve), creeping among the roots of *Arizona pætula* (shrub); and upon examining it anatomically, found great numbers of the *Arizona pætula* (infusorium) living in its gills. The *Arizona pætula* (bird) was feeding upon these crabs with great voracity," etc.

Another point. A genus may contain a vast number of species, and yet from want of profound investigations no one may see the propriety of dividing it up. As occurs very commonly, in the course of time some new species belonging to it are described under names, which, being preoccupied in that genus, are very properly changed. The new designations become established and may be used for years. At last it becomes necessary to divide the genus, and the species whose names have been referred to are found to belong to different genera. Shall the old reduplicated specific name, or the substituted one be now adhered to? Naturalists are about equally divided in opinion upon this point.

The propriety of using small initial letters to proper specific names, nouns or adjectives, has been made the subject of discussion. Whatever method be followed here, it would seem that uniformity is desirable; if any of these proper names are to have small initials, why not all? Most zoologists and botanists seem in this matter to follow the usage of their own language rather than that of the Latin, or any uniform system. The Germans will have all nouns begin with a capital, and all adjectives with a small letter, as *Ocypode Cursor*, *Chiton emersonianus*, whereas the English write common nouns with a small initial, and all *proper* appellations, whether nouns or adjectives, with a capital, as *Ocypode cursor*, *Chiton Emersonianus*. The truly convenient system will be to write all specific names without exception, with a small initial letter, as is done by one of the most eminent zoologists of this country, and by many of those of Europe. We shall then have no difficulty in distinguishing specific from generic names, and may discuss the relations of species without the necessity of repeating the generic name or its initial every time they are mentioned. A proper name, modified for use as a specific appellation, becomes a part of a new title, and involves a different idea.

We will not detain the reader by discussing other mooted points, as whether ante-Linnæan names shall be accepted, if binomial; whether names of faulty etymology shall be corrected, etc. The above are only mentioned as instances of the necessity of establishing many rules to produce uniformity of usage among naturalists. In pointing out how this may be satisfactorily done, we proceed to our promised suggestion.

We have somewhere read, that when any orthographical or other difficulty occurs in the use of the French language, the case is immediately referred,—in accordance with the admirable *system* for which the nation are remarkable both in science and literature,—to the Academy, who decide upon it, arbitrarily it may be, but finally. The action of this tribunal is respected, and no farther uncertainty or diversity in the use or spelling of the word can occur to embarrass French authors. Now why may not a similar mode of action be of use in science, and enable us at last to settle all our difficulties. Science is cosmopolitan, not national. Let a convention meet at Paris or some other central point, composed of delegates from all the scientific societies of the earth, and representing at least all the departments of zoology and botany. Here they may hold sessions of the entire body, for a sufficient length of time to establish all the general rules of nomenclature which can be conveniently applied. But as we have before observed, there are some particular cases for which no rule will serve, and which must in some way be decided separately and *arbitrarily*. For the settlement of these cases let the convention divide itself into as many sections or committees as there are classes of plants and animals; a committee of ornithologists for the birds, of entomologists for the insects, etc. These committees being composed of experts in the various branches, will not find it difficult to discuss and pass judgment by vote upon the name of each contested genus or species. Let these decisions be respected, and let the names of those who will not abide by them be placed upon a new edition of that black-book which LINNÆUS kept of old,—the list of *Damnati!*

2. *Les genres Loriope et Peltogaster*, H. Rathke; par W. LILJEBORG, Professeur de Zoologie à Upsala en Svède. pp. 35, 4to, pl. 3. (Extr. des Nova Acta Reg. Soc. Sc. Upsal, Ser. 3, Vol. iii.)—M. Liljeborg has unraveled a singular history in relation to the curious sac-like parasites found on the abdomens of Decapod crustaceans. They were first observed by Cavolini, and in more recent times by Rathke, who considered them to be worms, probably Entozoa, and instituted the genus *Peltogaster* for their reception, describing two species, *P. paguri* and *P. carcini.* Diesing placed them among his Myzelmintha, and formed a new genus, *Pachybdella*, for *P. carcini.* These forms were made the subject of discussion by Kroyer, Steenstrup, O. Schmidt, and Lindstrom, who agreed with Cavolini in referring them to the Crustacea, but could only conjecture their more intimate relations, although suggesting those with the Entomostraca, the Lerneidæ, or the Bopyridæ. From a study of their larvæ M. Liljeborg now ascertains their true place to be among the Cirripedes, and describes two new species.

Within the body of *Peltogaster paguri*, Rathke found a minute Tetradecapod, scarcely a line in length, which he considered to have been swallowed as food by that animal, and described it as an Amphipod under the name of *Loriope.* It was afterward referred to the Tanaidæ by Dana, who described a new allied genus *Cryptothir.* Its history however remained very obscure, although it was demonstrated that, being always found alive, it was not the food of the *Peltogaster*, and some naturalists even suspected it to be the male of that parasite. By a fortunate discovery M. Liljeborg has now cleared up the difficulty. In examining a *Peltogaster* taken from the abdomen of *Pagurus pubescens*, he found attached to it another sac-like body filled with Loriopes, which might well have been taken for the egg-pouch of the *Peltogaster* itself, but which after careful study proved to be a distinct animal,—a parasite upon a parasite! It proved, in fact, to be the female of the *Loriope*, grown monstrous by a process of degradation similar to that observed in the female of the *Bopyridæ*, to which family indeed the *Loriope* must now be referred. The occurrence of the young Loriopes in the digestive cavity of *Peltogaster* is, then, simply adventitious.

3. *Neue Wirbellose Thiere, beobachtet und gesammelt auf einer Reise um die Erde*, 1853 bis 1857, von LUDWIG K. SCHMARDA; 1st Band, 1st Hälfte, 4to, pp. 66, and 15 colored plates. Leipzig, 1859. (New York, B. Westermann & Co.)—A quarto volume, handsomely bound and splendidly illustrated, containing descriptions of the Turbellaria and Rotatoria collected and observed by Dr. Schmarda during a voyage round the world. The descriptions of the animals belonging to the former order are made additionally clear by woodcuts showing the shape of the head and the distribution of the ocelli. A review is given of the genera of

each order. The Dendrocœla are divided into two divisions, *Acarena* and *Carenota*, the former having no distinct head, which those of the latter group possess. We should judge this to be a character of much less importance than those derived from anatomical characters, which forbid such a division. Following it, the author separates *Planaria gonocephala* from the other freshwater Planariæ and places it with such forms as *Cephalolepta* and *Planaria oceanica*, Darwin! There is also a large number of genera hitherto considered sufficiently well established, which are entirely ignored by M. Schmarda, as *Prosthiostomum*, *Dendrocœlum*, *Procerodes*, *Fovia*, *Bdelloura*, *Geoplana*, and *Rhynchodemus*. The new genera of Dendrocœla are *Dicelis*, *Prostheceræus*, *Homaloceræus*, *Goniocarena*, *Carenoceræus*, and *Sphyrocephalus*. Of these several have been previously established under different names. *Prostheceræus* may be adopted, as Quatrefages' name *Proceros* is preoccupied. *Goniocarena* is *Dugesia* Girard, *Carenoceræus* is *Nautiloplana* Stm., and *Sphyrocephalus* is *Bipalium*.

The Nemertinea are subdivided upon more certain grounds than the preceding order, but we are at a loss to understand why the proboscis should not be considered as the mouth, as it is certainly the aperture through which food is introduced into the body. Oken's name *Borlasia* is adopted (although it is exactly synonymous with *Lineus*, Sow.) and made to include *Amphiporus*, *Acrostomum* and *Baseodiscus* of Diesing. A new genus, *Loxorrhochma*, is established for the *Polia coronata* of Quatrefages. The figures are excellent, showing well the colors, which among these animals form the most reliable specific characters, and notwithstanding errors of nomenclature and arrangement, we have to thank M. Schmarda for an exceedingly interesting and beautiful work. W. S.

4. *A Supplement to the "Terrestrial Air-breathing Mollusks of the United States and the Adjacent Territories of North America;"* by W. G. Binney. Boston, 1859. 8vo, pp. 207, and 6 colored plates. (Extracted from the Journal of the Boston Society of Natural History.)—This much needed supplement to Dr. Binney's great work contains the additions and corrections which have accumulated in the rapid progress of the science during the seven years which have elapsed since the publication of that work. Several doubtful species which were omitted in that work have since been investigated, and are now included or referred to their proper place in the synonymy. Besides these, we have figures and descriptions of all the recently discovered species, of which the number is large, particularly among those of the Pacific coast. These are placed together in the first part of the volume. The writings of foreign authors upon our land-snails are properly discussed, and their descriptions reprinted in full wherever there was any doubt as to the species to which they should be referred. Thus the whole subject is thoroughly posted up to the date of Jan. 1st, 1859, and the work forms a most acceptable contribution to American conchology. W. S.

5. *Catalogue of the Recent Marine Shells found on the Coasts of North and South Carolina;* by J. D. Kurtz. 8vo, pp. 10. Portland, Me., 1860.—This catalogue shows the results of the author's researches in the conchological fauna of our southern coast made in the years 1848–52. The number of species given is 204, an increase of 78 over that given

by Prof. Gibbes in his catalogue published in Tuomey's "Geology of South Carolina," published in 1848. Several species are mentioned as occurring on the marl-bottom off the N. C. coast which have not been hitherto observed north of Florida. Four new species are briefly described, viz., *Venus trapezoidalis*, (which is perhaps the same as *V. pygmæa*,) *Arca Holmesii*, *Scalaria rupicola* and *Chemnitzia textilis*; and several new ones are mentioned by name only. Capt. Kurtz has also contributed very largely to our knowledge of the marine animals of the same coasts, in other departments, as our zoological archives abundantly show. W. S.

PROCEEDINGS PHILADELPHIA ACAD. NAT. SCI., 1859.—p. 281, Resolutions on the death of Thomas Nuttall.—A new Unio from the Isthmus of Darien; *I. Lea.*—Additions to the Coleopterous Fauna of Northern California and Oregon; *J. L. LeConte.*—p. 294, Notes and Descriptions of Foreign Reptiles; *E. D. Cope.*—p. 297, A new Myalina and Posidonia from the Carboniferous of Texas; *W. M. Gabb.*—p. 297, New birds from Cape St. Lucas; *J. Xantus.*—p. 299, Notes on Birds collected at Cape St. Lucas by Mr. John Xantus; *S. F. Baird.*—p. 306, Mineralogical Notices; *W. J. Taylor.*—p. 310, New Histeridæ; *J. Leconte.*—p. 317, Contributions to American Lepidopterology, No. 2; *B. Clemens.*—p. 329, On the soft parts of certain Unionidæ; *I. Lea.*—p. 331, Descriptions of three new species of exotic Uniones; *I. Lea.*—Notice of Shells collected by Xantus at Cape St. Lucas: *P. P. Carpenter.*—p. 332, Catalogue of the Venomous Serpents in the Museum of the Academy, with notes on the families, genera, and species (new genus, Teleuraspis); *E. D. Cope.*—Index to vol. for 1859.

Transactions of the Am. Philos. Society (Philadelphia). Vol. XII (new series), Part II, contains:—Art. XI. Della Correlazione delle Forze Chimiche Colla Rifrangibitita delle Irradiazione; Di ZANTEDESCHI.—Esperimenti Eeguiti Col Calorico Solare.—Art. XII. Geological sketch of the estuary and freshwater deposit of the Bad Lands of the Judith, with some remarks upon the surrounding formations; by F. V. HAYDEN, M.D.—Art. XIII. Extinct Vertebrata from the Judith River and Great Lignite Formations of Nebraska; by JOSEPH LEIDY, M.D.—Art. XIV. A sketch of the Botany of the Basin of the great Salt Lake of Utah; by E. DURAND.—Art. XV. Observations on the Magnetic Dip in the United States; by Prof. ELIAS LOOMIS.

REPORT OF THE TWENTY-EIGHTH MEETING OF THE BRITISH ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE, held at Leeds, Sept. 1858. London, 1859.—Contains the following zoological papers of more general interest:—On the Anatomy of the Araneidea, particularly of their Spinning organs (2 plates); *R. H. Meade.*—Various reports of Dredging committees.—On the reproductive organs of Sertularia tamarisca; *Allman.*—On the Migration of Birds; *Collingwood.*—Anatomy of the Brain in some small quadrupeds; *Garner.*—On the formation of the Cells of Bees; *Tegetmeier.*

DET KONGELIGE DANSKE VIDENSKABERNES SELSKABS SKRIFTER, 5te Raekke, Naturv. og Math. Afdeling, 4de Binds. Copenhagen, 1856–59.—Contains the following zoological papers:—Coroctoca and Spirachtha, Staphylines which bring forth living young and are domesticated with a Termite (with 2 plates); *J. C. Shiödte.*—On the Hectocotyle forms of the Octopod genera Argonauta and Tremoctopus (with 2 plates); *J. J. Sm. Steenstrup.*—Attempt at a monographic exposition of Sergestes, a genus of Shrimps, with remarks upon the organs of hearing in the Decapod Crustacea (5 plates); *H. Kroyer.*—On Mephitis Westermanni, a new "Stinkdyr" from Brazil; *J. Reinhardt.*—5te Binds, 1ste Hefte, 1859: Additamenta ad historiam Ophiuridarum,—descriptions of new or little known species of Serpent-stars; 1st and 2d parts, 7 plates, (new genera, Ophiocten, Ophionereis, Ophiactis, Ophiostigma, and Ophioblenna); *Chr. Fr. Lütken.* W. S.

6. *New Zoological Journal.*—Dr. H. F. WEINLAND, already well known to American zoologists, has commenced, Oct. 1, 1859, at Frankfort on the Maine, a monthly journal (8vo, 16 pp.) entitled *Der Zoologische Carten. Organ für die Zoologische Gesellschaft.*

IV. ASTRONOMY AND METEOROLOGY.

1. *Supposed intra-Mercurial planet.*—The announcement of M. Le Verrier that the existence of one or more planets within the orbit of Mercury is rendered highly probable by his computations on the movement of the perihelion of Mercury, has called out former observations of the transits of bodies over the sun's disc, in addition to those mentioned in vol. xxviii, pp. 445 and 446 of this Journal.

(1.) Dr. Lescarbault at Orgères (Dept. Eure et-Loir), France, observed with a telescope, March 26th, 1859, a small black circular spot moving across the upper limb of the sun, at a rate which would occupy $4^h\ 26^m\ 48^s$ to traverse the entire disc. Its apparent diameter was less than a quarter that of Mercury in transit. M. LeVerrier thinks the observation worthy of credit, and computes that on the supposition of a circular orbit the time of the planet's revolution around the sun is $19^d{\cdot}7$, and the inclination of its orbit 12° 10′. Its greatest elongation from the sun would not exceed eight degrees, and its light be less than that of Mercury. This planet seems however insufficient to account for all the movement of the perihelion of Mercury.—*Comptes Rend. Acad. Sci., Jan.* 2, 1860.

(2.) Mr. Benj. Scott, of London, states that about midsummer in 1847, he chanced to turn a telescope towards the sun then near its setting, and saw on the sun's disc a well defined black spot, which was not to be seen there at sunrise the next morning. Its angular diameter appeared as large as that of Venus. Mr. Scott mentions that a similar body, or spot, was seen by Mr. Lloft, January 6, 1818.

2. *Mr. Alvan Clark's New Micrometer for measuring large Distances*, (Extracted from the Monthly Notices of the Royal Astronomical Society for July 1859).—At the monthly meeting of the Society in June, Mr. Alvan Clark, of Boston U. S., exhibited a micrometer invented by himself, which is capable of measuring with accuracy any distance up to about one degree. It is also furnished with a position-circle. Its character is essentially the same as that of the parallel-wire micrometer; but it has some peculiarities not, it is believed, previously introduced, and on which its wide range depends.

The most remarkable of these peculiarities consists in its being furnished with *two eye-pieces*, composed of small single lenses, mounted in separate frames, which slide in a grove, and can be separated to the required distance. A frame carrying two parallel spider-lines, each mounted separately with its own micrometer-screw, slides in a dove tailed grove in front of the eye-pieces; and, by a free motion in this frame, each web can be brought opposite to its own eye-lens.

In using this micrometer, the first step is, to set the position-vernier to the approximate position of the objects to be measured. Then the eye-lenses are separated till each is opposite to its own object. The frame containing the webs and their micrometer-screws is then slid into its place; and, the webs having been separated nearly to their proper distance by their free motion in the frame, they are placed precisely on the objects by their fine screws, the observer's eye being carried rapidly from one eye-lens to the other a few times, till he is satisfied of the bisection of each of the objects by its own web. The frame is then removed for reading off the measure by means of an achromatic microscope, on the stage of which it is placed. One of the webs is brought to the intersection cross-wires in the eye-piece of the microscope; and by turning a screw (the revolutions of which are counted), the frame travels before the microscope, and the other web is brought to the intersection of the cross-wires. The *parts* of a revolution are read off by a vernier from a large divided circle attached to the screw.

The advantages arising from the peculiar construction of this micrometer are the following:—

1. Distances can be observed with great accuracy up to about *one degree*, and the angles of position also.

2. The webs, being in the same plane, are free from parallax, and are both equally distinct, however high the magnifying power may be.

3. The webs are also free from distortion and from color.

4. A different magnifying power may be used on each of the objects; which may be advantageous in comparing a faint comet with a star.

3. *New Double Stars discovered by Mr. Alvan Clark;* communicated by the Rev. W. R. Dawes. (From Monthly Notices of the Royal Astronomical Society, xx., p. 55. Second series.)

Con't No.	Designation.	R.A. 1860.	N.P.D. 1860.	Mag.	Dist.	Date of discovery.
		h m s	° ′		″	
13	Lalande 1980	1 0 51	45 32·4	8, 8	0·45	1858, Nov. 29
14	Lalande 2634	1 20 7	47 56·6	8, 9	0·8	1858, Oct. 13
15	99 Herculis	18 1 43	59 27·4	5½, 10½	1·7	1859, July 10
16	✱ Vulpeculæ	19 52	63 7	7½, 8	0·4	1858, Aug. 30
17	Cygni 153 B / B.A.C. 6959	20 8 3·8	38 57·4	6, 11½	3·8	1859, July 19
18	44 Cygni	20 25 41	53 32·0	6½, 11½	2·8	1859, July 6
19	A.Z. xxiv. 11	21 10 50	26 10·2	7½, 7½	+0·9	1859, July 8
20	75 Cygni	21 34 41	47 21·6	5½, 11	2·9	1859, July 19

All the double stars in this series may be considered as good test-objects for telescopes of from 6 to 8 or 9 inches aperture. But it is not merely as such that they are interesting: they become especially so from the fact that they are all situated in the northern hemisphere, and all but one at more than 30° from the equator. They consequently attain a good altitude at Pulkova; notwithstanding which they have escaped the acute search of M. Otto Struve with the 15-inch refractor, in addition to the previous one by his father with the Dorpat telescope of 9·6 inches aperture. Either, therefore, they have recently undergone considerable change; or, if not, it appears that objects of great difficulty and delicacy may be detected with very perfect telescopes of smaller size, which have escaped the most diligent scrutiny with far larger instruments. The whole of these objects were discovered by Mr. Clark with his own object-glasses of 8 and 8¼ inches aperture, and five of them in my own observatory, during his visit to me last summer; since which I have met with *seven others*, of similar character and situation; and it should be mentioned that none of the objects recorded are below Struve's eighth magnitude for the larger star; all smaller, of which many have been found, having been systematically rejected. It appears, therefore, that there is still much occupation for telescopes of moderate dimensions, even in this department of astronomy, which might reasonably have been supposed to have been long since exhausted. The *distances* stated in the list are from my own recent measurements.

Haddenham, Thame, November, 1859.

4. *Notice of the Meteor of Nov.* 15, 1859; by Prof. E. Loomis.—In the last No. of this Journal, p. 137, I gave a brief notice of this meteor, but from want of space was compelled to limit myself to a brief summary of results. I have received a large amount of documents relating to this meteor, most of which however are too indefinite to be of much value. I now proceed to present a brief summary of what appear to me the most reliable observations.

At New Haven, Ct., Judge W. W. Boardman saw the meteor descend at an angle of 25° to 35° with the vertical, and it passed from his view at the edge of the dome of a steeple in azimuth S. 35° 34′ W. Continuing the meteor's path down to an altitude of 3° or 2°, we have S. 37° W. for the azimuth of the place where the meteor would have disappeared to him had his view been unobstructed.

At New York city Mr. Tatham was riding in the Bowery, and saw the meteor descend at an angle of 20° with the vertical, and in a range with the middle of the street opposite the Old Bowery theatre. According to the map of the city, this direction was S. 27½° W. The diameter of the meteor appeared to be about one-third that of the full moon.

A correspondent of the Evening Post, walking down Broadway, saw the meteor disappear in azimuth S. 25° W.

Mr. Gould, also in Broadway, saw the meteor disappear behind a building in azimuth S. 23½° W.

Mr. Pirsson, also in Broadway, saw the meteor disappear behind a high building in azimuth S. 21° W.

Mr. Bradley, also in Broadway, reports that the meteor disappeared in azimuth S. 16° W.

Several other observers agree as to the general direction of the meteor, but their statements are less precise than those of the preceding. As Mr. Bradley's observation differs materially from the others, I reject it, presuming that his memory must have been in fault, either in respect to his point of observation, or that of the meteor's disappearance. The mean of the other four estimates is S. 24¼° W., or allowing for the effect of the high buildings which obstructed the view of three of the observers, the mean would be about S. 26° W. This result differs five degrees from my former estimate; a difference which is explained by my having obtained two new observations, and by my rejection of Mr. Bradley's observation.

At Washington, the apparent path of the meteor was vertical, and its point of disappearance was estimated at four degrees north of east.

A gentleman four miles west of Dover, Del., was riding towards Dover. His wife saw the meteor; he only saw the smoky trail which he describes as a nearly vertical column, with its base 20°, and its top 40° from the horizon; direction due east.

Mr. Parsons, at Salisbury, Somerset county, Md., saw the meteor descend in a slanting direction to the earth, when it exploded with a dull sound. Its direction was from the N.E.

At Lewistown, Del., the meteor was seen to fall in the N.E. The report was heard five minutes later—loud but distant.

If we mark upon a map all the preceding directions, we find that the lines do not intersect at one point, but they indicate the most probable point of the meteor's disappearance to have been near lat. 39° 10′ and long. 75° 5′.

At New Haven, the path of the meteor was estimated to make an angle of from 25° to 35° with the vertical. Mr. Wilder Smith, near Waterbury, Ct., estimated the inclination to the vertical at about 30°.

At New York, Mr. Tatham estimated the angle with the vertical at 20°, Mr. Gould 10°, Mr. Pirsson 35°, and Mr. Bradley 45°. The mean of these four estimates is 27°.

At Washington, the path was pronounced exactly vertical. The actual path of the meteor was therefore such as, if continued, must undoubtedly have struck the earth. It must have passed vertically over the extreme southern part of New Jersey, and must have struck the earth in Delaware Bay, or near its shore.

That this conclusion is a near approximation to the truth, is confirmed by observations from the southern part of New Jersey.

Mr. Mills was surveying in the forest four miles west of Stephens Creek in Atlantic county, and heard a noise nearly overhead. He looked up and saw a cloud of a rounded form like a puff of smoke about 15 degrees south of the zenith.

At Millville, Cumberland county, a strange rumbling noise was heard somewhat resembling thunder, and one or more clouds of smoke were seen in a southeast direction at an elevation very roughly estimated at 45°.

At Newport, Cumberland county, a rumbling noise, which lasted two minutes, was heard in an east or southeast direction.

At Maurice River Cove, Cumberland county, the captains of the oyster boats saw a flash and smoke in an easterly direction.

At Dias Creek, Cape May county, Mr. Smith states that the noise was great and lasted two or three minutes. The flash was brilliant, and the smoke was seen in a northeast direction at an elevation of 75° or 80° above the horizon.

At Goshen, Cape May county, a noise was heard in a northeast direction, and a cloud of a rounded form was seen in the northeast.

At Dennisville, Cape May county, the noise appeared directly overhead. There was a small cloud or belt of white smoke left in the train of the meteor, about five degrees northwest of the zenith, the atmosphere being perfectly clear at the time. The detonations lasted somewhat over a minute.

The directions indicated in the preceding notices have a decided convergence towards a point near lat. 39° 13′, and long. 74° 52′. This result accords so nearly with that derived from observations made at a distance of a hundred miles and upwards, as to show that the observations are in the main reliable, but subject to that uncertainty which attends all estimates made without instruments, and not reduced immediately to writing. We must then conclude that this meteor passed vertically over the southern part of New Jersey, nearly on the parallel of 39° 13′, and that it struck the earth near the eastern shore of Delaware Bay, probably between Dennis Creek and Maurice River.

I assume that this meteor was a solid body. We are acquainted with two classes of meteors quite distinct from each other and differing greatly in density. Ordinary shooting stars have never been known to reach the earth's surface, or to produce any audible noise. Another class of meteors like that of Agram in 1751 is composed chiefly of iron, and another like that of Weston, Ct. (1807), partly of iron and partly of silicates. They frequently strike the earth, or at least let fall fragments to the earth, and are attended not only by a brilliant flash of light, but by a tremendous noise. The New Jersey meteor bore a striking resemblance to the Weston meteor, not only in the brilliancy of its light, but in the noise which attended it. We cannot doubt that it was a body of considerable density; and the direction of its motion was such that the entire mass must have struck the earth. It may have sunk into Delaware Bay and not a single fragment have fallen upon dry land; but there is reason to hope that at least some fragments of it may yet be discovered. Such fragments, if they exist, are probably scattered along an east and west line coinciding nearly with the parallel of 39° 13′, and the entire mass probably lies near the meridian of 75°.

5. *Sandwich Island Meteor of Nov.* 14, 1859.—A meteor of remarkable size and brilliancy was seen from the slope of Mauna Kea north of the great volcanoe of Kilauea, S. I., soon after dark of the 14th November last, shaped like a cross, having the light of the moon at full, moving vertically south from a point a little below the zenith and disappearing near the crater.—*Pacific Com. Advertiser, Dec.* 15.

6. *Der Meteoreisenfall von Hraschina bei Agram am* 26 *Mai* 1751; von W. Haidinger. Wien, 1859.—Prof. Haidinger has here revised all the contemporary evidence respecting the fall of this remarkable meteoric iron mass, the details of which are fortunately well authenticated. This history is of great interest at the present moment when the late meteors of August and November last have called up anew the discussion of this subject.

V. MISCELLANEOUS SCIENTIFIC INTELLIGENCE.

1. *Monthly varying level of Lake Ontario, measured, in inches, from a fixed point above the surface downwards, for fourteen years, at Charlotte, mouth of Genesee River, N. Y.:*

Year.	Jan.	Feb	Mar	Apr	May	Jun.	July	Aug	Sep	Oct.	Nov	Dec	Mean	Range.	
1846	51	54	48	45	42	39	39	42	45	45	48	45	45·2	15	Low.
1847	48	42	36	36	29	25	25	25	36	39	43	46	35·8	23	Higher.
1848	29	34	43	38	38	37	38	39	44	49	54	53	41·3	25	Lower.
1849	50	50	52	46	36	33	44	39	45	38	38	41	42·7	19	do.
1850	45	40	40	40	32	29	34	46	47	52	55	43	41·9	26	do.
1851	44	54	48	47	44	38	35	38	42	47	53	51	45·0	19	Low.
1852	50	51	48	44	26	26	22	24	30	23	38	34	34·7	29	Higher.
1853	35	32	32	25	20	14	27	20	24	28	38	39	27·8	25	Mean.
1854	39	39	38	38	27	24	25	27	36	44	48	50	36·2	26	Lower.
1855	52	53	36	40	40	36	34	36	36	34	33	33	38·6	20	do.
1856	35	35	33	31	23	18	23	30	37	46	53	53	34·7	35	Higher.
1857	54	56	46	44	35	24	19	12	14	9	24	22	30·0	47	Mean.
1858	19	13	13	18	4	6	4	2	8	12	14	16	10·9	17	Very high.
1859	20	24	16	10	6	2	8	11	17	22	28	25	15·7	26	Lower.
Mean.	41	41	38	36	29	25	27	28	33	35	40	39	34·3	25	

1. As more water falls usually in the warmer months, the Lake is higher in those months generally than in the colder months.

2. The range has been only 54 inches, the lowest being in February, 1857, and the highest in August, 1858, and in June, 1359; the mean of the two is 27 inches.

3. In 1846 and 1857 the mean level *lowest*, and in 1858 and 1859 *highest*.

4. In 1853 the Lake was near the *mean level*, and in 1857 only a little less, though the first half of the year gave low water and the last half high.

5. The Lake was near the highest, or within four inches of it, in May, June, July and August, 1858, and in May and June, 1859, and of course the average of both years was high.

6. The Lake down to 50 inches or more in January and February, 1846; in November and December, 1848; in January, February and March, 1849; in October and November, 1850; in February, November and December, 1851; in January and February, 1852; in December, 1854; in January and February, 1855; in November and December, 1856; and in January and February, 1857.

These statements show that the changes of the level must be owing to the ordinary causes of supply or diminution of water over this great watershed, and disprove any notion of *periodic* rise and fall under any but meteoric laws. As the water was high in the Lake through 1858, it was suggested that the average fall of water must continue it high in 1859, as the measures now prove. In November, 1859, the water fell to the mean, and rose afterwards from the great autumnal rains at the west which had flowed into Lake Ontario. C. D.

2. *Eruption of Mauna Loa, Sandwich Islands*, (in a letter to Prof. Dana from Prof. R. C. Haskell, Oahu College, dated Honolulu, Nov. 5, 1859).—Since my last dates (June 22d)* the lava continues to flow from the place of the recent eruption. With scarcely any cessation since the middle of June it has been flowing into the sea. Hawaii has been increased in area by many acres at least, by several hundred acres it is said.

After writing you from Kona in June, I visited Kilauea, which I found *very quiet*. There has, however, been considerable action since you were there in 1840, for the crater is now filled up even with the "black ledge" of which Wilkes speaks.

From Kilauea, passing through Hilo, I went to Waimeu, intending to ascend Mauna Kea, but the weather proved so rainy and foggy that I was unable to do so. From Waimeu I went direct to Kóna, crossing the lava stream without difficulty on a mule, between the three mountains. The stream was fully three miles wide where I crossed, and at some points above appeared to be five or six miles wide. At this time the lava was flowing into the sea, and of course running under me as I crossed, yet the lava on the surface was in no place so hot as to burn the hoofs of the mule, or even to be noticed by myself, unless I touched my hand to it.

After arriving at Kona I went by canoe to visit the place where the lava was then and is still flowing into the sea. Without attempting to give an adequate description of the sight presented as I passed, by night, a few rods in front of the stream, which was more than a mile wide, I will only mention one fact.

* Vol. xxviii, [2], 284.

The lava was at a light-red heat, and flowed into the sea with a velocity of two or three miles per hour. And yet this point is forty miles from the source of the stream, and at least twenty-five miles from the lowest point to which the "fissure" in Mauna Loa can possibly extend. Therefore the lava flows twenty-five miles at least, without receiving any heat from the interior of the earth, and yet is still of a light-red heat. It will be remembered, of course, that the stream is covered over with solid lava all the way from the source to within a few feet of the sea, with the exception of a small opening here and there, once in a mile perhaps.

Rev. T. Coan adds, under date of Hilo, Hawaii, Nov. 25, 1859.—"The old lake of fusion in Kilauea is slowly enlarging, and the area around it is subsiding. Probably it may in time resume its old size of half a mile in diameter. Recent visitors have found it active. On one occasion it was thought to throw up jets to the height of 70 feet.

The present eruption has now been in progress ten months, and our last advices report it still active. Several streams have fallen into the ocean along the coast of Kona. These are of different widths, and some of them are separated miles from each other. A small village, Kibele, has been covered of late with the lava, and a large and valuable fish-pond filled up. The people in Kibele pulled down their houses, and also the church, on the approach of the lava stream, and carried off the materials. Just above the church the fiery stream parted, flowed along on each side of the ground where the church had stood, reunited below it, and continued in one stream to the sea. This fact struck the Hawaiians as marvellous, and they regretted having removed their house of worship.

During the early stages of this eruption there were many splendid exhibitions along the line of flow. Canals, cataracts, lakes, fountains and jets of fusion were seen along the slope of the mountain. Forests were consumed, rocks were rent, loud and startling detonations were heard, and the heavens were shrouded with a pall of darkness. Now, and for a long season past, little or no fire is seen, except where the red lava pours into the sea. Here a broken line of fusion is seen coming out from under its self-made counterpane of hardened lava, and pouring down the face of a low and cragged precipice into the ocean, keeping up a constant boiling and sending up clouds of vapor into the air.

The central parts of Kilauea are more quiet than any other part of the crater. We have occasional earthquakes. Two shocks occurred in February, one in July, and two in November of the current year."

Book Notices.—

1. *Trübner's Bibliographical Guide to American Literature*: a classed list of books, published in the United States of America during the last forty years. With Bibliographical Introduction, Notes and Alphabetical Index. Compiled and edited by Nicholas Trübner. London, 1859. 8vo, 554 pp.—This work is beyond all question the best guide which we have to recent American literature and science. Not only is it better than all other bibliographical works of a similar scope but it is excellent in itself. Our limits permit us to mention only one of its most valuable features. Special attention has been bestowed on works in natural science, not only those which appear with an author's name, and are accordingly easy to trace, but more particularly on serial works, such as scientific journals, the transactions of learned societies, and reports of the

state and national legislatures, which are often very difficult to discover by the ordinary apparatus of the trade. Mr. Trübner not only mentions what constitute complete sets of such works; but he enumerates the contents of the several volumes,—so that by means of his excellent index a multitude of articles and essays, often overlooked, are brought to the knowledge of every student. This book should be owned by every book-buyer. D. C. G.

2. *Manual of Public Libraries, Institutions and Societies in the United States and British Provinces of North America;* by WILLIAM J. RHEES, chief clerk of the Smithsonian Institution. 8vo, pp. 687. Lippincott. Philadelphia, 1859.—This volume contains a great amount of useful information on Public Libraries, and gives evidence of much labor in its compilation. The list of libraries in the various States extends to over three thousand titles. In a second edition the Author will be able to supply some obvious deficiencies which are inseparable from the first cast of such a work.

3. *The New American Cyclopedia: a popular Dictionary of general Knowledge;* edited by GEO. RIPLEY and CHAS. A. DANA. Vol. I—VIII. 8vo. New York and London: D. Appleton & Co.—Since our former notice of this Cyclopedia it has advanced rapidly, until now we have before us eight volumes of eight hundred pages each, the last article being on the too famous HAYNAU. Such promptness in issuing so large a mass of elaborately prepared matter speaks well not only for the energy of the publishers and the industry of its editors, but also of the public appreciation of the work. Like its predecessor, the "*Encyclopedia Americana,*" 1829–47, by Dr. LIEBER and others, it gives a satisfactory response to almost all questions coming within the range of its plan. The *New Cyclopedia*, however, besides its greater range of topics, has the advantage derived from a vast progress in many departments of knowledge, developed by a numerous corps of contributors skilled each in his own speciality.

In looking over its articles with a peculiar reference to our own departments, we are often tempted to linger among its miscellaneous topics, so rich in various and interesting information. The fine arts, religion, law, politics and war, share our attention with history and biography, ancient and modern, foreign and American, including persons still living, with sketches of events within their respective eras; geography, with the physical and picturesque features and the mineral treasures of particular countries; common and useful arts, agriculture, mechanics, and their various productions; gas lighting, gun-powder, its history, manufacture and uses; caoutchouc, gutta percha, and kindred topics of technical chemistry, with their diversified applications, and a multitude of other subjects, more or less practical and interesting to society at large.

In the American Cyclopedia the articles on science are numerous and valuable, and elevate the work to the character of a compendium of modern science. These articles are in most cases written with decided ability, and evidently by persons who are familiar with the topics which they discuss. While many of the less important subjects are presented with luminous brevity, others are more fully expanded. Among these are many topics of natural history; Chemistry is presented with its equivalents and laws of combination illustrated by many of its modern discoveries and practical applications; of the latter an example is found in the full account of the manufacture of gelatine, of beer and bricks, and in the ample history of gas lighting. Geology, voltaic electricity, magnetism, and other departments of pure or applied science, are treated with reasonable fulness. As a literary work the Cyclopedia is written in a pure and chaste style, and exhibits the candor and fairness which should ever adorn a record of universal knowledge. B. S.

An Introduction to Practical Pharmacy: designed as a text-book for the Student, &c.; by Edward Parrish. Philadelphia. 2d edition. 246 illustrations. 8vo, pp. 720. Blanchard & Lea, 1859.—A well arranged and carefully prepared treatise, adapted to the state of this art in the United States.

Elements of Inorganic Chemistry, including the applications of the Science in the Arts; by Thos. Graham, F.R.S., L. and E. Edited by Henry Watts, B.A., and Robt. Bridges, M.D. 2d American edition in one volume. 233 woodcuts, 8vo, pp. 852. Philadelphia: Blanchard & Lea. 1858.—The American publishers issued the 430 first pages of this volume in 1852 under the editorship of Dr. Bridges. The remainder is reproduced without alteration from the English edition.

Nautical Monographs, No. I. (Washington Observatory) Oct. 1859. The winds at sea, their mean direction, and annual average duration from each of the four quarters. With four plates of diagrams of winds and calms. 4to, pp. 8. By Lt. M. F. Maury.

Caloric: its Mechanical, Chemical and Vital Agencies in the Phenomena of Nature; by Samuel J. Metcalf, M.D. 2 vols. 8vo, pp. 630 and 481. Philadelphia: J. B. Lippincott & Co.

Announced.

A Dictionary of English Surnames; by John Henry Alexander, Esq., of Baltimore, Maryland.—The work will be comprehended in about one thousand pages, and it will be sent to press directly after the necessary commercial and technical arrangements can be made.

Little, Brown & Company, of Boston, propose to publish by subscription a series of photo-lithographic plates of the Fossil Footprints found on the Connecticut River Sandstone, prepared by the late Dr. James Deane, of Greenfield, in one volume, 4to. Price $5.00.—The work will be issued under the superintendence of T. T. Bouve, Esq., A. A. Gould, M.D., and Henry I. Bowditch, M.D., and for the benefit of the family of Dr. Deane, and will be published in the best style, similar to Prof. Agassiz's "Contributions to the Natural History of the United States." Two hundred subscribers are required.

Obituary.—

Mr. Gustavus Wurdemann died on the 29th of Sept. 1859 at Swedesboro, N. J., aged 41 years. Mr. Wurdemann was employed in the U. S. Coast Survey since 1837, in the last twelve years of his life principally as a tidal and meteorological observer in Florida and the Gulf of Mexico. The observations made by him are of great value for their completeness and faithfulness. The short intervals of time left to him by the confining nature of his duties he employed with much success in collecting objects of natural history, and as he was mostly stationed on parts of our coast seldom visited by naturalists, he succeeded in obtaining several species new to science and still more which were new to the fauna of the United States. His collections are in the museums of the Smithsonian Institution and of Prof. Agassiz in Cambridge. Most of his zoological acquisitions have been already published to the world. The largest of our North American herons, *Ardea Wurdemanni*, was discovered by him.

James P. Espy, one of the most successful meteorologists of our time, died in Cincinnati, on the 24th of January, after a short illness, in the 75th year of his age. We expect to present a notice of his life in our May number.

Jean-Fréd.-Ludw. Hausmann, the eminent mineralogist, died at Göttingen, Dec. 26, 1859, aged 77 years 10 months.

FOSSIL SHELLS FROM NEBRASKA.

[illegible]ci. V. XXIX, No. 85, [illegible] 1860. PLATE I.

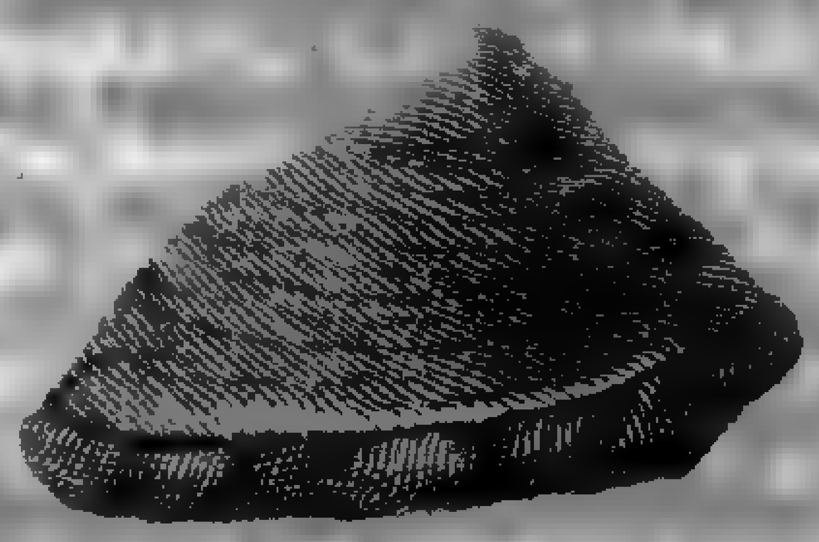


Fig. 1. [illegible]
Fig. 2. Dorsal view of same, with a portion [illegible] muscular scar on the internal cast.
Fig. 3. Internal cast of another specimen of same species, show[illegible] [illegible]ular scar.

U.S. COAST SURVEY

A.D. BACHE Supdt.

DIAGRAMS SHOWING THE RESULTS

of the

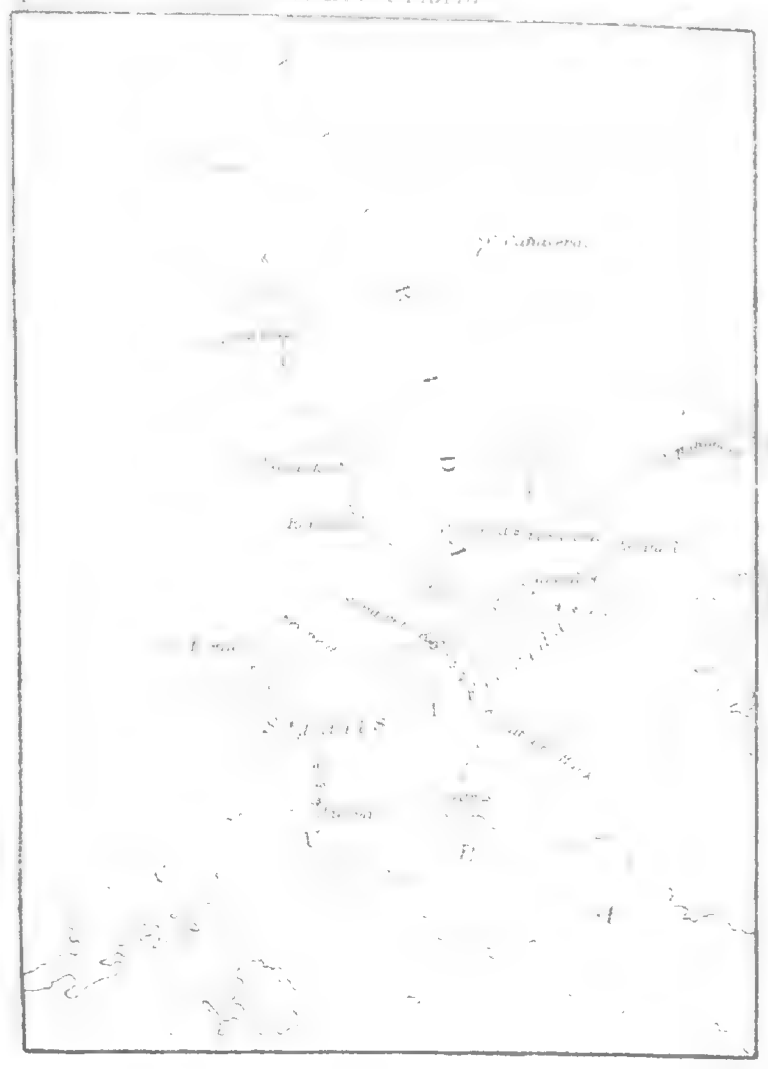
GULF STREAM EXPLORATIONS

From

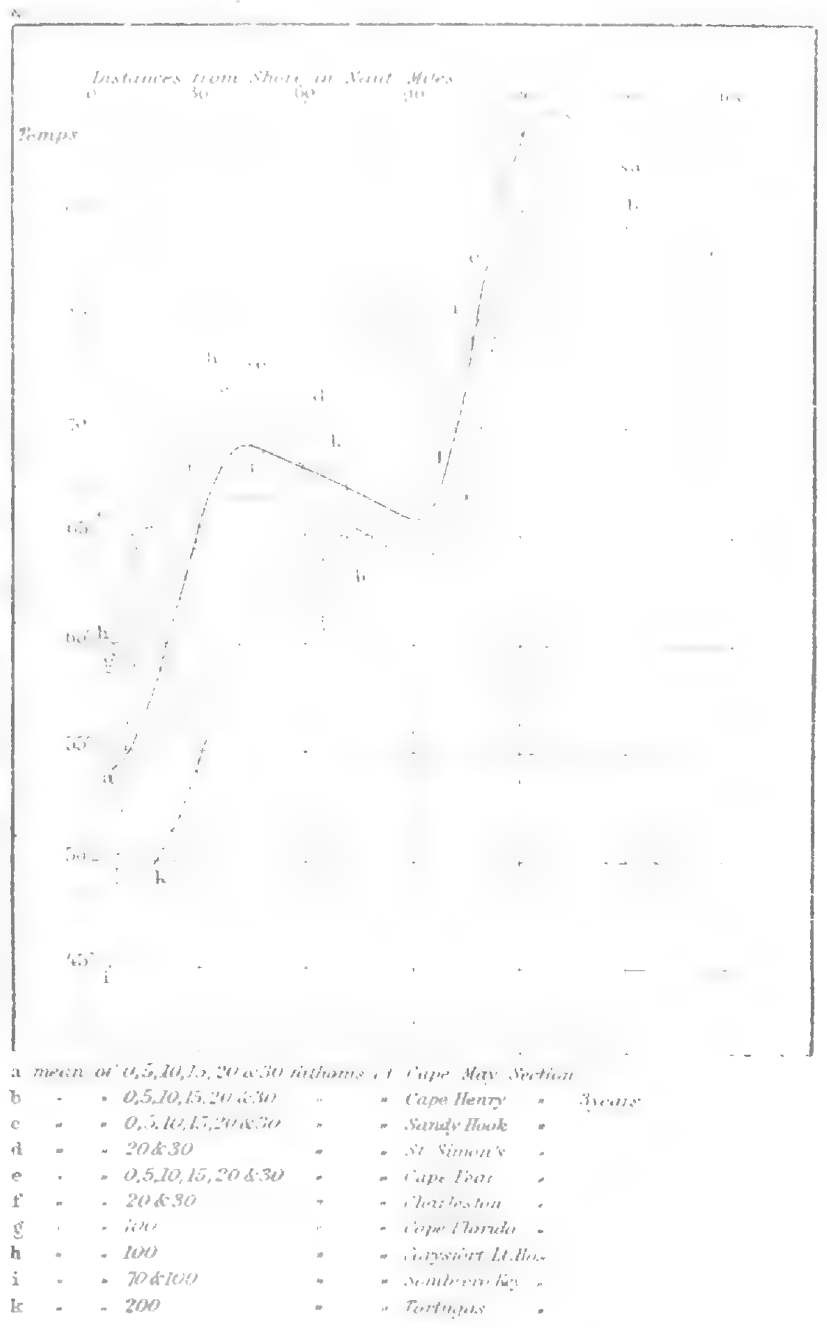
Observations made in 1845, 56, 47, 48, 53, 55, 58 & 59

1859

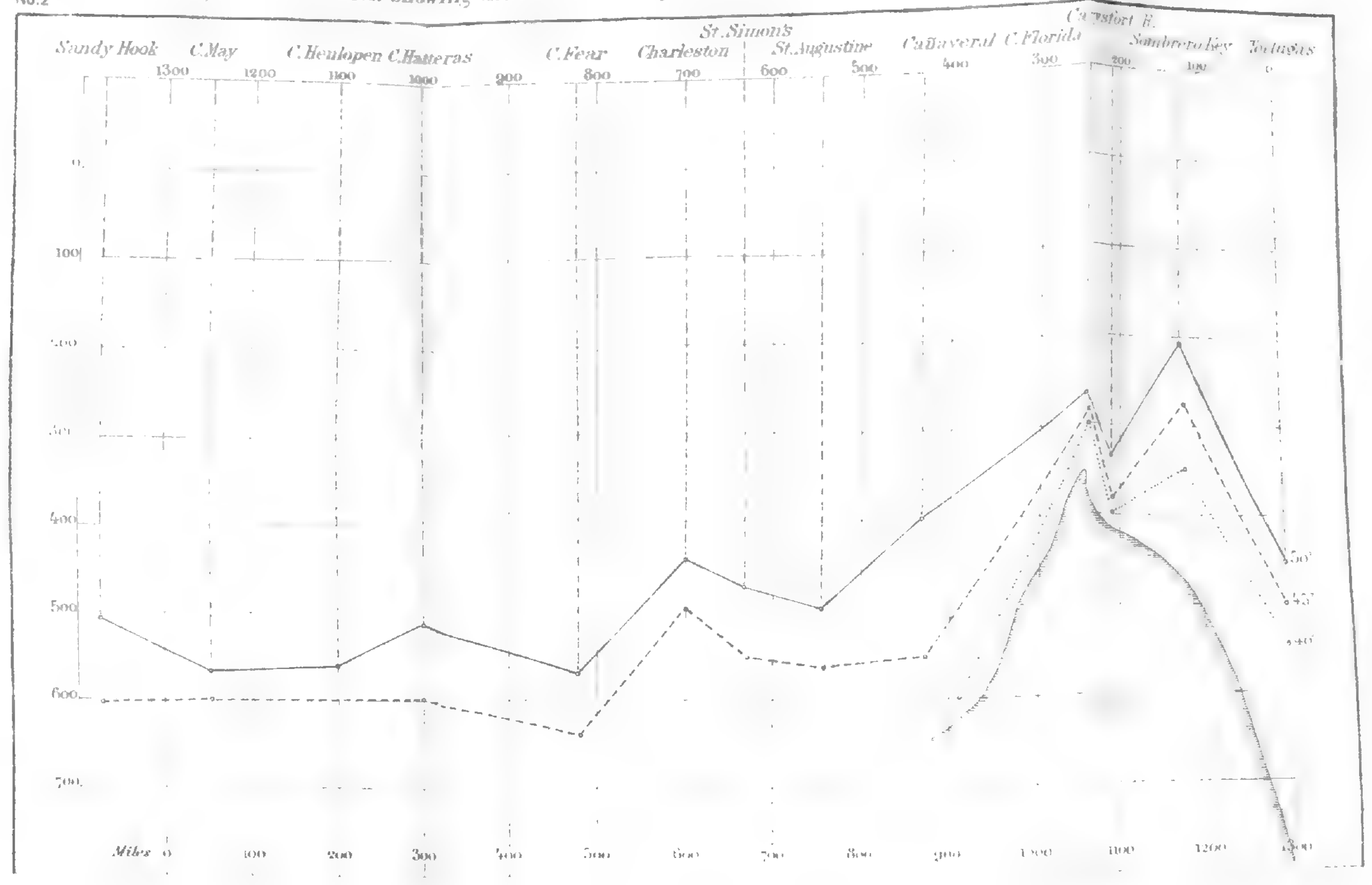
Showing the lines of Sections run in the Straits of Florida



Comparative curves of the cold wall



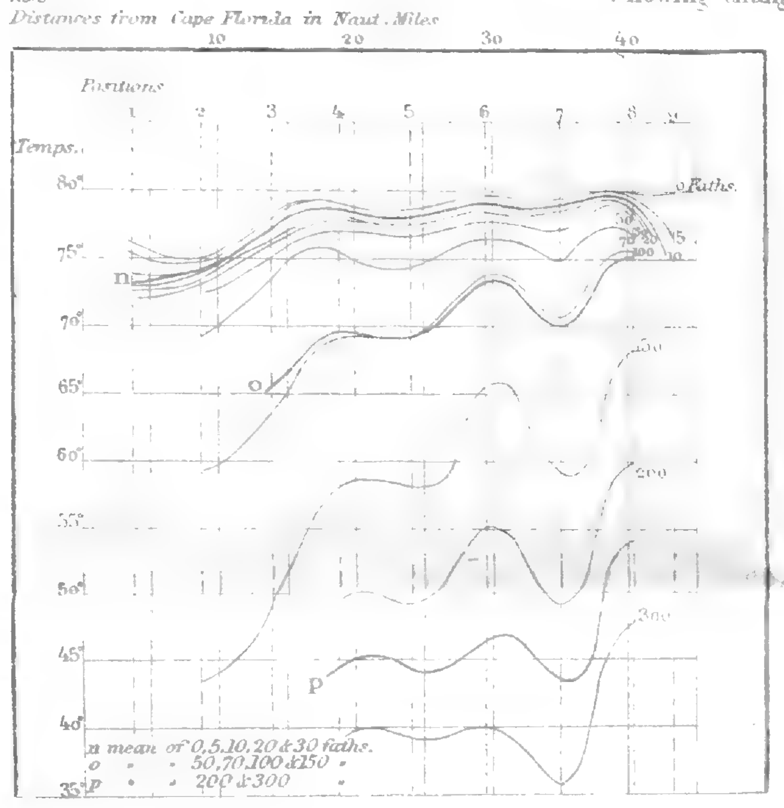
No.2 Longitudinal Section showing the Bottom & Temperatures near the bottom in the Straits of Florida



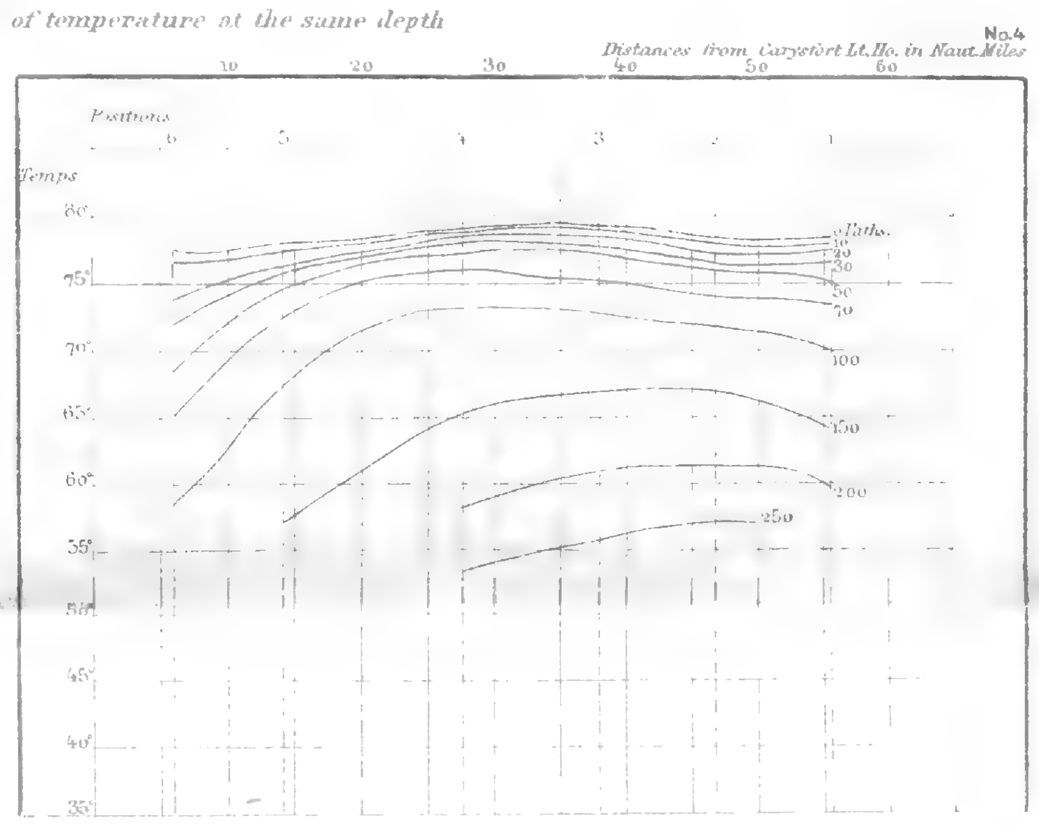
Sections off Cape Florida and Carysfort Lt. Ho. Diagrams of Sections

Showing changes of temperature at the same depth

No.3



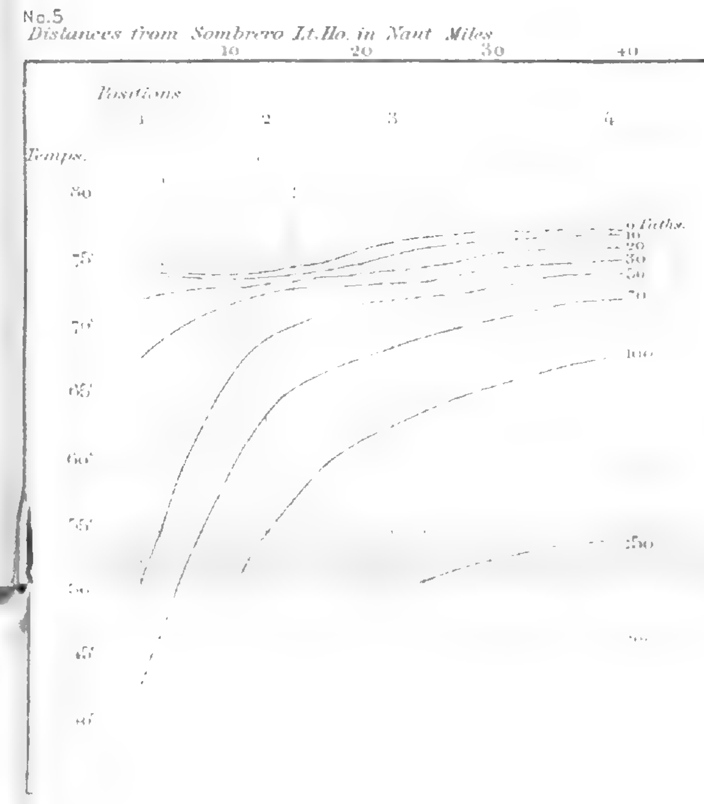
No.4



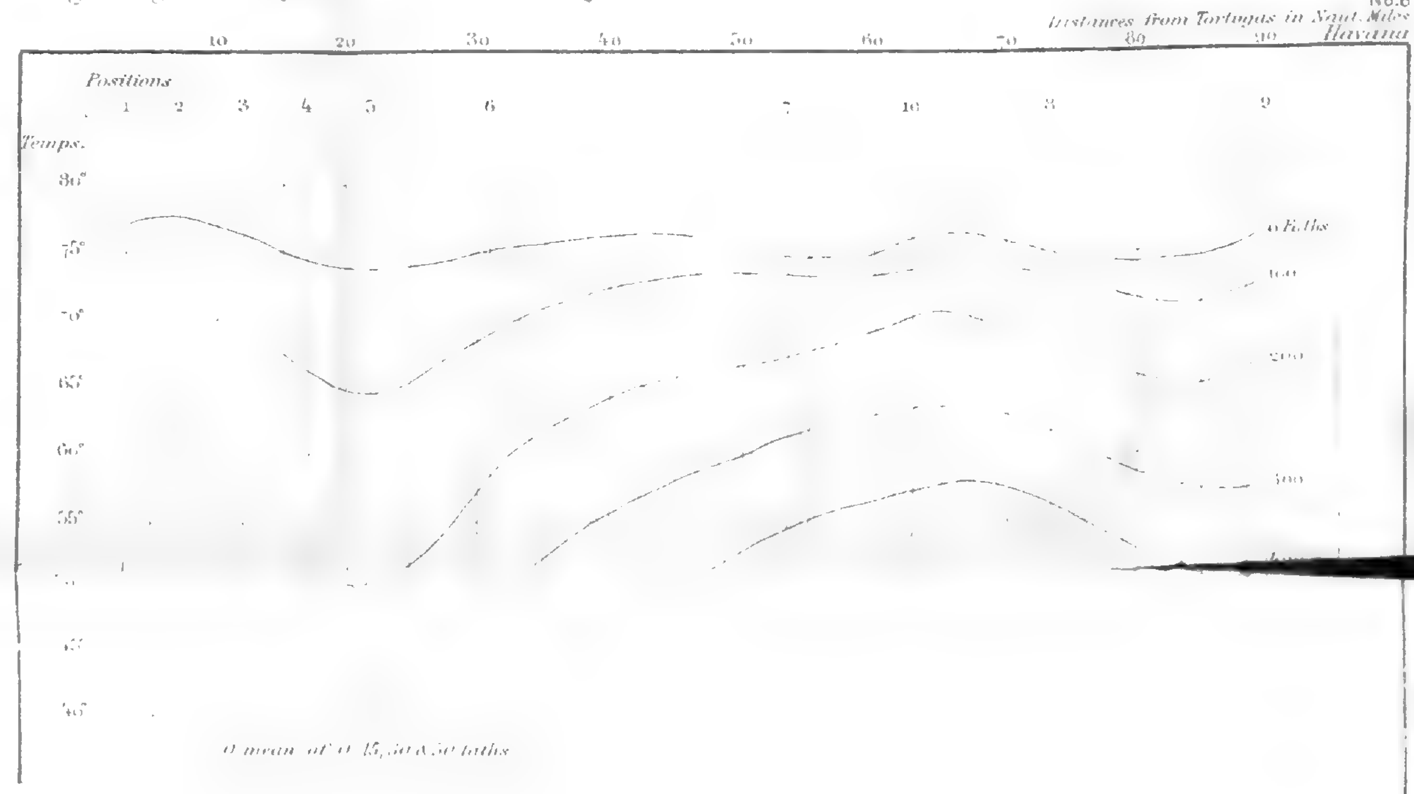
Sections off Sombrero Lt. Ho. and Tortugas. Diagrams of Sections

Showing changes of temperature at the same depth

No.5



No.6

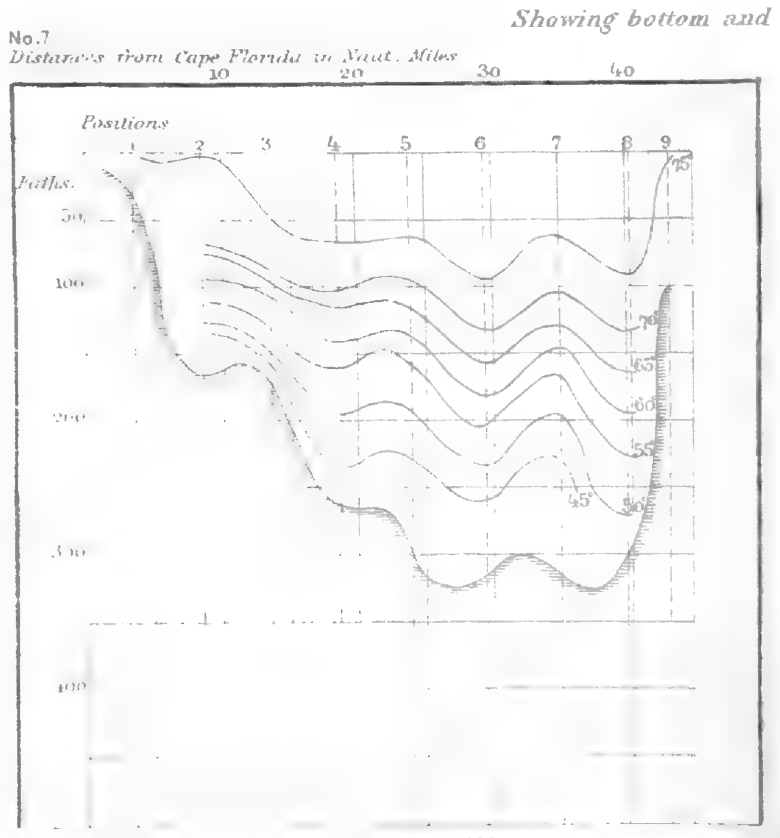


Section 1

Section 2

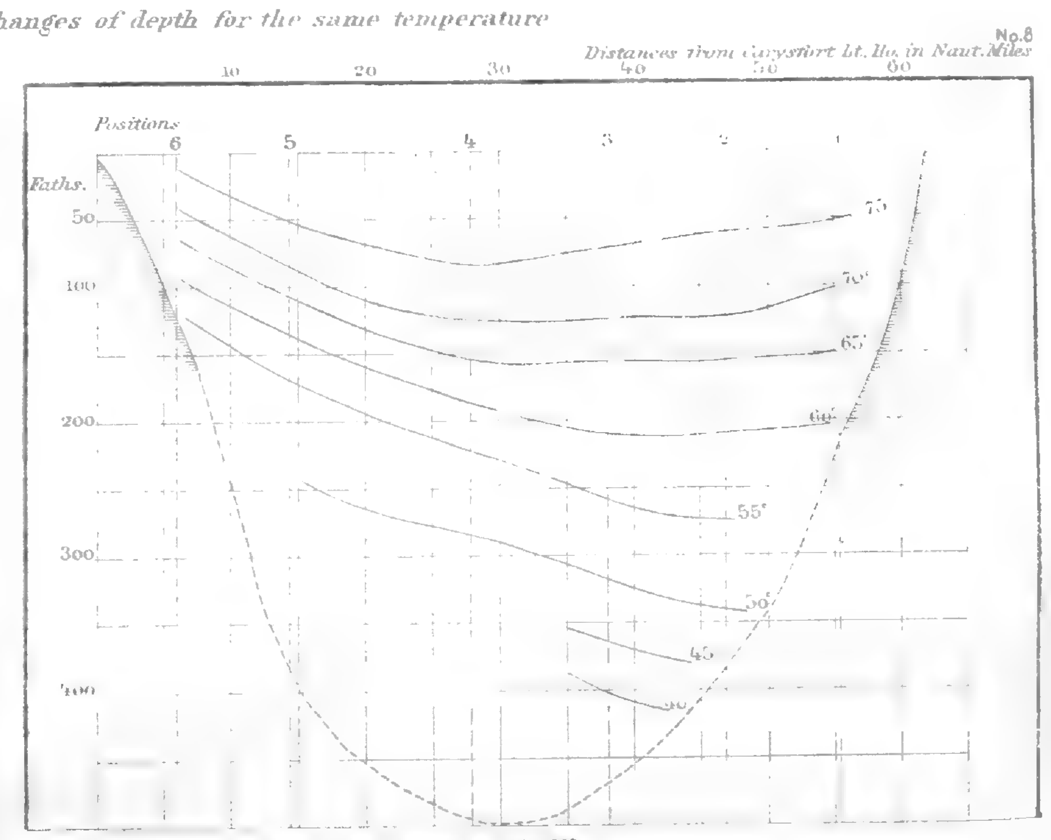
Showing bottom and changes of depth for the same temperature

No.7



The observations were made in May 1855

No.8



The observations were made in April and May 1856

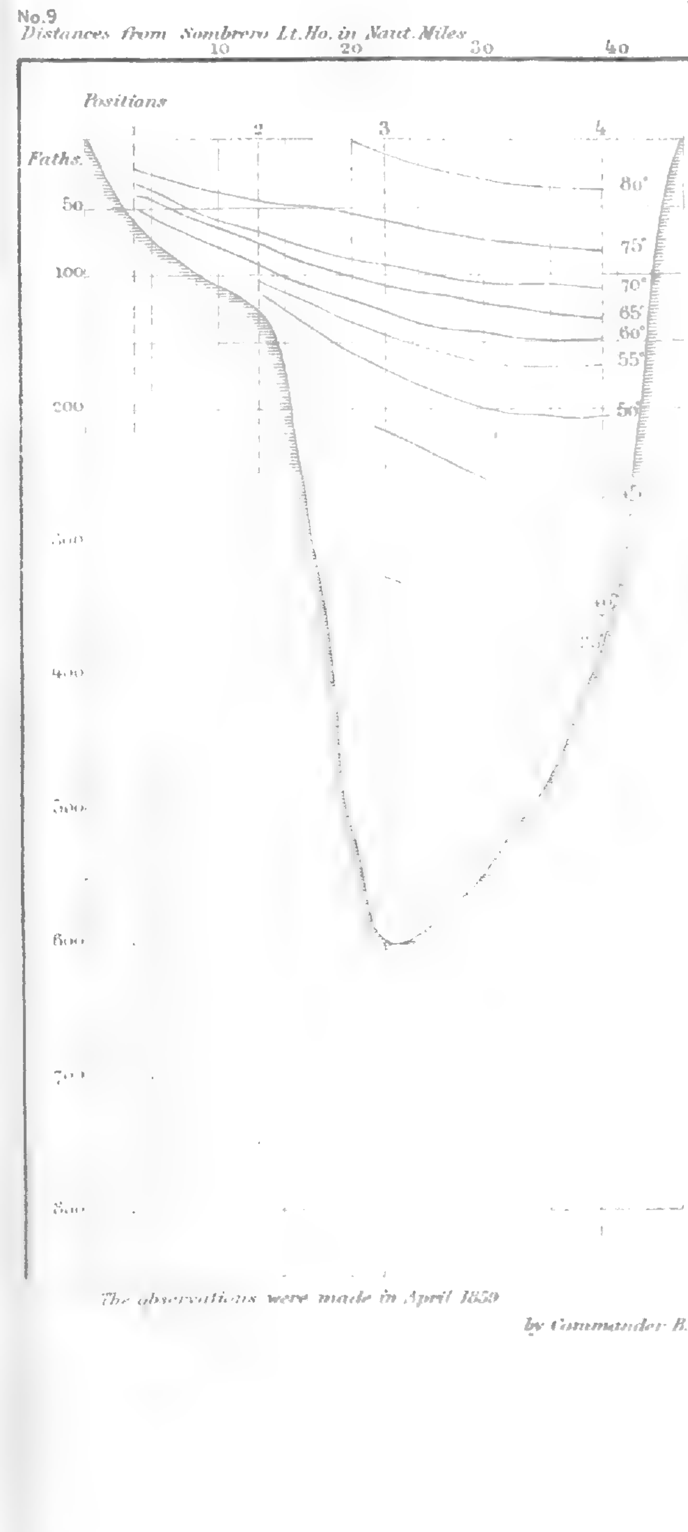
by Lieut. Comdg. T.A. Craven U.S.N. Asst. in the Coast Survey

Section 3

Section 4

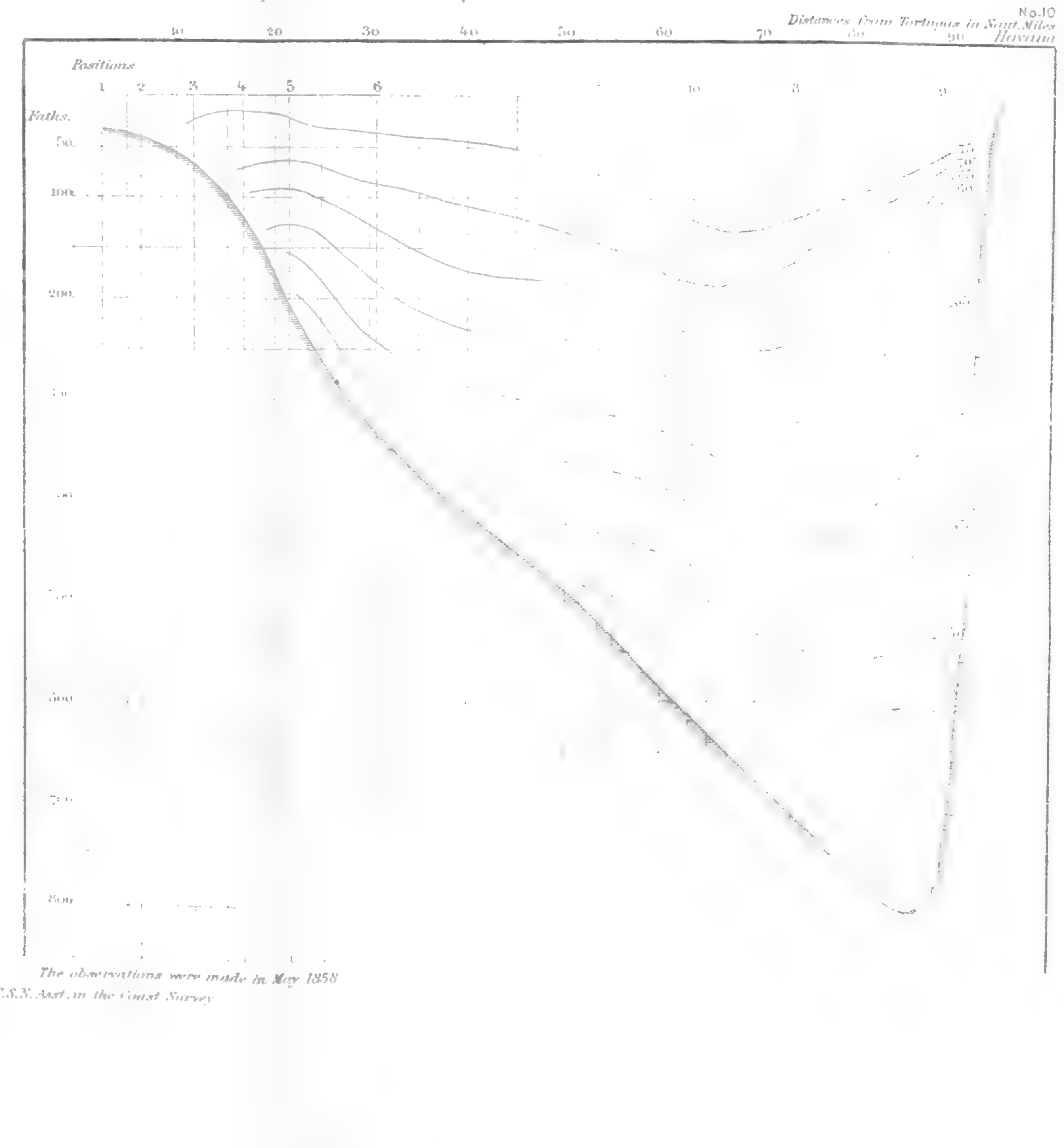
Showing bottom and changes of depth for the same temperature

No.9



The observations were made in April 1859

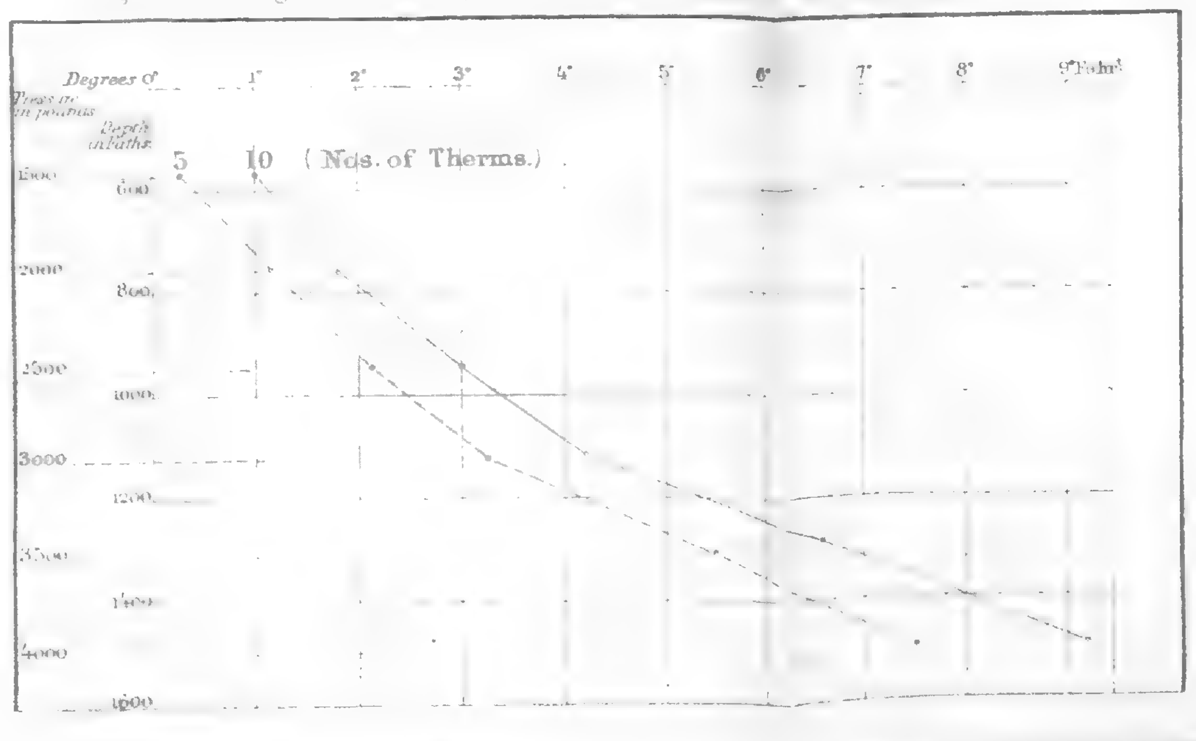
No.10



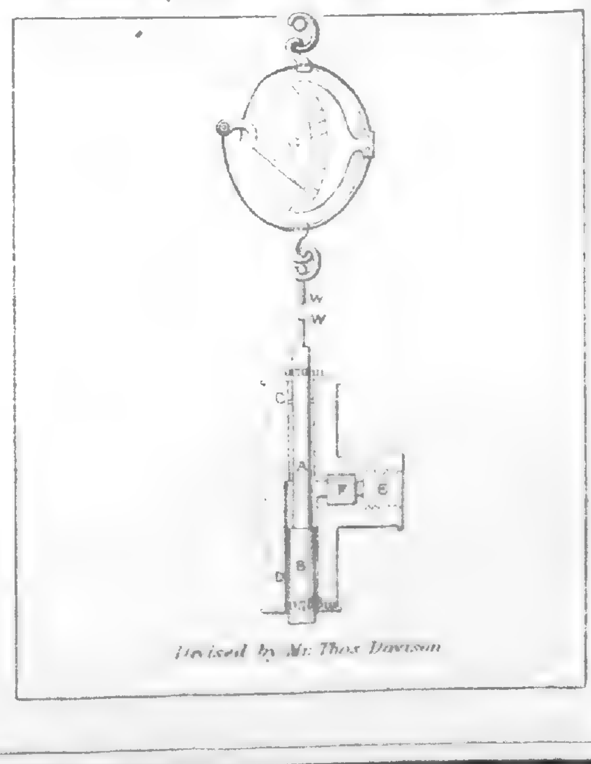
The observations were made in May 1858

by Commander B.F. Sands U.S.N. Asst. in the Coast Survey

No.11 *Diagram showing the effect of pressure upon Saxton's Deep Sea Thermometer*



No.12 **GAUGE** *for measuring the pressure in hydraulic cylinders*



Devised by Mr. Thos. Davidson

THE

AMERICAN

JOURNAL OF SCIENCE AND ARTS.

[SECOND SERIES.]

ART. XXVI.—*On the Origination and Distribution of Vegetable Species:—Introductory Essay to the Flora of Tasmania*; by Dr. JOSEPH D. HOOKER.

(Continued from p. 25.)

§4. *On the General Phenomena of the Distribution of Plants in Time.*

A THIRD class of facts relates to the antiquity of vegetable forms and types on the globe, as evidenced by fossil plants. The chief facts relating to these are the following:—

31. The earliest Flora of which we know much scientifically, is that of the Carboniferous formation. We have indeed plants that belonged to an earlier vegetation, but they do not differ in any important respects from those of the carboniferous formation. Now the ascertained features of the coal vegetation may be summed up very briefly. There existed at that time,—

Filices, in the main entirely resembling their modern representatives, and some of which may even be generically, though not specifically, identical with them.

Lycopodiaceæ; the same in the main characters as those now existing, and, though of higher specialization of stem, of greater stature, of different species, and perhaps also genera, from modern *Lycopodiaceæ*, yet identical with these in the structure of their reproductive organs and their contents, and in the minute anatomy of their tissues.

Coniferæ. The evidence of this order is derived chiefly from the anatomical characters of the Dicotyledonous wood so abundantly found in the coal, and which seems to be identical in all important respects with the wood of modern genera of that order, to which must be added the probability of *Trigonocarpon* and *Nœggerathia* being Gymnospermous, and allied to *Salisburia.** On the other hand, it must not be overlooked that no Coniferous strobili have been hitherto detected in the Carboniferous formation.

Cycadeæ. Some fragments of wood, presenting a striking similarity in anatomical characters to that of *Cycadeæ*, have been found in the carboniferous series.

In the absence of the fructification of *Calamites*, *Calamodendron*, *Halonia*, *Anabathra*, etc., there are no materials for any safe conclusions as to their immediate affinities, beyond that they all seem to be allied to Ferns or *Lycopodiaceæ*. But the same can hardly be said of the affinities of *Volkmannia*,† *Antholites*, and others, which have been referred, with more or less probability, to Angiospermous Dicotyledons.

The Permian Flora is for the most part specifically distinct from the Carboniferous, but many of its genera are the same. The prevalent types are Gymnospermous Dicotyledons, especially *Cycadeæ*, and a great abundance of Tree-ferns.

The New Red Sandstone, or Trias group, presents plants more analogous to those of the Oolite than to those of the Carboniferous epoch, but they have also much in common with the latter. *Voltzia*, a remarkable genus of Conifers, appears to be peculiar to this period.

In the Lias numerous species of *Cycadeæ* have been found, with various Conifers and many Ferns. No other Dicotyledonous or any Monocotyledonous plants have as yet been discovered, but it is difficult to believe that none such should have existed at a period when wood-boring and herb-devouring insects, belonging to modern genera, were extremely abundant, as has been proved by the researches of Mr. Brodie and Mr. Westwood.‡

The Oolite contains numerous *Cycadeæ*, *Coniferæ*, and Ferns, and more herbivorous genera of insects; and here Monocotyledonous vegetables are recognizable in *Podocarya* and other Pandaneous plants. A cone of *Pinus* has been discovered in the Purbeck, and one of *Araucaria* in the inferior Oolite of Somersetshire.

* Phil. Trans., 1855, p. 149.

† See Quarterly Journal of the Geological Society, May, 1854.

‡ These insects include species of the existing common European genera, *Elater*, *Gryllus*, *Hemerobius*, *Ephemera*, *Libellula*, *Panorpa*, and *Carabus*. Of all conspicuous tribes of plants the *Cycadeæ*, *Filices*, *Coniferæ*, and *Lycopodiaceæ* perhaps support the fewest insects, and the association of the above-named insects with a vegetation consisting solely or mainly of plants of these orders is quite inconceivable.

In the Cretaceous group, Dicotyledons of a very high type appear. A good many species are enumerated* by Dr. Debey, of Aix-la-Chapelle, including a species of *Juglans*, a genus belonging to an order of highly-developed floral structure and complex affinities.†

Characeæ appear for the first time at this epoch, and are apparently wholly similar in structure to those of the present day.

The Tertiary strata present large assemblages of plants of so many existing genera and orders, that it can hardly be doubted but that even the earliest Flora of that period was almost as complex and varied as that of our own. In the lowest Eocene beds are found *Anonaceæ*, *Nipa*, *Acacia*, and *Cucurbitaceæ*.‡ In the Bagshot sands some silicified wood has been found, which may confidently be referred to *Banksia*, and which is, in fact, scarcely distinguishable from recent and fossil Australian Banksia wood.§

In the brown coal of the Eocene and Miocene periods, Fan-palms, Conifers, and various existing genera of *Myriceæ*, *Laurineæ*, and *Plataneæ* are believed to have been identified. Wesel and Weber describe from the brown coal of the Rhine a rich and varied Flora, representing numerous families never now seen associated, and including some of the peculiar and characteristic genera of the Australian, South African, American, Indian, and European floras.‖

* Quart. Journ. Geol. Soc., vii, pt. 1, misc., p. 110.

† Professor Oswald Heer, of Zurich, in an interesting little paper (Quelques Mots sur les Noyers), in Bibl. Univ. Genev., Sep. 1858, argues from the fact of the early appearance of *Juglans* in the geological series, that this genus must be a low type of the Dicotyledonous class to which it belongs. The position of *Juglans* is unsettled in the present state of our classification of Dicotyledonous orders, as it has equal claims to be ranked with *Terebinthaceæ*, which are very high in the series, and with *Cupuliferæ*, which are placed very low; and were the grounds for our thus ranking these orders based on characters of ascertained relative value, such an argument might be admissible; but the system which sunders these orders is a purely artificial one, and *Juglans* with its allies would prove it so, if other proofs were wanting; for it absolutely combines *Terebinthaceæ* and *Cupuliferæ* into one natural group, in which (as in so many others) there is a gradual passage from great complexity of floral organs to great simplicity.

‡ I am far from considering the identification of these and the other genera which I have enumerated in various strata as satisfactory, but I conclude that they may be taken as evidence of as highly developed and varied plants having then existed as are now represented by these genera.

§ I am indebted to the late Robert Brown for this fact, and for the means of comparing the specimens, which are beautifully opalized. I ascertained that he was satisfied with the evidence of this wood having really been dug up near Staines, though it is so perfectly similar in every respect to the opalized Banksia-wood of Tasmania as to suggest to his mind and my own the most serious doubts as to its English origin.

‖ See Quart. Journ. Geol. Soc., xv, misc. 3, where an abstract is given, with some excellent cautions, by C. J. F. Bunbury, Esq. The Australian genera include *Eucalyptus*, *Casuarina*, *Leptomeria*, *Templetonia*, *Banksia*, *Dryandra*, and *Hakëa*. I am not prepared to assert that these identifications, or the Australian ones of the Molasse, are all so unsatisfactory that the evidence of Australian types in the brown coal and Mollasse should be altogether set aside; but I do consider that not one of the above-named genera is identified at all satisfactorily, and that many of them are not even problematically decided.

In the Mollasse and certain Miocene formations at Œningen and elsewhere in Germany, Switzerland, and Tuscany,* 900 species of Dicotyledons† have been observed, all apparently different from existing ones. They have been referred, with more or less probability, to Fan-palms, Poplars (three species), evergreen *Laurineæ*, *Ceratonia*, *Acacia*, *Tamarindus*, *Banksia*, *Embothrium*, *Grevillea*, *Cupressus*, several species of *Juglans* (one near the North American *J. acuminata*, another near the common walnut of Europe and Asia, *J. regia*, and a third near the North American *J. cinerea*); also a Hickory, near the *Carya alba* (a genus now wholly American), and a *Pterocarya* closely allied to *P. Caucasica*.

The rise of the Alps was subsequent to this period; and in the European deposits immediately succeeding that event, in Switzerland (at Durnten and Utznach) are found evidences of the following existing species,—Spruce, Larch, Scotch Fir, Birch, a Hazel (different from that now existing), *Scirpus lacustris*, *Phragmites communis*, and *Menyanthes trifoliata*.

The glacial epoch followed, during and since which there has probably been little generic change in the vegetation of the globe.

32. So much for the main facts hitherto regarded as established in Vegetable Palæontology. They are of little value as compared with those afforded by the Animal Kingdom, even granting that they are all well made out, which is by no means the case. In applying them theoretically to the solution of the question of creation and distribution, the first point which strikes us is the impossibility of establishing a parallel between the successive appearances of vegetable forms in time, and their complexity of structure or specialization of organs, as represented by the successively higher groups in the natural method of classification. Secondly, that the earliest recognizable Cryptogams

* During the printing of this sheet I have received from my friend M. DeCandolle a very interesting memoir on the tertiary fossil plants of Tuscany, by M. C. Gaudin and the Marquis C. Strozzi, in which some of the genera here alluded to are described. The age of these Tuscan beds is referred by Prof. O. Heer to a period intermediate between those of Utznach and Œningen. The most important plants described are, Coniferæ 6 sp., Salix 2, Liquidambar 1, Alnus 1, Carpinus 1, Populus 2, Fagus 1, Quercus 5, Ulmus 2, Planera 1, Ficus 1, Platanus 1, Oreodaphne 1, Laurus 2, Persea 1, Acer 2, Vitis 1, Juglans 4, Carya 1, Pterocarya 1. There are 49 extinct species in all, of which 46 are referred, without even a mark of doubt or caution, to existing genera, and this in almost all cases from imperfect leaves alone! Without questioning the good faith or ability of the authors of this really valuable and interesting memoir, I cannot withhold my protest against this practice of making what are at best little better than surmises, appear under the guise of scientifically established identifications. What confidence can be placed in the positive reference of supposed fossil Fungi to *Sphæria*, or of pinnated leaves to *Sapindus*, and other fragments of foliage to existing genera of *Laurineæ*, *Ficus*, and *Vitis*!

† O. Heer, Sur les Charbons feuilletés de Durnten et Utznach, in Mem. Soc. Helvet. Sc. Nat. 1857; Bibl. Univers. Genev., August, 1858.

should not only be the highest now existing, but have more highly differentiated vegetative organs than any subsequently appearing; and that the dicotyledonous embryo and perfect exogenous wood with the highest specialized tissue known (the coniferous, with glandular tissue*), should have preceded the monocotyledonous embryo and endogenous wood in date of appearance on the globe, are facts wholly opposed to the doctrine of progression, and they can only be set aside on the supposition that they are fragmentary evidence of a time further removed from that of the origin of vegetation than from the present day; to which must be added the supposition that types of *Lycopodiaceæ*, and a number of other orders and genera, as low as those now living, existed at that time also.

33. Another point is the evidence,† said to be established, of genera now respectively considered peculiar to the five continents having existed cotemporaneously at a comparatively recent geological epoch in Europe, and the very close affinity, if not identity, of some of these with existing species. The changes in the level and contour of the different parts of the earth's surface which have occurred since the period of the chalk, or even since that preceding the rise of the Alps, imply a very great amount of difference between the past and present relations of sea and land and climate; and it is no doubt owing to these changes that the *Araucariæ*, which once inhabited England, are no longer found in the northern hemisphere, and that the Australian genera which inhabited Europe at a period preceding the rise of the Alps have since been expelled.

34. Such facts, standing at the threshold of our knowledge of vegetable palæontology, should lead us to expect that the problem of distribution is an infinitely complicated one, and suggest the idea that the mutations of the surface of our planet, which replace continents by oceans, and plains by mountains, may be insignificant measures of time when compared with the duration of some existing genera and perhaps species of plants, for some of these appear to have outlived the slow submersion of continents.

* The vexed question of the true position of Gymnospermous plants in the Natural System assumes a somewhat different aspect under the view of species being created by progressive evolution. In the haste to press the recent important discoveries in vegetable impregnation and embryogeny into the service of classification, the long-established facts regarding the development of the stem, flower, and reproductive organs themselves of Gymnospermous plants have been relatively underrated or wholly lost sight of; and if an examination of the doctrines of progression and variation lead to a better general estimation of the comparative value of the characters presented by these organs, the acceptance or rejection of the doctrines themselves is, in the present state of science, a matter of secondary importance.

† See fifth foot-note of p. 307 (‖): what I have there said of the supposed identifications of the Australian genera applies to many of those of the other enumerated quarters of the globe.

35. From the sum then of our theories, as arranged in accordance with ascertained facts, we may make the following assumptions:—That the principal recognized families of plants which inhabited the globe at and since the Palæozoic period still exist, and therefore have as families survived all intervening geological changes. That of these types some have been transferred, or have migrated, from one hemisphere to another. That it is not unreasonable to suppose that further evidence may be forthcoming which will show that all existing species may have descended genealogically from fewer pre-existing ones; that we owe their different forms to the variation of individuals, and the power of limiting them into genera and species to the destruction of some of these varieties, etc., and the increase of individuals of others. Lastly, that the fact of species being with so much uniformity the ultimate and most definable group (the leaves as it were of the family tree), may possibly be owing to the tendency to vary being checked, partly by the ample opportunities each brood of a variety possesses of being fertilized by the pollen of its nearest counterpart, partly by the temporary stability of its surrounding physical conditions, and partly by the superabundance of seeds shed by each individual, those only vegetating which are well suited to existing conditions. An appearance of stability is also, in the case of many perennials, due to the fact that the individuals normally attain a great age,* and thus survive many generations of other species, of which generations some present characters foreign to their parents.

36. In the above line of argument I have not alluded to the question of the origin of those families of plants which appear in the earliest geological formations, nor to that of vegetable life in the abstract, conceiving these to be subjects upon which, in the present state of science, botany throws no light whatever. Regarded from the classificatory point of view, the geological history of plants is not altogether favorable to the theory of progressive development, both because the earliest ascertained types are of such high and complex organization,† and because there are no known fossil plants which we can certainly assume to be-

* In considering the relative amount and rate at which different plants vary, it should be remembered that we habitually estimate them not only loosely but falsely. We assume annuals to be more variable than perennials, but we probably greatly overrate the amount to which they really are so, because a brief personal experience enables us to study many generations of an annual under many combinations of physical conditions; whereas the same experience embraces but a fractional period of the duration of (comparatively) very few perennials. It has also been well shown by Bentham (in his paper on the British Flora, read [1858] before the Linnæan Society) that an appearance of stability is given to many varieties of perennials, through their habitual increase by buds, offsets, etc., which propagate the individual; and in the case of *Rubi*, which comparatively seldom propagate by seed, a large tract of ground may be peopled by parts of a single individual.

† I have elsewhere stated that I consider the evidence of *Algæ* having existed at a period preceding vascular Cryptogams to be of very little value. (Lond. Journ. Bot., viii, p. 254.)

long to a non-existing class or even family, or that are ascertained to be intermediate in affinity between recent classes or families.*

The progress of investigation may ultimately reveal the true history of the unrecognized vegetable remains with which our collections abound, and may discover to us amongst them new and unexpected organisms, suggesting or proving a progressive development; but in the meantime the fact remains that the prominent phenomena of vegetable palæontology do not advance us one step towards a satisfactory conception of the first origin of existing natural orders of plants.

Taking the Conifers for an example, whatever rank is given to them by the systematist, that they should have preceded Monocotyledons and many Dicotyledons in date of appearance on the globe, is a fact quite incompatible with progressive development in the scientific acceptation of the term, whilst to argue from their apparently early appearance that they are low in a classificatory system is begging the question.

Another fact to be borne in mind is, that we have no accurate idea of what systematic progression is in botany. We know little of high and low in the Vegetable Kingdom further than is expressed by the sequence of the three classes, Dicotyledons, Monocotyledons, and Acotyledons; and amongst Acotyledons, of Thallogens being lower than Acrogens, and of these that the Mosses, etc., are lower than Filices and their allies. It is true that we technically consider multiplication and complexity of floral whorls in phænogamic plants as indications of superior organization; but very many of the genera and orders most deficient in these respects are so manifestly reduced members of others, which are indisputably the most complex in organization in the whole Vegetable Kingdom, that no good classification even has been founded on these considerations alone.†

* It must not be supposed that in saying this I am even expressing a doubt as to there having been plants intermediate in affinity between existing orders and classes. Analogy with the animal kingdom suggests that some at any rate of the plants of the coal epoch do hold such a relationship; but should they not do so, I consider this fact to be of little value in the present inquiry, for I incline to believe that the ascertained geological history of plants embraces a mere fraction of their whole history.

† The subject of the retrogression of types has never yet been investigated in botany, nor its importance estimated in inquiries of this nature. To whatever order we may grant the dignity of great superiority or complexity, we find that order containing groups of species of very simple organization; these are moreover often of great size and importance, and of wide geographical distribution. Such groups, if regarded *per se*, appear to be far lower in organization than other groups which are many degrees below them in the classified series; and our only clue to their real position is their evident affinity with their complex co-ordinates;—destroy the latter by a geological or other event, and all clue to the real position of the former may be lost. Are such groups of simply-constructed species created by retrogressive variation of the higher, or did the higher proceed from them by progressive variation? If the latter, did the simpler forms precede in origin the highest forms of all other groups which rank below them in the classified series?

37. Again, it is argued by both Mr. Darwin and Mr. Wallace that the general effects of variation by selection must be to establish a general progressive development of the whole animal kingdom. But here again in botany we are checked by the question, What is the standard of progression? Is it physiological or morphological? Is it evidenced by the power of overcoming physical obstacles to dispersion or propagation, or by a nice adaptation of structure or constitution to very restricted or complex conditions? Are cosmopolites to be regarded as superior to plants of restricted range, hermaphrodite plants to unisexual, parasites to self-sustainers, albuminous-seeded to exalbuminous, gymnosperms to angiosperms, water plants to land, trees to herbs, perennials to annuals, insular plants to continental? and, in fine, what is the significance of the multitudinous differences in point of structure and complexity, and powers of endurance, presented by the members of the Vegetable Kingdom, and which have no recognized physiological end and interpretation, nor importance in a classificatory point of view? It is extremely easy to answer any of these questions, and to support the opinion by a host of arguments, morphological, physiological, and teleological; but any one gifted with a quick perception of relations, and whose mind is stored with a sufficiency of facts, will turn every argument to equal advantage for both sides of the question.

To my mind, however, the doctrine of progression, if considered in connection with the hypothesis of the origin of species being by variation, is by far the most profound of all that have ever agitated the schools of Natural History, and I do not think that it has yet been treated in the unprejudiced spirit it demands. The elements for its study are the vastest and most complicated which the naturalist can contemplate, and reside in the comprehension of the reciprocal action of the so-called inorganic on the organic world. Granting that multiplication and specialization of organs is the evidence and measure of progression, that variation explains the *rationale* of the operation which results in this progression, the question arises, What are the limits to the combinations of physical causes which determine this progression, and how can the specializing power of Nature stop short of causing every race or variety ultimately to represent a species? While the psychological philosophers persuade us that we see the tendency to specialize pervading every attribute of organic life, mental and physical; and the physicists teach that there are limits to the amount and duration of heat, light, and every other manifestation of physical force which our senses present or our intellects perceive, and which are all in process of consumption; the reflecting botanist, knowing that his ultimate results must accord with these facts, is perplexed at feeling that he has failed to establish on independent evidence the doctrines of variation and progressive specialization, or to co-ordinate his attempts to do so with the successive discoveries in physical science.

38. Before dismissing this subject, I may revert once more to the opposite doctrine, which regards species as immutable creations, and this principally to observe that the arguments in its favor have neither gained nor lost by increased facilities for investigation, or by additional means for observation. The facts are unassailable that we have no direct knowledge of the origin of any wild species; that many are separated by numerous structural peculiarities from all other plants; that some of them invariably propagate their like; and that a few have retained their characters unchanged under very different conditions and through geological epochs. Recent discoveries have not weakened the force of these facts, nor have successive thinkers derived new arguments from them; and if we hence conclude from them that species are really independent creations and immutable, though so often illimitable, then is all further inquiry a waste of time, and the question of their origin; and that of their classification in Genera and orders, can, in the present state of science, never be answered; and the only known avenues to all means of investigation must be considered as closed till the origin of life itself is brought to light.

39. Of these facts the most important, and indeed the only one that affords a tangible argument, is that of genetic resemblance. To the tyro in Natural History all similar plants may have had one parent, but all dissimilar plants must have had dissimilar parents. Daily experience demonstrates the first position, but it takes years of observation to prove that the second is not always true. There are, further, certain circumstances connected with the pursuit of the sciences of observation which tend to narrow the observer's views of the attributes of species; he begins by examining a few individuals of many extremely different kinds or species, which are to him fixed ideas, and the relationships of which he only discovers by patient investigation; he then distributes them into genera, orders, and classes, the process usually being that of reducing a great number of dissimilar ideas under a few successively higher general conceptions; whilst with the history of the ideas themselves, that is, of species, he seldom concerns himself. In a study so vast as botany, it takes a long time for a naturalist to arrive at an accurate knowledge of the relations of genera and orders if he aim at being a good systematist, or to acquire an intimate knowledge of species if he aim at a proficiency in local floras, and in both these pursuits the abstract consideration of the species itself is generally lost sight of; the systematist seldom returns to it, and

the local botanist, who finds the minutest differences to be hereditary in a limited area, applies the argument derived from genetic resemblance to every hereditarily distinct form.

40. It has been urged against the theory that existing species have arisen through the variation of pre-existing ones and the destruction of intermediate varieties, that it is a hasty inference from a few facts in the life of a few variable plants, and is therefore unworthy of confidence, if not of consideration; but it appears to me that the opposite theory, which demands an independent creative act for each species, is an equally hasty inference from a few negative facts in the life of certain species,* of which some generations have proved invariable within our extremely limited experience. These theories must not, however, be judged of solely by the force of the very few absolute facts on which they are based. There are other considerations to be taken into account, and especially the conclusions to which they lead, and their bearing upon collateral biological phenomena, under which points of view the theory of independent creations appears to me to be greatly at a disadvantage. For according to it every fact and every phenomenon regarding the origin and continuance of species, but that of their occasional variation, and their extinction by natural causes, and regarding the *rationale* of classification, is swallowed up in the gigantic conception of a power intermittently exercised in the development, out of inorganic elements, of organisms the most bulky and complex as well as the most minute and simple; and the consanguinity of each new being to its pre-existent nearest ally, is a barren fact, of no scientific significance or further importance to the naturalist than that it enables him to classify. The realization of this conception is of course impossible; the boldest speculator cannot realize the idea of a highly organized plant or animal starting into life within an area that has been the field of his own exact observation† and research; whilst the more cautious advocate hesitates about admitting the origin of the simplest organism under such circumstances, because it compels his subscribing to the doctrine of the "spontaneous generation" of living beings of every degree of complexity in structure and refinement of organization.

* See paragraph 4, where I have stated that the grand total of unstable species probably exceeds that of the stable.

† It is a curious fact (illustrative of a well-known tendency of the mind), that the few writers who have in imagination endeavored to push the doctrine of special creations to a logical issue, either place the scene of the creative effort in some unknown, distant, or isolated corner of the globe, removed far beyond the ken of scientific observation, or suppose it to have been enacted at a period when the physical conditions of the globe differed both in degree and kind from what now obtain; thus in both cases arguing *ad ignotum ab ignoto.*

On the other hand, the advocate of creation by variation may have to stretch his imagination to account for such gaps in a homogeneous system as will resolve its members into genera, classes, and orders; but in doing so he is only expanding the principle which both theorists allow to have operated in the resolution of some groups of individuals into varieties. And if, as I have endeavored to show, all those attributes of organic life which are involved in the study of classification, representation, and distribution, and which are barren facts under the theory of special creations, may receive a rational explanation under another theory, it is to this latter that the naturalist should look for the means of penetrating the mystery which envelops the history of species,—holding himself ready to lay it down when it shall prove as useless for the further advance of science, as the long serviceable theory of special creations, founded on genetic resemblance, now appears to be.

The arguments deduced from genetic resemblance being (in the present state of science), as far as I can discover, exhausted, I have felt it my duty to re-examine the phenomena of variation in reference to the origin of existing species. These phenomena I have long studied independently of this question; and when treating either of whole floras or of species, I have made it my constant aim to demonstrate how much more important and prevalent this element of variability is than is usually admitted, as also how deep it lies beneath the foundations of all our facts and reasonings concerning classification and distribution. I have hitherto endeavored to keep my ideas upon variation in subjection to the hypothesis of species being immutable; both because a due regard to that theory checks any tendency to careless observation of minute facts, and because the opposite one is apt to lead to a precipitate conclusion that slight differences have no significance; whereas, though not of specific importance, they may be of high structural and physiological value, and hence reveal affinities that might otherwise escape us. I have already stated how greatly I am indebted to Mr. Darwin's* *rationale* of the phenomena of variation and natural selection in the production of species; and though it does not positively establish the doctrine of creation by variation, I expect that every additional fact and observation relating to species will gain great additional value from being viewed in reference to it, and that it will materially assist in developing the principles of classification and distribution.

* In this Essay I refer to the brief abstract only (Linn. Journ.) of my friend's views, not to his work now in the press, a deliberate study of which may modify my opinion on some points whereon we differ. Matured conclusions on these subjects are very slowly developed.

[This extensive Essay proceeds to consider the Flora of Australia under § 1. General Remarks. 2. Estimate of the numbers, distribution, and affinity of the Classes, Orders, &c. 3. The Australian distribution of Natural Orders. 4. The Genera of the Australian Flora. 5. The Tropical Australian Flora. 6. The Flora of Extra-tropical Australia. 7. The Flora of Countries around Spencer's Gulf. 8. The Tasmanian Flora, an analysis of its elements and the geographical distribution of the species and their allies. 9. The New Zealand and Polynesian features of the Australian vegetation. 10. The Antarctic plants of Australia. 11. The South African features of Australian vegetation. 12. The European features of the Australian Flora. 13. On the Fossil Flora of Australia and its Geology in relation to the existing Flora. 14. On some of the naturalized Plants of Australia. 15. A List of some of the Esculent Plants of Australia. 16. Outlines of the progress of Botanical Discovery in Australia, etc.—From the large amount of matter suitable to our pages, we have space for only two of the shorter of the sections.]

General Remarks on the Flora of Australia.

The Flora of Australia has been justly regarded as the most remarkable that is known, owing to the number of peculiar forms of vegetation which that continent presents. So numerous indeed are the peculiarities of this Flora, that it has been considered as differing fundamentally, or in almost all its attributes, from those of other lands; and speculations have been entertained that its origin is either referable to another period of the world's history from that in which the existing plants of other continents have been produced, or to a separate creative effort from that which contemporaneously peopled the rest of the globe with its existing vegetation; whilst others again have supposed that the climate or some other attribute of Australia has exerted an influence on its vegetation, differing both in kind and degree from that of other climates. One of my objects in undertaking a general survey of the Australian Flora, has been to test the value of the facts which have given rise to these speculations, and to determine the extent and comparative value of a different and larger class of facts which are opposed to them, and which might also give some clue to the origin of the flora, and thus account for its peculiarities. This I pursued under the impression that it is the same with the study of whole floras as of single species or their organs, viz., that it is much easier to see peculiarities than to appreciate resemblances; and that important general characters which pervade all the members of a family or flora, are too often overlooked or undervalued, when associated with more conspicuous differences which enable us to dismember them. The result has proved, as I anticipated, that, the great difficulty being surmounted of collecting all the materials and so classifying them as to allow of their being generalized upon, the peculiarities of the flora, great though they be, are found to be more apparent than real, and to be due to a multitude of specialities affecting the species, and to a certain extent the genera, but not extending to the more important characteristics of the vegetation, which is not fundamentally different from that of other parts of the globe.

Before proceeding to the discussion of the elements of the Australian Flora, I shall shortly describe its general character, viewed in the double light of a peculiar vegetation and as a part of the existing flora of the globe. Its chief peculiarities are:—

That it contains more genera and species peculiar to its own area, and fewer plants belonging to other parts of the world, than any other country of equal extent. About two-fifths of its genera, and upwards of seven-eighths of its species are entirely confined to Australia.

Many of the plants have a very peculiar habit or physiognomy, giving in some cases a character to the forest scenery (as *Eucalypti*, *Acaciæ*, *Proteaceæ*, *Casuarinæ*, *Coniferæ*), or are themselves of anomalous or grotesque appearance (as *Xanthorrhœa*, *Kingia*, *Delabechea*, *Casuarina*, *Banksia*, *Dryandra*, etc.).

A great many of the species have anomalous organs, as the pitchers of *Cephalotus*, the deciduous bark and remarkable vertical leaves of the *Eucalypti*, the phyllodia of *Acacia*, the fleshy peduncle of *Exocarpus*, the inflorescence and ragged foliage of many *Proteaceæ*.

Many genera and species display singular structural peculiarities, as the ovules of *Banksia*, calyptra of *Eucalyptus*, stigma of *Goodeniaceæ*, staminal column of *Stylidium*, irritable labellum of various *Orchideæ*, flowers sunk in the wood of some *Leptospermeæ*, pericarp of *Casuarina*, receptacle and inner staminodia of *Eupomatia*, stomata of *Proteaceæ*.

On the other hand, if, disregarding the peculiarities of the flora, I compare its elements with those of the floras of similarly situated large areas of land, or with that of the whole globe, I find that there is so great an agreement between these, that it is impossible to regard Australian vegetation in any other light than as forming a peculiar, but not an aberrant or anomalous, botanical province of the existing Vegetable Kingdom. I find:—

That the relative proportions of the great classes of Monocotyledons to Dicotyledons, of genera to orders, and of species to genera, are the same as those which prevail in other floras of equal extent.

That the subclasses distinguished by a greater or less complexity of the floral envelops, or their absence, as *Thalamifloræ*, *Calycifloræ*, *Corollifloræ*, etc., are also in the same relative proportions as prevail in other floras.

That the proportion of Gymnospermous plants to other Dicotyledons is not increased.

That all the Australian natural orders, with only two small exceptions, are also found in other countries; that most of those most widely diffused in Australia are such as are also the most widely distributed over the globe; and that Australia wants no known order of general distribution.

That the only two absolutely peculiar natural orders contain together only three genera, and very few species; they are, further comparatively local in Australia, and are rather aberrant forms of existing natural families than well-marked isolated groups: *Brunoniaceæ* being intermediate between *Goodeniaceæ* and *Compositæ*, and *Tremandreæ* between *Polygaleæ* and *Buettneriaceæ*.

That the large natural orders and genera, which, though not absolutely restricted to Australia, are there very abundant in species and rare elsewhere, and for which I shall hence adopt the term Australian, stand in very close relationship to groups of plants which are widely spread over the globe (as *Epacrideæ* to *Ericeæ*, *Goodeniaceæ* to *Campanulaceæ*, *Stylideæ* to *Lobeliaceæ*, *Casuarineæ* to *Myriceæ*).

That these Australian orders are exceedingly unequally distributed in Australia; that there is a greater specific difference between two quarters of Australia (southeastern and southwestern) than between Australia and the rest of the globe; and that the most marked characteristics of the flora are concentrated at that point which is geographically most remote from any other region of the globe.

That most of those Australian orders and genera which are found in other countries around Australia, have their maximum development in Australia at points approximating in geographical position towards those neighboring countries. Thus the peculiarly Indian features of the flora are most developed in northwestern Australia, the Polynesian and Malayan in northeastern, the New Zealand and South American in southeastern, and the South African in southwestern Australia.

That of the nine largest natural orders, which together include a moiety of the Australian species of flowering plants, no fewer than six belong to the nine largest natural orders of the whole world, and five belong to the largest in India also.

That in Australia itself, in advancing from the tropics to the coldest latitudes, or from the driest to the most humid districts, or from the interior to the seashore, or in ascending the mountains, the changes in vegetation are in every aspect analogous to what occur in other parts of the globe.

That the relations between the epochs of the flowering and the fruiting of plants, and the seasons of the year, are the same in Australia as elsewhere, and most remarkably so; the *Orchidæ* being spring flowers, the *Leguminosæ* summer, the *Compositæ* autumn, and the *Cryptogamia* winter.

That the peculiarities of the Australian flora in no way disturb the principles of natural arrangement derived from the study of the flora of the globe apart from that of Australia. For, after having attempted to consider the Australian vegetation in a classificatory point of view, shutting out of my view, as far as I could, that of other countries, I have been led to the conclusion that the authors of the Natural System—Ray, Linnæus,* and the Jussieus—might have developed the same Natural System had they worked upon Australian plants instead of upon European.

I find further, that the classes, orders, genera, and species, may be about as well (or as ill) fixed or limited by a study of their Australian members as by those of any other country similarly circumstanced; and that there is the same vagueness as to the exact limits of natural groups, a similar inequality amongst them in numerical value and botanical characters, and an analogous difficulty in forming subclasses intermediate between classes and orders, as other floras present. The Australian flora, in short, neither breaks down nor improves the Natural System of plants as a whole, though it throws great light on its parts; the Australian genera fall into their places in that system well enough, though that system was developed before Australia was known botanically, and was chiefly founded upon a study of the vegetation of its antipodes.

Thus, whether the Australian flora is viewed under the aspect of its morphology and structure, as exhibited by its natural classification, or its numerical proportions or geographical distribution, it presents essentially the same primary features as do those of the other great continents: and it hence appears to me rash to assume that its origin belongs to another epoch of the earth's

* The real merits of Linnæus as a founder of the Natural System have never been appreciated. In the well deserved admiration of the genius and labors of the Jussieus, it is forgotten that the powers displayed by Linnæus in constructing the Genera Plantarum was not less (perhaps greater) than that exercised in grouping these into those genera of a higher value, which are now called Jussieuan Orders. The history of our Natural System presents but four salient points:—I. Ray's division of all plants into Phænogams and Cryptogams, and of the former into Monocotyledons and Dicotyledons. II. Linnæus's forming natural groups called genera, and rendering a knowledge of them acccessible to scientific minds by means of a binomial nomenclature and a mixed natural and artificial system of classes and orders. III. The Jussieus' combining most of the genera of Linnæus into truly natural orders, under Ray's classes, which classes they divided into subclasses as artificial as many of Linnæus's classes were. IV. The separation of Gymnosperms, by Brown, which is the first step towards a natural classification of the Jussieuan orders of Dicotyledons. (See Lond. Journ. of Bot. and Kew Gard. Misc., ix, 314, *note.*)

history than that of other floras, when the proportions of its classes, etc., are identically the same with these; or that it should be attributed to a distinct creative effort, if this is manifested only in effecting morphological differences requisite to constitute species and genera in our classification, without disturbing the proportions of these; or that the local influence of the Australian climate should be essentially different from that of other countries, and yet effect no physiological change in the periods of flowering and fruiting, or produce any other functional disturbances of the vegetable organisms, or affect the agency of humidity, temperature, soil, and elevation, on plants.

On the Fossil Flora of Australia, and its Geology in relation to the Existing Flora.

The fossiliferous rocks of Australia do not throw much light upon the antiquity of its existing flora, because of the hiatus which geologists seem to consider exists between the palæozoic and tertiary strata of that country. Mr. Jukes* has called attention to the curious fact that this deficient series in Australia is largely developed in Europe, and there presents such Australian forms of life as marsupiate quadrupeds, *Trigonia* and other fossil shells, together with Cycadeous plants. To the latter no importance can be attached, as this order is far more characteristic of tropical America, of India, and even of southeast Africa, than of Australia; but on the other hand the *Araucaria* of the English oolite, and other fossils alluded to at p. 308, would seem to tend to confirm Mr. Jukes's observation.

The so-called Palæozoic rocks of Australia contain fossil plants of which so little, botanically, is known, that it would be rash to speculate on their affinities, even if we knew the age of the beds they are found in, as compared with the European, which we do not. Their fossils comprise ferns of several genera, including the genus *Glossopteris*, which is found in the oolite beds of England, and in India;† *Phyllotheca*, a plant somewhat similar to *Casuarina*, but of extremely doubtful affinity; *Vertebraria*, also an Indian fossil, as to the affinities of which no plausible guess has been made; *Sphenopteris* and *Zygophyllites*, of which little more can be said. To these the Rev. W. B. Clarke‡ adds the following well-known British coal fossils,—*Lepidodendron*, *Halonia*, *Sigillaria*, *Ulodendron*, *Calamites*, and *Stigmaria*.

Many of the tertiary fossil plants of Australia would seem to be very closely allied to existing ones; these include the *Casuarina* cones of Flinders Island, the *Banksia* and *Araucaria* wood

* J. B. Jukes, 'Physical Structure of Australia,' p. 89, etc.
† M'Coy in Ann. Nat. Hist., vol. xx, p. 152.
‡ Journ. Geolog. Soc. Lond., vol. iv, p. 60.

of Tasmania, the *Banksia* cones of Victoria (which seem identical with those of *B. ericifolia*, though buried under many feet of trap). The leaves of the calcareous tuffs on the banks of the Derwent,* etc., appear however to belong to a different and warmer period.

From the above it would appear that the extinct flora of Australia was not entirely different from that now existing, and, following Mr. Jukes's line of argument, that Australia continued as dry land during the European Oolitic and Cretaceous periods. At this epoch Mr. Jukes assumes that the peculiar flora of Australia was introduced, and that the continent was again submerged during the Tertiary epoch, when it presented the appearance of two long islands, or chains of islands, one, the larger, representing the elevated land of eastern Australia and Tasmania, the other that of southeastern Australia, together with subsidiary groups in the western and northern parts of the continent.

These are the speculations of an able geologist and voyager, which I introduce without comment, and chiefly to observe that such a partition of the continent may be supposed to be favorable to the multiplication of forms of vegetable life out of fewer pre-existing ones, by the segregation of varieties. These groups of islands would present a precise analogy with the Galapagos and Sandwich groups, where we have the small islands of one Archipelago peopled by different species, and even genera. The subsequent elevation of these islets, and consequent union of them into larger ones, would further, according to Darwin's hypothesis (of the struggle of very different kinds of species and families for occupation of the soil resulting in a further separation of varieties into species), tend to enlarge the genera numerically within comparatively small geographical limits, and thus effect such a geographical distribution of plants as Australia now presents.

In our complete ignorance as to the condition of all the continents during the Palæozoic epoch, it is impossible to speculate on the earlier condition of the Australian flora. That previous to some Tertiary submersion of a great part of the continent, it was not altogether specifically different from what it now is, would appear from a fact insisted on by Mr. Jukes, that it was during such a submersion that those volcanos were active, the lavas of which now cover large tracts of southern Australia, and which we know to have buried a plant apparently identical with *Banksia ericifolia*, which is still one of the commonest trees in that part of the country: but the question of where the *Banksias* and their allies were created, and, if in other lands than

* Darwin's Journal, p. 535, and Volcanic Islands, p. 140; Strzelecki, p. 254; Milligan in Tasman. Journ., i, 131.

Australia, how they migrated thither, we have no means of answering. If the identifications of *Banksia* and other Proteaceous leaves in the Cretaceous and Miocene formations of Europe are worthy of confidence, it is possible that the Australian types may have migrated from the northern to the southern hemisphere, as, according to Darwin's speculations, the existing European plants in Australia have.

Some arguments in favor of the antiquity of the Australian flora as compared with the European may be derived from a consideration of its generic and ordinal peculiarities. If, as I have expressed it, a genus or order is rendered peculiar, that is, unlike its allies, by the extinction of the intermediate species, it follows that the greater the peculiarity the greater the number of lapsed forms. Applying this argument to the Australian flora, we must assume an extraordinary destruction of species that once linked it with the general flora of the globe, to account for its many peculiar genera, and these being represented by so many species. But as this destruction of species is primarily due to geological causes that influence climates, and so directly and indirectly lead to the extinction of species, and as geological events are of slow progress, it follows that we must regard the Australian flora as a very ancient one. Again, Darwin argues that a rich flora or fauna, marked by a preponderance of highly developed types, must have required a large area for its development: this is because, according to his view, the principle of natural selection favors the high forms, and is unfavorable to the low. Now it could easily be shown that the Australian flora is of as high a type as any in the globe, but under existing conditions has a very small area for its development, and presents fewer representatives of other floras to contend with than most; and we must hence, under these hypotheses, assume not only the antiquity of the flora, but that it was developed in a much larger area than it now occupies.

The only other geological speculation, founded upon anything like plausible grounds, that bears upon the origin of any of the plants now inhabiting Australia, is that of Mr. Darwin in reference to the European species, to which I have alluded at p. 23. It implies of course that the existing European types were introduced into the continent long subsequently to the peculiar Australian, and are plants of a later creation. I have already pointed out the difficulties attending its adoption, the chief of which is the admission of such a cold climate in the intertropical latitudes as that not merely a temperate, but a decidedly northern flora should have migrated across them; and that this migration, if conceded, must have been extensive and have introduced very many genera and species into the tropics appears likely, when we consider the fragmentary character of the assemblage of northern forms still left in Australia; for even when reduced to its most typical examples, it consists of nearly as many natural orders as species. The little colony of south Australian genera found under the equator, on Kini Balou, in Borneo, presents another difficulty, except indeed it be regarded as evidence of that previous southern migration of Australian forms from Europe to Australia, which I have just mentioned as conceivable.

There are then the Antarctic types to account for; were they of more recent introduction than the European or Australian? Darwin has alluded to the possibility of these having been transported by icebergs from higher southern latitudes, during a period of greater cold than now obtains in the southern hemisphere (as the Scandinavian and Arctic plants are supposed by Forbes to have been transported to Britain, etc., during the glacial period), and, with the north European plants already in Australia, to have ascended the mountains during the subsequent rise of temperature. This would imply that Australia was, during a cold Tertiary period, simultaneously peopled by all those Antarctic, European, and Australian types which now inhabit it, but that the latter flora was much less developed in number of species and genera than now; for I cannot but regard the Antarctic flora in the same light as the European, and as a mere fragment of a much more extensive one, whose other members perished in the battle for place waged with the European and Australian during those changes of climate and level that succeeded their first introduction. The ultimate numerical ascendancy of the Australian botanical element may have been gained during the subsequent partition of the continent into archipelagos of islands, which became so many colonies of Australian types of vegetation, prepared on the final rise of the land to descend and occupy the intermediate ground. The paucity of alpine plants of Australian genera is a fact which lends itself well to this idea; it implies that, during either the rise of land or increase of temperature, the tendency of the species of Australian type was to seek warmer regions, and that the boreal and antarctic types being better suited to a colder climate prevented to a great extent the establishment of such varieties of Australian type as might otherwise have been adapted to inhabit the same climate as themselves.

When I take a comprehensive view of the vegetation of the Old World, I am struck with the appearance it presents of there being a continuous current of vegetation (if I may so fancifully express myself) from Scandinavia to Tasmania; along, in short, the whole extent of that arc of the terrestrial sphere which presents the greatest continuity of land. In the first place, Scandinavian genera, and even species, reappear everywhere from Lapland and Iceland to the tops of the Tasmanian alps, in rapidly diminishing numbers it is true, but in vigorous development throughout. They abound on the Alps and Pyrenees, pass on to the Caucasus and Himalaya, thence they extend along the Khasia mountains, and those of the peninulas of India to those of Ceylon and the Malayan archipelago (Java and Borneo), and after a hiatus of 30°, they appear on the alps of New South Wales, Victoria and Tasmania, and beyond these again on those of New Zealand and the Antarctic Islands, many of the species remaining unchanged throughout! It matters not what the vegetation of the bases and flanks of these mountains may be; the northern species may be associated with alpine forms of Germanic, Siberian, Oriental, Chinese, American, Malayan, and finally Australian and Antarctic types; but whereas these are all, more or less, local assemblages, the Scandinavian asserts his prerogative of ubiquity from Britain to beyond its antipodes.

Next in importance and appearance along the arc indicated is that flora which may be called Himalayan,* and which consists of the endemic plants of that range, with a mixture of Siberian, Caucasian, and Chinese genera; this, gathering strength in its progress southeastward along the ranges of northern and eastern India, occupies the flanks of all the mountain-chains I have enumerated between the Caucasus and Malay Islands; but there the Himalayan flora disappears, and does not reappear in Australia or New Zealand, and scarcely a trace of it is found in Polynesia.

The Malayan flora† is in many respects closely allied to the Himalayan, but is wholly tropical in character. This also very gradually appears in the valleys of the western and central Himalaya, and multiplying in genera and species in the eastern Himalaya and Khasia ranges, it sweeps down the Malayan peninsula, occupies all the Malayan Islands, and then it too stops short without entering Australia, being, however, continued eastward in tropical Polynesia..

Lastly, there is the flora of the plains and lower hills of India,‡ which is of a drier character than the Malayan, and is equally characteristic of Africa. This commences gradually in northwest India, or even in eastern Persia, and occupies all central India, the Gangetic plain, the whole of the Madras peninsula, except the western coast and mountains, the valley of the Irrawaddi, and the lower flat districts of the Malay Islands, whence it is continued in great force over the whole of tropical Australia.

* Characterized by *Cupuliferæ, Magnoliaceæ, Ternstrœmiaceæ, Laurineæ, Balsamineæ, Ericeæ, Fumariaceæ*, etc.

† *Vaccineæ, Rhododendron, Begoniaceæ, Quercus:* and equally typified by *Cyrtandraceæ, Dipterocarpeæ, Myristiceæ, Anonaceæ, Menispermeæ.*

‡ It consists of *Acanthaceæ, Sterculiaceæ*, and other orders, enumerated at p. xiii, *et seq.* [in the original Essay].

Reversing the position, and beginning at the southern extreme of this arc of vegetation, there is first the Antarctic flora (the complement of the Scandinavian), with its decided Australian representatives in *Centrolepideæ* and *Stylidieæ*, commencing in Fuegia, the Falklands, and Lord Auckland's and Campbell's group, reappearing in the alps of New Zealand, Tasmania, and Australia, and disappearing under the equator, on the alps of Borneo, being thus strictly confined to the southern hemisphere. Next there is the Australian flora proper, a large and highly developed one, diminishing rapidly after crossing the southern tropic, and as it advances towards the northwestern shore of the continent, reappearing in very small numbers in the Malay Islands, and terminated by a *Casuarina* on the east coast of the Bay of Bengal, and a *Stylidium* on the west. Not one representative of this vegetation advances further northwest.

Analogous appearances are presented by Africa and America. In Africa Indian forms prevail throughout the tropics, and, passing southwards, occupy the northern boundary of the south temperate zone; but there a very copious and widely different vegetation succeeds, of which but few representatives advance north to the tropic, and none to India, but with which are mingled Scandinavian genera and even species. In the New World, Arctic, Scandinavian, and North American genera and species are continuously extended from the north to the south temperate and even Antarctic zones; but scarcely one Antarctic species, or even* genus (*Forstera, Calceolaria, Colobanthus, Gunnera*, etc. etc.) advances north beyond the Gulf of Mexico.

These considerations quite preclude my entertaining the idea that the southern and northern floras have had common origin within comparatively modern geological epochs. On the contrary, the European and Australian floras seem to me to be essentially distinct, and not united by those in intervening countries, though fragments of the former are associated with the latter in the southern hemisphere. For instance, I regard the Indian plants in Australia to be as foreign to it, botanically, as the Scandinavian, and more so than the Antarctic; and that to whatever lengths the theory of variation may be carried, we cannot by it speculate on the southern flora being directly a derivative one from the existing northern. On the contrary, the many bonds of affinity between the three southern floras, the Antarctic, Australian, and South African, indicate that these may all have been members of one great vegetation, which may once have covered as large a southern area as the European now does a northern. It is true that at some anterior time these two

* *Acæna* is a remarkable exception.

floras may have had a common origin, but the period of their divergence antedates the creation of the principal existing generic forms of each. To what portion of the globe the maximum development of this southern flora is to be assigned, it is vain at present to speculate; but the geographical changes that have resulted in its dismemberment into isolated groups scattered over the Southern Ocean, must have been great indeed. Circumscribed as these floras are, and encroached upon everywhere by northern forms, their ultimate destiny must depend on that power of appropriation in the strife for place which we see in the force with which an intrusive foreign weed establishes itself in our already fully peopled fields and meadows, and of the real nature of which power no conception has been formed by naturalists, and which has not even a name in the language of biology. Everywhere, however, we see the more widely distributed, and therefore least peculiar forms of plants, spreading, and the most peculiar dying out in small areas, and the progress of civilization has introduced in man a new enemy to the scarce old forms, and a strong ally of those already common. Nor can it be doubted but that many of the small local genera of Australia, New Zealand, and South Africa, will ultimately disappear, owing to the usurping tendencies of the emigrant plants of the northern hemisphere, energetically supported as they are by the artificial aids that the northern races of man afford them.

ART. XXVII.—*On the Coloring Matter of the Privet and its application in the Analysis of Potable Waters;* by Mr. JEROME NICKLÈS.

THE berries of the privet (*Ligustrum vulgare*), which are often employed in Europe to color wines, contain, besides water and ligneous matter, a portion of glucose, a waxy substance and a beautiful crimson coloring matter, which is the principal element. This matter is soluble in water, alcohol and ether; it contains no nitrogen, and is much more stable than many allied substances. When exposed to a sufficient heat it gives a black porous charcoal, but the uncharred portions remain unchanged. It was not altered by boiling for forty-eight hours with distilled water, nor by digestion during six weeks with sulphurous acid. The fixed alkalies and their neutral carbonates turn its color to green, but the red is restored by acids so that it may be employed as a delicate test in place of litmus or the coloring matter of the dahlia. With a solution of acetate of alumina it gives a violet blue liquid, from which by boiling a fine blue lake is precipitated, which is insoluble in acetic acid, but dissolves in tartaric, citric and mineral acids to a red liquid, from which alkalies throw down again the blue lake. The basic, and even the neutral acetate of lead, yield with the red coloring matter of the privet a blue precipitate, which is soluble in acetic acid. Ammonia readily alters this coloring matter, giving rise to a yellow substance not well defined in its character. From these observations it would appear that the red coloring principle of the berries of the privet is a substance *sui-generis* and distinct from any hitherto known. I therefore propose to designate it by the name of *liguline.*

In order to obtain liguline in a state of purity, the filtered juice of the berries was precipitated by neutral acetate of lead, and the well washed lake suspended in a small quantity of water was decomposed by sulphuretted hydrogen. The residue was then thoroughly washed by ether, in which the liguline is insoluble.* Being taken up by alcohol, and again treated by acetate of lead, sulphuretted hydrogen and ether, it might be supposed to be pure. I was, however, unable to obtain concordant results in a series of elementary analyses, the carbon of the direct lead compound varying between 21·56 and 23·00 per cent, and the hydrogen from 1·89 to 2·58.

It is probable that the process described by Mr. Glénard for the preparation of *oenoline*, the red coloring matter of wines (*An. de Chim. et de Phys.*, Dec. 1858, p. 368), would be preferable for the extraction of liguline. I accordingly applied it, but the berries having been gathered too late in the season, the coloring matter had become so far altered that my trial was unsuccessful, so that the question of the elementary composition of liguline remains unsettled.

The following further observations on this coloring matter are not without interest. It is not precipitated by gelatine, which throws down the red coloring matter of wines. With hypochlorite of lime it gives a yellow color and a yellow precipitate. With chlorid of gold, a yellow color and reduction of the metal. With chlorid of platinum, no change in the cold, but a brown color by heat. With chromate of potash a green; with bichromate brown, and with sesquichlorid, and ferroso-ferric sulphate of iron the same color. Chlorine destroys the color of liguline. The chlorids of sodium, barium and mercury, the nitrates of baryta, lead, mercury and bismuth, as also the sulphates of starch, soda, lime, zinc, manganese and cadmium are without action on the coloring matter of the privet.

The bicarbonates of lime and of the alkalies (unlike the neutral alkaline carbonates which turn it to green) give a blue color with liguline, and the same is true of the chlorids and nitrates of zinc and calcium. The colors thus obtained offer however some

* The author has previously stated that the coloring matter is soluble in ether—there is apparently some error of the copyist.—Note of the Translator.

peculiar differences when seen by transmitted light; in this way the blue produced by a chlorid of zinc and bicarbonate of lime appears red, while it is green with the chlorid of calcium or the nitrate of lime or zinc. The blue color produced by a solution of bicarbonate of potash, on the contrary, offers no variation when thus viewed by transmitted light.

The recent juice of the berries of privet alters readily even when mixed with alcohol; its fine crimson color turns to red, and the liquid then mingled with a solution of bicarbonate of lime gives a gray instead of a blue color, and gives a dirty blue with acetate of lead. This change appears to depend upon the development of ammonia from the transformation of the azotized matter of the juice; when separated from these matters and isolated, on the contrary, liguline may be preserved without change, either in aqueous or alcoholic solution. Its color is then an intense crimson.

Even the strong mineral acids in the cold do not alter liguline, but in the presence of alkalies on the contrary, it is rapidly altered, although the red color can be, to a certain extent, restored by an acid. This alteration is dependent upon the absorption of oxygen, as may be shown by introducing a mixture of liguline and potash ley in a glass tube over mercury, when rapid absorption takes place.

The property of liguline to produce blue with solutions of bicarbonate of lime renders it a delicate reagent for the detection of this salt in potable waters. For this purpose it suffices to let fall a drop of an aqueous or alcoholic solution of liguline into the water, the crimson tint which this communicates to distilled water is replaced by a beautiful blue. In place of the solution we may employ a test paper impregnated with the coloring matter, which is best as prepared from the lead precipitate. We may, however, employ the recent juice of the berries, taking care to redden the paper slightly by exposing it to the vapor of acetic acid before drying.

As a reagent for the detection of bicarbonate of lime in waters, liguline is greatly to be preferred to a tincture of logwood, and the paper prepared with it becomes a valuable reagent for the laboratory as well as for the naturalist in the field. I have found by this reagent that while bicarbonate of lime is indicated in the springs which flow from the jurassic strata, and especially those that supply the city of Nancy, no change of color is produced by a solution of liguline with the waters of other streams which have their source in rocks destitute of calcareous matter.

The observations which I have given above were made for the most part with the fruit of the privet gathered in the autumn of 1856, and I have in fact indicated in a note in the Bulletin of the local Society of Acclimation for the North-West district

(Nancy, 1857, p. 121). I have delayed publication in the hope to render my research more complete by a good elementary analysis, but I am now induced to publish the results already obtained that I may claim the right to continue and complete the investigations, having learned that Mr. Glenard proposes to undertake a similar research.

In conclusion, we may remark that the coloring matter of the privet offers a great analogy with that of the wines of Villefranche isolated and examined by Mr. Glenard; this analogy is shown by their composition and their properties. Its reaction with bicarbonate of lime may render it a valuable reagent in chemical analysis. The fact that it is not precipitated by gelatine, which, as is well known, throws down the red coloring matter of wines, will serve to distinguish the two when associated. It still remains to be decided whether the coloring principle of all red wines is the same, but this is a question foreign to our present subject.

ART. XXVIII.—*On the Method of Measurements, as a diagnostic means of distinguishing Human Races, adopted by Drs. Scherzer and Schwarz, in the Austrian circumnavigatory Expedition of the "Novara"*; by JOSEPH BARNARD DAVIS.

WEIGHT and measure have been very frequently applied as means to determine the physical proportions of different human races, and to ascertain their essential diversities. But it may well be doubted whether they have ever been employed in that systematic and comprehensive manner, which will afford the results they are capable of yielding. Travellers have generally contented themselves by speaking in indefinite comparative terms of the people with whom they have come into contact. But few have submitted any considerable number of these people to the test of measurement, and thus ascertained their dimensions. Anthropology stands in need of many more accurate and extended observations, to derive the full results from these sources of knowledge.

The subject itself is a large one, and some have confined themselves to one branch of it, some to others. Where actual measurements have been carried out, many have contented themselves with taking the *stature* of a few, or a number, of the people; others have, besides, ascertained the length of the *limbs*; and a few have subjected the *head* to a series of superficial measurements. As we are fully assured that this latter division of the body is the seat of those faculties which lie at the base of all the peculiarities of human races; bearing essentially and intimately upon their manners and customs, all their institutions, their religious impulses; their capacity for civilization, and the development to which it has attained, it is not surprising that it should have attracted the chiefest attention. Besides the superficial measurements of the head, a more extensive series of observations has been made upon the bony skull itself, with a view of determining its relative proportions, for comparison in the same race, or among different races. Many observers, advancing a step nearer, have endeavored to ascertain, by measure and by weight, the *internal capacity* of this marble palace. And, lastly, some have laboriously devoted their inquiries to the great central mass of the nervons system, and availed themselves of the opportunities that have occurred to them, to determine the size and the weight of the brain, and its different parts. As this last investigation comes nearest of all to the specialties of human beings—who are so finely discriminated by Professor Owen, as *archencephala*—it is to be regretted that the occasions for research among distinct races are so few, and have been so little availed of, and the investigation itself is so elaborate and nice, that hitherto this most interesting part of anthropological anatomy is, as it were, a *tabula rasa*, to use the language of one of the most laborious inquirers in this branch of science—Prof. Huschke, of the University of Jena. It is, however, fortunate that gauging the internal capacity of the skull should afford the means of so accurate an approximation to the volume and the weight of the brain; and thus, for the comparison of these important points among the different families and tribes of men. Hence, the labors of Tiedemann, the distinguished physiologist, who, with a very amiable design, undertook to show that the brain of a negro was not smaller than that of the European—an attempt similar to that of the late Sir William Hamilton. Tiedemann might have succeeded in impressing us with his own conclusion, had he not published the tables on which this conclusion was based, and which themselves refute such an erroneous opinion. To Tiedemann succeeded Professor Morton, of Philadelphia, Professor Van der Hoeven, of Leyden, and others. Among the most recent, is Prof. Huschke, of Jena, one of whose results of whose own estimation of the capacity of the skull, and of the size of the brain, is, that the Germanic races, among whom through our Anglo-Saxon forefathers we rank, as one great branch, have the largest brains of any people. They distinctly exceed the French in this respect.

That great diversities, capable of metrical appreciation, prevail among human races is very well known. Some of the tribes of North American Indians are remarkable for their great stature. Catlin assures us that the men among the *Crows*, whose hair will frequently reach the calves of their legs, are most of them six feet or more. Other tribes are of a decidedly lower stature. Of the gigantic Patagonians of South America, the most extravagant accounts have been given by travellers. But Capt. King affirms them, upon measurement, to be from 5 feet 10 inches to 6 feet high, which is supported by the statement of M. D'Orbigny, that some are 6 ft. 3½ inches, and the medium stature is above 5 ft. 8 inches English. On the contrary, the average height of the Bushmen is only 4 feet 4 inches. This gives a range of very nearly two feet between the tallest and the shortest races of men we are acquainted with. The other races of mankind are comprised within these limits of difference. Some tribes of the Negritos average about 4 feet 8 inches; the so-called Malay races, ascending to a mean of 5 feet 3 inches. But among the Negrito tribes of the Pacific there is, as that eminent ethnologist, Mr. Crawford, has clearly shown, a great diversity of stature. They dwell in islands scattered over a large extent of ocean, and although some tribes do not reach 5 feet in height, others, as those of New Caledonia, attain to 6 feet, and individuals among them even more. In the recent expeditions to the Andaman Islands, for the purpose of selecting a spot for a penal settlement, the inhabitants are spoken of as "dwarf Negrillos," and as "men of middle size." An individual who was measured, gave a stature of 4 feet 9½ English inches. (Selections from the Records of the Government of India, No. xxv: the Andaman Islands.) Thus, in stature alone, a very great diversity prevails. And it is remarkable that tribes in close proximity to each other frequently exhibit startling contrasts. Dr. Livingstone, whose opportunities had he been an ethnologist were so extraordinary, observed in the plains of the interior of Southern Africa, scattered among the Kafirs, who are a tall, fine and robust race, the hordes of the diminutive Bushmen. He was deeply impressed with what he saw, so contrary to all his preconceptions; and expresses his great surprise that such dissimilar races should be everywhere scattered about the country without being mingled, where they have dwelt for unlimited ages, exposed to all the same influences of air, climate, food, &c. The tall Patagonians and some tribes of the Fuegians, distinguished for their dwarf stature, afford a similar example of contrast.

The brothers Schlagintweit, following in the train of Mr. Hodgson, carried on an extensive series of metrical observations on the tribes of the Himalaya and of India. Many curious results, chiefly pointing to the different proportions of parts of the bodies and limbs of these people from those of Europeans, have been attained, which will be published in the ethnological portion of their projected work. After ascertaining the weight of the individual and his strength, by means of the dynamometer, they made from 25 to 28 different measurements, chiefly of the head, and of other parts of the body and limbs. But Drs. Scherzer and Schwarz have striven, by a more complex and complete system of observation and measurement, to gain an image of the size and form of the individual, and of all his parts;—thus not merely to subserve the purposes of the anatomist, the physiologist and the ethnologist, but those of the artist also. Their more ambitious object of obtaining, in this way, to a natural classification of human races, is an evidence of laudable zeal; but we can hardly hope that their labors can do more than contribute towards the solution of this difficult problem. Although, it ought to be mentioned, that the late Baron Humboldt, a short time before his death, expressed his great satisfaction with the system of measurements of Drs. Scherzer and Schwarz; by which, he thought, we may at length arrive at a safer result in distinguishing and determining human races than by any other means.

After recording the age, weight, height, strength, color of the hair and eyes, and number of the pulsations of the radial artery, they divide their measurements into three sections, those of the *head*, the *trunk*, and the *extremities;* and of these they take no less than 70 different dimensions in all, by means of different instruments.

Their external measurements of the head are the most complete that have ever been employed. They embrace the face as well as the other parts of the head, and by means of a perpendicular line with plummet, and a small metre scale, they are able to ascertain pretty correctly the profile of the countenance. The number of their different measurements of and about the *head*, consisting of superficial distances, diameters, circumferences, &c. amounts to 31, those of the *trunk* to 18, and those of the *extremities* to 21.

When the frigate "Novara" reached Sydney, these gentlemen printed an account of their system of measurements, "for private circulation" among men of science, which is preceded by a number of ingenious observations. In these, they dwell upon the ease with which travellers intuitively discriminate the different nations and tribes of mankind; and yet the difficulty in some selected individuals and cases to carry out this diagnosis, especially when the eye is deceived by a substitution of dress; and express great confidence in a more minute examination by a systematic method of measurements. They insist with equal confidence that nature must recognize a definite plan by which man's different types are formed and distinguished; and conclude that we should dedicate the same amount of study and inquiry to the systematic arrangement of our own species, as has long been applied to thousands of species of the vegetable and animal kingdoms.

In the course of these introductory remarks they mention their examination of the Chinese inmates of the prison at Hong Kong. Among these they found persons belonging to the *Hakka Tribe*, with stout and vigorous constitutions, fine, well-shaped, aquiline or long and straight noses, and a form of the eyes not resembling the specific obliquity of other Chinese. As criminals, they had been deprived of their tails, and Drs. Scherzer and Schwarz affirm that they had such a resemblance to the figures of some Europeans of the lower class, that, by a change of dress, they might pass amongst us without being recognized. They also mention how successfully Gützlaff, Medhurst, Huc and others have travelled the Empire in a Chinese dress without detection. And, no doubt, there are individuals so capable of assuming, and, as it were, substituting, the manners and expressions of others that the ordinary and slight attention which is paid to persons on a journey and among numbers, does not suffice to discriminate them.

Still, *the rule* must run counter to such a confusion; or the statement of the Austrian voyagers could not be true—that an anthropologist on the Island of Java is able, at first view, to classify most of the Malay tribes inhabiting the larger and smaller Islands on the Indian Archipelago, without ever mistaking. And the very remarkable account of the Abbé Huc proves that if there are differences among the races of men too subtle to be detected by the eye, yet they are not the less certainly appreciable. He informs us that he and his companion successfully eluded the detection of the unsuspecting or inattentive Chinese, but that to the Chinese *dogs* they always stood at once revealed as Europeans, by their peculiar smell. "The dogs barked continually at us, and appeared to know that we were foreigners." This is not the proper time to refer to the distinguishable odors of the different races of mankind, which travellers allude to. Huc said he could easily distinguish those of the Negro, the Malay, the Tartar, the Thibetan, the Hindoo, the Arab and the Chinese. Indeed, it is the same, with those having a delicate sense of smell, as to the French and other European races. And with respect to the fact of the penetrating and offensive scent attached to man, more especially to civilized man, Mr. Galton and others, who have traversed desert countries teeming with wild animals, give distinct and prominent testimony—which testimony is, in truth, not very complimentary to us.

We have been informed, on the authority of one who has seen much of the North American Indians, that they describe an odor to them peculiarly disgusting as being attached to the Jews. A fact, which, if correct, is little accordant with the extraordinary hypotheses which would derive the Indians themselves from the lost tribes.

Finally, it may be mentioned, that by a recent communication from Dr. Scherzer we are informed that during the cruise of the "Novara," about 200 individuals of different races, but of about the same age, males and females, were subjected to measurement. The whole number of measures taken amount to nearly 12,000. Dr. Scherzer adds, that he does not consider these observations sufficient, but merely as a commencement of a system of thorough metrical examination;—that the paper on measurements has been translated into different languages, and copies of it left in the hands of physicians, and other men of science, in the different places and islands visited by the expedition, who promised to complete the observations on the aborigines, and to forward the results to Europe;—that the measurements already effected embrace those made on Negroes, Malays, Mongols, Papuans and Indians;—and that the greatest number were taken on individuals in the Nicobar Islands, Batavia, where natives of almost all the islands of the Indian Archipelago were met with; Manilla, Hong Kong, Sydney (Austral negroes), New Zealand, Tahiti (where were aborigines of New Caledonia and Norfolk Island), Chili and Peru.

The results obtained by the extensive series of measurements thus procured, will shortly be published to the world, in the volumes now in preparation at Vienna. The History of the important Voyage of the "Novara," a popular illustrated work, from the Journals of the Commanders, Commodore Wüllustorf and Dr. Scherzer, may be expected to be issued from the Imperial printing office, in Vienna, to be followed by an English translation, in the early part of the present year. It is proposed that this shall be succeeded by a number of other volumes on distinct subjects. 1. Those on nautical, astronomical, meteorological, magnetical, and other observations relating to Physical Geography, by Commodore Wüllustorf. 2. Geology, by Dr. Hochstetter. 3. Zoology, by Messrs. Frauenfeld and Zelebor. 5. Ethnography, by Dr. Scherzer. 6. Statistics and Natural Economy, by the same. 7. Medicine, (Pathological and Pharmacognostical Researches,) by Dr. Schwarz. And, lastly, 8. an Album selected from nearly 2,500 sketches made by Mr. Sellery, the artist of the expedition. Whenever this grand programme, which will have the best wishes of men of science in all countries, shall have been completed, the rich results of the first Austrian Circumnavigatory Expedition, placed as it has been in able and well instructed hands, will, we have no doubt, vindicate the national character in a new and much nobler field of enterprise; and give to that country a far more lasting and more dignified fame than any she has hitherto required.

Note.—Such is the inconvenience resulting from the use of a variety of metre scales, and such a number of methods of measurement, frequently taking quite different points for measures bearing the same name, as in the case of the skull especially, that the distinguished Professor Von Bær, of St. Petersburg, has just proposed a Congress of Anthropologists, to determine upon one uniform scale and to establish one system. By this means, all the results of measurements of the human body would be rendered of universal applicability.—*Nachrichten über die ethnog. craniol. Sammlung zu St. Petersburg. S.* 81.

ART. XXIX.—*Report of Assistant Charles A. Schott, on the latest results of the Discussion of the Secular Change of the Magnetic Declination, accompanied by tables showing the declination (variation of the needle) for every tenth year from the date of the earliest reliable observations, for twenty-six stations on the Atlantic, Gulf, and Pacific coasts of the United States.*

[Published in this Journal by permission of the Treasury Department, and communicated by Prof. A. D. BACHE, Superintendent U. S. Coast Survey.]

IN accordance with the Superintendent's letter of January 21, 1859, I have prepared a set of tables for practical use, giving the secular change of the magnetic declination and showing for every tenth year, from the date of the earliest reliable observations to the present time, the magnetic declination (commonly called the variation of the magnetic needle) for stations on or near the northeastern coast of the United States and also for some stations on our southern and western coasts—as derived from my several discussions of the secular change in which have been included the latest data in possession of the Coast Survey. For the eastern and southern coasts, the following papers may be referred to: Coast Survey report for 1855, Appendix No. 48, pp. 306–337; Coast Survey report for 1858, Appendix No. 25, pp. 192–195, and Appendix No. 26, pp. 195–197. For the western coast, Coast Survey report for 1856, Appendix No. 31, pp. 228–235 may be consulted.

In general the secular change of the declination appears to be of a periodic character, but in no instance has a whole cycle been completed on either coast. Its length therefore remains necessarily in a great measure uncertain, and the tentative analytical process so far followed has for its main object the proper representation of all reliable observations made at any one station, so as to furnish the means of interpolation and also to enable us to calculate the magnetic declination for any required place and date, within the limits of the discussion. In the investigation of 1855 a linear function was used in the discussion which does not involve the duration of the period, and on this account the results were, in regard to time, of rather limited extent (see remark on p. 337 of Report for 1855). For the western coast stations, I still prefer to retain this form of the discussion. Subsequently, by means of the knowledge gained in that discussion, an attempt was made to substitute a circular function, directly involving a period or periods, the length of which, as well as all other numerical co-efficients in the formula for the secular change, has been determined by applying the method of least squares. The use of a circular function—commenced in 1858 with two stations, is now extended to eighteen, within the limits stated above, and it has been applied to some stations in Canada, the southern coast of the United States and Central America, in order to furnish material for the generalization of the law, so far as ascertained, in reference to epochs and rates of change. A secondary period within the first was traced at several stations, its length, however, being much more variable and uncertain, was found fluctuating between one-half and one-fifth of the primary period, while its amplitude was on the average fifteen times smaller than that of the primary wave for stations forming group 1, or within the geographical limits of Portland, Burlington and Williamsburg. This smaller amplitude was found nearly constant and equal to 0°·4.

To make the present paper more complete it contains also the record of all observations used in the discussion not heretofore published in the Coast Survey reports.

As long as the cause producing the secular change remains altogether unknown, it is not safe to trust too far to the continuation of the law thus empirically derived, and in the following tables no value, deduced by the formula, has been inserted antecedent to the first observation by more than. ten years. The tabular values may therefore be regarded in the light of a strict interpolation between actual observations, and since the analytical treatment will equalize and remove, in a measure, accidental errors of observation, they may be considered as certainly more trustworthy than any single observation, particularly in cases where the number of observations available for the discussion exceeds half a dozen, properly distributed in relation to time. The probable error of any single representation will be found in the second table. For all ordinary use by the surveyor (or navigator) the tabular values are sufficiently precise, when greater accuracy is required the annual inequality of the declination and the diurnal variation for the time required must be taken into account; the former correction will probably not exceed, in any case, one minute, and the latter may amount in summer, in maximo, to minus or plus six minutes, and in winter to minus or plus three minutes, numbers which were derived from Prof. Bache's discussion of the Philadelphia observations. The tables will also answer for intermediate places, for which they furnish the necessary data of interpolation.

It is proper to state that the present formulæ should be considered as liable to future changes and improvements depending on the accumulation of additional observations, and it is hardly necessary to state that their number also may hereafter be considerably increased by the accession of new material. The utility of a publication of tables showing the declination for every tenth year was suggested by Mr. T. B. Brooks. In the numerical calculations I was assisted by Mr. G. Rumpf of the Computing Division.

Formulæ expressing the secular change of the magnetic declination (commonly called variation of the magnetic needle) used for calculating the tabular values.—Group 1. Stations between Portland, Me., and Williamsburg, Va.

A positive sign of D indicates west declination, a negative sign, east declination. n equals the number of years (and fraction of a year) from 1830; positive for years after and negative for years before this epoch. Longitudes are reckoned from Greenwich.

No.	Locality.	Lat.	Long.	
1	Burlington, Vt.	44° 27′	73° 10′	$D=+11^{\circ}.55-4^{\circ}.10\cos(1^{\circ}.30n+36^{\circ})+0^{\circ}.21\cos(7^{\circ}.2n+290^{\circ})$
2	Portland, Me.	43 39	70 16	$D=+10.70-2.63\cos(1.33n+87)$
3	Portsmouth, N. H.	43 05	70 43	$D=+10.20-2.45\cos(1.37n+72)$
4	Rutland, Vt.	43 36	72 55	$D=+9.89-3.66\cos(1.5n+45)$
5	Cambridge, Mass.	42 23	71 07	$D=+9.65-2.78\cos(1.30n+71)+0.22\cos(2^{\circ}.7n+220^{\circ})$
6	Newburyport, Ms.	42 48	70 49	$D=+9.55-2.56\cos(1.4n+78)$
7	Boston,	42 20	71 02	$D=+9.16-2.55\cos(1.39n+76)+0.22\cos(3^{\circ}.6n+222^{\circ})$
8	Providence, R. I.	41 50	71 24	$D=+9.11-2.99\cos(1.45n+58)+0.19\cos(7^{\circ}.2n+246^{\circ})$
9	Hartford, Conn.	41 46	72 40	$D=+8.60-3.59\cos(1.25n+45)$
10	New Haven, Conn.	41 17	72 55	$D=+8.13-3.49\cos(1.33n+39)$
11	Albany, N. Y.	42 39	73 43	$D=+7.65-2.74\cos(1.42n+62)$
12	Oxford, N. Y.	42 27	75 42	$D=+6.65-3.69\cos(1.3n+40)$
13	New York,	40 43	74 00	$D=+6.47-2.32\cos(1.6n+55)$
14	Philadelphia,	39 58	75 10	$D=+5.37-3.44\cos(1.6n+39)$
15	Hatboro', Pa.	40 07	75 08	$D=+5.23-3.28\cos(1.54n+47)+0.22\cos(4^{\circ}.1n+347^{\circ})$
16	Baltimore,	39 16	76 35	$D=+2.70-2.25\cos(1.5n+49)$
17	Washington, D. C.	38 53	77 00	$D=+2.42-2.0\cos(1.5n+49)$
18	Williamsburg, Va.	37 15	76 40	$D=+2.22-2.6\cos(1.5n+22)$

The following table contains the number (n) of observations (single or combined) upon which each formula is based; the probable error (E_o) of an observation expressed in minutes, as a measure of the degree of accuracy with which the observations are represented; the epoch of the last minimum of west declination (or of maximum east declination) together with the least west declination (greatest east), and lastly, the annual variation for the years 1840, 1850, and 1860, expressed in minutes. The positive sign expresses west declination increasing (east diminishing).

Locality.	n	E_o	Epoch of min. W. decl'n.	Least W. decl'n	Annual change.		
					v1840	v1850	v1860
Burlington, Vt.	9	± 5′	1813	+7·4°	+4·1′	+3·4′	+4·5′
Portland, Me.	5	14	1765	+8·1	+3·6	+3·4	+3·0
Portsmouth, N. H.	4	10	1777	+7·7	+3·5	+3 5	+3·2
Rutland, Vt.	4	18	1800	+6·2	+4·9	+5·5	+5·7
Cambridge, Mass.	22	12	1782	+6·9	+4·3	+4·3	....
Newburyport, Mass.	4	12	1774	+7·0	+3·7	+3 6	+3·3
Boston,	8	10	1782	+6·7	+4·5	+4·3	+3·7
Providence, R. I.	30	5	1779	+6·1	+5·3	+3·8	+3·0
Hartford, Conn.	6	14	1794	+5·0	+4·0	+4·4	+4·6
New Haven,	14	10	1801	+4·6	+3·8	+4·4	+4·7
Albany, N. Y.	10	3	1787	+4·9	+3·9	+4·0	+3·9
Oxford, N. Y.	10	11	1799	+3 0	+4·0	+4·6	+4·9
New York,	13	13	1795	+4·1	+3·7	+3·9	+3·8
Philadelphia,	11	16	1805	+1·9	+4·7	+5·3	+5·4
Hatboro, Pa.	18	5	1796	+1·8	+4·2	+4·3	+4·4
Baltimore,	3	13	1798	+0·5	+3·2	+3·4	+3·4
Washington, D. C.	6	8	1798	+0·4	+2·8	+3·1	+3·1
Williamsburg, Va.	3	15	1815	−0·4	+2·4	+3·2	+3·7

Table of magnetic declinations for eighteen stations forming group 1, on or near the northeastern coast of the United States, between the years 1680 and 1860. West declination is indicated by a plus sign, east declination by a minus sign, and is expressed in degrees and fractions of a degree.

Year.	Burlington, Vt.	Portland, Me.	Portsmouth, N. H.	Rutland, Vt.	Cambridge, Mass.	Newburyport, Mass.	Boston.	Providence, R. I.	Hartford, Conn.
	°	°	°	°	°	°	°	°	°
1680	....	......	....	....	....	......	....	....	.....
1690	....	......	....	....	....	......	....	....	.....
1700	....	......	....	....	+ 9·9	......	+ 9·7	....	.....
1710	....	......	....	....	9·4	......	9·0	+10·4	.....
1720	....	......	....	....	8·8	......	8·3	9·5	.....
1730	....	......	....	....	8·4	......	7·8	8·9	.....
1740	....	......	....	....	7·9	......	7·4	8·3	.....
1750	....	......	....	....	7·5	......	7·2	7·7	.....
1760	....	+ 8·1	....	....	7·2	......	7·0	6·9	.....
1770	....	8·1	+ 7·8	....	7·0	+ 7·0	6·8	6·3	.....
1780	....	8·3	7·7	....	6·9	7·0	6·8	6·1	+5·2
1790	+ 7·8	8·5	7·9	+6·3	6·9	7·2	6·8	6·3	5·0
1800	7·5	8·9	8·1	6·2	7·1	7·5	7·0	6·4	5·0
1810	7·3	9 4	8·5	6·3	7·5	7·9	7·3	6·5	5·2
1820	7·6	10·0	8·9	6·7	8·0	8·4	7·8	6·8	5·6
1830	8·30	10·6	9·4	7·3	8·58	9·0	8·41	7·46	6·1
1840	9·07	11·2	10·0	8·1	9·28	9·6	9·13	8·38	6·7
1850	9·69	11·8	10·6	8·9	10·0	10·3	9·88	9·14	7·4
1860	+10·30	+12·3	+11·2	+9·9	+.....	+10·8	+10·56	+ 9·68	+8·1

Year.	New Haven, Conn.	Albany, N. Y.	Oxford, N. Y.	New York.	Philadelphia.	Hatboro', Pa.	Baltimore.	Washington, D. C.	Williamsburg, Va.
	°	°	°	°	°	°	°	°	°
1680	......	......	......	+8·8	......	+8·5	......	......	......
1690	......	......	......	8·7	......	8·3	......	......	+4·8
1700	......	......	......	8·5	+8·8	7·9	......	......	+4·8
1710	......	......	......	8·0	8·4	7·5	......	......	......
1720	......	......	......	7·6	7·9	7·0	......	......	......
1730	......	......	......	7·0	7·1	6·3	......	......	......
1740	......	......	......	6·4	6·3	5·6	......	......	......
1750	......	......	......	5·8	5·3	4·7	......	......	......
1760	+6·1	......	......	5·2	4·4	3·8	......	......	......
1770	5·5	......	......	4·7	3·5	2·9	......	......	+1·2
1780	5·0	......	......	4·4	2·8	2·2	......	......	+0·7
1790	4·8	......	+3·0	4·2	2·2	1·8	......	......	+0·2
1800	4·6	......	3·0	4·2	2·0	1·8	......	+0·4	−0·2
1810	4·7	+5·4	3·1	4·3	1·9	2·1	+0·6	0·5	−0·4
1820	5·0	5·8	3·4	4·7	2·2	2·6	0·8	0·8	−0·4
1830	5·42	6·3	3·82	5·16	2·70	3·20	1·2	1·1	−0·2
1840	5·98	7·0	4·43	5·73	3·41	3·89	1·7	1·5	+0·1
1850	6·71	7·7	5·15	6·37	4·25	4·61	2·4	2·0	+0·6
1860	+7·46	+8·3	+5·95	+7·01	+5·19	+5·32	+2·9	+2·6	+1·2

Note.—At Cambridge, Mass., the observations after 1855 require farther examination. At Williamsburg, the values between 1700 and 1770 were not considered sufficiently reliable for insertion. The expression for Baltimore depends for length of period and time of minimum on the Washington formula.

The total number of observations upon which the tabular values and the formulæ are based is 180, the average number of any one station is 10, and the average probable error of any single representation is ±11′.

If we arrange the stations geographically, we find that at the eastern stations the minimum (west) declination occurred earlier than at the more western and southern stations; thus, from six stations between Portland and Providence it occurred about the year 1777; in the Connecticut and Hudson valleys and along the sea-coast as far south as Washington, the year of the minimum does not differ much from 1797; Williamsburg in Virginia gives 1815. The transition as we pass from the New England states is somewhat abrupt, but too well marked to be accidental. Extending the investigation farther north, I find for Quebec, Canada, the year of the minimum 1769; going farther west we find that at Toronto it must have occurred before the year 1842, and at York Fort, Hudson Bay, I find the year 1842 (as already ascertained by General Sabine, after the receipt of Capt. Blakiston's observations of 1857). This latter station is nearly halfway across the continent, and if we proceed to the western coast we find that the eastern declination there has not yet reached its maximum (equivalent to a western minimum), but it is highly probable that it will reach it before the close of the present century. The present reverse or western motion of the isogonic lines in our eastern states which commenced about the year 1777 will gradually be communicated to the more westerly stations, and will, it is highly probable, be participated in our western coast before or at the close of the present century, the direction of the motion in this latter locality being at present still to the eastward and southward, though with a diminishing rate (see p. 235 of C. S. report of 1856).

The following equations constructed for the two northernmost stations may be added here:

York Fort, Hudson Bay, $D = +5^\circ\cdot1 - 14^\circ\cdot2 \cos(1^\circ\cdot6n + 340^\circ)$
Quebec, Canada, $D = +12\cdot84 - 3\cdot7 \cos(1\cdot6n + 97)$

The second group comprises the stations on the southern portion of the Atlantic coast and Gulf coast; only three in number, to which have been added some stations located further south.

Group II. *Southern Stations.*

No.	Locality.	Lat.	Long.	Magnetic declination.
		° ′	° ′	
1	Charleston, S. C.	32 45	79 51	$D = -2^\circ\cdot12 - 2^\circ\cdot02 \cos(1^\circ\cdot55n + 56^\circ)$
2	Savannah, Ga.	32 05	81 05	$D = -2\cdot95 - 1\cdot24 \cos(1\cdot5n + 20)$
3	Mobile, Ala.	30 41	88 02	$D = -6\cdot5 - 0\cdot77 \cos(1\cdot6n + 16)$

Locality.	n	E_0	Epoch of max. east declination.	Max. east declination	Annual change.		
					1840	1850	1860
Charleston, S. C.	5	$\pm 9'$	1794	$-4^\circ\cdot1$	$+3'\cdot1$	$+3'\cdot2$	$+3'\cdot2$
Savannah, Ga.	4	12	1817	$-4\cdot2$	$+1\cdot1$	$+1\cdot5$	$+1\cdot8$
Mobile, Ala.	6	12	1820	$-7\cdot3$	$+0\cdot7$	$+0\cdot9$	$+1\cdot1$

Proceeding in a southerly direction the next station discussed outside of the boundaries of the United States is Havana, Cuba, lat. 23° 09′, long. 82° 22′, for which place I found $D = -4^\circ\cdot82 - 1\cdot45 \cos(1\cdot3n + 26^\circ)$ with 1810 as the year of maximum east declination. The values collected for Jamaica were not discussed, but the nine values I was able to obtain, will be found in the appended record. For Panama, New Granada, lat. +8° 57′, long. 79° 29′, the southernmost station discussed, I find $D = -6^\circ\cdot9 - 1^\circ\cdot04 \cos(1\cdot2n + 74^\circ)$, an equation satisfying the observations, but not considered as preferable to the following expression, $D = -5^\circ\cdot57 - 2^\circ\cdot21 \cos(1\cdot2n + 34^\circ)$, which supposes the maximum to occur in 1802.

Going westward and northward I found for Vera Cruz, Mexico, lat. 19° 12′, long. 96° 09′, $D = -4^\circ\cdot2 - 5\cdot04 \cos(1\cdot1n + 7^\circ)$, with the maximum east declination in 1824.

The following table has been calculated from the preceding equations:

Year.	Charleston, S. C.	Savannah, Ga.	Mobile, Ala.
	°	°	°
1770	−3·7	....	....
1780	−4·0	....	....
1790	−4·1	....	....
1800	−4·1	−4·1	−7·1
1810	−4·0	−4·2	−7·2
1820	−3·6	−4·2	−7·3
1830	−3·2	−4·1	−7·2
1840	−2·8	−4·0	−7·1
1850	−2·2	−3·7	−7·0
1860	−1·7	−3·5	−6·8

The following formulæ for stations of the western coast, between San Diego and Cape Disappointment, forming group 3, have been copied from p. 234 of the report for 1856.

No.	Locality.	Lat.	Long.	
		° ′	° ′	
1	San Diego,	32 42	117 13	$D=-12^{\circ}17-0{\cdot}019n+0{\cdot}00018n^2$
2	Monterey,	36 38	121 54	$D=-14{\cdot}19-0{\cdot}050n+0{\cdot}00047n^2$
3	San Francisco,	37 48	122 27	$D=-15{\cdot}14-0{\cdot}028n+0{\cdot}00025n^2$
4	Cape Mendocino,	40 25	124 22	$D=-16{\cdot}29-0{\cdot}029n$
5	Cape Disappointment,	46 17	124 02	$D=-19{\cdot}65-0{\cdot}019n$

The total number of observations used for the construction of the above formulæ is 21, the greatest number for any one station being 6, the least 3, the average probable error of any single representation is ±12′. The annual change (increasing east declination) may be taken the same for all stations, viz., in 1840, −1′·6, in 1850, −1′·2, in 1860, −0′·8.

Year.	San Diego.	Monterey.	San Francisco.	Cape Mendocino.	Cape Disappointment.
	°	°	°	°	°
1790	−11·1	−11·4	−13·6	−15·1	−18·9
1800	11·4	12·3	14·1	15·4	19·1
1810	11·7	13·0	14·5	15·7	19·3
1820	12·0	13·6	14·8	16·0	19·5
1830	12·2	14·2	15·1	16·3	19·7
1840	12·3	14·6	15·4	16·6	19·8
1850	12·5	15·0	15·6	16·9	20·0
1860	−12·6	−15·3	−15·8	−17·2	−20·2

The next station discussed, south of California, is San Blas, Mexico, lat. 21° 32′ N., long. 105° 16′ W. of Gr., which gave the following expression (see p. 234, C. S. report for 1856),

$$D=-8^{\circ}{\cdot}63-0{\cdot}042n-0{\cdot}00031n^2,$$

which equation, when compared with those above, shows a reversal in the sign of the coefficient of n^2 or an opposite curvature. The annual easterly increase at San Blas in 1850 according to the above formula was 3′·3. This station, however, is already within the area of the peculiar form of the isogonic lines, which position may possibly render an immediate comparison impracticable. The station Sitka, in Russian America, is the next place discussed north of Washington Territory. I find for it the approximate formula,

$$D = -28^{\circ}{\cdot}12 - 0{\cdot}0607n - 0{\cdot}00025n^2.$$

It depends for its latest declination (1858) on the tabular value assigned by Mr. Evans on his late map of the lines of equal magnetic variation reduced to 1858.

Record of all observed declinations made use of in the above paper, not heretofore published in the U. S. Coast Survey reports.

The following record, containing only additional observations, we have to consult the preceding reports of 1854,* 1855–'56, and '58, if we desire to collect all results which may have been used at any one station. The stations are arranged geographically, commencing with the northern and eastern stations and concluding with the stations on the western coast. $D=$ observed declination.

York Fort, Hudson Bay.

From the Proceedings of the Royal Society of London for January 7, 1858, by Major Gen. Sabine.

1725,	$D=$19° 00′ W.	Capt. Middleton.
1787,	5 00 "	Hansteen's map.
1819, Sept.,	6 00 E.	Sir J. Franklin.
1843, July,	9 25 "	Capt. Lefroy.
1857, Aug.,	7 37 "	Capt. Blakiston.

Quebec, Canada.

1649,	$D=$16° 00′ W.	P. Bressau, Hansteen's Erdmag's, Barlow Cycl. Met.
1686,	15 30 "	DeHayes, " "
1810,	11 00 "	Becquerel, traité du magnetisme.
1814,	11 50 "	Kent, Becquerel traité du magnetisme.
1831,	13 38 "	Bayfield, " " " "
1842,	14 12 "	Capt. Lefroy.
1859, July,	16 17 "	Chas. A. Schott, Asst. U. S. Coast Survey.

Burlington, Vt.

See former observations in 1855 report, pp. 326–337.

1837,	$D=$ 8° 45′ W.	Prof. Benedict.
1840,	9 42 "	J. Johnson, Thompson's Hist. of Vermont.
1845, June,	9 22 "	Dr. J. Locke, Smiths. Contrib. to Knowledge, vol. iii, 1852.

Portland, Me.

1763,	$D=$ 7° 45′ W.	J. Winthrop, Sill.'s Journal, xxxiv, 1838, Prof. Loomis's collection.
1775,	8 30 "	J. F. DeBarre's Atlantic Neptune, London, 1781.

* The table of the declinations in that report is reprinted and enlarged in the report of 1855.

1845, June,	11 28 "	Dr. J. Locke, Smiths. Contrib. to Knowledge, vol. iii, 1852.
1859, July,	12 20 "	Chas. A. Schott, Asst. U. S. Coast Survey.

See also C. S. report of 1856, p. 215.

Portsmouth, N. H.

1771,	$D=$ 7° 46′ W.	Holland, Sill.'s Journal, xxxiv, 1838, Prof. Loomis's collection.
1771,	7 48 "	Holland.
1775,	7 45 "	J. F. DeBarre's Atlantic Neptune.
1859, July,	11 15 "	Chas. A. Schott, Asst. U. S. Coast Survey.

See also C. S. report of 1856, p. 215.

Rutland, Vt.

1789, April,	$D=$ 7° 03′ W.	Dr. Williams, Sill.'s Journal, xvi, 1829.
1810, May,	6 04 "	" " " " " "
1811, Sept.,	6 01 "	" " " " " "
1859, July,	9 49 "	Chas. A. Schott, Asst. U. S. Coast Survey.

Cambridge, Mass.

See pp. 317–318 of C. S. report of 1855, also C. S. report, 1856, p. 222.

1845, June,	$D=$ 9° 32′ W.	Dr. J. Locke, Smiths. Contrib. to Knowledge, vol. iii, 1852.
1855, May,	10 54·6 "	W. C. Bond (in a letter to Supt. of C. S.)
1856, May,	10 50·3 "	" " " "
1856, July,	10 06 "	Karl Friesach, Imp. Acad. of Sciences, Vienna, vol. xxix, 1858.

Note.—More recent observations still require examination.

Newburyport, Mass.

1775,	$D=$ 6° 45′ W.	J. F. W. DeBarre's Atlantic Neptune.
1781,	7 18 "	Dr. Williams, Sill.'s Journal, xxxiv, 1838, Prof. Loomis's collection.
1859, July,	10 58 "	Chas. A. Schott, Asst. U. S. Coast Survey.

See also C. S. report, 1856, p. 215.

Boston, Mass.

See C. S. report, 1855, pp. 316, 317–337.

Providence, R. I.

See C. S. report, 1855, pp. 307, 308, 309, 337.

Hartford, Conn.

1786,	$D=$ 5° 25′ W.	Dr. Williams,	Prof. Loomis's collection in Sill.'s Journal, vol. xxxiv, 1838.
1810,	4 46 "	Asher Miller,	
1824,	5 45 "	N. Goodwin,	
1828,	6 03 "	"	
1829,	6 03 "	"	
1859, July,	8 04 "	An interpolated value from observations at Springfield and New Haven in 1859 and 1855.	

New Haven, Conn.

See C. S. report, 1855, pp. 319, 320, 337.

Albany, N. Y.

1847, Nov.,	$D=$ 7° 35′ W.	Regent's report (geological survey).

1856, Sept., 8 35 " Karl Friesach, Imp. Acad. of Sciences, Vienna, vol. xxix, 1858.

See also C. S. report, 1855, pp. 328–337, and C. S. report, 1858, p. 191.

Oxford, N. Y.

The following observations marked E. B. W. C. are from a letter of Mr. E. B. W. Call to the Superintendent C. S. Dec. 22, 1858.

1792–95,	$D=$ 3° 00′ W.	E. B. W. C.
1817,	3 00 "	"
1828, July,	4 30 "	"
1834, Oct.,	3 52 "	Regent's report, Sill.'s Jour., xxxiv, 1838.
1836, Oct.,	4 09 "	" " " " "
1838, July,	4 30 "	" " observed at Guilford.
1849, Nov.,	5 11 "	E. B. W. C.
1857, April,	5 44 "	"
1858, Feb.,	5 47 "	"
1858, Dec.,	5 50 "	"

New York.

See C. S. report of 1855, pp. 320, 321, 333, and 337, also C. S. report, 1856, p. 217.

Philadelphia.

See C. S. report of 1855, pp. 313, 314, and 337.

Hatboro', Pa.

See C. S. report of 1858, pp. 192, 193, 194, and 195.

Baltimore, Md.

1808, $D=$ 0° 10′ to 15′ W. D. Byrnes, vol. xviii, 1830, Sill.'s Journal.

See also C. S. report, 1856, pp. 219, 227, also C. S. report, 1858, p. 191.

Washington, D. C.

See C. S. report, 1858, pp. 195, 196, 197.

Williamsburg, Va.

1694,	$D=$ 5° 00′ W.	Sill.'s Journal, vol. xxxiv, 1838, Prof. Loomis's collection.
1780,	0 50 "	" " " " "
1809,	0 33 E.	" " " " "
1856, Aug.,	1 04 W.	Deduçed from observations at Petersburg, Old Point Comfort, and Norfolk.

Charleston, S. C.

1857, April, $D=$ 1° 56′ E. Derived from observations at Savannah in 1852 and 1857.

See C. S. report, 1855, pp. 322, 323.

Savannah, Ga.

1817,	$D=$ 4° 00′ E.	Becquerel traité du magnetisme.
1838,	5 05 "	Silliman's Journal, xxxiv, 1840.
1839,	3 31 "	" " " "

See also C. S. report, 1856, p. 220, and C. S. report, 1858, p. 192.

Mobile, Ala.

See C. S. report, 1855, p. 323, also C. S. report, 1858, p. 192.

Havana, Cuba.

See C. S. report, 1855, p. 324.

1857, Jan.,	$D=$ 5° 15′ E.	Karl Friesach, Imp. Acad. of Sciences, Vienna, vol. xxix, 1858.

Jamaica, W. Indies.

1732,	$D=$ 6° to 6° 05′ E.	J. Harris at Black river, in March and April. Phil. Trans., 1733.
1789–93,	6 50 "	J. Leard, map of Port Royal.
1791–92,	6 45 "	" " "
1819,	4 50 "	DeMackau, Becquerel's traité du magnetisme, Paris, 1846.
1821,	4 50 "	DeMayne, " "
1822,	4 54 "	Owen, " "
1832,	5 13 "	Foster, " "
1833?	4 40 "	From a map.
1840?	4 00 "	Gen. Sabine's isogonic map of the Atlantic Ocean.
1857, March,	3 40 "	Karl Friesach, Imp. Acad. of Sciences, Vienna, vol. xxix, 1858.

Panama, New Granada.

1775, Nov.,	$D=$ 7° 49′ E.	Encycl. Brit.
1791, Dec.,	7 49 "	" "
1802,	8 00 "	" "
1822,	7 00 "	Hall, Becquerel's traité du magnetisme.
1837,	7 02 "	Sir E. Belcher.
1849,	6 55 "	Major Emory (Mexican Bound. Survey).

See also C. S. report, 1856, p. 223.

Vera Cruz, Mexico.

1726–27,	$D=$ 2° 15′ E.	J. Harris, Phil. Trans. R. S. anno 1728.
1769,	6 40 "	Encyc. Brit., 7th edition, 1842.
1769, March,	6 28 "	"
1776,	7 30 "	Don Ulloa, Encyc. Brit.
1815,	10 37 "	Malony.
1819, April,	9 16 "	Wise.

See also C. S. report, 1856, p. 214.

San Diego, Monterey, San Francisco and Cape Mendocino, California, and for Cape Disappointment, Washington Territory, see C. S. report, 1856, pp. 228 to 235.

Sitka, Russian America.

1804,	$D=$26° 45′ E.	Lissiansky,	Becquerel's traité du magnetisme.
1824,	27 30 "	Kotzebue,	
1829,	28 19 "	Erman,	
1858,	30 00 "	From Evans' map of isogonic lines for 1858.	

Washington, D. C.

ART. XXX.—*Caricography;* by PROF. C. DEWEY.

(Continued from vol. xxviii, p. 232, Second Series.)

No. 260. *Carex argyrantha*, Tuckerman.

Spica composita; spiculis 4–8, ovato-rotundis vel obovatis sub-approximatis alternis albis, inferiore subremota, supernè staminiferis, squamosi-bracteatis, distigmaticis; fructibus ovatis compressis erectis vel sub-patulis nervosis margine membranaceo-alatis viridibus acuminato-rostratis brevi-bifidis, squamam membranaceam albam lanceolatam aequantibus.

Culm 1½–3 feet high, smooth, lax, reclining, and twice longer than the leaves; spikelets nearer above and white; fruit margined or winged widely for its length and width; light green.

Rocky Woods, Amherst and Sunderland, Mass.; *Prof. Tuckerman*, by whom the plant and his description have been kindly presented. It is related to *C. Deweyana*, and the white-glumed family, silver-flowered, according to its name, but appears to be new and distinct; discovered the last season.

Note. *C. Rugeliana*, Kunze, suppl. to Schkuhr, No. 56, p. 189, is *C. aestivalis*, Curtis, according to Boott, Illust., p. 54, No. 133. *C. miser*, Buckley, in this Journal, vol. xlv, p. 173, and vol. xlviii, p. 140, is considered by Dr. Boott to be *C. juncea*, Willd. System. Veg., 1826, No. 226. See Boott, Lin. Trans. vol. xx, p. 116, and Illust., p. 55. The descriptions of Willd. & Kunze sustain this conclusion of Dr. Boott.

261. *C. paludosa*, Goodenough. Schk. fig. 103.

Spicis pistilliferis, 2–4, sæpe 3, cylindraceis erectis oblongis, arctifloriferis sub-approximatis, superioribus sessilibus, inferiore sæpe longo-pedunculata vix vaginata inferne attenuata et hinc sublaxiflora, alternatis et foliaceo-bracteatis; perigyniis (fructibus) ovatis in breve rostrum bidentatum attenuatis vel ovalibus acuminatis brevi-rostratis distinctè et multi-nervosis subcompressis subglabris tristigmaticis, squamam angustam lanceolatam aequantibus vel lanceolata cuspidata brevioribus.

Culm 1½–2 feet high, erect, triquetrous, scabrous above, longer than the rough-edged leaves, with leafy bracts equal to or surpassing the culm. Varies, like the European plant, in the length and thickness of its spikes, and in its glumes or scales.

Near Boston—*Wm. Boott, Esq.*, in 1859, probably introduced not many years since. Common in England, Germany, and Sweden.

262. *C. monile*, Tuckerman. Boott, Illust., No. 71.

Spicis staminiferis, 2–4, longis, cylindraceis, gracilibus cum squama longo-lanceolata; pistilliferis 2, raro 1, oblongo-cylindraceis, subremotis, brevi-pedunculatis, sub-densifloris, infima interdum basin attenuata sub-nutante, foliaceo-bracteatis vix vaginatis; fructibus globosis vel ellipsoideis inflatis brevi-rostratis bidentatis glabris multi-nervatis stramineis cum ore rostri sub-obliquo, squama angusta oblonga lanceolata sub-duplo longioribus.

Culm 15-30 inches high, erect, triquetrous, longer than the leaves; bracts surpassing the culm, bright yellowish green.

In marshes, not abundant, N. England—*Tuckerman;* Rhode Island—*Olney.;* New Jersey, Ohio and westward.

Note 1. The Carex, No. 197, vol. xlix, p. 47 of this Journal, accidentally misnamed, is *C. Vaseyi*, Dew., of which No. 197 contains the accurate description. From the preceding, *C. monile*, Tuck., the fruit is very different, being ovate, long-conic, subtriquetrous-inflated, glabrous and scabro-rostrate. *C. Vaseyi* is the plant referred to by Dr. Boott, Illust., No. 71, as different from *C. vesicaria* and *C. monile*, but not named by him among those sent him from Penn Yan, N. Y., by Dr. Sartwell. The reference of No. 197 above to No. 71 in the Illust. is, of course, an oversight. The correction on 197 is *C. Vaseyi*, Dew.

Note 2. *C. bullata*, Schk., Fig. 166, described in this Journal, vol. ix p. 71, 1825, from living specimens, and named from its (ball-shaped) nearly globose fruit, seems to me, as it does to Carey in Gray's Manual, distinct from *C. cylindrica*, Schw., and from the following number.

263. *C. physema*, Dew. *C. bullata*, Boott, Illust., No. 71, and Carey (non Schk).

Spicis staminiferis, 2–3, cylindraceis, gracilibus, contiguis, infima bracteata; pistillifera 1, interdum 2, subrotunda vel oblongo-cylindracea perdensiflora et crassa remota et subfulva, infima pedunculata et subnutante vel erecta longi-foliacea bracteata; fructibus turgidi-ovatis longi-cylindraceis rostratis bifurcatisque, inflatis glabris et scabro-dentatis vel serratis, squama lanceolata acuta albi-marginata longioribus et latioribus.

Culm 1–2 foot high or more, firm, slender for the thickness of pistillate spike, stiff, triquetrous and often roughish, shorter than the narrow, flat and prim leaves; bright green.

Humid meadows, New England to Pennsylvania.

On *C. bullata*, Schk., Fig. 166, the pistillate spikes are shown as long and loose-flowered, and the fruit globose (ovate-globose), inflated abruptly, contracted into a slender, round, long, scabrous bifurcate beak, with a glume about half as long.

On the Carex, Boott, Illust., No. 39, the fruit is very large and inflated-ovate, with a conic, tapering, three-sided beak, scabrous-dentate, forming thick, dense and large spikes. If the figure by Dr. Boott shows *C. bullata*, Schk., then the figure in Schk. is a palpable caricature. Trusting to the correctness of the figure of Schk., I may yet say that Dr. Boott has presented a well known and distinct form. It is obvious that it should have another name, which designates the *inflation* of the fruit or perigynium, like the other.

264. *C. Olneyi*, Boott, Illust., No. 40.

Spicis stamiferis, 2–4, saepe 3, cylindraceis approximatis gracilibus; pistilliferis saepe 2, vel 1–3, cylindaceis sub-crassis densifloris stramineis approximatis plus minus pedunculatis, infima basin tereti laxiflora saepe subnutante, bracteatis; fructibus inflati-ovatis brevi-conico-rostratis scabro-bifurcatis nervosis divergentibus, squama lanceolata acuta vel cuspidata longioribus.

Culm 15–22 inches high, strong, obtusely triquetrous, scabrous above, shorter than the long and stiff margined leaves.

Rhode Island—*Olney;* probably associated with this group over the country. Were it not for the very different spikes, the fruit would closely ally this species to *C. physema*, as being a smaller form of it.

Remark.—This Vesicaria group is of difficult determination, and there has been much confusion in consequence. Hoping that some light has shined upon it, I can only say with Dr. Boott, that "future observation must determine in America the value to be affixed to the species of this group." The following shows the group as here exhibited:

C. monile, Tuckerman. Boott, Illust., No. 39.
C. Vaseyi, Dew. *C. monile*, Sart., Exsic. No. 151, and Sill. Journ., [1] vol. 48, p. 47, putting *C. Vaseyi* for *C. monile.*
C. bullata, Schk. Dew., Sill. Journ. vol. 9, p. 71 (not Boott or Carey).
C. physema, Dew. *C. bullata*, Boott, and Carey in Manual.
C. Tuckermani, Boott. Dew., Sill. Journ., vol. 49, p. 47. *C. cylindrica*, Carey.
C. Olneyi, Boott, Illust., and as above.

As Dr. Boott has found *C. lenticularis*, Mx., among the northern Carices, it is now distinguished and identified. The great difficulty had been in the imperfection of the description of Michaux. This is removed by Dr. Boott, which the younger botanists will be glad to see, and also the more complete description.

C. lenticularis, Mx. Boott, Illust., No. 76, Fig. 77. *C. concolor*, R. Br. in this Journal.

Spicis cylindraceis obtusis approximatis et sessilibus; staminifera unica, interdum 2, infima brevi, vel terminali superne sæpe fructifera; pistilliferis 2–5, vel rarò pluribus, foliato-bracteatis, interdum inferne staminiferis, infima vix sessili; fructibus ellipticis convexo-lenticularibus, interdum subovatis, per-brevi-rostellatis nervosis stramineis glabris, squama oblonga *obtusa* pallida longioribus; planta matura concolore.

Culm 8–14 inches high, triquetrous, erect, leafy towards the base, leaves about the length of the culm, and sheaths longer than the culm; terminal spike staminate, often only staminate below, sometimes a shorter staminate sessile spike near it; pistillate spikes 3–4, or more or less, cylindrical, erect, the lowest sometimes vaginate-bracted; stigmas 2; fruit sub-ovate, longer than the oblong obtuse white-edged scale; whole plant light green, nearly of the same color.

Lake of Swans and Arctic America, *Michaux and Richardson.* Var. Albi-montana, has fruit less oval, or ovate and acutish, tapering above, resembling somewhat *C. torta*, but taller than the Arctic plants. White Mountains, N. H., about ponds—*Tuckerman.* In Harrison, Me., about ponds, in 1859—*Rev. J. Blake.*

Errata.—On p. 232, vol. xxviii, for *C. gynocratis* read *C. gynocrates.* On p. 231, same vol., for vol. xxiv, p. 48, read vol. xxvii, p. 81, Sec. Ser., and for the numbers 254, 255 and 256, read 257, 258 and 259.

ART. XXXI.—*On Numerical Relations existing between the Equivalent Numbers of Elementary Bodies;* by M. CAREY LEA, Philadelphia. Part II.

(Concluded from p. 111.)

On Geometrical Ratios existing between Equivalent Numbers.

THE First Part of this paper was devoted to the examination of relations between the equivalent numbers of certain elementary bodies depending upon the number 44–45, and it was attempted to show:—

1st. That such relations extend to nearly all the elements:—

2d. That the particular groups collected together by this relation consist of bodies whose properties are analogous, and that the classification is in harmony with the distinguishing characteristics of the substances classified.

The first portion of this Second Part will present a species of relation wholly distinct, it is believed, from any that has hitherto been pointed out, and which may be not inappropriately termed Geometrical Ratios, to distinguish them from the more familiar arithmetical relations which have been heretofore exclusively studied by chemists.

The arithmetical relations are susceptible of at least an hypothetical explanation, on the supposition that the common difference in a series of elements may represent the equivalent number of a substance as yet undetermined, which, by its combinations in varying proportions, gives rise to the bodies constituting the successive terms of the series. The analogies which are now to be considered are more difficult of explanation, even by hypothesis. Their accuracy, sometimes absolute, renders improbable the supposition that they are mere casual coincidences. In science it is not permitted to neglect facts merely because we cannot satisfactorily account for them.

The nature of these relations consists in this, that if we take two substances and examine the ratio which subsists between the numbers representing their atomic weights, we may find in certain cases, that it is identical with the ratio subsisting between the atomic weights of two other substances, and so on through a considerable number of elements. The ratio between the atomic weights, for instance, of oxygen and nitrogen, is that of four to seven, so likewise is that between those of zirconium and potassium, potassium and barium, with absolute exactitude. What renders this the more remarkable is, that all three of these last substances are striking exceptions to Prout's law that the equivalents of the elements are exact multiples of that of hydrogen; they all have decimals, zirconium 22·4, potassium 39·2, barium 68·6. Now the ratio just mentioned gives these numbers with their decimals with perfect exactness. The same species of relation exists between many other elements, as will be seen by the table below :—

(1.)

Oxygen-Nitrogen Ratio, or that of Four to Seven.

			Atomic wts. calculated.		Atomic wts. received.
Nitrogen	= 14,	$\frac{4}{7}$ of 14	= 8	Oxygen	= 8
Barium	= 68·6,	$\frac{4}{7}$ " 68·6	= 39·2	Potassium	=39·2
Potassium	= 39·2,	$\frac{4}{7}$ " 39·2	= 22·4	Zirconium	=22·4
Calcium	= 20,	$\frac{4}{7}$ " 20	= 11·43	Magnesium	=12
Magnesium	= 12,	$\frac{4}{7}$ " 12	= 6·86	Glucinum	=7*
Strontium	= 43·75,	$\frac{4}{7}$ " 43·75	= 25	Titanium	=25
Lead	= 103·5,	$\frac{4}{7}$ " 103·5	= 59·16	Tin	=59
Antimony	= 120·3,	$\frac{4}{7}$ " 120·3	= 68·76	Vanadium	=68·60
Bismuth	= 208,	$\frac{4}{7}$ " 208	= 118·84	Antimony	=120·3
Mercury	= 100,	$\frac{4}{7}$ " 100	= 57·16	Cadmium	=56
		$\frac{4}{7}$ " 57·16	= 32·66	Zinc	=32·60
Molybdenum	= 48,	$\frac{4}{7}$ " 48	= 27·42	Chromium	=26·70
Chlorine	= 35·5,	$\frac{4}{7}$ " 35·5	= 20·28	Silicon	=21
Fluorine	= 19,	$\frac{4}{7}$ " 19	= 10·86	Boron	=10·9†
Boron	= 10·9,	$\frac{4}{7}$ " 10·9	= 6·23	Carbon	=6

It will be remarked that mercury, cadmium and zinc are here again brought together. Although four-sevenths of the atomic weight of mercury is not that of cadmium exactly, nevertheless four-sevenths the resulting number so obtained, viz. 57·16, gives for the atomic weight of zinc 32·66, an exceedingly close approximation.

The last three equations in the table show us that the atomic weight of silicon stands nearly in the same numerical ratio to that of boron, as that of chlorine does to that of fluorine, and that both these ratios, especially the latter, approach nearly to that existing between the atomic weight of boron and carbon.

Now as the atomic weight of boron is by no means positively determined, it may be allowable to examine how far an hypothetical number will fulfill these several ratios. Let us assume:—

Fluorine : Chlorine : : Carbon : Boron

we find :—

$$\frac{\text{Chlorine } 35{\cdot}5 \times \text{Carbon } 6}{\text{Fluorine } 19} = 11{\cdot}2104,$$

and obtain for the hypothetical equivalent of boron the number 11·21 (neglecting the last decimal figures).

Let us now examine how far the number 11·21 will fulfill the second proposed ratio, viz.

Carbon : Boron : : Boron : Silicon

* Taking Glucina as G_2O_3.

† See Jahresbericht, 1858.

we have $\frac{(\text{Boron } 11{\cdot}21)^2}{\text{Carbon } 6} = 20{\cdot}944$:—

a close approximation to 21, the received number.

If, therefore, we assume the atomic weight of boron to be a mean proportional between those of carbon and silicon, which we may do by an alteration certainly within the bounds of possible error in determinations so far made, we find that the proportion between the weights of carbon and boron, of boron and silicon, is the same as that between those of fluorine and chlorine.

(2.)

Carbon-Nitrogen Ratio, or that of Three-Sevenths.

The atomic weight of Carbon stands to that of Nitrogen in the ratio of 3 to 7, a proportion which is found exactly or approximately to extend to certain other elements.

Nitrogen	= 14,	$\frac{3}{7}$ of	14	= 6	Carbon	= 6
Fluorine	= 19,	$\frac{3}{7}$ "	19	= 8·14	Oxygen	= 8
Iron	= 28,	$\frac{3}{7}$ "	28	= 12	Magnesium	= 12
Cerium	= 47,	$\frac{3}{7}$ "	47	= 20·14	Calcium	= 20
Phosphorus	= 31,	$\frac{3}{7}$ "	31	= 13·29	Nitrogen	= 14
Bromine	= 80,	$\frac{3}{7}$ "	80	= 34·29	Chlorine	= 35·5
Arsenic	= 75,	$\frac{3}{7}$ "	75	= 32·14	Zinc	= 32·6
Lead	= 103·5	$\frac{3}{7}$ "	103·5	= 44·4	Strontium	= 43·75
Cadmium	= 56,	$\frac{3}{7}$ "	56	= 24	2 Magnesium	= 24
Uranium	= 60,	$\frac{3}{7}$ "	60	= 25·71	Chromium	= 26·7

(3.)

The proportions expressed by the preceding tables may be differently presented, and perhaps rendered more striking. As the numbers which express the equivalent weights of the elements are altogether relative, it is of course a mere question of convenience which is selected as unity. If in place of adopting the equivalent of one substance, as that of hydrogen or oxygen, as a permanent unit, we successively make those of the substances contained in the right hand column of the table in section 1. our unit, and consider the effect of such a change upon the equivalents of the substances contained in the left-hand column, we shall obtain the following results:

Making the equivalent of	Oxygen	100,	the eq. of	Nitrogen	becomes 175
"	Potassium	100,	" " "	Barium	" 175
"	Zirconium	100,	" " "	Potassium	" 175
"	Titanium	100,	" " "	Strontium	" 175
"	Tin	100,	" " "	Lead	" 175·4
"	Vanadium	100,	" " "	Antimony	" 174·9
"	Boron	100,	" " "	Fluorine	" 174·3
	&c.			&c.	

The same view may be extended to Section 2. Assuming the atomic weight of the substances in the left-hand column successively as unity, or one hundred, we obtain the following results:

Making the equivalent of	Nitrogen	100	that of	Carbon	becomes 42·8
	Fluorine	100	" "	Oxygen	" 42·1
	Iron	100	" "	Magnesium	" 42·8
	Cerium	100	" "	Calcium	" 42·6
	&c.			&c.	

It is therefore clear that the atomic weight of each substance in the left hand column of these several tables bears to that of the corresponding substance in the right-hand column a definite numerical ratio, which is identical, or nearly so with that borne by the atomic weight of any other substance in the same column of the same table to that of the corresponding substance in the other column.

(4.)

We have seen that many elements stand to other elements in the same relation as nitrogen to oxygen—many in the same relation as nitrogen to carbon. Apart from these more general ratios, many elements may be classed together in double or treble pairs, such that the two elements in one pair stand to each other in the same numerical ratio as the two elements of a second or third pair, the two elements constituting each pair being more or less closely allied to each other in properties, though the pairs are not necessarily analogous with those with which they are compared.

For example, arsenic stands to antimony in the same numerical ratio as selenium to tellurium, within an extremely small fraction, so that by multiplying and dividing we have:—

$$\text{Arsenic } 75 \times \frac{\text{Tellurium } 64}{\text{Selenium } 40} = 120, \text{ Antimony} = 120{\cdot}3,$$

So in like manner magnesium stands to zirconium in the same ratio as fluorine to chlorine:—

$$\text{Magnesium } 12 \times \frac{\text{Chlorine } 35{\cdot}5}{\text{Fluorine } 19} = 22{\cdot}42, \text{ Zirconium} = 22{\cdot}40,$$

So carbon stands to boron in the same ratio as silver to gold:—

$$\text{Carbon } 6 \times \frac{\text{Gold } 197}{\text{Silver } 108} = 10{\cdot}94, \text{ Boron} = 10{\cdot}90.$$

These ratios, as well as those that follow, are very close approximations, certainly within the limits of error which may easily exist in the determinations on which we depend. For antimony we find 120, the recent determinations vary from 119 to 122, that which appears most reliable is 120·3, as adopted by the Jahresbericht. In the second case, the number found for zirconium varies but $\frac{1}{1121}$ from that generally received. In the third, the number found for boron is intermediate between the number adopted in the Jahresbericht 10·90 and that found by Deville, 11. Discrepancies such as these are trifling in the extreme, and have not the slightest real significance.

Phosphorus stands to nitrogen in nearly the same ratio as strontium to calcium:—

$$\text{Phosphorus } 31 \times \frac{\text{Calcium } 20}{\text{Strontium } 43{\cdot}77} = 14{\cdot}16,\ \text{Nitrogen} = 14.$$

Tin stands to titanium in nearly the same ratio as iron to magnesium:—

$$\text{Tin } 59 \times \frac{\text{Magnesium } 12}{\text{Iron } 28} = 25{\cdot}28,\ \text{Titanium} = 25.$$

Tin stands to zinc in almost exactly the same ratio as gold to silver, or as boron to carbon:—

$$\text{Tin } 59 \times \frac{\text{Silver } 108}{\text{Gold } 197} = 32{\cdot}35,\ \text{Zinc} = 32{\cdot}60.$$

$$\text{Tin } 59 \times \frac{\text{Carbon } 6}{\text{Boron } 10{\cdot}9} = 32{\cdot}48,\ \text{Zinc} = 32{\cdot}60.$$

These and many other analogies of the same character are brought together in the following table, by which the ratios of comparison between each pair are shown, up to the second place of decimals, beyond which it is pseudo-accuracy to go, in view of the data.

Ratio of tellurium to selenium, or	$\frac{\text{Te } 64}{\text{Se } 40}$	$= 1{\cdot}60$
Ratio of antimony to arsenic, or	$\frac{\text{Sb } 120}{\text{As } 75}$	$= 1{\cdot}60$
Ratio of zirconium to magnesium,	$\frac{\text{Zr } 22{\cdot}4}{\text{Mg } 12}$	$= 1{\cdot}87$
Ratio of chlorine to fluorine,	$\frac{\text{Cl } 35{\cdot}5}{\text{F } 19}$	$= 1{\cdot}87$
Ratio of boron to carbon,	$\frac{\text{B } 10{\cdot}9}{\text{C } 6}$	$= 1{\cdot}82$
Ratio of gold to silver.	$\frac{\text{Au } 197}{\text{Ag } 108}$	$= 1{\cdot}82$
Ratio of strontium to calcium,	$\frac{\text{Sr } 43{\cdot}77}{\text{Ca } 20}$	$= 2{\cdot}19$
Ratio of phosphorus to nitrogen,	$\frac{\text{P } 31}{\text{N } 14}$	$= 2{\cdot}21$

Ratio of iron to magnesium, $\frac{\text{Fe } 28}{\text{Mg } 12} = 2{\cdot}33$

Ratio of tin to titanium, $\frac{\text{Sn } 59}{\text{Ti } 25} = 2{\cdot}36$

Ratio of tin to zinc, $\frac{\text{Sn } 59}{\text{Zn } 32{\cdot}6} = 1{\cdot}81$

Ratio of gold to silver, $\frac{\text{Au } 197}{\text{Ag } 108} = 1{\cdot}82$

Ratio of zirconium to aluminum, $\frac{\text{Zr } 22{\cdot}4}{\text{Al } 13{\cdot}7} = 1{\cdot}64$

Ratio of calcium to magnesium, $\frac{\text{Ca } 20}{\text{Mg } 12} = 1{\cdot}67$

Ratio of potassium to sodium, $\frac{\text{K } 39{\cdot}2}{\text{Na } 23} = 1{\cdot}70$

Ratio of arsenic to phosphorus, $\frac{\text{As } 75}{\text{P } 31} = 2{\cdot}42$

Ratio of cobalt to magnesium, $\frac{\text{Co } 29{\cdot}5}{\text{Mg } 12} = 2{\cdot}46$

Ratio of nickel to magnesium, $\frac{\text{Ni } 29{\cdot}6}{\text{Mg } 12} = 2{\cdot}47$

Ratio of lead to tin, $\frac{\text{Pb } 103{\cdot}5}{\text{Sn } 59} = 1{\cdot}75$

Ratio of cadmium to zinc, $\frac{\text{Cd } 56}{\text{Zn } 32{\cdot}6} = 1{\cdot}72$

(5.)

It has been the object of this paper to develope as far as possible those numerical relations existing between the atomic weights of the elements which have not been previously observed. It is, perhaps, impracticable in the present state of chemical science to explain why such relations should exist, nevertheless, nothing which tends to an exacter knowledge of the laws which govern the proportions in which the elements combine should be neglected. In this way, little by little, the materials are collected for future generalizations, with the reasonable hope of eventually arriving at an intimate knowledge of the true constitution of the materials which compose our globe.

The substances which compose the chlorine group form a well marked family to which fluorine seems also to belong. The analogies which unite chlorine, bromine and iodine, appear to multiply with each new investigation of their properties. By none have they been more strikingly illustrated than by the recent experiments of Plücker on the spectra obtained from the light of electric discharges through gases in a state of extreme rarefaction, demonstrating that those given by Cl, Br and I, resembled each other in exhibiting great numbers of black lines of extreme fineness, so fine indeed as to be almost mathematical lines, and that in this respect these elements differed wholly from all others. With so remarkable an experiment as this before us, supported by the very numerous other relations which exist between these substances, we might naturally look to find striking numerical correspondences between their atomic weights, but the analyses upon which we at present depend give us numbers which afford no relations positively exact. Of the four numbers 19, 35·5, 80 and 127, two are prime, one fractional, and one has many divisors. Such numbers are very unpromising for the development of numerical relation.

Thus far, however, at least relations do exist: that it is possible to express the value of the equivalent of any one of these substances in terms of any two of the remaining—each equivalent is a function of any other two, thus:—

$$I = 10\,Cl - 12\,F \qquad Cl = \frac{12\,Br - 7\,I}{2}$$

$$I = \frac{5\,Br - F}{3} \qquad Cl = \frac{I + 12\,F}{10}$$

$$I = \frac{12\,Br - 2\,Cl}{7} \qquad Cl = \frac{Br - 7\,F}{6}$$

$$Br = 6\,Cl - 7\,F \qquad F = \frac{10\,Cl - I}{12}$$

$$Br = \frac{2\,Cl + 7\,I}{12} \qquad F = \frac{6\,Cl - Br}{7}$$

$$Br = \frac{3\,I + F}{5} \qquad F = 5\,Br - 3\,I$$

If these equations represented approximations, no matter how close, they would not be worthy of consideration. They are perfectly exact. The quasi-connection of oxygen with this series will be pointed out further on.

(6.)

It has been remarked that the atomic weights of the members of the oxygen series proper differ from each other by whole multiples of the atomic weight of oxygen, but the extent to which differences of this kind exist amongst the atomic weights of the elementary bodies has perhaps not been pointed out.

The elements having the highest atomic weights are bismuth and gold, commencing with the first of these substances, and diminishing its atomic weight successively by whole multiples (in one case by a sub-multiple) we obtain the following results:—

Differences.			
	208, Bismuth = 208		
8×10			
	128	Iodine = 127	
8			
	120, Antimony = 120·3		
8×2			
	104		Lead = 103·5
8			
	96, Osmium = 97–99		
8×2			
	80	Bromine = 80	
8×½			
	76, Arsenic = 75		
8			
	68, Vanadium = 68·6		Barium = 68·6
8			
	60, Uranium = 60		
8			
	52, Ruthenium = 52·2		
8			
	44		Strontium = 43·77
8			
	36	Chlorine = 35·5	
8×2			
	20	Fluorine = 19	Calcium = 20
8			
	12		Magnesium = 12

It has not been usual to classify vanadium, osmium or ruthenium with the arsenic series, but Schafarik in a paper on the vanadium compounds,* and Hallwachs and Schafarik in a subsequent paper† on allied subjects (to neither of which the author had access while engaged on the first part of this paper except in the form of a brief notice) are of opinion that vanadium should be removed from the molybdenum group in which it has heretofore been classed, and be transferred to the arsenic series, an opinion which they support by arguments founded on the fact that the specific volume as well of vanadic acid as of the

* Sitzungsbericht of the Acad. of Sciences of Vienna, Bd. xxxiii, ext. in Chemisches Centralblatt, 1859, 97.

† Same vol. of Sitzungsbericht, ext. in Chemisches Centralblatt, 1859, p. 161.

metal itself, corresponds with those of the metals of the arsenic series, viz.:

		Specif. Vol.	
Vanadic acid,	VO_3	26·5	
Arsenious acid,	AsO_3	26·6	
Antimonic oxyd,	SbO_3	25·9	
Bismuthic oxyd,	BiO_3	25·9*	28·4†

They also point out the fact that vanadinite (vanadinbleierz) is isomorphous with the analogous compounds of the arsenic series, mimetene (kampylit) and pyromorphite. Professor Mallet has shown that reasons can be given for classifying osmium also in the arsenic series.‡

Again, commencing with gold we obtain the following succession of numbers.

Differences.				
	196 Gold	=197		
8×11				
	108 Silver	=108		
8				
	100 Mercury	=100		
8				
	92 Tungsten	= 92		
8×3½				
	64 Tellurium	= 64		
8				
	56		Cadmium	=56
8				
	48 Molybdenum	= 48		
8				
	40 Selenium	= 40		
8				
	32		Zinc	=32·6
8×2				
	16 Sulphur	= 16		
8				
	8 Oxygen	= 8§		

In an interesting paper published in the Memoirs of the Am. Acad. of Boston, vol. v, Prof. J. P. Cooke, of Harvard University, attempted a classification of all the elementary bodies by making them all functions of a general series of the form $a+dx$. By giving fixed values to a and d particular expressions were obtained, giving rise to series by the changes in the value of the variable x. These particular series were characterized by the value of d, which in different series became successively 3, 4, 5,

* Boullay. † Karsten. ‡ Am. Jour. Science, xxix, 49.

§ If to the at. wt. of Li which may be taken at 7, we add 16=2×8, we have 23 the exact at. wt. of Na, and a further addition of 16 gives very nearly the at. wt. of K.

6, 8 and 9. The values of a were likewise always small, never exceeding 8. The values of x were restricted to whole numbers.

This view, although new and ingenious, is evidently exposed to a serious objection: the series include too much. The form entitled by Prof. Cooke the "Three Series," and expressed by $1+3x$, includes one-third of all possible whole numbers, and so to a proportionally great extent with all the other forms. As the author did not make positive exactness essential, it is evident that a wide scope was given for the classification of elements under any particular series: therefore this theory could only find favor as far as it might be made to conform to a wholly unobjectionable classification of the elements. In this it only partially succeeded, as will appear from an examination of the substances included in the various groups.

In the first series, chromium, manganese, osmium and gold are classed as affiliated to the chlorine group, into which however the series only admits fluorine with an error of 2, or 12 per cent.

In the second, arsenic and manganese are made affiliated with the sulphur group, which includes also molybdenum, vanadium, &c.

In the third, oxygen is classed with the nitrogen-phosphorus group, into which antimony only enters with an error of 7·7.*

In the fifth we find tin and titanium, the platinum group, gold, mercury, and most of the magnesia group.

In the sixth we have part of the remainder of the magnesia group, the metals of the alkaline earths and lead, the alkaline metals, hydrogen and silver, with copper and manganese as affiliations. Rendering all justice to the author for the originality and ingenuity of his views, it must be admitted that the flexibility of his series has led to a classification not in all respects supported by analogies. Five out of his six series contain one or more members of the magnesia group either directly or as affiliated members.

Dumas has followed out these ideas of Prof. Cooke and extended them. In place of referring his series to the type of the formic acid series alone, he includes the types of substituted ammoniums and stannethyls, under the general form $na+xd+yd'$; thus adopting an expression even more general than that of Prof. Cooke, but restricting it more closely in its application.

Perhaps the most beautiful and important of the relations pointed out by Dumas, are the parallel series of numbers, which by the subtraction of each term of the one from the corresponding term of the other, exhibit a constant difference within a certain degree of approximation.

* This probably arises from the fact that the equivalent of antimony as generally received at the time when Prof. Cooke wrote was higher than it is now known to be, and by diminishing the value of the variable x corresponding to this term, by 1, a nearer approximation would be obtained.

The analogies pointed out in the several parts of this paper evidently lead directly to the construction of series of the same kind some of which, together with others depending upon yet different relations are given below.

−Oxygen =−8	Chlorine =35·5	Bromine =80	Iodine =127
−Zinc =−32·6	Magnesia =12	Cadmium =56	Lead =103·5
24·6	23·5	24	23·5

In connection with these two parallel series we may remark that oxygen appears to constitute a negative term in the chlorine series, precisely as nitrogen does in the phosphorus and zinc in the cadmium series, with considerable approximation to exactness. If the atomic weight of chlorine were taken at 36 instead of 35·5 it would constitute an exact numerical mean between the atomic weights of bromine and oxygen supposing the latter to be taken with a negative sign. We have before seen that nitrogen, the negative member of the phosphorus series, is less closely allied with the rest than they are with each other. Still greater is the step between chlorine and oxygen, so great indeed that it is very doubtful whether they can properly be classed in the same series, although certain cases of isomorphism can be urged in favor of such a classification.

We here see a new instance of the existence of the relation of 44—45 developed in the first part of this paper.

Differences.	Calculated equivalents.		Received equivalents.
	− 8	- -	Oxygen = 8
44			
	36	- -	Chlorine = 35·5
44			
	80	- -	Bromine = 80
44			
	124	- -	Iodine =127

Gold =197	M =152·5	Silver =108	Mercury =100
Mercury =100	Cadmium = 56	Magnesium = 12	Glucinum = 4·7
97	96·5	96	95·3

M here represents a possible metal as yet unknown. In the first part of this paper mention was made of the possibility of the existence of such a metal, having an atomic weight representing the arithmetical mean between those of gold and silver.

Bismuth =208	M′ =164	Antimony =120·3	Lead =103·5
Antimony =120·3	Arsenic = 75	Phosphorus = 31	Nitrogen = 14
87·7	89	89·3	89·5

In this double series we find nearly the same number (89 approximatively) obtained by subtraction, as by addition in the last of the double series, part first, section 8. If in that just presented we replace lead by arsenic and give a negative sign to the atomic weight of nitrogen, we shall still have a number conforming to the character of the series, viz., 89.

In this and the following series the symbol M′ represents a possible metal constituting an intermediate term between antimony and bismuth. That such a metal might exist was pointed out in the consideration of the phosphorus series.

Bismuth	=208	M′	=164	Antimony	=120·3	Arsenic	=75
Gold	=197	M	=152·5	Silver	=108	Copper	=63·4
	11		11·5		12·3		11·6

M as before represents an hypothetical metal having for its atomic weight a mean term between those of silver and gold.

Uranium	60	Vanadium	68·6	Tungsten	92
Magnesim	12	Calcium	20	Strontium	43·75
	48		48·6		48·25

Molybdenum	48	Vanadium	68·6	Tungsten	92	2 Tantalum	137·6
Magnesium	12	Zinc	32·6	Cadmium	56	Mercury	100
	36		36		36		37·6

The parallelism between the three first terms of these last two series commencing respectively with molybdenum and magnesium is positively exact, perhaps the only known case in which absolute exactitude obtains. It is probable that other cases of parallel series exist, and will be discovered.

It is not easy to fix the exact amount of importance which attaches to the numerical relations up to this time ascertained to exist between the atomic weights of the elements. Some are no doubt mere casual coincidences, and relations remarkably exact and symmetrical may exist between the atomic weights of bodies which have no analogies in their properties: for example we may take calcium 20, selenium 40, uranium 60, bromine 80, mercury 100. Here the differences are not only exact, but all the subsequent numbers are multiples of the first, and this between bodies remarkably dissimilar in their properties—a striking proof of the necessity of caution in inferring relations of properties as following from relations of numbers. But on the other hand, to reject the relations of number when accompanied by analogy of properties, as unmeaning and unimportant, would be to err quite as much on the other side. When the received equivalent of an element forming a term in a well marked series differs from that obtained by calculation, it naturally leads, as Prof. Mallet has remarked, to suspect an error and desire a redetermination. The fact that a group of elements allied in their chemical characters may be arranged in a series having a common difference or a definite ratio between its terms, confirms the propriety of grouping those elements together, and such analogies may in doubtful cases assist us in arriving at a correct classification.

Philadelphia, Feb. 27, 1860.

Art. XXXII.—*Ornithichnites, or tracks resembling those of Birds;* by Roswell Field, of Greenfield, Mass.

When fossil footprints were first discovered in the sandstones of the Connecticut Valley, it was indeed thought to be a great discovery, but that the tracks, thus found were made by birds was received by men of scientific attainments with great distrust and skepticism. That they were tracks made by once living animals there could be no doubt, but that they were ornithichnites was very much doubted.

It was not indeed until after my esteemed friend Dr. Hitchcock had spent much time in comparing, describing, and in distributing specimens, that the scientific public became satisfied that they were the tracks of once living birds.

The great and only proof that they were the tracks of birds, is found in the organization of the fossil foot, in the numbers of joints or lateral expansions in the toes; in this they are supposed to agree, and probably do, with living types; this with alternate steps of right and left feet is all the evidence we have that they were the tracks of birds.

Living in the immediate vicinity of Turner's Falls, the locality that has furnished the most numerous, and beyond all comparison the most beautiful specimens, my attention was drawn many years since to this particular subject. It was from my farm that the late Dexter Marsh obtained his choicest specimens. And it was in the vicinity of these Falls where my much lamented friend Dr. Deane found "new walks in an old field:" Where our barren and rocky wastes became to him a garden of delight.

It was here I witnessed their labors with pleasure, and in a more obscure way have followed in their footsteps. I think I may safely say that I have uncovered more footprints, and found more new species, and a greater variety of tracks than any other man, I think I might also say with propriety than all others that have preceded me, and if I have learnt anything on this subject I have learnt it at the quarries. It is there, and there only I have studied the history of Triassic days, and the more I have studied, the greater have been my doubts as to the ornithic character of any of the tracks which these tables of stone contain. I have seen thousands of tracks that others have not seen. With injudicious blasting, and the carelessness of workmen many choice specimens have been broken and lost; other slabs literally covered with footprints, have been spoilt by suncracks the shrinkage of the mud in drying; the stratum over which the animal moved being either too hard or too soft to receive or retain good impressions, all such are rejected and lost to the student, at the quarries.

I have no new theory to advance, and none to build up, but if I can rightly decipher these fossil inscriptions impressed on the tombstones of a race of animals that have long since ceased to exist, they should all of them be classed in the animal kingdom as Reptilia. If I have not studied this subject in vain they were all quadrupedal. That they usually walked on two feet I admit, and that they could as readily walk on four when necessary is equally true. In proof of this we find tracks as perfect as if made in plaster or wax, which to all appearance as to the number of toes, and the phalangeal or lateral expansions in the toes agree perfectly with those of living birds, and still we know by the impressions made by their forward feet that these fossil tracks were made by quadrupeds. In other cases where the animals sunk deep in the muddy stratum over which they moved, it is plain that they dragged their tails in the mud, leaving a groove plowed up from one half inch to an inch in width. This groove is not always found on the surface where the foot rested, the weight of the animal causing the foot to sink through the yielding stratum, whilst the tail dragged on the one above. This we know was the case with animals that were surely quadrupeds, but they show the appendage of a tail only when their feet sunk deeply in the plastic clay. Thus the proof that we once relied upon to prove them birds can be relied upon no longer.

That there were quadrupeds in those sandstone days and that these had hind feet perfectly agreeing with the stony bird-tracks, throws great doubt and distrust on the question, whether there were any true birds in this age of reptiles.

If there were birds they were doubtless apterous and naked, for we should naturally suppose that where so large a number of birds congregated upon the muddy banks, that in dressing and pluming their feathers some of them must have been trodden under foot, but the impression of feathers has never been found, although we find the smallest leaves of vegetables, and the path-way and tracks of annelids, and insects, some of them so small that they can hardly be seen with the naked eye. Even the Otozoum whose giant-like track measures twenty inches in length, once supposed to have been a biped reptile, by later discoveries is proved to have been four-footed. Other new discoveries have reduced the number of so-called birds, transferring them to the class of quadrupeds, which I verily believe is the proper place for all of them. The smoothness of the bottom of the foot in our fossil tracks agrees better with some species of batrachians that now live in and about the water, than it does with such animals as live on the land. Had birds existed at this early geological period, when the sandstone of the Connecticut valley was being deposited, there has indeed been a woful gap in their history, from then up to near the historical period, while the die from which they were struck at their creation was not broken, but a new edition produced in these latter days. The work perhaps may have been revised but has not been enlarged as respects the size of the animal. I know that many eminent men and men of great scientific attainments, men who have spent much time and labor for many years in the investigation of this subject have come to different conclusions, and it may not become me to say that their conclusions are wrong. I would only add that when fossil tracks were first discovered there was so little known of the formation of the feet of fossil or of living animals, and particularly of their footprints that it is possible the first discoverers might have been mistaken as to the ornithic character of the footprints. The study of these fossils so very interesting to the geologist and naturalist, still merits their earnest attention. There is no known locality where they are found in such abundance, and in such perfection as at Turner's Falls, the northern terminus of the sandstone beds. Very few indeed have any conception of the marvelous perfection of this fossil inscription, or of the multitudes of once living creatures whose existence they commemorate.

March, 1860.

[Mr. Field is a plain farmer, who makes no claim to be an authority in science, but like Hugh Miller has hammered his geology out of the rocks on which he lives. He is well known as one of the most successful collectors of the foot-marks of the Connecticut sandstone, and his testimony as to the impression made on himself of their probable character and origin, has the merit of a conviction making head in an honest mind against all the weight and bias of opposing authorities.—EDS.]

ART. XXXIII.—*Eighth Supplement to Dana's Mineralogy;* by GEO. J. BRUSH, Professor of Metallurgy in Yale College.*

List of Works, etc.

F. X. M. ZIPPE: Lehrbuch der Mineralogie mit naturhistorischer Grundlage. pp. 450, 8vo, mit 334 Fig. Wien, 1860.

A. DESCLOIZEAUX: Sur l'emploi des propriétés optiques biréfringentes pour la détermination des espèces cristallisées. p. 88, 8vo. Extrait des Annales des Mines, tome xiv, 1858.

A. DELESSE: Etudes sur le Métamorphisme. Roches granitiques, pp. 77, 8vo. Extrait des Annales des Mines, tome xii.

CARL F. NAUMANN: Elemente der Mineralogie, fünfte vermehrte und verbesserte Auflage. 8vo, pp. 460, mit 483 Fig. Leipzig, 1859.

N. VON KOKSCHAROW: Materialen zur Mineralogie Russlands, dritter Band, Lieferung 1—3, pp. 128, mit Atlas. St. Petersburg, 1859.—This continuation of Kokscharow's great work, contains monographs of the species garnet, magnetite, anal-

* In the absence of Professor Dana I have endeavored to give an abstract of the results of the mineralogical researches, published since the appearance of the seventh Supplement.—G. J. B.—New Haven, April 1, 1860.

cime and euclase, together with important additional notes upon beryl, cancrinite, nepheline, phenacite, and apatite.

Adam: Extrait du Tableau Minéralogique. 8vo, pp. 14. Paris.

J. F. L. Hausmann: Ueber die Krystallformen des Cordierites von Bodenmais in Bayern. 4to. pp. 16. Göttingen, 1859.

Gustav Rose: Ueber die heteromorphen Zustande der kohlensauren Kalkerde, zweite Abhandlung, mit drei Kupfertafeln. 4to. pp. 48. Berlin, 1859.

Victor von Lang: Versuch einer Monographie des Bleivitriols. 8vo, pp. 54, mit xxvii Tafeln. Wien, 1859.

D. D. Owen: *First Report of a Geological Reconnaissance of the Northern Counties of Arkansas.* Little Rock, 1858.—In this report Dr. Elderhorst has contributed much that is of interest in regard to the ores of zinc, lead and manganese, and presented his facts in a manner that will make them of permanent practical value.

H. Kopp und H. Will: Jahresbericht über die Fortschritte der Chemie und verwandter Theile anderer Wissenschaften für 1858. Giessen, 1859. pp. 859.—Pages 673 to 812 contain Dr. Kopp's excellent review of the progress of mineralogical science for the year 1858.

H. C. Sorby: On the Microscopical Structure of Crystals, indicating the Origin of Minerals and Rocks. 8vo, pp. 48, with plates. Quart. Jour. Geol. Soc. Lond., xiv, pp. 458–500.

H. Dauber: Ermittelung krystallographisher Constanten und des Grades ihrer Zuverlässigkeit. Pogg. Ann., cvii, 267, 343, cviii, 439.

W. H. Miller: On the employment of the Gnomonic Projection of the Sphere in Crystallography. L., E. and D. Phil. Mag. for July, 1859.

Daubrée: Memoire sur la relation des sources thermales de Plombières avec les filons métalifères, et sur la formation contemporaine des zéolithes. Extrait du Bulletin de la Société Géologique de France, [2], xvi, p. 562–591.

A. Kenngott: Uebersicht der Resultate mineralogischer Forschungen im Jahre, 1858. 8vo, pp. 229. Leipzig, 1860.—This is a continuation of Dr. Kenngott's excellent reports, and contains a résumé of the results of mineralogical researches made in 1858. Its completeness, and the careful criticisms by Dr. Kenngott render it a most valuable auxiliary in the study of mineralogy.

F. Hessenberg: Mineralogische Notizen, No. 3. 4to, pp. 32. Frankfurt, 1860.—This number of Hessenberg's mineralogical notices, contains contributions to the crystallography of the species lievrite, realgar, heavy-spar, calcite, sphene, anatase, crocoisite, and malachite.

*Descriptions of Species.**

Albite [p. 240, VI].—Analyses of albite, (1.) from Oberhalbstein in Graubunden by Desclabissac (Zeitsch. d. deutschen geolog. Gesell., x, 207). (2.) from Calveras County, California, associated with auriferous pyrites and native gold by F. A. Genth (this Journal, [2], xxvii, 249):

	Si	Al	Fe	Ċa	Ṁg	Ṅa	K̇	ign.	
1.	68·50	18·11	——	0·56	0 66	12·17			=100·00
2.	68·39	19·65	0·41	0·47	——	10·97	*tr.*	0·21	=100·10

Allanite [p. 208, I—VI].—Analyses of *orthite* (1.) from Arendal by Zettel (Ann. d. Chem. u. Pharm., cxii, 85). (2.) mean of four analyses of orthite from Suontaka in Finland by Mendeljef (Kopp's Jahresbericht, 1858, 703):

	Si	Al	Fe	Ṁn	Ċe	La, Ḋi	Ċa	Ṁg	K̇	Ṅa	H	C	
1.	32·70	17·44	16·26	0·34	3·92	15·41	11·24	0·90	0·51	0·24	2·47	0·28	=101·71
2.	48·0	2·4	34·8	——	3·3	Ẏ 1·5	9·3	——	——	——	0·7	——	=100·00

* The paging refers to Dana's Mineralogy, and the Roman numerals, in many places added, to the preceding Supplements.

ANORTHITE [p. 234, II, VI].—Potyka (Pogg. Ann., cviii, 110) has found *anorthite* to be a constituent of the rock occurring associated with hornblende at the Konchekowskoi Kamen in the Urals. The sp. gr. in fragments was 2·731 (17·1° C.), in powder 2·7325 (16·8°). B.B. difficultly fusible. Only partially decomposed, and with gelatinization, by chlorhydric acid.

$\bar{Si}$	$\ddot{Al}$	$\ddot{Fe}$	$\dot{Ca}$	$\dot{Mg}$	$\dot{K}$	$\dot{Na}$	
45·31	34·53	0·71	16·85	0·11	0·91	2·59	= 101·01

corresponding very closely with the formula $\dot{Ca}^3\bar{Si}+3\ddot{Al}\bar{Si}$, or considering silica as $SiO_2=\dot{Ca}\bar{Si}+\ddot{Al}\bar{Si}$.

Anorthite from Carlingford, Ireland, gave on analysis by Prof. Haughton, $\bar{Si}$ 45·87, $\ddot{Al}$ 34·73, $\dot{Ca}$ 17·10, $\dot{Mg}$ 1·55=99·25 (Greg, L., E. and D. Phil. Mag., [4], xix, 13).

ANTIGORITE [p. 281, I, IV].—For description of optical properties of, see Haidinger's article in Pogg. Ann., lxxvii, 94, and Descloizeaux, Ann. des Mines, xiv, (1858.)

APATITE [p. 396, I—VII].—A description of crystallized *apatite* from Pfitsch-Thal in Tyrol is given by G. vom Rath.—Pogg. Ann., cviii, 353.

ATACAMITE [p. 138, I, III, IV, V].—Bibra gives for the composition of *atacamite* from Algodan Bay in Bolivia:

$\dot{Cu}$	Cu	Cl	$\dot{H}$	$\bar{Si}$	
56·00	14·54	16·11	12·13	0·91	= 99·60

—Kopp's Jahresbericht, 1858, 740.

ARAGONITE [p. 448, II, III, IV, V, VII].—Luca has described a variety of aragonite, occurring in the Lias of Gerfalco in Tuscany, which he calls *mossottite.* The mineral is a prismatic fibrous radiated aggregate of a light green color. Sp. gr.=2·384. On heating crumbles and loses its color. Composition:

$\dot{Ca}$	$\dot{Sr}$	$\ddot{C}$	$\dot{Cu}$	$\ddot{Fe}$	Fl	$\dot{H}$	
50·08	4·69	41·43	0·95	0·82	*tr.*	1·36	= 99·33

According to Marcel de Serres, the same variety of aragonite occurs in the province of Messima.—(Kopp's Jahresbericht, 1858, 732.)

A variety of aragonite from Nertschinsk has been named *oserskite* by Breithaupt (B. and H. Zeit., xvii, 54).

AXINITE [p. 213].—This species has been observed at Cold Spring in New York.

BISMUTH [p. 20].—A specimen of *native bismuth,* associated with native gold, from the Peak of Sorato, analyzed by F. A. Genth (this Jour., [2], xxvii, 247) contained:

Bi	Te	Fe	
99·914	0·042	*tr.*	= 99·956

BORACITE [p. 393, II, III, IV].—The identity in chemical composition of *boracite* and *stassfurthite* was shown by Chandler (Suppl. IV), and is further confirmed by the analyses of Siewert and Drenkmann (Zeitschrift f. d. ges. Naturwissen, xi, 365, in Kopp's Jahresbericht, 1858, 735). The direct determinations made on the washed and ignited mineral in seven partial analyses gave as the mean:

$\dot{Mg}$	$\ddot{Fe}$	$\bar{B}$	
30·83	0·32	69·05	= 100·20

But H. Ludwig found (Arch. Pharm., [2], xcvi, 129, in Kopp's Jahresbericht, 1858, 735) that the unwashed air-dried stassfurthite contained

MgCl	$\dot{Mg}$	$\bar{B}a$	$\dot{H}$
11·75	23·80	58·45	6·00

(*a*) By the difference.

A direct determination of $\bar{B}$ gave 59·72 p. c., and Ludwig considered it a hydrous boracite containing a variable mixture of chlorid of magnesium. Heintz, however, has shown that chlorid of magnesium is an essential constituent of the mineral and that even after long washing with hot water it still contains a considerable amount of this chlorid. His results (calculated from five partial analyses) gave (Jour. f. prakt. Chem., lxxvi, 243):

Cl	Mg	$\dot{M}g$	$\overline{F}e$	$\ddot{B}$	$\dot{H}$	
8·14	2·84	25·74	0·43	61·22[a]	1·63	= 100

a By the difference.

from which he draws the formula $2(\dot{M}g^3\ddot{B}^4)+MgCl, \dot{H}$. In communicating these observations of Heintz to the Berlin Academy, H. Rose (loc. cit.) adds that the boracite from Lüneberg also contains chlorine as an essential constituent, and this has been substantiated by the recent analyses of Dr. Julius Potyka (Pogg. Ann., cvii, 433), and also by Heintz (Jour. f. prakt. Chem., lxxvii, 338).

Potyka analyzed both boracite and stassfurthite, and four analyses of boracite are given by Heintz, having been made under his direction by Siewert and Geist. The results are as follows:

	MgCl	$\dot{M}g$	$\overline{F}e$	$\ddot{B}$	$\dot{H}$	
1. Clear boracite crystals,	10·90	25·24	1·59	62·91	0·55=101·19	Potyka.
2. Clouded " "	10·41	26·19	1·66	61·19	0·94=100·39	"
3. Stassfurthite,	10·73	26·15	0·40	60·77[a]	1·95=100·00	"
4. Boracite,	11·14	26·00	1·52	61·34[a]	——=100·00	Siewert.
5. "	11·71	24·86	1·13	62·30[a]	——=100·00	"
6. "	11·11	25·45	1·83	61·61[a]	——=100·00	Geist.
7. "	11·54	25·43	1·05	61·98[a]	——=100·00	"

a. By the difference.

Potyka's specimens, after pulverization, were washed with cold water until the wash water no longer gave any reaction with nitrate of silver or chlorid of barium; he found that both boracite and stassfurthite were slightly soluble in hot water. All the specimens of boracite were from Lüneberg. Heintz was unable to find a weighable quantity of water. Both Potyka and Heintz express the composition of boracite by the formula $2(\dot{M}g^3\ddot{B}^4)+MgCl=\ddot{B}$ 62·50, $\dot{M}g$ 26·86, MgCl 10·64=100. Stassfurthite appears to be a boracite with one atom of water, $2(\dot{M}g^3\ddot{B}^4)+MgCl, \dot{H}$ $=\ddot{B}$ 61·27, $\dot{M}g$ 26·33, MgCl 10·42, $\dot{H}$ 1·98=100.

BROMYRITE [p. 93].—F. Field has analyzed the *bromyrite* which occurs in octahedrons imbedded in carbonate of lime at Chañarcillo in Chile (Quar. Jour. Chem. Soc., x, 241). The crystals had the color and lustre of amber, and are much harder than the chloro-bromids or chlorid, and appear to be little affected by light. Composition,

Ag	Br	
57·43	42·57	= Ag Br

CANCRINITE [p. 233, II].—P. v. Pusirewsky has analyzed the *cancrinite* from the Ilmen Mts. and from Mariinskaja in the Tunkinsk Mts. (Kokscharow, Mat. Min. Russlands, iii, 76):

	$\dddot{S}i$	$\overline{A}l$	$\overline{F}e$	$\dot{N}a$	$\dot{C}a$	$\ddot{C}$	$\dot{H}$	$\dddot{S}$
1. Ilmen Mts.,	35·71	29·58	—	18·78	5·56	5·56	3·76	0·32=99·27
2. " "	36·21	29·56	0·19	18·27	5·81	5·54	3·64	——=99·22
3. Tunkinsk Mts.,	37·72	27·75	—	21·60	3·11	5·61	4·07	——=99·86

The variety from the Ilmen Mts. had a light rose-red color and sp. gr.=2·489. That from Mariinskaja was yellow, sp. gr.=2·454. Pusirewsky writes the formula $2(\dot{N}a^2\dddot{S}i+2\overline{A}l\dddot{S}i+[\dot{N}a, \dot{C}a]\ddot{C})+3\dot{H}$, containing one atom more of water than the formula given by Whitney for the cancrinite of Litchfield, Maine.

CARNALLITE [III].—Heintz gives the composition of *carnallite*, as found by Siewert:

MgCl	KCl	NaCl	$\dot{C}a\dddot{S}$	$\dot{H}$
36·03	27·41	0·23	1·14	36·33—38·01

—Kopp's Jahresbericht, 1858, 739.

CASSITERITE [p. 118, V, VI, VII].—In a recent letter (Boston, Jan. 8th, 1860) to Prof. B. Silliman, Jr., Dr. C. T. Jackson mentions having received from Los Angelos in California a so-called silver ore, which on examination proved to be oxyd of tin mixed with some peroxyd of iron. On assay it gave 60·5 per cent of metallic tin. From the size of the masses of ore, Dr. Jackson is led to suppose that a vein of workable magnitude exists, some specimens being eight inches in thickness.

CERITE [p. 312].—Rammelsberg (Pogg., cvii, 632) has published several analyses of this species. The mean of the results gives—

$\dddot{\mathrm{Si}}$	$\dot{\mathrm{Ce}}$	$\dot{\mathrm{La}}, \dot{\mathrm{Di}}$	$\dot{\mathrm{Ca}}$	$\dot{\mathrm{Fe}}$	$\dot{\mathrm{H}}$
19·18	64·55	7·28	1·35	1·54	5·71=99·61

from which he draws the formula $(\dot{\mathrm{Ce}}, \dot{\mathrm{La}}, \dot{\mathrm{Di}}, \dot{\mathrm{Ca}}, \dot{\mathrm{Fe}})^2\dddot{\mathrm{Si}}+\mathrm{Aq}$, or $\dot{\mathrm{R}}^3\dddot{\mathrm{Si}}+\mathrm{Aq}$. An important fact observed by Rammelsberg is, that when cerite is treated with chlorhydric acid it is partially decomposed, leaving an insoluble residue of a different composition from that contained in the solution. He remarks that it appears as if cerite was composed of a mixture of silicates, differing in not being equally acted upon by acids.

CINNABAR [p. 48, II, IV, V].—Hugo Müller has analyzed and described tetrahedral crystals of cinnabar, from Asturia, Spain, which he supposes to be pseudomorphs of either tetrahedrite or chalcopyrite.—Quar. Jour. Chem. Soc., xi, 240.

CHLORITE [p. 294, IV, V].—For analyses of a chlorite-like substance from the melaphyr-porphyry of Ilfeld by Streng, see Zeitschrift d. deutschen geolog. Gesellschaft, x, 136.

CHRYSOCOLLA [p. 309, II].—P. Herter gives analyses of two varieties of chrysocolla from the crystalline slates of Ober and Nieder-Rochlitz in Transylvania, in Zeitsch. d. deutschen geolog. Gesellschaft, ix, 372. In the same paper Herter mentions a mineral which he considers a new species. It occurs in a geode of quartz. It is amorphous; brittle; color dark pistachio-green to liver-brown and dirty yellowish-green; has a strong pitchy lustre and an almost conchoidal fracture. Sp. gr.=2·991. Contains $\dddot{\mathrm{Si}}$ 14·24, $\mathrm{SbO_5}$ 24·68, $\dddot{\mathrm{As}}$ 7·24, $\dot{\mathrm{Cu}}$ 31·49, $\dot{\mathrm{Pb}}$ 0·68, $\dot{\mathrm{Ag}}$ 2·05, $\dot{\mathrm{Fe}}$ 8·38, $\dot{\mathrm{Ca}}$ 2·16, $\dot{\mathrm{Mg}}$ 0·56, $\dddot{\bar{\mathrm{Al}}}$ 0·21, $\dot{\mathrm{H}}$ 8·03=99·71. Another specimen gave but 16 per cent copper, showing the composition to be variable. In matrass gives water. Fuses easily in the forceps coloring the flame emerald green, with soda on charcoal gives a metallic bead. The centre of several of the masses was found to contain tetrahedrite, of which the above substance is a product of decomposition.

CLAYITE, *W. J. Taylor* (Proc. Acad. Nat. Sci., Philad., Nov. 1859.).—This mineral is a sulphid of lead, with about twenty-five per cent of arsenic, copper and antimony, and appears to be intermediary between galena and cupro-plumbite. It is from Peru, and occurs in small monometric crystals, a combination of the tetrahedron with the dodecahedron. It is also found amorphous, forming a coating a thirty-second of an inch thick on a layer of quartz. Color and streak blackish-gray; sectile; hardness about 2·5. B.B. on charcoal fuses easily, giving reactions for lead, arsenic and antimony, and with soda a brilliant metallic globule which becomes lustreless on cooling. Carefully selected crystals gave—

	S	As	Sb	Pb	Cu	Ag
1.	8·22	9·78	6·54	68·51	7·67	*tr.*
2.	8·14	——	——	67·40	5·62	—

No. 2 was not entirely free from extraneous matters. Prof. Taylor gives the formula (Pb, $\bar{\mathrm{C}}\mathrm{u}$)(S, As, Sb). A confirmatory result was obtained on a specimen of the amorphous variety. The mineral was received from Joseph A. Clay, Esq., of Philadelphia, having been sent to him by his brother, Hon. J. Randolph Clay, United States Minister in Peru, and it is named in honor of these gentlemen. [The amount of sulphur is extremely small, and the presence of arsenic and antimony seems to indicate an analogy with *steinmannite*, which has recently been shown to be a galena, containing some twenty or more per cent of the sulphids of arsenic, antimony and zinc, although this composition varies exceedingly.—G. J. B.]

COAL [p. 26, II, IV, VI].—O. Matter has analyzed (Jour. f. prakt. Chem., lxxvii, 39) the so-called Bog-head coal from Torbane-Hill in Scotland, with the following result:

C	H	N	O	S	$\dot{\mathrm{H}}$	$\dddot{\mathrm{Si}}$	$\dddot{\bar{\mathrm{Al}}}$	$\dddot{\bar{\mathrm{Fe}}}$	$\dot{\mathrm{Ca}}$
60·81	9·18	0·78	4·39	0·32	0·39	13·19	9·50	1·22	0·27=100·05

CONDURRITE [p. 36, V].—An examination of this mineral by C. Winkler (B. u. H. Zeit., xviii, 383) confirms the results obtained by v. Kobell and Rammelsberg, showing this mineral to be a mixture of arsenolite, cuprite, copper-glance, and arsenid of copper.

COPPERAS [p. 380].—A cupriferous variety of copperas from a mine of chalcopyrite in Turkey has been analyzed by Pisani (Comptes Rendus, April 18, 1859). Color like that of cyanosite, on exposure assumes an ochreous tint. Composition—

$\dot{C}u$	$\dot{F}e$	$\dddot{S}$	$\dot{H}$
15·56	10·98	29·90	43·56

giving the formula $(\dot{F}e\ \dot{C}u)\dddot{S}+7\dot{H}$, or copperas in which a portion of the iron is replaced by copper.—(L. E. and D. Phil. Mag., [4], xvii, 409.)

COPPER NICKEL [p. 52, VI].—According to G. Rose and Nöggerath the copper-nickel from Sangerhausen crystallizes in the hexagonal form.—(Zeitschrift d. deutschen geolog. Gesellsch., x, 91; Verhandl. d. naturhist. Ver d. Rheinlande, xv, xv.)

CYANOLITE, CENTRALLASSITE, CERINITE, *H. How*, (Ed. new Phil. Jour., x, 84).—Prof. How has described three new species of silicates occurring in a reniform nodule in the trap of the Bay of Fundy, one mile east of Black Rock. The nodule was about half the size of a fist. It was covered with a green chlorite-like coating, and on breaking it presented a curious internal structure; immediately beneath the coating was a narrow band of a yellowish-white mineral resembling wax (*cerinite*), then a portion having a stellated appearance and a highly pearly lustre (*centrallassite*), while the centre was principally made up of a bluish-gray opaque mineral in rounded spots (*cyanolite*). A careful separation of the constituents showed—1. *Cyanolite*, comprising the centre of the nodule, was amorphous; hardness=4·5; sp. gr.=2·495; fracture flat-conchoidal, even; streak white, lustre dull, color bluish-gray; sub-translucent in thin pieces, and the powder transparent under the microscope. Decomposed with chlorhydric acid, affording slimy silica, but does not gelatinize either before or after heating. In matrass becomes white and gives off water. B.B. in platinum forceps fuses only on the thin edges, with soda and borax gives transparent beads, with salt of phosphorus a translucent glass. Analysis gave—

	$\dddot{S}i$	$\bar{\bar{A}}l$	$\dot{C}a$	$\dot{M}g$	$\dot{K}$	$\dot{H}$
1.	74·15	0·84	17·52	*tr.*	0·53	7·39 = 100·43
2.	72·52	1·24	18·19	*tr.*	0·61	6·91 = 99·47

Analysis No. 2 was made on a specimen not perfectly free from *centrallassite*. Disregarding the small amount of alumina and potash in No. 1, we have the oxygen ratio $\dot{C}a : \dddot{S}i : \dot{H}$ as 1 : 7·85 : 1·31 or 4 : 31·40 : 5·2, from which Prof. How draws the formula $\dot{C}a^4\dddot{S}i^{10}+5\dot{H}=\dddot{S}i$ 74·26, $\dot{C}a$ 18·36, $\dot{H}$ 7·37. Considering the water as basic the ratio of the oxygen in all the bases to that of the silica is as 1 : 3·2, approximating to that of Edelforsite, $\dot{C}a\ \dddot{S}i$ or 1 : 3. The name *cyanolite* is in allusion to the blue tint which distinguishes this mineral from its associates.

2. *Centrallassite* occurs in spherical concretions between the cyanolite and the rind. The concretions when broken have a lamellar structure and consist of plates diverging from a centre; the plates have a pearly lustre, but the mineral passes into an opaque white form. Centrallassite has a white, sometimes yellowish, color; translucent, transparent in thin plates; brittle; lustre sub-resinous; hardness=3·5; sp. gr.=2·45—2·46. In matrass yields water, becomes opaque and silvery-white. B.B. fuses readily, with spirting, to an opaque glass, with the fluxes gives a clear bead. Decomposed by chlorhydric acid without gelatinizing. The result of two analyses were:—

$\dddot{S}i$	$\bar{\bar{A}}l$	$\dot{C}a$	$\dot{M}g$	$\dot{K}$	$\dot{H}$
59·05	1·00	27·86	0·20	*undet.*	11·40
58·67	1·28	27·97	0·13	0·59	11·43 = 100·07

The oxygen ratio of the mean of these analyses for the lime, silica and water is 1 : 3·91 : 1·27=4 : 15·64 : 5·08, from which Prof. How deduces the formula $\dot{C}a^4\dddot{S}i^5+5\dot{H}=\dddot{S}i$ 59·06, $\dot{C}a$ 29·20, $\dot{H}$ 11·74. From two determinations of the water and an estimation of the silica, the opaque mineral was proved to have the same composition as the transparent variety.

3. *Cerinite.* The narrow band enveloping the two preceding minerals (an eighth of an inch in thickness) was an opaque mineral, translucent in very thin fragments; amorphous; lustre sub-resinous, resembling white or yellowish-white wax; H.=3·5. B.B. fusible without intumescence. It was imperfectly decomposed by chlorhydric acid. Two analyses gave—

	Si	Al	Fe	Ca	Mg	K	H
1.	58·13	12·21	1·01	9·49	1·83	0·37	15·96=99·00
2.	57·02	13·11	1·27	10·15	1·91	*undet.*	15·42=98·88

The iron and potash in No. 1 were dissolved out by chlorhydric acid—a fusion with carbonate of soda was made to complete the decomposition. The loss in the analyses is supposed by Prof. How to be due to alkali not determined. The ratio between R, R, Si, H is as 1 : 2 : 9 : 4, and gives the formula 3CaSi+2AlSi³+12H= Si 58·06, Al 14·60, Ca 11·96, H 15·38.

Datholite [p. 334, I—IV, VI].—J. D. Whitney has described a peculiar variety of this mineral, which occurs in nodules in the Minnesota mine, Lake Superior (this Jour. [2], xxviii, 13). The mineral is quite compact and breaks with a conchoidal fracture; it is perfectly white and opaque, resembling in physical character the purest and most close-grained marble. H.=4·5; sp. gr. 2·983. Analysis by C. F. Chandler gave—

Si	Fe, Al	Ca	B (loss)	H
37·41	0·35	25·11	21·40	5·73 = 100.

Q. Sella has in his collection, a crystal of datholite from Baveno, 4½ inches long by 3¼ inches broad, and 1½ inches in thickness.—(Wien Akad. Berichte, xxix, 239.)

Diaspore.—See under *Natrolite.*

Embolite [p. 93].—F. Field has given analyses of three varieties of chloro-bromid of silver from Chañarcillo, in the province of Atacama (Quar. Jour. Chem. Soc., x, 239):

	Ag	Br	Cl		
1.	68·22	16·84	14·92	=	99·98
2.	66·94	19·82	13·18	=	99·94
3.	61·07	33·82	5·00	=	99·89

No. 1 had a pale green color, and its formula is 2AgCl+AgBr. No. 2, of a darker color, and of more frequent occurrence, is identical with Breithaupt's *embolite*, as analyzed by Plattner, 3AgCl+2AgBr. No. 3 was of a very dark green color, sometimes having a purple tint, its formula is AgCl+3AgBr.

Under the names *megabromite* and *mikrobromite* Breithaupt has described two new chloro-bromids of silver (B. u. H. Zeit., xviii, 449). I. *Megabromite.* Lustre adamantine; color siskin to pistachio-green, changing on exposure to the light, to blackish-gray; streak pale green. Crystalline form cubic; cleavage cubic, though not always distinct; fracture conchoidal and uneven; slightly malleable and sectile. H.=2·75—3; sp. gr. 6·230—6·234. Occurs in compact limestone. Analysis by T. Richter:

Ag	Br	Cl	I
64·19	26·49	9·32	*tr.*

The relations of Ag, Br and Cl are as 2·26 : 1·26 : 1, or as 9 : 5 : 4=4AgCl+5AgBr. It bears a very strong resemblance to embolite in physical characters.

II. *Mikrobromite.* Lustre adamantine; color between asparagus and greenish-gray, on exposure becomes ash-gray and opaque; streak white, translucent; crystalline form, cubic; fracture irregular, and without any regular cleavage; very sectile and malleable. H.=2·5—3. Sp. gr. 5·75—5·76. Occurs with native silver in a yellowish-red compact limestone at Copiapo in Chile. Two analyses by Richard Müller gave—

	Ag	Br	Cl
1.	70·28	12·35	17·37
2.	69·81	12·44	17·75

The ratio of Ag, Br and Cl is as 4 : 1 : 3, giving the formula AgBr+3AgCl.

[In this connection it may be interesting to notice the remarks of Domeyko upon the chloro-bromids of silver (Elementos de Mineralogia, 202). He says: "the chloro-bromids vary in color, from grayish-green or yellow, to asparagus and pistachio-green. In general the specimens that have a yellow color have more bromine, and consequently less silver, than those of a gray or pearly green color." Analyses of three specimens of the yellow variety from the mines of Chañarcillo gave Domeyko:

	AgCl	AgBr		Ag	Br	Cl
1.	51·00	49·00	=	66·53	20·85	12·62
2.	52·80	47·20	=	64·84	20·09	13·07
3.	51·00	49·00	=	66·53	20·85	12·62

The ratio of the atoms of Ag, Br and Cl in Nos. 1 and 3, are as $2\frac{1}{3} : 1 : 1\frac{1}{3}$ or 7:3:4, giving the formula $4AgCl+3AgBr$. No. 2 has the ratio $2\frac{1}{2} : 1 : 1\frac{1}{2}$ or 5 : 2 : 3, giving the same formula as Breithaupt's embolite, $3AgCl+2AgBr$. Domeyko also gives four analyses of the grayish green variety which occurs in masses an inch or more in thickness. Of these Nos. 4, 5 and 6, as analyzed below, were from Chañarcillo and 7, from Quillota. All the specimens examined were remarkably homogeneous and pure, some of them as translucent as wax.

	AgCl	AgBr		Ag	Br	Cl	Ratio.
4.	72·9	27·1	=	70·44	11·53	18·03	9 : 2 : 7
5.	65·6	34·4	=	69·14	14·63	16·23	7 : 2 : 5
6.	81·4	18·6	=	71·94	7·92	20·14	20 : 3 : 17
7.	66·4	33·6	=	69·28	14·30	16·42	15 : 4 : 11

If we now recapitulate these together with Field's analyses and the analyses of the three species of Breithaupt, in the order of the increase of bromine, we have:

	Ag	Br	Cl	
Pearly green, Chañarcillo, (6),	71·94	7·92	20·14	Domeyko.
" " " (4),	70·44	11·53	18·03	"
Mikrobromite, Copiapo,	69·84	12·39	17·77	Müller.
Pearly green, Quillota, (7),	69·28	14·30	16·42	Domeyko.
" " Chañarcillo, (5),	69·14	14·63	16·23	"
Light green, "	68·22	16·84	14·92	Field.
Embolite, "	66·94	19·82	13·18	"
" "	66·86	20·08	13·05	Plattner.
" " (2),	66·84	20·09	13·07	Domeyko.
Yellow, " (1 and 3),	66·53	20·85	12·62	"
Megabromite,	64·19	26·49	9·32	Richter.
Dark green, Chañarcillo,	61·07	33·82	5·00	Field.

Here then are ten distinct chemical compounds formed by the union of various proportions of AgCl and BrCl, and so far as known, they all crystallize in the monometric system. As both *bromyrite* (AgBr) and *kerargyrite* (AgCl) crystallize in the monometric system, and as Cl and Br are isomorphous and may replace each other in an infinite number of proportions, it is well to ask, where we shall stop in the making of new species. The five varieties which we have quoted from Domeyko, together with the two varieties analyzed by Field, deserve to rank as species quite as much as embolite, megabromite, and mikrobromite. The varieties of chloro-bromid of silver seem to shade insensibly into each other—the specific gravity increases, and the color deepens in proportion with the increase of bromine. We have already the name embolite admitted in the science, and if the native chloro-bromid should be found which has the ratio 2 : 1 : 1 or $AgCl+AgBr$, it would have the same right to be ranked a species as dolomite, but we protest against making ten or more species of Ag (Br, Cl), for the same reasons that we should protest against making distinct species of all of the varieties of dolomitic limestone.—G. J. B.]

Fergusonite [p. 350, III].—R. Weber has analyzed *fergusonite* from West Greenland (Pogg. Ann., cvii, 590):

$\ddot{\mathrm{C}}\mathrm{b}$	$\ddot{\mathrm{S}}\mathrm{n}$	$\ddot{\mathrm{Z}}\mathrm{r}$	$\dot{\mathrm{Y}}$	$\dot{\mathrm{C}}\mathrm{e}$	$\overline{\mathrm{F}}\mathrm{e}$	$\overline{\mathrm{U}}$	
48·84	0·35	6·93	38·61	3·05	1·48	0·35	= 99·61

This, although differing somewhat from the former analysis by Hartwall, proves the mineral to be distinct from the Norwegian *tyrite*.

Franklinite [p. 166, I, VII].—The chemical composition of this mineral has been carefully studied by Rammelsberg (Pogg. Ann., cvii, 312), and the causes of error, and the disagreement in former analyses pointed out. His analyses, partly from massive, and partly from crystallized specimens, gave:

		1.	2.	3.	4.	5.
G.=5·21	$\overline{Fe}$	64·28	65·32	64·92	63·40	64·64
	$\overline{Mn}$		13·08	13·87	13·28	13·81
	$\dot{Zn}$			25·09	26·83	25·51
				103·88	103·51	103·96

Analyses 4 and 5 were made by Schulz in Rammelsberg's Laboratory. The mean is $\overline{Fe}$ 64·51, $\overline{Mn}$ 13·51, $\dot{Zn}$ 25·30 (excluding $\dot{Zn}$ in No. 4) =103·52, equal to

	Fe	Mn	Zn	O	
	45·16	9·38	20·30	25·16	= 100·00
Equivalents,	4·8	1·	1·8	9·3	.

The atomic proportion between the metals and the oxygen is 7·6 : 9·3=1 : 1·2=5 : 6 or R^5O^6. Rammelsberg proves by experiment that at least a *portion* of the manganese must be sesquioxyd, and after the consideration of several hypotheses in regard to the oxydation of the iron and manganese, which show the impossibility of the composition of the mineral corresponding to the spinel formula, he is led to assume the *whole* of the manganese to exist as sesquioxyd. He then calculates the oxygen remaining, after deducting that united with the zinc and manganese, as belonging to the iron. This gives—

	a	Oxygen.		*b*	Oxygen.	
$\overline{Mn}$	13·51	4·13	13·61	13·51	4·13	12·38
$\overline{Fe}$	31·64	9·48		27·50	8·25	
$\dot{Fe}$	29·55	6·55	11·55	33·31	7·38	12·38
$\dot{Zn}$	25·30	5·00		25·30	5·00	
	100·00		25·16	99·62		24·76

Column *b* is a correction of the calculation so as to make the oxygen of the protoxyds and sesquioxyds equal, and corresponds to the amount of iron found in the analyses. The oxygen thus calculated differs but 0·4 per cent from the total oxygen.

The calculation of the compound R^5O^6 in which Mn : Zn : Fe=1 : 2 : 5 gives Fe 45·24, Mn 8·92, Zn 21·02, O 24·81, or $\overline{Fe}$ 64·66, $\overline{Mn}$ 12·80, ZnO 26·19=103·65, corresponding very well with the results of the analyses, and giving the formula $(\dot{Fe}\dot{Zn})^3(\overline{Fe}\overline{Mn})$ or $\dot{R}^3\overline{R}$. Rammelsberg considers the isomorphism of $\dot{R}\overline{R}$ with $\dot{R}^3\overline{R}$ as a necessary consequence of the isodimorphism of the oxyds $\dot{R}$ and $\overline{R}$, already shown in his recent investigations on hornblende, augite, and the different varieties of specular and titanic iron. [An analysis of *franklinite*, made by the writer in the autumn of 1858, gave results agreeing very nearly with the composition obtained by Rammelsberg, viz. $\overline{Fe}$ 65·05, $\overline{Mn}$ 14·77, $\dot{Zn}$ 23·30, insoluble 0·30=103·12.—G. J. B.]

Galena [p. 36, II—IV, VII].—The so-called *steinmannite*, already referred to galena by Kenngott (Uebersicht, 1855, 109), has recently been analyzed by Schwarz (*Reuss*, Wien. Akad. Ber., xxv, 561), with the following results:

PbS	As_2S_3	SbS_3	ZnS	FeS	
76·48	9·25	0·77	11·38	2·10	= 99·98

Another specimen gave 2 p. c. less lead, a trace of zinc, and scarcely any arsenic, but contained a large amount of antimony. In a third sample a small portion of silver was found. From these facts Prof. Reuss draws the conclusion that sulphid of lead is the only constant constituent, and that *steinmannite* is an impure galena.

Garnet [p. 190, I—VII].—Analyses of *lime-iron-garnet* from the Schischimsk Mts. (1.), and from Achmatowsk (2.), and the *grossular* from the Slüdianka River (3), made under the direction of N. v. Iwanow in the Mining-Department Laboratory in St. Petersburg gave (Kokscharow, Mat. Min. Russlands, iii, 79):

		$\ddot{S}i$	$\ddot{\bar{A}}l$	$\ddot{\bar{F}}e$	$\dot{C}a$	$\dot{M}g$	$\dot{M}n$
1.	G.=3·798.	35·21	*tr.*	34·11	30·96	*tr.*	*tr.*=100·28
2.	———	37·22	6·04	24·81	31·07	0·49	*tr.*= 99.63
3.	G.=3·427.	40·99	14·90	10·94	32·94	0·98	—=100·75

An extended notice of the occurrence of *garnet* in Russia, with description of interesting crystals is given by Kokscharow in his Materialen zur Mineralogie Russlands, iii, 1—40.

GERSDORFFITE [p. 58, VII].—Dr. Genth has detected crystals of this mineral occurring as an incrustation upon partially decomposed galena and blende at Phœnixville, Pa. The crystals are cubes with octahedral planes, and sometimes, though rarely, pentagonal dodecahedrons are found.—This Jour., [2], xxviii, 248.

GLASERITE [p. 365, III].—Prof. W. J. Taylor refers to *glaserite*, with a query, a sulphate of ammonia and potash from the Chincha Islands. It occurs in concretions a half or three-quarters of an inch in diameter. Color yellowish-white; structure crystalline; taste pungent and bitter; opaque; permanent in air. Hardness 2. B.B. blackens and fuses with difficulty, giving a white bead. The results of two analyses were:

	SO_3	NH^4O	KO	NaO	
1.	48·40	5·37	43·45	1·68	= 98·90
2.	48·30	5·10	46·49		= 99·89

Both specimens contained traces of organic matter. The composition gives the formula $(NH^4O, KO, NaO)SO^3$, which differs from that of glaserite only in having a portion of the potash replaced by ammonia and soda (Proc. Acad. Nat. Sci. Philad., Nov., 1859).

HALLOYSITE [p. 251].—Nöggerath has described as a variety of opal, a mineral occurring in a soft gelatinous state in trachyte in the opal mine at Czerweitsa in Hungary (Verhand. d. naturhist. Ver. d. Rheinlande, xv, cii). Upon exposure to the atmosphere the mineral hardens, and its characters approach those of jasper-opal. Analysis by Landolt showed the mineral to lose 5·30 p. c. by drying over sulphuric acid, and the dried substance gave:

$\ddot{S}i$	$\ddot{\bar{A}}l\ddot{\bar{F}}e$	$\dot{C}a$	$\dot{H}$
46·96	36·56	*tr.*	16·10 = 99·62

[Assuming the iron to be an unimportant ingredient, this composition corresponds to the varieties of *halloysite* from Anglar and Housscha (Min., p. 151, anal. 1, 2).—G. J. B.]

HAYESINE [p. 394, III, IV].—Analyses of very pure *hayesine* by Reichardt (Kopp's Jahresbericht, 1858, 737):

	$\ddot{\bar{B}}^a$	$\dot{C}a$	$\dot{N}a$	$\dddot{S}$	Cl	Insol.	$\dot{H}^b$	$\dot{H}^c$
1.	52·05	11·56	*tr.*	0·53	0·94		33·53	1·38
2.	50·42	12·10	*tr.*	1·07	1·21	0·67	33·67	0·87

(*a*) by the difference. (*b*) expelled at a red heat. (*c*) expelled at 100° C.

No. 1 was a specimen from the German importers, and 2 was received direct from Lima. Reichardt gives the formula $\dot{C}a\ddot{\bar{B}}^4+10\dot{H}$. An analysis of *hayesine* by F. W. Helbig (Dingler's Polytech. Jour., cxlvii, 319) gave $\ddot{\bar{B}}$ 46·46, $\dot{C}a$ 14·03, $\dot{N}a$ 5·17, $\dot{H}$ 32·61, NaCl 1·89, $\dot{M}g$ and $\ddot{S}i$ traces. Additional analyses of the commercial article are given in Barreswil's Réperetoire de Chimie Appliquée, i, 215.

HEMATITE [p. 113, II, III, IV, VII].—A specimen of tabular crystalline hematite from Vesuvius analyzed by Rammelsberg (Pogg. Ann., cvii, 453) gave $\ddot{\bar{F}}e$ 98·05, $\dot{M}g$ 1·40=99·45. It contained no protoxyd of iron.

Analyses of hematite from the Lake Superior region by Prof. J. D. Whitney (this Jour., [2], xxviii, 13):

	I. a.	I. b.	I. c.*	II. a.	II. b.	III. a.	III. b.
Insoluble,	1·02	·80	·54	7·92	7·96	1·99	2·05
Iron,	69·41	70·22	69·96	64·42	64·01	68·81	
Oxygen and traces, lime, &c.,	29·57	28·98	29·50	27·66	28·03	29·20	

* Mean of three closely-agreeing determinations.

I. from the Jackson Mountain, II. from the Cleveland, and III. from the Burt or Lake Superior Mountain.

Homichline [VII].—This mineral was referred to *barnhardtite* in the last supplement, but the recent analysis of Richter, published by Breithaupt in the B. u. H. Zeitung, xviii, 321, gives its composition as Fe 25·81, Cu 43·76, S 30·21, and the formula $(Cu^2S)^3, Fe^2S^3+2FeS$. Sp. gr. 4·47—4·48, (Breithaupt.) [The identity of crystalline form of this mineral with chalcopyrite, together with its less degree of hardness, and the difficulty of obtaining it pure and free from admixture with chalcopyrite, would seem to indicate that it might be a product of decomposition of this latter species, or perhaps a mixture of this species, with some of the richer sulphids of copper, such as erubescite or copper-glance. It is interesting in this connection to note Dr. Genth's remarks upon the occurrence of barnhardtite with copper-glance and chalcopyrite at the Pioneer Mills mine (this Journal, [2], xxviii, 248).—G. J. B.]

Hornblende [p. 170, I—IV, VI, VII].—A. Knop has published a description and several analyses by himself and W. Hoffman of an interesting soda hornblende from the serpentine rock at Waldheim in Saxony (Ann. d. Chem. u. Pharn., cx, 363). Color leek-green; translucent; occurring in veins of an inch in thickness and resembling actinolite. H.=5. Sp. gr. 2·957.

	$\dddot{Si}$	$\dddot{\bar{Al}}$	$\dot{Fe}$	$\dot{Mn}$	$\dot{Ca}$	$\dot{Mg}$	$\dot{Na}$	
1.	58·71a	1·52	5·65	0·25	11·53	10·01	12·38=100·05	Knop.
2.	58·45a	1·92	5·53	0·51	10·28	11·12	12·61=100·42	Hoffmann.
3.	58·45a	1·74	5·79	0·32	10·76	10·83	12·93=101·12	"

(*a.*) Mean of two determinations.

It lost 0·5 per cent by ignition. [The analyses give too much silica for the hornblende formula, but this may be accounted for on the supposition that the mineral was partially decomposed, when treated by hydrochloric acid to free it from adhering carbonates. The large percentage of soda is remarkable.—G. J. B.]

Iodyrite [p. 95, 506].—An analysis of iodyrite from Delirio's Mine, Chañarcillo, afforded F. Field, Ag 45·98, I 54·02=AgI.—(Quar. Jour. Chem. Soc., x, 241).

Iridosmine [p. 19, I].—Analyses of *iridosmine* from various localities by Deville and Debray (Ann. Chim. et Phys., [3], lvi, 481).

		Ir	Rd	Pt	Ru	Os	Cu	Fe
1. Columbia,		70·40	12·30	0·10	—	17·20	—	—=100·
2. "		57·80	0·63	—	6·37	35·10a	0·06	0·10=100·06
3. California,		53·50	2·60	—	0·50	43·40	—	—=100·
4. Australia,		58·13	3·04	—	5·22	33·46	0·15	—=100·
5. Borneo,		58·27	2·64	0·15	—	38·94	—	—=100·
6. Russia,		77·20	0·50	1·10	0·20	21·00	tr.	—=100·
7. "	G.=18·9,	43·28	5·73	0·62	8·49	40·11	0·78	0·99=100·
8. "	G.=18·8,	64·50	7·50	2·80	—	22·90	0·90	1·40=100·
9. "	G.=20·4,	43·94	1·65	0·14	4·68	48·85	0·11	0·63=100·
10. "	G.=20·5,	70·36	4·72	0·41	—	23·01	0·21	1·29=100·

(*a.*) In this analysis, the osmium was determined directly, in the others by difference.

Iron [p. 17, II, VII].—F. A. Genth describes in this Journal, [2], xxviii, 246, a specimen of what appears to be telluric iron. It is said to occur near Knoxville, in Tennessee, although its exact locality is not known. It contains neither carbon, phosphorus or sulphur, and its peculiar appearance together with its being associated with a silicate of magnesia, iron and lime, render it probable that it may be a genuine specimen of *native iron*. Dr. Genth describes the mass examined, to have been about one and a half inches square, and three-eights of an inch in thickness. The iron had a grayish-white color, a hackly fracture, and broke easily into fragments, which though crystalline, did not show any distinct planes. It was soft, scarcely scratching fluor-spar. Lustre eminently metallic. Readily dissolved by nitric acid. Composition:—

Fe	Ni	Co	Mg	Ca	Si	
99·790	0·140	*tr.*	0·022	0·121	0·075	= 100·148

A similar mineral has been received by Dr. Genth from Northern Alabama, and it is exceedingly desirable that more definite information should be obtained in regard to the locality and mode of occurrence of this problematical substance.

Kerargyrite [p. 92, IV].—A specimen of chlorid of silver from the "Republicana Mine," Chañarcillo, analyzed by F. Field (Quar. Jour. Chem. Soc., x, 239) contained:

Ag	Cl	
75·27	24·73	= AgCl.

Labradorite [p. 237, II, VII].—Vom Rath gives as the composition of the *labradorite* from the gabbro of Marmorera in Graubündten:

	$\dddot{\mathrm{Si}}$	$\dddot{\mathrm{\bar{A}l}}$	$\dddot{\mathrm{\bar{F}e}}$	$\dot{\mathrm{C}}\mathrm{a}$	$\dot{\mathrm{M}}\mathrm{g}$	$\dot{\mathrm{K}}$	$\dot{\mathrm{N}}\mathrm{a}$
G. = 2·840.	55·45	22·12	4·28	9·68	1·30	1·64	5·73 = 100·20

This mineral lost 2·76 p. c. on ignition.—Zeitsch. d. deutschen geolog. Gesellschaft, ix, 246.

Libethenite [p. 420].—Analyses of *libethenite* from Congo in Portuguese Africa by Hugo Müller (1.) (Quar. Jour. Chem. Soc., xi, 242); from Libethen (2.) by Bergemann, and from Nischne-Tagilsk (3.) by Chydenius (Kopp's Jahresbericht, 1858, 726):

	$\dot{\mathrm{C}}\mathrm{u}$	$\dddot{\mathrm{\bar{P}}}$	$\dddot{\mathrm{\bar{A}}}\mathrm{s}$	$\dot{\mathrm{H}}$	$\dot{\mathrm{F}}\mathrm{e}$	$\ddot{\mathrm{C}}$	
1. Congo,	67·21	28·76		4·03			= 100·00
	66·76	29·02		4·22			= 100·00
2. Libethen,	66·29	26·46	2·30	4·04			= 99·09
3. Nischne-Tagilsk,	64·47	29·48	*tr.*	3·68	1·77	0·82	= 100·22

LILLITE, *Reuss* (Wien Akad. Ber., xxv, 550).—This name has been given by Reuss to a mineral which occurs at Przibram in Bohemia. In physical characters it resembles glauconite, and appears to be a product of the decomposition of pyrites. It is an amorphous, lustreless, earthy substance, having a hardness = 2, and sp. gr. 3·043. Color blackish-green, in very fine powder under the microscope is leek-green by transmitted light. Material selected as pure as possible gave Payr on analysis:

$\dddot{\mathrm{Si}}$	$\dddot{\mathrm{\bar{F}e}}$	$\dot{\mathrm{C}}\mathrm{a}\ddot{\mathrm{C}}$	FeS (insol.)	$\dot{\mathrm{H}}$	
32·48	54·95	1·96	0·63	10·20	= 100·22

On treating the mineral with nitric acid, Payr found after ignition and making allowance for the water, the sulphur of the pyrites, and the carbonic acid, that the iron in the mineral had absorbed 3·43 per cent of oxygen, so that deducting this from the 54·95 $\dddot{\mathrm{\bar{F}e}}$ we have 51·52 per cent of iron plus oxygen. This gives a total of 96·79, a loss of 3·21 per cent in the analysis. The author places the species near hisingerite and cronstedite. It corresponds very closely in chemical composition with the variety of hisingerite from Riddarhyttan in which Rammelsberg found (Min., p. 290):

$\dddot{\mathrm{Si}}$	$\dddot{\mathrm{\bar{F}e}}$	$\dot{\mathrm{F}}\mathrm{e}$	$\dot{\mathrm{C}}\mathrm{a}$	$\dot{\mathrm{M}}\mathrm{g}$	$\dot{\mathrm{H}}$
33·07	34·78	17·59	2·56	0·46	11·54

Magnesite [p. 447, II, III].—Analyses of the magnesite from Snarum and Frankenstein by T. Scheerer (Jour. f. prakt. Chem., lxxvi, 424):

	$\ddot{\mathrm{C}}$	$\dot{\mathrm{M}}\mathrm{g}$	$\dot{\mathrm{F}}\mathrm{e}$	$\dot{\mathrm{C}}\mathrm{a}$	
Snarum (crystallized),	52·13	46·66	0·78	0·43	= 100·00
Frankenstein (amorphous),	55·34	47·43	—	0·22	= 100·00

The small amount of mechanical mixture, amounting in the Snarum specimen to 0·05—0·1405 per cent, and in the Frankenstein specimen to 0·048 p. c., have been subtracted from the above.

MAGNOFERRITE.—Rammelsberg (Pogg. Ann., cvii, 451) gives this name to the octahedral iron which occurs interlaminated with hematite, in the fumaroles formed at Vesuvius after the eruption of 1855. His former analyses showing the presence of a considerable amount of magnesia are contained in Suppl. VII. Two additional analyses of portions selected out by means of the magnet from the finely pulverized mineral gave:

		F̶e	Ṁg	Ċu	Insol.
1.	G.=4·568.	82·91	13·60	0·99	2·51 = 100·01
2.	G.=4·638.	83·30	13·41	0·59	2·00 = 99·30

which, excluding the oxyd of copper and insoluble portions, gives (1.) F̶e 85·92, Ṁg 14·09=100·01, and (2.) F̶e 85·51, Ṁg 13·77=99·28.

The former analyses (Suppl. VII) thus calculated are: *a.* F̶e 86·96, Ṁg 12·59=99·55; *b.* F̶e 84·20, Ṁg 16·00=100·20; *c.* F̶e 84·35, Ṁg 15·65=100. Analysis *a* was made from selected crystals, *b* was a portion extracted by the magnet from the associated hematite, while *c* was a specimen thus selected from one of the older Vesuvian hematites. Rammelsberg considers the composition of the crystals as $\dot{M}g^m$ $F̶e^n$ in which probably $m=3$ and $n=4$, the regular (monometric) form being due to the isodimorphism of Ṙ and R̶.

MARGARODITE [p. 223].—An analysis of an authentic specimen of margarodite from the original locality at Pfitsch gave Hlasiwetz (Kenngott's Uebersicht, 1858, 67):

S̈i	A̶l	F̶e	Ċa	K̇	Ṅa	Ign.
45·48	33·80	6·25	0·48	7·31	6·22	0·36 = 99·90

Kenngott remarks that this composition may be represented by the formula Ṙ S̈i+2R̶ S̈i, but adds that this is of little value, as on closer examination with the magnifier, the specimen proved to be an intimate mixture of a mica with granular quartz and minute crystals of feldspar. [The mica from Lane's Mine, analyzed by Smith and myself, and referred by Dana to margarodite, is distinctly foliated and apparently perfectly homogeneous. It is identical in composition with the so-called margarodite from St. Etienne analyzed by Delesse (Min., p. 224).—G. J. B.]

MARIONITE (*Elderhorst*), see *Zinc-bloom.*

MEGABROMITE (*Breithaupt*), see *Embolite.*

MIKROBROMITE (*Breithaupt*), see *Embolite.*

MISPICKEL [p. 62, 509, I, II, III, V].—Analyses of mispickel from Sahla in Sweden by J. Potyka (Pogg. Ann., cvii, 304):

	S	Fe	As	Sb	Bi
G.=6·095.	19·13	34·78	43·26	1·29	0·14 = 98·60

These results give the received formula, $FeAs_2+FeS_2$, differing from the analysis by Behncke (Suppl. III), which corresponded to $3FeS^2+2Fe^2As^3$. Potyka shows the want of agreement between the analyses to be due to the fact that mispickel suffers partial decomposition by simply boiling in water. The sp. gr. of small fragments he found to be 6·043—6·047, that of the powder boiled for some time in water was only 5·819 to 5·874, and on examination of the water appreciable quantities of sulphuric acid, iron and arsenic were found in solution.

C. v. Hauer obtained in two analyses of mispickel from Kindberg in Styria:

S̈i	A̶l	Ċa	Fe	As	S
5·0	1·0	0·3	30·8	43·2	18·9 = 99·2
0·7	0·3	*tr.*	32·7	45·0	21·0 = 99·7

—Jahrb. d. k. k. geolog. Reichsanstalt in Kopp's Jahresbericht f. 1858, p. 678.

According to Daubrée (Compt. Rendus, 1858, xlvii, 959) the lignite of the tertiary formation at Lobsann (Lower Rhine) contains from 0·002—0·0008 per cent of arsenic, and on dissolving the bituminous limestone from the same locality a fine amorphous residue (amounting to about 2 p. c.) is obtained, which gives the reactions of mispickel.

MOLYBDATE OF IRON [p. 144, I, II].—The so-called *molybdate of iron* described by D. D. Owen has recently been examined by Dr. Genth (this Jour., [2], xxviii, 248), and from the varying proportions of the iron—in one case 35 p. c., in another 24·3—he questions whether the substance may not be a mechanical mixture of molybdine and limonite.

MOSSOTTITE.—See *Aragonite.*

NAGYAGITE [p. 65].—Folberth has analyzed the foliated-tellurium from Nagyag. It occurs in six-sided tables in a pearl-gray quartz, and has a specific gravity =6·68. Treatment with sulphid of carbon extracted 25 p. c. of the amount of sulphur. Two analyses gave (Verhandl. d. siebenbürg. Ver. f. Naturwissensch., viii, 98, in Kenngott's Uebersicht f. 1856–7, 179):

Pb	Au	Sb	S	Te	Se
60·83	5·84	3·69	9·76	17·22	*tr.* = 97·34
60·27	5·98	3·86	9·68	18·04	*tr.* = 97·83

differing very materially from the previous analyses by Klaproth, Brandes and Schönlein.

NATROLITE [p. 327, VI, VII].—A variety of transluscent natrolite from Fassa in Tyrol, analyzed by Hlasiwetz, gave the following composition (Kenngott's Uebersicht, 1858, 72):

$\ddot{\text{S}}\text{i}$	$\ddot{\bar{\text{A}}}\text{l}$	$\dot{\text{C}}\text{a}$	$\dot{\text{M}}\text{g}$	$\dot{\text{N}}\text{a}$	$\dot{\bar{\text{H}}}^{a}$	$\dot{\bar{\text{H}}}^{b}$
43·34	27·43	3·60	0·40	9·00	10·30	0·90 = 99·97

(*a*) basic water. (*b*) hygroscopic water.

agreeing very nearly with the composition of galactite, which has been shown to be a variety of natrolite by Dana and Heddle (Suppl. I and III).

In an article upon *Spreustein* (natrolite) (Pogg. Ann., cviii, 431) Scheerer shows the cause of color, of the red and brown varieties of this mineral, to be due to mechanical impurities. A microscopical examination of several varieties showed that only the perfectly white specimens were entirely free from mechanical mixture. The white varieties were perfectly decomposed by chlorhydric acid giving a homogeneous jelly, while with the colored varieties the gelatinized mass always contained suspended more or less of an opaque white powder. If however, the decomposition was made with nitric acid, this powder retained the original color of the natrolite. A separation of the insoluble powder on some twenty grams of the mineral gave material to determine the character of this substance. The results of two analyses prepared from different varieties gave:

	$\ddot{\text{S}}\text{i}$	$\ddot{\bar{\text{A}}}\text{l}$	$\ddot{\bar{\text{F}}}\text{e}$	$\dot{\bar{\text{H}}}$
1.	1·58	76·75	6·77	14·70 = 99·80
2.	0·82	82·56	1·52	15·00 = 99·90

These give the formula $\ddot{\bar{\text{R}}}\dot{\bar{\text{H}}}$, and the powder is *diaspore* in which a portion of the alumina is replaced by iron. The quantity of this mineral in the specimens of natrolite analyzed by Scheerer varied from 4 to 7 p. c. This will explain the reason of the different analyses of Spreustein differing from each other, and also from pure natrolite. The following analyses may serve as examples. I. Crystallized colorless natrolite from Brevig analyzed by Dr. Sieveking in Scheerer's laboratory. II. Dark brownish-red Spreustein from an island of Brevigfjord, by Scheerer.

	$\ddot{\text{S}}\text{i}$	$\ddot{\bar{\text{A}}}\text{l}$	$\ddot{\bar{\text{F}}}\text{e}$	$\dot{\text{C}}\text{a}$	$\dot{\text{N}}\text{a}$	$\dot{\bar{\text{H}}}$
I.	47·16	26·13	0·53	0·53	15·60	9·47 = 99·42
II.	44·50	30·05	0·98	0·83	13·52	9·93 = 99·81

II. contained $6\frac{3}{4}$ p. c. of diaspore, which when subtracted from the above gave $\ddot{\text{S}}\text{i}$ 47·47, $\ddot{\bar{\text{A}}}\text{l}$ 26·83, $\ddot{\bar{\text{F}}}\text{e}$ 0·60, $\dot{\text{C}}\text{a}$ 0·88, $\dot{\text{N}}\text{a}$ 14·42, $\dot{\bar{\text{H}}}$ 9·61=99·81; this is almost exactly the composition obtained for I. Scheerer adds that the so-called *brevicite* (Min., 327, anal. 14, 15, and 16) is nothing more than natrolite, which contains a considerable percentage of diaspore. For the further consideration of the disputed points upon the paramorphous nature of *Spreustein* (Paläo-Natrolith) see the original paper in Pogg. Ann., cviii, pp. 416—435.

Nepheline [p. 232, II].—P. v. Pusirewsky has analyzed the *elæolite* which is associated with cancrinite, zircon and other minerals in the Graphite-Mine at Mariinskaja in the Tunkinsk Mts. (Kokscharow Mat. Min. Russlands, III, 78), and J. P. Kimball has described the same variety of nepheline, occurring with sodalite at Salem, Mass. (this Jour., [2], xxix, 65):

	$\dddot{Si}$	$\ddot{\bar{Al}}$	$\ddot{\bar{Fe}}$	$\dot{Ca}$	$\dot{Mg}$	$\dot{Na}$	$\dot{K}$	Ign.	
1. Mariinskaja,	44·94	30·29	0·72	1·15	0·15	21·80	1·48	——	=100·53 P.
2. Salem, G.=2·63.	44·31	32·80	*tr.*	0·40	——	16·43	5·50	1·47	=100·91 K.

Nickel and Copper, arseniuret of.—This ore, mentioned by T. Sterry Hunt (this Jour., [2], xix, 417) as a mixture of *domeykite* and *copper-nickel*, has since been thoroughly examined by both Prof. Hunt (Rep. Geol. Survey, Canada, 1853–6, p. 388) and Prof. J. D. Whitney (this Jour., [2], xxviii, 15) giving analyses which confirm the above conclusion.

Nickel-gymnite [p. 286, VII].—An ore, apparently an impure variety of this mineral, is described by T. Sterry Hunt (this Jour., [2], xix, 417, and Rep. Geol. Survey, Canada, 1853–6, p. 389), as occurring with the nickel ores of Michipicoten Island, Lake Superior.

Oligoclase [p. 239, I].—Vom Rath has given analyses of *oligoclase* from the granite of Albulaberge, and also of a compact *lime oligoclase* from the diorite of Piz-Rosag, in Granbündten (Zeitschr. d. deutsch. geol. Gesell., ix, 226, 259):

	$\dddot{Si}$	$\ddot{\bar{Al}}$	$\ddot{\bar{Fe}}$	$\dot{Ca}$	$\dot{Mg}$	$\dot{K}$	$\dot{Na}$	
Albulaberge, G.=2·725.	62·01	21·16	2·54	3·53	0·78	4·33	5·94	=100·29
Piz-Rosag, G.=2·835.	57·64	22·99	3·92	8·09	0·37	1·79	5·25	=100·05

The analyses were made on the ignited mineral. The specimen from Albulaberge lost 1·05 per cent, and that from Piz-Rosag 1·32 per cent on ignition.

The leek-green *feldspar* associated with pyrrhotine at Bodenmais (Bavaria) has been analyzed by Potyka (Pogg. Ann., cviii, 366). It is triclinic, and has the characteristic striæ on the cleavage surfaces. Sp. gr. 2·604. Composition:

$\dddot{Si}$	$\ddot{\bar{Al}}$	$\dot{Fe}$	$\dot{Ca}$	$\dot{Mg}$	$\dot{K}$	$\dot{Na}$	
63·12	19·78	1·51	0·66	0·13	12·57	2·11	= 99·87

This gives an oxygen ratio of $\dot{R} : \ddot{\bar{R}} : \dddot{Si}$, of 1 : 2·86 : 10·17 or nearly 1 : 3 : 10,—a ratio intermediary between that of oligoclase and orthoclase; the sp. gr. (2·604) is also between that of oligoclase (2·56) and orthoclase (2·67).

Orthoclase [p. 242, II, III, V—VII].—J. D. Whitney has analyzed the interesting crystallized *orthoclase* which is associated with native copper, calcite and the zeolites in many of the Lake Superior copper mines (this Jour., [2], xxviii, 16). It occurs in distinct crystals of a reddish flesh color, having a striking resemblance to stilbite. The crystals are rarely as much as one-tenth of an inch in length. Composition:

$\dddot{Si}$	$\ddot{\bar{Al}}$	$\ddot{\bar{Fe}}$	$\ddot{\bar{Mn}}$	$\dot{K}$	$\dot{Na}$	
65·45	18·26	0·57	*tr.*	15·21	0·65	= 100·14

Oserskite (*Breithaupt*), see *Aragonite*.

Pectolite [p. 305, II, III, VI, VII].—Analyses of three specimens of the very pure variety of this mineral from Bergen Hill, N. J., by J. D. Whitney (this Jour., [2], xxix, 205):

	$\dddot{Si}$	$\dot{Ca}$	$\dot{Mn}$	$\dot{Fe}$	$\dot{Na}$	$\dot{H}$
1.	54·82	33·12	0·66	2·6	8·78	2·36
2.	54·76	32·88		1·16	9·17	2·03
3.	54·27	32·83		1·24	8·94	2·72

(*a*) By the difference.

The direct determination of the water on the substance dried at 60° C. gave, for 2, 3·03, and for 3, 2·75 p. c. These results agree very closely with the previous analysis published by Prof. Whitney (Min., anal. 5). Specimen 3, from the Wheatley Collection in Union College, was considered the purest, and the analyses gives the oxygen ratio for $\dot{H}$, $\dot{Na}$, $\dot{Ca}$, $\dddot{Si}$ as 1 : 1·05 : 3·83 : 11·84 or nearly 1 : 1 : 4 : 12, or the formula, as expressed by Prof. Whitney, $\dot{Na}^3\dddot{Si}^4+4\dot{Ca}^3\dddot{Si}^2+3\dot{H}$ = $\dddot{Si}$ 54·22, $\dot{Ca}$ 33·73, $\dot{Na}$ 9·33, $\dot{H}$ 2·74. This corresponds much better with the results obtained than v. Kobell's formula, in which the oxygen ratio is 1 : 1 : 4 : 11.

Prof. Whitney calls attention to the relations of pectolite to spodumene, and also to wollastonite and pyroxene, the latter connection being more apparent when the formula is written $(\dot{Ca}\tfrac{4}{6}, \dot{Na}\tfrac{1}{6}, \dot{H}\tfrac{1}{6})^3\dddot{Si}^2$ or $\dot{R}^3\dddot{Si}^2$.

Pennine [p. 295, II, IV, V].—A new analysis of *pennine* from Zermatt by Victor Merz, gave (Kenngott's Uebersicht f. 1858, 63):

$\dddot{Si}$	$\dddot{Al}$	$\dot{Fe}$	$\dot{Mn}$	$\dot{Mg}$	$\dot{H}$
33·26	11·69	7·20	*tr.*	35·18	12·18 = 99·51

not differing materially from the previous results of Marignac, Schweizer and MacDonnel. For an extended discussion of the chemical composition of this mineral by Dr. Kenngott, see loc. cit., pp. 62—66.

Pholerite (p. 251].—F. A. Genth describes (this Jour., [2], xxviii, 251) *pholerite* as occurring in the coal mines of Schuylkill Co., Pa., in yellowish white scales which become of a snow white color, and pearly lustre, on being treated with chlorhydric acid. Under the microscope the scales appear to be clinorhombic, having the planes *i-i* and −1-*i*. The specimens examined were from Tamaqua near Pottsville. Analysis No. 1, was made on the original mineral, in Nos. 2 and 3 the substance had been previously treated with chlorhydric acid. Nos. 1 and 2, were decomposed by fusion with carbonate of soda—No. 3 by treatment with sulphuric acid.

	$\dddot{Si}$	$\dddot{Al}$	$\dddot{Fe}$	$\dot{Ca}$	$\dot{Na}$	$\dot{K}$	$\dot{H}$
1.	46·93	37·90	0·18	0·93	*undetermined*		13·98 = 99·92
2.	46·98	39·65	—	—	0·11	0·06	13·69 = 100·49
3.	46·81	39·56	—	—	0·11	0·06	13·91 = 100·45

giving the formula $\dddot{Al}^3\dddot{Si}^4+6\dot{H}$ = $\dddot{Si}$ 47·06, $\dddot{Al}$ 39·20, $\dot{H}$ 14·71.

Phosphochalcite [p. 425, II, VI, VII].—Bergemann has found arsenic acid in all the native phosphates of copper.—Analysis of the *phosphochalcite* from Linz gave:

	$\dot{Cu}$	$\dddot{P}$	$\dddot{As}$	$\dot{H}$
Phosphochalcite,	69·97	19·89	1·78	8·21 = 99·85

(Abstract from Pogg. Ann., civ, 190, in Kopp's Jahresbericht, 1858, 726).

Pitchblende [p. 107, IV, V].—Hermann has given (Jour. f. prakt. Chem., lxxvi, 326) the name *uranoniobite* to the crystallized pitchblende from Strömsheien in Norway, previously described and analyzed by Scheerer (see Min., p. 108, anal. 5, under pitchblende). Scheerer remarks in his description (Pogg. Ann., lxxii, 568) that it is possible that the metallic acids found in the analysis may be due to admixture with a substance he calls *Niob-pelopsaures Uran-Manganoxydul* (columbate of uranium and manganese) with which the pitchblende is associated;—further investigation is needed to establish its claims to be considered a distinct species.

Hermann also gives a new analysis of the *pitchblende* from Joachimsthal:

	PbS	$\dddot{Si}$	$\dddot{Al}$	$\dddot{Fe}$	$\dddot{Bi}$	$\dddot{U}$	$\dot{U}$	$\dot{Pb}$	$\dot{Mn}$	$\dot{Ca}$	$\dot{Mg}$	$\dot{H}$
G.=6·97.	2·84	2·45	0·33	1·88	1·23	52·37	28·84	0·74	0·14	5·78	0·41	2·59

with traces of arsenic. In the same paper Hermann communicates an analysis of the so-called *pittinite* (Pittinerz, Breithaupt) from Joachimsthal. The mineral occurs in amorphous opaque masses of a pitch-black color. It has an uneven and slightly conchoidal fracture, and a highly resinous lustre. Streak greenish-brown. H.=4. Sp. gr. 5·16. Heated in tube yields water containing traces of fluorine and ammonia; fused with soda on charcoal gives a globule containing lead and bismuth. Easily decomposed by nitric acid with separation of gelatinous silica on evaporation. Composition:

$\dddot{Si}$	$\dddot{U}$	$\dddot{Fe}$	$\dddot{Bi}$	$\dot{Pb}$	$\dot{Ca}$	$\dot{Mg}$	$\dot{H}$	Insol.
5·00	68·45	4·54	2·67	2·51	2·26	0·55	10·06	3·20 = 99·24

with traces of fluorine, ammonia, phosphoric and carbonic acids. [This substance is evidently very nearly related to pitchblende, and is probably a result of the alteration of that mineral. Hermann endeavors to show that silica is an essential constituent of pitchblende and allied uranium minerals, but as most of these substances are amorphous, and as their composition varies considerably, it seems possible that the silica may be due to admixture with some earthy silicate.—G. J. B.]

PITTINITE.—See *Pitchblende.*

PLATINUM [p. 12, I—IV].—Analyses of platinum from various localities, by H. St.Claire Deville and H. Debray (Ann. de Chimie, [3], lvi, 449):

	Pt	Ir	Rh	Pd	Au	Cu	Fe	X[a]	Sand	Pb(?)	Os & loss	
1.	86·20	0·85	1·40	0·50	1·00	0·60	7·80	0·95	0·95	—	—	=100·25
2.	80·00	1·55	2·50	1·00	1·50	0·65	7·20	1·40	4·35	—	—	=100·15
3.	76·82	1·18	1·22	1·14	1·22	0·88	7·43	7·98	2·41	—	—	=100·28
4.	85·50	1·05	1·00	0·60	0·80	1·40	6·75	1·10	2·95	—	—	=101·15
5.	79·85	4·20	0·65	1·95	0·55	0·75	4·45	4·95	2·60	—	0·05	=100·00
6.	76·50	0·85	1·95	1·30	1·20	1·25	6·10	7·55	1·50	0·55	1·25	=100·00
7.	51·45	0·40	0·65	0·15	0·85	2·15	4·30	37·30	3·00	—	—	=100·25
8.	45·70	0·95	2·65	0·85	3·15	1·05	6·80	2·85	35·95	—	0·05	=100·00
9.	59·80	2·20	1·50	1·50	2·40	1·10	4·30	25·00	1·20	—	0·80	=100·00
10.	61·40	1·10	1·85	1·80	1·20	1·10	4·55	26·00	1·20	—	—	=100·20
11.	77·50	1·45	2·80	0·85	*undet.*	2·15	9·60	2·35	1·00	—	2·30	=100·00
12.	76·40	4·30	0·30	1·40	0·40	4·10	11·70	0·50	1·40	—	—	=100·50

(*a*.) Iridosmine.

Nos. 1, 2, and 3 from Choco (Columbia), South America; 4, 5, and 6 from California; 7, Oregon; 8, Spain; 9, 10, Australia; 11, 12, Russia.

For analyses of platinum ore from Goenoeng Lawack in Borneo by Prof. Bleekrode see Pogg. Ann., cvii, 189.

PYRITES [p. 54, I, IV].—G. Rose has described a pseudomorph of *pyrites* after *pyrrhotine*, the crystals are six-sided prisms, two inches across and one inch in thickness (Zeitschrift d. deutsch. geolog. Gesellschaft, x, 98).

PYROMORPHITE [p. 400, II, IV].—Analyses of Russian *pyromorphite* by Struve (Kokscharow, Mat. zur Min. Russlands, iii, 42):

		PbCl	$\dot{Pb}$	$\dot{Fe}$, $\ddot{Cr}$	$\dddot{As}$	$\dddot{V}$	$\dddot{P}$	
1. Beresowsk,	G.=6·715.	9·94	73·36	0·59	—	*tr.*	15·82	= 99·71
2. Altai (Tomsk),	G.=5·537.	10·13	73·40	—	2·61	—	12·90	= 99·04

PYROXENE [p. 158, I, II, V—VII].—Reuss has described a compact white *pyroxene* from Oberrochlitz in Bohemia. Under the microscope it shows a crystalline structure. The mineral is snow white when pure, but sometimes has a light green color from admixture with chrysocolla, malachite and allophane. H.=5·5—6. G.=3·398. Decomposed by chlorhydric acid with gelatinization. Analysis by v. Payr (Wien Akad. Ber., xxv, 557):

$\ddot{Si}$	$\dot{Ca}$	$\dot{Mg}$	$\dot{Fe}$	$\dot{Mn}$
55·03	20·72	15·71	4·84	3·16=99·46

PYRRHOTINE [p. 50, I, II].—Analysis of *pyrrhotine* from Bernkastel on the Moselle by Baumert gave Fe 61·0, S 39·4, and no nickel (Verhandl. d. naturhist. Ver. d. Rheinlande u. Westphalens, xiv, s. lxxxv). For observations on nickeliferous pyrrhotine from Snarum see Müller in B. u. H. Zeitung, xvii, 304.

QUARTZ [p. 145, II—IV, VII].—Blum and Carius have described *quartz* as pseudomorph of *celestine*, from Girgenti. The crystals contained $\ddot{Si}$ 98·80, $\dot{Sr}\dddot{S}$ 1·78 (Pogg. Ann., ciii, 628, in Kopp's Jahresbericht, 1858, 745).

REALGAR [p. 31, VI].—Analysis of *realgar* from Pola de Lena in Asturia, Spain, by Dr. Hugo Müller (Quar. Jour. Chem. Soc., xi, 242) gave S 30·00, As 70·25.

RIPIDOLITE [p. 296, I, V].—An interesting and peculiar variety of this mineral from Steele's Mine, Montgomery Co., North Carolina, has been described by Dr. F. A. Genth in this Journal, [2], xxviii, 250. Composition:

S̈i	A̶l	F̶e	Ḟe	Ṁn	Ṁg	Ḣ
24·90	21·77	4·60	24·21	1·15	12·78	10·59

SAPONITE (Nicklès).—For a more extended description of this silicate noticed in the Suppl. VII, see Ann. de Chimie, [3], lvi, 46.

Saualpite.—A synonym for a variety of zoisite from the Saualp in Carinthia.

SCHEELITE [p. 347].—F. A. Genth has found *scheelite* at the Bangle Mine in Cabarras Co., and also at Flowe mine, Mecklenburgh Co., North Carolina. At the former place it occurs in granular masses three-fourths of an inch in diameter; it has a pale yellowish-brown color, and a distinct octahedral cleavage. Composition (this Jour., [2], xxviii, 252):

W⃛	S̈n	Ċu	F̶e	Ċa	
79·52	0·13	0·08	0·18	19·31	= 99·22

The variety from Flowe mine was observed in crystals, in one case a modification of the octahedron 1, truncated by 1-*ī*,—crystal about three-tenths of an inch in length; another specimen, half this size, had an orange color and was a combination of the planes $\frac{1}{8}$ and *i-i.*

Another variety from Flowe mine, forming what Dr. Genth calls *rhombic tungstate of lime*, occurs in small indistinct crystals—the largest one-quarter of an inch long. Each crystal has a nucleus of wolfram, and the following planes are given: *I*, *i*-ī, $\frac{1}{2}$-ī, 1, and 1-ĭ; cleavage could not be observed. Dr. Genth does not believe these crystals to be pseudomorphs, and suggests that tungstate of lime is dimorphous,—a conclusion which, though extremely interesting, we hesitate to accept until the subject has been more fully investigated.

SERPENTINE.—Observations on the crystalline structure of serpentine by Websky in Zeit. d. deutschen geol. Gesellsch., x, 277.

SMITHSONITE [p. 447, I, III, VII].—For analyses of zinc ores from Arkansas by Dr. Elderhorst see First Geological Report of Arkansas, pp. 147–155.

SODALITE [p. 229, II, VI].—J. P. Kimball has published a description and analysis of sodalite, from an erratic block of compact syenite at Salem, Mass. (this Jour., [2], xxix, 67). The mineral was associated with elæolite, orthoclase, biotite, zircon, and albite (?) Occurs in crystalline, sub-translucent masses; cleavage indistinct; lustre greasy; color lavender-blue. Sp. gr. on three specimens 2·294, 2·303, 2·314. Chemical composition:

S̈i	A̶l	F̶e	Ṅa	Cl	
37·33	32·70	*tr.*	24·31	6·99	= 101·33

Calculating the chlorine to exist as chlorid of sodium we have:

S̈i	A̶l	F̶e	Ṅa	Na	Cl	
37·33	32·70	*tr.*	18·17	4·57	6·99	= 99·76

corresponding very closely to the analyses of the sodalite from Litchfield in Maine by Whitney (Min., anal. 5, 6). Dr. Kimball remarks that the sodalite from both Litchfield and Salem, is found in erratic blocks, but the absence of cancrinite as an associating mineral in the Salem specimens, would seem to favor their being derived from different sources.

STROYMEYERITE [p. 48].—Prof. W. J. Taylor has described and analyzed a variety of *stroymeyerite* occurring at Copiapo in Chile (Proc. Acad. Nat. Sci. Phila., Nov. 1859). It is found in small six-sided trimetric crystals not larger than one-eighth of an inch in diameter. Its hardness is 2·5—3. Lustre metallic; color dark steel-gray; streak nearly black and shining. Sectile, crystals brittle. It occurs in barytes in small cavities associated with quartz crystals, and upon the latter are implanted the crystals of stroymeyrite, together with small crystals of pyrargyrite. Analysis gave:

S	Ag	Cu	Fe	
16·35	69·59	11·12	2·86	= 99·92

This composition differs materially from the published analyses of stroymeyerite, although not more than the analyses of specimens from different localities vary from each other. $\mathrm{\bar{C}u}$ and Ag appear to replace each other in this mineral in all proportions. The formula is ($\mathrm{\bar{C}u}$, Ag, Fe)S.

TALKOID, *Naumann* (Mineralogie, 5te, Aufl. 255).—The sparry crystalline talc from Presnitz described by Scheerer (Pogg. Ann., lxxiv, 321, this Jour., [2], xiv, 39) has been named *talkoid* by Naumann. It is snow white and broadly foliated occurs with magnetite at Presnitz. Sp. gr. 2·48. Composition, according to Scheerer and Richter:

	$\dddot{Si}$	$\bar{Al}$	$\dot{Fe}$	$\dot{Mn}$	$\dot{Ca}$	$\dot{Mg}$	$\dot{H}$	
1.	58·46	0·09	1·09	——	0·61	32·83	6·56	= 99·64
2.	58·70	0·06	1·01	0·39	0·81	32·07	6·56	= 99·50

for which Naumann gives the formula $\dot{Mg}^3\dddot{Si}^5+\dot{H}$.

TANTALITE [p. 351, III—VI].—A. E. Nordenskiöld has analyzed tantalite from a new locality at Björtboda in Finland (Pogg. Ann., cvii, 374):

$\dddot{Ta}$	$\ddot{Sn}$	$\dot{Fe}$	$\dot{Mn}$	
83·79	1·78	13·42	1·63	= 100·62

the oxygen ratio between the bases and the metallic acids is 1 : 4·83, most nearly resembling the composition of the Tammela tantalite.

TENNANTITE [p. 84, II].—Vom Rath has published the following analyses of tennantite from Cornwall (Verhandl. d. naturhist. Ver. d. Rheinlande u. Westphalens, xv, s. lxxii, in Kopp's Jahresbericht f. 1858, 680):

	Density.	S	Cu	Fe	Zn	As	
1*a*.	4·652.	25·22	46·88	6·40	1·33	18·72	= 98·55
1*b*.	——	27·13	44·43	6·88	1·43	20·13	= 100·00
2.	4·69.	26·34	52·97	2·82	——	18·06	= 100·19

No. 1*a* is the direct result of the analysis—the mineral was associated with black oxyd of copper, and assuming the amount of this substance to be seven per cent, and averaging the analysis to one hundred, gives the result as in No. 1*b*. Analysis No. 2 was by Baumert. According to v. Rath, the ratio between the metallic sulphids and the sulphid of arsenic in tennantite is 5 : 4, while the analogous ratio in tetrahedrite is 4 : 3.

TOURMALINE [p. 270, II, IV, VII].—Jenzsch (Pogg. Ann., cviii, 648) has examined a crystal of *tourmaline* from Elba which he considers to be optically bi-axial. He suggests, from his investigations, that although the tourmaline crystals from Elba and Penig (Saxony) approach very nearly the hexagonal form, that they belong either to the trimetric or monoclinic system—a view previously suggested by Breithaupt's measurements. Breithaupt publishes a preliminary notice in the Berg und Hüttenmannische Zeitung, xix, 93, of a forthcoming monograph on this subject.

TRIPHYLINE [p. 406, 513].—F. Oesten obtained from the analysis of a very pure specimen of triphyline from Bodenmais in Bavaria (Pogg. Ann., cvii, 438):

	$\bar{P}$	$\dot{Fe}$	$\dot{Mn}$	$\dot{Ca}$	$\dot{Mg}$	$\dot{Li}$	$\dot{K}$	$\dot{Na}$	$\dddot{Si}$	
G.=3·545—3·561.	44·19	38·21	5·63	0·76	2·39	7·69	0·04	0·74	0·40	=100·05

This gives the oxygen ratio between the bases and phosphoric acid 15·34 : 24·77 = 3·09 : 5, and the formula, $\dot{R}^3\bar{P}$, the same as first proposed by Fuchs. Wittstein, in a recent note (Pogg. Ann., cvii, 511), calls attention to the fact that eight years since he published results giving the above formula, and says moreover, that a portion of the iron exists as sesquioxyd. Oesten has since (Pogg. Ann., cviii, 648) published proof that the specimen he examined was entirely unaltered, and that all the iron existed as protoxyd.

TYRITE (?) [I, III, IV].—Potyka (Pogg. Ann., cvii, 590) has analyzed specimens of supposed tyrite from Norway which prove to be a new columbate containing several per cent of potash, and distinct from the tyrite of Forbes. The chemical composition was found to be:

$\bar{C}b$	$\ddot{Zr}$	$\dddot{W}$	$\ddot{Sn}$	$\dot{Pb}$	$\dot{Cu}$	$\dot{Y}$	$\dot{Ce}$	$\dot{Fe}$	$\dot{U}$	$\dot{Ca}$	$\dot{Mg}$	$\dot{K}$	$\dot{H}$	
48·49	0·80	1·35	0·09	0·41	0·35	31·90	3·68	1·12	4·12	1·95	*tr.*	7·23	3·71	= 100·20

The ratio between the metallic acids and bases, exclusive of the water is, as 1 : 1·04 or $\dot{R}^3\ddot{C}b$. The mineral occurs implanted in red feldspar in small irregular masses having an uneven fracture, but no distinct cleavage. Lustre, sub-metallic; color black, in thin splinters reddish-brown and translucent on the edges; streak reddish-brown; hardness that of apatite (5). Sp. gr. in coarse powder =5·124 (16·6° C.). When hot water is poured upon fragments a crepitation or crackling takes place. B.B. with borax gives a reddish-yellow bead while hot, which on cooling becomes yellow; with salt of phosphorus is completely dissolved to a greenish-yellow bead while hot, becoming green on cooling. No reaction for manganese with soda. Treatment with concentrated sulphuric acid gave no reaction for fluorine. [This mineral corresponds in many of its physical and blowpipe characters with the *bragite* of Forbes (see Suppl. III). Possibly a thorough analysis of authentic specimens of bragite would show them to be very nearly related, if not identical. —G. J. B.]

Uranium, silicates of, see Hermann's paper in Jour. f. prakt. Chem., lxxvi, 320.

Uranoniobite (*Hermann*), see *Pitchblende.*

URANOCHALCITE, *Hermann.*—This name has been given by Hermann to a mineral from Joachimsthal (Jour. f. prakt. Chem., lxxvi, 321). It occurs in reniform amorphous masses having a metallic appearance. Fracture compact, and slightly conchoidal, with a feeble metallic lustre; brittle; opaque; color between steel-gray and pinchbeck-brown; streak black. H.=4. Sp. gr. 5.04. Heated in a closed tube the mineral at first gives off water, and then a sublimate of realgar, and finally metallic arsenic, leaving a black residue consisting chiefly of bismuth, uranium, copper, and iron. Treated with nitric acid the mineral is dissolved with separation of sulphur. On evaporation of the solution, silica separates in the gelatinous form. The analysis gave:

S	As	Cu	Ni	Fe	$\dddot{Si}$	$\bar{Bi}$	$\bar{U}$	$\bar{Fe}$	$\dot{Fe}$	$\dot{H}$	Ag
5·79	7·23	10·21	0·97	2·31	4·40	36·06	14·41	11·95	3·27	2·40	*tr.*=99·00*

Hermann writes the formula $5(\dot{R}^4\dddot{Si}+4\bar{R}\dddot{Si}+10\dot{H})+R(AsS)$. [It is quite improbable that this composition is that of a simple mineral, and until further investigation we may reasonably doubt the homogeneousness of the substance analyzed.—G. J. B.]

Vanadinite [p. 362, II—IV].—Kokscharow considers the *vanadinite* crystals from Beresowsk to be pseudomorphs of *pyromorphite.* Struve found in the interior of each vanadinite crystal a portion of unaltered pyromorphite. The mean of two analyses gave:

	PbCl	$\dot{P}b$	$\bar{Fe}\bar{Cr}$	$\dddot{V}$	$\dddot{P}$
G.=6·863.	9·60	71·13	0·43	15·92	2·92

Struve represents this composition by the formula $PbCl+\dot{P}b(\dddot{P}\frac{1}{2}, \dddot{V}2\frac{1}{2})$ or $(3\dot{P}b^3\dddot{P}+PbCl)+5(3\dot{P}b^3\dddot{V}+PbCl)$.—(Kokscharow, Mat. Min. Russlands, iii, 44).

Vivianite [p. 415, III, IV].—For an article on the composition and formation of *vivianite* by Alphonse Gages, see L., E. and D. Phil. Mag., [4], xviii, 182.

Water [p. 110].—Analysis of water from the Dead Sea, by Dr. F. A. Genth.—Ann. d. Chem. u. Pharm., cx, 240.

Wolfram [p. 351, I—IV].—F. A. Genth has published (this Jour., [2], xxviii, 253) an analysis of the wolfram which forms the nucleus of the peculiar tungstate of lime crystals alluded to under scheelite. One crystal showed the planes *I*, *i*-$\bar{i}$, $\frac{1}{2}$-$\bar{i}$, and 1-$\bar{i}$. Sp. gr. 7·496 (at 25° C.). Composition:

$\dddot{W}$	$\ddot{Sn}$	$\dot{Fe}$	$\dot{Mn}$	$\dot{Ca}$
75·79	*tr.*	19·80	5·35	0·32 = 101·26

corresponding to variety II. (Min., p. 352), having the formula $4\dot{Fe}\dddot{W}+\dot{Mn}\dddot{W}$.

* The original gives 100, but owing to a typographical, or other error the analysis adds up only 99.

WULFENITE [p. 349, II, V].—The massive wulfenite from Garmisch, is a mixture of molybdate of lead, with carbonate of lead and other substances, as shown by Wittstein's analysis (Kopp's Jahresbericht, 1858, 721):

$\dot{Pb}$	$\dot{Ca}$	$\dot{Mg}$	$\ddot{\overline{Fe}}$	$\dddot{Mo}$	$\dddot{Si}$	$\overline{\dot{P}}$	Loss ($\ddot{C}$ & trace $\dddot{V}$)
33·80	10·85	3·57	2·80	20·00	16·20	0·04	12·74

ZINC-BLOOM [p. 460, 513, VII].—Dr. Elderhorst has described a hydrous carbonate of zinc from Marion County, Arkansas, as a new species under the name *marionite* (First Geol. Rep. Arkansas, p. 153). The chemical composition he found:

$\dot{Zn}$	$\ddot{C}$	$\dot{H}$		
73·26	15·01	11·81	=	100·08

[This is identical with analyses 1*a*, of zinc-bloom from Santander in Spain, by Peterson and Voit, published in the last supplement. This analysis gave $\dot{Zn}$ 73·1, $\ddot{C}$ 15·1, $\dot{H}$ 11·8. These analysts found that zinc-bloom undergoes a change on exposure to the air, thereby losing both carbonic acid and water. A specimen of 1a, exposed to the air for three months was found to contain $\dot{Zn}$ 74·73, $\ddot{C}$ 13·81, $\dot{H}$ 11·45. Other analyses by Braun are quoted in the last supplement. Peterson and Voit (Ann. d. Chem. u. Pharm., cviii, 50) give the formula for zinc-bloom $\dot{Zn}_5$, $\ddot{C}_2$, $\dot{H}_3$, which is the same as that given by Dr. Elderhorst for *marionite;*—it is an interesting fact that this is also the composition of the precipitate, produced by adding an equivalent of carbonate of soda to a zinc salt at the boiling temperature. Marionite may be considered as zinc-bloom, and the earlier analyses of this species by Smithson and Berthier, are undoubtedly less correct than those of Karsten and the more recent ones by Peterson, Voit, Braun, and Elderhorst.—G. J. B.]

Terreil mentions the occurrence of zinc-bloom at Santander in oolitic grains (L'Institut, No. 1347).

ART. XXXIV.—*Theoretical Determination of the Dimensions of Donati's Comet;* by Prof. W. A. NORTON.

IT is proposed in the present article to investigate the dimensions of the great comet of 1858, at certain specified dates, upon the theory developed in this Journal, (vol. xxvi; No 79), and compare the theoretical determinations with the results of observation. Resuming the equation of the approximate orbit of a particle emitted from the nucleus, obtained in the investigation alluded to, viz.

$$\frac{\sqrt{2pr}}{k\sin a}\left(1-\sqrt{1-\frac{k\sin a}{pr}z}\right)=\sqrt{\frac{2x}{k\cos a}} \quad . \quad . \quad . \quad (1.)$$

in which the axis of z coincides with the original direction of motion, a denotes the angle of inclination of this initial line of direction to a line perpendicular to the radius-vector, r the radius of the nucleus, p the acceleration due to the repulsive force of the nucleus at its surface, and k the opposite acceleration produced by the sun's repulsion; let us pass to a new system of rectangular axes, x' and z', of which the axis of z' is coincident with the radius-vector of the orbit of the comet. Effecting the transformation of coördinates, reducing, and denoting by H the distance of the vertex of the cometary envelope from the nucleus $\left(=\frac{pr}{k}\right)$, we have

$$x'^2-2H\sin 2a\,.\,x'=-2H\sin 2a\cot a\,.\,z'. \quad . \quad . \quad . \quad . \quad (2.)$$

Putting $H\sin 2a=K$,

$$x'^2-2Kx'=-2K\cot a\,.\,z'. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3.)$$

Let $z'=0$, and we obtain for the half-breadth of the envelope, $\frac{b}{2}=2K$; and thence, for the coördinates of the vertex of the curve described by the particle, $X'=\frac{b}{4}=K$, and $Z'=h=\frac{K}{2}.\tan g\,a$. Transferring the coördinates to this point, we get for the equation of the curve, referred to its vertex,

$$x''^2=2K\cot a\,.\,z''. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (4).$$

This is the equation of a parabola, of which the parameter, $2p_1=2K\cot a=4h\cot^2 a$; and the distance from the focus to the vertex $=\frac{K}{2}\cot a=h\cot^2 a$.

It is also the equation of the curve that would be described by a particle if it were projected from the nucleus with a certain velocity, and subsequently repelled by the sun alone. From which it appears that the path pursued by a particle repelled from the nucleus is very nearly the same, and, for the purposes of the present investigation, may be regarded as the same, as that which would be followed if the particle were simply projected from the nucleus. If we had occasion to trace accurately the trajectory of the particle in the vicinity of the nucleus, another investigation would become necessary. It should also be observed, that in the case of any particle, which, on its return from its excursion toward the sun, comes into proximity to the nucleus, the parabolic projectory becomes materially modified by its repulsive action, and equations (3 and (4) are inapplicable.

We may conclude from the result just obtained that, so far as the form and dimensions of the nebulous envelope are concerned, the theory of a repulsion exerted by the mass of the nucleus does not differ materially from that of the projection of the cometary matter by an instantaneous force from its surface; which, it appears, has been advocated and discussed by Bessel.

Other determinations relative to the envelope of the comet may be effected by the following formulas; in which Z = the greatest distance attained by a particle, in the initial direction of motion; Y = the actual distance from the nucleus, of the particle when in this extreme position; ϱ = the angle included between Z and Y; β = the inclination of the tangent drawn to any point of the curve followed by the particle, to the radius-vector of the orbit of the comet; $v=$ the velocity of the particle at the vertex of its parabolic path; and $v'=$ its velocity at any other point of the curve;

$$H=\frac{pr}{k}. \quad Z=\frac{H}{\sin a}. \quad Y=\frac{H}{\sin^2 a}. \quad \ldots\ldots \quad (5.)$$

$$\varrho=\text{co}.\, a. \quad \text{tang}\,\beta=\sqrt{\frac{H \sin 2a \cot a}{2z''}}=\sqrt{\frac{p_1}{2z''}}. \quad \ldots \quad (6.)$$

$$v=\sqrt{kp_1}. \ldots \quad (7.) \qquad v'=\frac{v}{\sin\beta}. \quad \ldots\ldots \quad (8.)$$

$$v'=\sqrt{k(p_1+2z'')}=\sqrt{k\left(p_1+\frac{x''^2}{p_1}\right)}. \quad \ldots \quad (9.)$$

More accurately, we may obtain the velocity v'' at right angles to the radius-vector, for any point of the actual curve, from the following equation:

$$v''=\cos a\sqrt{2pr\left(1-\frac{r\cos a}{x'}\right)}. \quad \ldots\ldots \quad (9^a.)$$

in which $x'=$ distance of the point from the radius-vector. The distance from the nucleus to any point of the trajectory of the particle, whose coördinates are known, may be readily obtained from the polar equation of the curve.

Equs. (1) to (9^a) have been obtained on the supposition that the nucleus is at rest; or, in other words, they refer to the relative motion of the cometary particle and nucleus, on the supposition that the two have the same velocity, and a constant direction of motion through space. Strictly speaking, there is not a perfect accordance between the two motions, even during the short interval of time that the particle remains within the limits of the envelope; but no material modifications of the theoretical results are required on this account, in investigating the form and dimensions of the envelope. But when we undertake to follow the cometary particle, after it has left the region of the envelope, and is receding from both the nucleus and the sun, under the influence of the solar repulsion, it will no longer answer to neglect the orbitual motion of the nucleus.

The general problem, to find the relative positions of a repelled cometary particle, and the nucleus of a comet, after any interval of time, appears to have been first effectually solved by Bessel. This important problem has recently been taken up independently, and solved anew by Prof. Peirce; who has shown that the orbit of the repelled particle is a hyperbola convex towards the sun, and has verified the supposed law of variation of the sun's force of repulsion. In pursuing the line of investigation in hand, we are led to take a point of view somewhat different from that occupied by either of these eminent astronomers. It is now proposed to determine both the true and apparent positions of the receding particle, after the lapse of any interval of time, directly from the initial velocity and direction of motion; in order to take account of the various circumstances of the original motion of the different particles supposed to proceed from the nucleus. The following formulas will serve for this purpose. Equs. (12) to (15) have been deduced from the general equations of motion of a body around a centre of attraction, by changing the sign of the force, and adapting them to convenient computation. Equ. (17) for calculating the true anomaly of the particle in its hyperbolic orbit, from the time, was independently investigated. It is sufficiently accurate for our purpose, and the calculation can be more readily effected with it than by the intervention of the eccentric anomaly. The constants which enter into the equation can be determined by very simple formulas, for any comet the elements of whose orbit are known, and for any position of the comet in its orbit; their values having been determined by other means for the perihelion of any one comet. They depend upon the initial circumstances of motion of the particle emitted from the nucleus. Equ. (16) was deduced from equ. (17).

If any particle, on leaving the sphere of influence of the nucleus, is subject to a diminished attraction from the sun, it will describe a hyperbola concave toward the centre of attraction, and will recede from the nucleus, though less rapidly than if it were effectively repelled by the sun. Equs. (22) to (25) serve for this case. There will be occasion to make use of them when we shall undertake to determine all possible particles that at any assumed date may go to make up the concave outline of the tail.

New Haven, March 28th, 1860.

(*To be continued.*)

Art. XXXV.—*The Great Auroral Exhibition of Aug. 28th to Sept. 4th,* 1859.—4th Article.

In the three preceding numbers of this Journal we have given observations of the Aurora of Aug. 28th to Sept. 4th, from almost every part of North America between the parallels of 13° and 48° north latitude. We now present a summary of observations of the same aurora in Europe, with some reports from Asia, and accounts of a simultaneous auroral exhibition in the southern hemisphere.

1. *Observations at Christiania, Norway,* (lat. 59° 54′), *by* Prof. CHRISTOPH HANSTEEN.

1859, Aug. 28th. At 10 P. M. only an indistinct coruscation behind the clouds in the north.

Aug. 29th, $12^h\ 10^m$ A. M. perfectly bright, almost as at full moon; the air dim with cirro-stratus, nevertheless the aurora shone through everywhere with strong radiating and flaming motion, very irregularly and unsteady. Corona was often formed; best formed at $12^h\ 17{\cdot}5^m$. Altitude 71° 37′ from south, azimuth 9° 57′ east. At $12^h\ 18{\cdot}5^m$ a purple-colored beam shot in east to γ Andromedæ. At $12^h\ 21{\cdot}5^m$ altitude of corona 72° 27′; azimuth 14° 55′ east. At no time were there regular bows. There was always a vacant space over the south horizon, but often of a suspicious character. It continued after 1 A. M. without essential variation in strength or character.

Aug. 29th, evening, rain—heavens covered.

Aug. 30th, " " " "

Aug. 31st, $11\frac{1}{2}$ P. M., lightning and thunder in southwest.

Sept. 1st, heavy rain—thunder.

Sept. 2d, radiating and strong flaming aurora, $12\frac{1}{2}^h$.

Sept. 3d, radiating aurora over the whole northern heavens to a little south of zenith; rather dimly. It continued to illuminate the heaven after it was almost covered. At $1\frac{3}{4}^h$ A. M. very clear behind the skies everywhere.

Sept. 4th, 10 P. M., radiating aurora in the north to 30° altitude. Later in the night, vehemently flaming with broad flames.

Sept. 5th, 10 P. M., elegant radiating aurora which dilated from the whole northern horizon to south of zenith, mostly behind a veil of cirro-stratus. At an altitude of 45° it was partly flaming. At 12^h it had nearly ceased.

Sept. 6th, at 10 P. M., an arc from 6° to 8° broad, the lower edge of which had an altitude of 5°.

The following table shows the state of the Bifilar magnetometer between Aug. 28th and Sept. 6th:—

1859.	Hour.	Bifilar.	1859.	Hour.	Bifilar.	1859.	Hour.	Bifilar.
	h m			h m			h m	
Aug. 28	9 23 A. M.	704·09	Aug. 30	9 15 A. M.	670·10	Sept. 2	4 27 P. M.	1438·91
" "	2 10 P. M.	764·98	" "	1 55 P. M.	751·18	" "	5 30 "	1104·69
" 29	9 28 A. M.	243 0	" "	5 30 "	732·08	" "	6 30 "	1020·41
" "	9 43 "	349·3	" 31	9 16 A. M.	667·72	" 3	9 21 A. M.	650·12
" "	10 9 "	638·78	" "	1 55 P. M.	737·95	" "	1 58 P. M.	926·13
" "	10 15 "	531·81	Sept. 1	9 21 A. M.	678·02	" 4	9 19 A. M.	670·05
" "	10 27 "	709·11	" "	2 5 P. M.	711·26	" "	4 28 P. M.	1034·2
" "	10 30 "	670·18	" 2	9 21 A. M.	609·70	" "	4 45 "	959
" "	10 33 "	723·43	" "	9 49 "	780·07	" 5	9 14 A. M.	705·15
" "	1 55 P. M.	941·50	" "	11 29 "	616·87	" "	9 15 P. M.	1060·49
" "	5 35 "	801·42	" "	2 25 "	1881·77	" 6	9 11 A. M.	641·85

The greatest difference observed during this period was 1195·91 parts of the scale. One division of the scale corresponds to $\frac{1}{15970}$ of the horizontal intensity. Hence the variation of the horizontal intensity from Aug. 29 to Sept. 2 amounted to nearly $\frac{1}{13}$ of its whole value.

The inclination of the magnetic needle was observed as follows:

Aug. 29th, 10^h 21^m A. M.	71° 31′·5	Sept. 2d, 10^h 23^m A. M.	71° 29′·0
5 23 P. M.	71 19·8	4 16 P. M.	70 26·9
		6 26 P. M.	71 5·8

The mean inclination of the needle in 1859 was 71° 18′.

The effect of this aurora upon the telegraph lines in Norway was much greater than in France and Germany. The effect was noticed from the opening of the stations at 7 A. M. On the 29th communication was interrupted till 11 A. M. on almost all the lines; and likewise Sept. 2d, but with a long repetition after 2 P. M. Sept. 3d, only towards 8½ A. M. During the remaining parts of those days, the perturbations were more or less uninterrupted, nevertheless communication could be maintained in some degree. Strong currents caused simultaneous attractions of all the armatures. The galvanometer showed strong deviations, sometimes with slow, sometimes with sudden movements, from one side to the opposite.

The intensity of the currents was greatest upon the longest lines going towards the north, on which sparks and uninterrupted discharges were from time to time observed. Pieces of paper were set on fire by the sparks of these discharges. In Bergen, where the line to Stavanger runs in a north and south direction, the current was at times so strong, especially Sept. 2d and 3d, that it was necessary to connect the lines with the earth in order to save the apparatus from destruction. The phenomena appeared less strong in Christiansand, in the southern part of Norway, where the lines run east and west.

2. *Observations made in different parts of England; extracted from the London Times.*

A. Durham (lat. 54° 46′).

Sept. 1, aurora; Sept. 2, vivid white aurora; Sept. 3, aurora Sept. 4, faint aurora.

B. Preston (lat. 53° 45′), by R. C.

Sept. 2d, there was a brilliant auroral display, continuing from 11 to 12 o'clock, and a second appearance, though not so brilliant, at a little before 2 o'clock on the morning of Sept. 3d. During the first display, the whole of the northern hemisphere was as light as though the sun had set an hour before, and luminous waves rolled up in quick succession as far as the zenith, some of a brilliancy sufficient to cast a perceptible shadow on the ground. To the northwest there was a large patch of light of a deep crimson hue, while the waves of light were white, as also were the streamers which occasionally shot across the northern part of the sky. It was the most brilliant aurora that has been witnessed here for many years.

C. Nottingham (lat. 52° 57′), by E. J. Lowe.

On the evening of Aug. 28th and morning of Aug. 29th there was an unusually brilliant auroral display. From 8^h 40^m P. M. until 9 P. M. Aug. 28th, curtains of red light were visible near the zenith. By 11^h 40^m P. M. the glare of orange light in the north was powerful enough (even through much cloud) to make the hands of a watch visible. At 12^h 25^m A. M. the light was so strong that it gave the impression of daylight. At 12^h 45^m an opening in the clouds near the zenith disclosed the cupola which was situated exactly on Alpha Andromedæ. At 1^h 15^m A. M. magnificent rays of light met two degrees east of Alpha Andromedæ. At this time three-fourths of the sky was covered with aurora. At 2^h 30^m, there being more clear sky, a splendid mass of aurora was visible, forming an ever changing cupola close to Gamma Trianguli. All the coruscations moved slowly eastward. At 3^h 15^m the cupola was formed near Gamma Andromedæ.

Sept. 3, strong aurora near the horizon.

Sept. 4, aurora.

D. Grantham (lat. 52° 55′).

Aurora Aug. 28th, 29th, 30th, 31st, and Sept. 3d.

E. London (lat. 51° 37′).

Aug. 28th, at 11^h 30^m P. M., auroral light in the north. At 0^h 15^m A. M. Aug. 29th it assumed the form of a luminous arch, similar to daybreak, and in the southwest there was an intense glare of red covering a very large extent; at 0^h 20^m streamers; at 0^h 25^m the streamers rose to the zenith and were tinged with crimson at their summits; at 0^h 45^m frequent coruscations; at 1^h 0^m the arch which had partially faded was re-formed, the body of light being very strong, but not sufficient to enable one to read any but very large print; at 1^h 30^m light equally strong, but outline indistinct; at 2 A. M. much less light and very indistinct. Continued till 2^h 30^m A. M.

Sept. 3, aurora.

F. Clifton (lat. 51° 27′), by William C. Burder.

Aug. 28th, about 10^h 45^m P. M., commenced a brilliant auroral display. At first there were several fine streamers, some of them white, and some faint crimson, extending from near the horizon almost vertically to α and β Ursæ Majoris. From that time till midnight there were generally very beautiful streamers, but without lateral motion, most of them being not quite vertical, but inclining slightly towards the east at the top. There was also always a general light, extending at midnight from northeast to west, and sometimes bright enough to enable a person to read the time on the face of an ordinary watch.

The aurora was repeated Sept. 1st, Sept. 2d, and Sept. 3d.

G. Aldershot (lat. 51° 15′).

Magnificent display of aurora Aug. 28th, and till early morning Aug. 29.

H. Brighton (lat. 50° 50′).

Aug. 29th, about half past one o'clock, a fine aurora occupied more than one-half the sky. It had the appearance of an irregular hemisphere of white light fringed with a band of crimson from twenty to thirty degrees broad, stretching from southwest to northeast by east.

3. *Observations at St. Valery, France,* (lat. 50° 10′ N., long. 1° 37′ E.), *by* H. Lartigue, *from Comptes Rendus,* T. xlix, p. 367.

Near St. Valery a white light of considerable intensity was noticed in the north at $11^h\,40^m$ P. M. Aug. 28th. A red column, with sides nearly parallel, and 4° or 5° in breadth, rose from the N.N.W. nearly to the zenith, but disappeared after a few minutes. About $12^h\,10^m$ the white light near the horizon had increased in intensity; a large part of the heavens was colored red, and the exhibition attained its greatest brilliancy at $12^h\,20^m$. Magnificent columns and brilliant rays, changing from red to green and white, rose to the zenith, sometimes passed beyond it, and occupied the entire space between Aquila and the meridian, and a few minutes later extended to the constellation Auriga. The light was bright enough to allow objects to be seen at a distance of one mile, as during a clear night with a full moon. The illumined portion of the sky increased till $12^h\,40^m$. After this time the brightness diminished near the meridian, but the east and west portions continued red. At $1^h\,15^m$ the vertical columns again appeared very brilliant, and nearly as extensive as at $12^h\,40^m$, but they soon disappeared. The red light grew fainter, and disappeared entirely at 2^h. The white light which marked the commencement of the phenomenon continued three-quarters of an hour longer.

4. *Observations at Paris, France,* (lat. 48° 50′), *by* M. Coulvier Gravier, *from Comptes Rendus,* T. xlix, p. 338.

The aurora was first noticed at Paris at 2^h on the morning of August 29th, and it soon rose to a great height above the horizon. About $2^h\,45^m$ the vertex of the grand arch had reached the trapezium in Cetus, being 150° from the northern horizon, and it extended from Monoceros to 10° south of θ Aquilæ; having an amplitude of more than 200°. The vertex of the small arch rose to η Draconis, being a height of 26°; and it extended from Cerberus to Leo Minor, having an amplitude of more than 100°. The exhibition continued until the morning twilight. A motion of translation from W.SW to E.SE. was suspected, but the motion was not very appreciable. When the aurora appeared in its greatest brilliancy, the substance which composed it appeared to be in a state of great agitation; and the rays exhibited a red color, sometimes like that of iron heated to redness and sometimes to a white heat. The space occupied by the small arch was, as usual, of a greenish color; the centre near the horizon being black, and the whole destitute of rays. The aurora exhibited the greatest brilliancy between the W. and N.E. points of the horizon. A few cirrus clouds were noticed during the exhibition; they were all black, without any reflection of the light of the aurora, proving that this light emanated from a region much above that of the clouds.

Magnetic effects of the Aurora; from the Comptes Rendus, T. XLIX, p. 473.

On the 26th of August some anomalies were noticed in the motions of the magnetic instruments at the Observatory of Paris, the declination having changed 22′ between 9½ A. M. and noon.

Aug. 28th at 5 P. M. the motion of all the magnetic instruments was very irregular. Between midnight and 1 A. M. of Aug. 29th the horizontal intensity varied 0·0074. At 9 A. M. of the 29th the horizontal intensity had diminished by 0·01, while the vertical component had increased 0·0013.

During the forenoon of the 29th the declinometer was very much disturbed, and at 11 A. M. it oscillated 41′ on each side of its mean position. Towards evening the disturbances disappeared; but a fresh disturbance commenced on the 1st of September, at $11^h\ 30^m$ A. M. About 4 P. M. Sept. 2d, there commenced a new magnetic storm, more violent than that of Aug. 29th. The magnets were carried beyond the range of their scales, showing a change of the horizontal intensity exceeding 0·014, but as the observations were only recorded photographically, the extreme range could not be determined.

Effect on the Telegraph Wires, from the Comptes Rendus, T. XLIX, p. 365.

From the evening of Aug. 28th until the morning of the 29th the needles of the magnetic telegraph at Paris were almost constantly in motion, as if a permanent current was passing through the telegraph wires. Business was therefore entirely interrupted, and could not be resumed until 11 A. M. Aug. 29th. The same effect was noticed on the telegraph lines from 4^h to 8^h on the morning of Sept. 2d, although no aurora was noticed on that day. Business was again interrupted, the needles were disturbed, and the bells were rung.

The galvanometers were violently deflected, sometimes to the right and sometimes to the left. The needles were turned from zero 10° or 20°, remained there stationary for a short time, then suddenly moved to 30° or 50°, then returned and were deflected in like manner on the other side of the zero point. The effect was more powerful and longer continued on the lines from Paris to Bordeaux, Marseilles and northward, than it was on the east and west lines. During the night of Aug. 29th some intelligible signals were received from Strasbourg.

During the day, Aug. 30th, the telegraph operators experienced frequent interruptions. On the afternoon of Sept. 1st some difficulty was experienced in telegraphing; but Sept. 2nd, at 4^h 50^m A. M., there was a general disturbance on all the lines, first on those to Bordeaux, Toulouse, Marseilles, London and Brussels, and a few minutes later on those to Basle, Strasbourg, Havre and Brest. At 7 A. M. *bright sparks* were noticed on the conductors of the lines to Bordeaux and Toulouse. The line to Strasbourg was less affected than the others. About 3 P. M. telegraphic communication was resumed on all the lines; but during the evening and the next morning it frequently happened that the communication was difficult.

Observations of Ozone.

Regular observations are made at Versailles on the amount of ozone in the atmosphere. During the auroras of Aug. 29 and Sept. 2, the quantity of ozone was decidedly greater than usual. The following table shows the sums of ozone collected during each period of six days, from Aug. 4, to Sept. 8, 1859:—

	Morning.	Evening.
From Aug. 4 to Aug. 10	64·0	55·6
Aug. 10 to Aug. 16	87·0	59·0
Aug. 16 to Aug. 22	82·0	60·0
Aug. 22 to Aug. 28	65·0	55·0
Aug. 28 to Sept. 2	97·0	64·0
Sept. 2 to Sept. 8	81·0	58·0

5. *Observations at Brussels, (lat.* 50° 51′), by M. Quetelet, from L'Institut of Feb. 1, 1860.

At 12^h 35^m A. M. Aug. 29th, the sky was overcast with a light and uniform veil, with the exception of the northern horizon, which presented a slight appearance of twilight. Soon there appeared in the N.W. a rosy light, which, in a few seconds, assumed enormous dimensions. It rose to an altitude of 60°, and illumined all that portion of the sky. The rosy light rapidly extended, and soon changed to purple, presenting the appearance of a vast conflagration. There was a constant oscillatory movement, and the light varied from a bright yellow to the deepest red. Near the horizon the sky presented a greyish and dirty appearance. There were faint traces of an obscure segment, whose centre was on the magnetic meridian. Bright rays of a yellowish white shot up from this part of the horizon, traversed the rosy light in the N. W., and terminated in a bundle at a distance of 90° from their origin.

About $12^h\ 45^m$ A. M., the twilight which illumined all the northern region became more intense; the general tint continued of a yellowish white, but on the eastern and western borders passed into a yellowish green. Then there appeared on the N. N. E. a second rosy light, but less decided than that of the N. W. This was also traversed by yellow rays; but the latter were much more brilliant and broader than those which traversed the light in the N. W. Those rays also terminated in a bundle at a distance of 43° from their origin.

Subsequently the aurora presented frequent alternations of brightness, but the general appearance continued the same until 2 o'clock, when observations were suspended.

At 9 A. M. Aug. 29th a disturbance of the magnetic instruments was noticed at the Observatory. The following table shows the extreme indications of the instruments for each hour, from 9 A. M. to 9 P. M. of Aug. 29th. Between 9^h and 10^h A. M. the fluctuations of the horizontal intensity were too great to be observed by the fixed telescope.

Hour.	Declination. Max.	Declination. Min.	Horizontal Inten. Max.	Horizontal Inten. Min	Hour.	Declination. Max.	Declination. Min.	Horizontal Inten. Max.	Horizontal Inten. Min.
h. h.	d.	d.	d.	d.	h. h.	d.	d.	d.	d.
9 to 10	50·12	58·63	?	?	3 to 4	53·54	57·55	9·63	4·93
10 to 11	49·33	53·60	1.07	– 2·84	4h 30m	57·96	"	8·83	4·60
11 to 12	51·77	55·89	1.89	–0·85	"	57·02	"	5·54	
12 to 1	51·68	53·58	6·50	2·53	6h 30m	56·60	"	3·78	
1 to 2	52·68	53·32	6·47	5·00	8h	55·43	"	6.12	
2 to 3	53·13	53·85	6·04	5·40	9h	55·73	"	5.85	

About midnight Aug. 28th—29th, the employés in the telegraph office at Brussels noticed signals from their bells, such as often occur during a storm. The employés in the offices at Mons, Antwerp, Gand and Ostend were also awakened by their bells, and enquired what was wanted. Communication with Paris, London, and Berlin were interrupted till $1^h\ 30^m$. Paris and London inquired of our operators if they saw a light in the heavens. The effect ceased at $1^h\ 30^m$ on all the lines except the submarine line from Ostend to Dover, which was charged with electricity throughout the entire morning. It was not till $3^h\ 30^m$, and after nearly doubling the battery, that communication was re-established.

September 2, between 5^h and 6^h A. M., there was a second disturbance on all the telegraph lines, and communication between

Brussels, Paris, and London was interrupted. The following observations were made at the Observatory of Brussels:—

Date.	Declination Max.	Declination Min.	Hor. Inten. Max.	Hor. Inten. Min.	Date.	Declination. Max.	Declination. Min.	Hor. Inten. Max.	Hor. Inten. Min.
	d.		d.						
Sept 2, 9 to 10 A. M.	54·75	57·65	10·43	6.25	Sept. 2, 9h 0m P.M.	60·62		5.64	
10 to 11 "	53·62	59·40	9·82	3·36	10 0	53·47		8 04	
11 to 12 "	53·72	58·15	0·30	4·64	Sept. 3, 9 to 10 A. M.	57.17	58·23	4·89	4·09
12 to 1 P. M.	52·06	66·24	8·05	7·36	10 to 11 "	54·56	56·53	5·21	4·46
1 to 2 "	49·34	62·81	?	?	11 to 12 "	53·73	54 96	7·08	5·55
2 to 3 "	43·40	57·87	17·79	0·00	12 to 1 P. M.	50·59	53·63	10·64	7·40
3 to 4 "	48·32	58·40	15·53	10·12	1 to 2 "	51·03	53·15	11·28	8·88
4 to 5 "	51·15	55·00	14·01	10·48	2 to 3 "	51·63	51·90	10·42	8·75
5 0m	54·75		10·24		3 to 4 "	48·37	51·52	13·75	8·36
6 0	54·44		8·87		4 30m	51·52		12·50	
7 5	56·67		7·80		5 0	53·50		14·83	
8 24	56·59		7·21		9 0	57·58		7·31	

6. *Effects of the Aurora upon the Telegraph Lines of Wurtemburg; from Poggendorff's Annalen,* Band 108, p. 506.

During the night of Aug. 28th, from 11^h 15^m P. M. to near noon of the 29th, there was remarked from time to time on all the telegraph lines proceeding from Stuttgard, an extraordinary attraction of the armatures, which continued from 20 to 40 minutes, and generally appeared first on the line to Heilbronn, after about 5 minutes on the Ulm line, next on the line to Carlsruhe, and last on the Tubingen line. This attraction was repeated every 5 or 10 minutes, and, towards morning, every 2 or 3 minutes. After 5 o'clock only bell signals could be obtained from the local stations, as the armatures were held fast. During this period the deflections of the galvanometers were very remarkable. In a single minute the needles changed their position 5 or 6 times even to 40° west. While on the Ulm line the deviation was easterly, on the Bruchsal line the deviation was westerly.

The cause of this phenomenon is found in a brilliant aurora which was everywhere observed from 9 P. M. Aug. 28th till towards morning of the 29th.

7. *Effects of the Aurora upon the Telegraph Lines of Prussia; from Poggendorff's Annalen,* Band 108, p. 504.

The electrical currents on the conducting wires exhibited themselves in violent deflections of the galvanometers. The needles swung violently from 30° to 70° to one side, returned slowly to zero, and then moved slowly to the other side. On the line proceeding from Berlin westward, the disturbance commenced between 1 and 2 o'clock on the morning of Aug. 29th, when all connection with the stations ceased. Notice had previously been received of disturbance at the easterly stations, Konigsberg, Kowno, Riga and Petersburg. During the day of the 29th, on the western lines, communication was uninterrupted, while on the eastern lines it was occasionally suspended.

On the 2nd of September, when at 7 A. M. almost all the lines were in use, the disturbance occurred on all the lines, and interrupted communication from 5 to 40 minutes. The interruption was first experienced at Konigsberg 5 A. M.; at Stettin 5^h 55^m; Coblentz and Cologne 6^h 45^m; Berlin 6^h 50^m; Kowno and Riga at 7 A. M. About 9 A. M. the disturbance was greatest, and it declined till 9^h 45^m, when communication was resumed with most of the stations. At Stettin communication on all the lines was resumed at 9^h 24^m, and at Cologne at 10^h. At Konigsberg the disturbance still continued, and at Berlin it increased to 1 o'clock, so that all communication was suspended with the west. In the course of the day, news was received of disturbance at Hamburg, Breslau, Brussels, Paris, and Amsterdam. From the latter station came the intelligence that the submarine line to England was also interrupted by the aurora.

8. *Auroral Observations in Austria; communicated by* Prof. W. HAIDINGER, Vienna, to Prof. SILLIMAN.

The Aurora of Aug. 28th to Sept. 4th, was seen at the following places in the Austrian empire:

	Latitude.		Long.		
Bodenbach, Bohemia,	50°	47′	14°	10′	Aug. 29th, from 2 to 3 A. M.
Schossl, do.	50	27	13	30	From 11¾ Aug. 28th to 2½ A. M. Aug. 29th.
Prague, do.	50	5	14	25	Aug. 29th, morning. Also faint in the night from Sept. 1st to 2nd.
Rzeszow, Galicia,	50	3	22	4	Sept. 2nd and 3rd, evening.
Biala, do.	49	50	19	5	Sept. 3, 8 o'clock.
Iristen, Austria,					Aug. 28, from midnight onwards.
Lintz, do.	48	17	14	15	September 3, 8 P. M.
Vienna, do.	48	13	16	23	Great magnetic disturbances were noticed from noon Aug. 28th to the evening of Aug. 29th. Also from Sept. 2 early in the morning to Sept. 3, morning; and from Sept. 4, morning, to Sept. 5th in the evening.
Kremsmünster, do.	48	3	14	7	Aurora, Sept. 3rd, from 8¼ to 8½ P. M.
Schemnitz, Hungary.	48	27	18	50	From Aug. 28th, 10 P. M. to Aug 29th, 3 A. M. Also Sept. 3rd, 9 P. M.
Neutra, do.	48	17	18	50	Sept. 3, 9 P. M.
Mitterdorf, Styria,					Sept. 2, evening.
Laibach, Carniola	46	3	14	30	Sept. 3, after 8 P. M.

9. *Effects of the Aurora upon the Telegraph Lines of Switzerland; from the Comptes Rendus,* T. XLIX, p. 662.

The intensity and direction of the currents excited in the telegraph wires, during the aurora of September 2d, were determined by M. Hipp, at Berne, by the deviation of a magnetic needle, surrounded by a wire, making thirty coils. The regular current employed in telegraphing should have a sufficient force to deflect this needle 30°. M. Hipp found that the short lines gave no indication of a current, while the most marked effects were indicated by the longest lines, and especially by those which were directed from north to south, as the line from Zurich to Berne, Fribourg and Lausanne. The current on this line, directed from Zurich to Lausanne, would increase gradually, until the needle was deflected 42°. It would then slowly decline, and at the end of two or three minutes become zero. It would then change its direction, returning from Lausanne to Zurich, and attain a maximum of 30 degrees. The latter current, after continuing 60 or 90 seconds, became zero, and again changed its direction.

It appears from these observations that two currents succeeded each other on the telegraph wires, having a general direction from north to south, the one proceeding from north to south having a double intensity and a double duration, the other proceeding from south to north having a less intensity and a less duration.

M. Hipp obtained deviations of 58 degrees between Zurich and Berne, and of 64 degrees between Berne and Basle, indicating currents at least threefold the ordinary current employed in telegraphing.

10. *Effects of the Aurora of Aug. 28th and 29th upon the Telegraph Lines of Tuscany, by* M. CH. MATTEUCCI; *from the Annales de Chimie et de Phys.,* Tom. LVII, p. 419.

About 6 A. M., Aug. 29th, the disturbance became sensible on the telegraph lines. About 10 A. M. a current, which marked 25 degrees on the galvanometer, and equal to about 30 feeble elements of Daniell, traversed the upper wire of our telegraph lines, in the direction from Pisa to Florence. The current slowly increased, attained its maximum in about five minutes, and then rapidly declined. These periods were renewed a great number of times, and, during the intervals, telegraphic communication was held in the usual manner. About 3 P. M. the auroral effect upon our telegraph lines had ceased.

During the disturbance, on all the lines where there are several wires stretched one above the other in the same vertical plane, the strongest current was uniformly observed in the upper wire, while in the wire nearest the earth, the current was either feeble or inappreciable. This extraordinary current was the most intense on the longest wires.

11. *Observations at Rome, Italy,* (*lat.* 41° 54′), *by* M. SECCHI; *from the Comptes Rendus,* T. XLIX, p. 347 and 458.

On the 29th of August we had a superb aurora. The sky at Rome was covered with a red veil, and was crossed by the most brilliant rays, in the form of luminous columns. The magnetic instruments were very much disturbed. The declinometer deviated 34′ from its normal position, and the inclination varied 42′. The instruments for measuring the horizontal and vertical force both passed beyond the range of their scales, showing that the variation of the horizontal force must have been at least 0·0135, and of the vertical force at least 0·0075. The disturbance continued for a long time during the forenoon, and the vertical magnet, which, before noon, was beyond the scale, in consequence of the elevation of its north pole, at one o'clock passed beyond the scale on the opposite side, from a depression of its north pole.

A still more remarkable disturbance of the magnetic instruments occurred on the 1st and 2nd of September. At 4 P. M. Sept. 1st, the vertical magnetometer passed beyond its scale, showing a diminution of vertical force.

Sept. 2, at 7 A. M., the magnets were very much disturbed. At 7^h 10^m the declinometer pointed 2° 50′ to the west of its ordinary position. After this the needle returned rapidly to the east, and at 7^h 30^m pointed 1° 23′ east of its mean position, thus describing an arc of 4° 13′ in less than half an hour. This disturbance is the more remarkable, as the greatest range heretofore observed at Rome was only 45′ or 50′.

The bifilar indicated a diminution of the horizontal component, amounting to 0·129, or about *one-eighth* of its mean value.

These disturbances continued with variable intensity all day. At 4^h 15^m P. M. the vertical magnet again passed beyond the range of its scale. At 9 P. M. the magnet was more tranquil, and at midnight they had all returned nearly to their normal condition.

The variations of the declinometer, the bifilar and the vertical magnetometer were not simultaneous, but their maxima occurred at different times. The great vibrations were cotemporaneous with the currents observed on the telegraph lines. The clouds observed in the heavens had the exact appearance of those of the aurora borealis when it occurs by day, and such as were noticed at Rome Aug. 29th.

Similar observations were made at Leghorn, where at 6^h 30^m A. M. Sept. 2nd the declination was 15° 10′, while at 6^h 30^m P. M. it was only 14° 18′. The inclination of the magnetic needle was also very much increased during the day.

12. *Observations from Western Asia.*

A. Yozgat (lat. 39° 45′), by FAYETTE JEWETT, M.D., American Missionary.

The auroral phenomena referred to in your circular were *not* observed at Yozgat. On the 28th of August, and for several days before and after that date, I was in Arabkir, a town nearly 300 miles almost east of Yozgat. The aurora was not noticed there. While I was at Arabkir, owing in part to the mildness of the temperature, and also to the peculiar clearness of the atmosphere, my attention was almost every evening directed to the study of the constellations. The natives, too, at that season, slept upon the roofs of their houses.

B. Kharpoot, (lat. 38° 40′), by Rev. C. H. WHEELER, American Missionary.

Aug. 28th and the following nights nothing unusual was seen here by me or by others of whom I have made inquiries. It is also a fact, so far as I know, that the usual displays of the aurora are less brilliant here than in New England.

C. Mosul, (lat. 36° 22′), by H. B. HASKELL, M. D. Missionary Physician.

No unusual appearance was observed Aug. 28th, 1859, either here, at Mardin, or Diarbekir. During the residence of American missionaries in Mosul (ten years) no auroral phenomena have been noticed.

13. *Observations in the Southern Hemisphere.*

Ship Southern Cross, (near lat. 50° S., long. 80 W.), from the Alta California.

On the night of Sept. 2d, during a tremendous gale, the rare spectacle of an aurora australis was witnessed. It commenced about half-past one o'clock in the morning, and increased in splendor until towards daylight, when it gradually faded before the light of day. The whole heavens were of a deep red, which color was reflected from the ocean. During the night a tremendous squall with hail burst upon the ship. Through the whole of this the flames assumed the same roseate hue; and when a spray flew over the ship, it fell to the leeward in ruddy showers. Between the squalls, in the clear places in the sky, the mysterious lights were seen shooting up in spiral streaks nearly to the zenith now flashing out with meteoric brilliancy, and now looming up against the horizon, as with the blaze of some terrible conflagration. During the gale, several times at night, brilliant balls of fire appeared flickering at the mast-heads, yard-arms, and other salient points. The captain and his officers say that they have never witnessed anything equaling this display for magnificence.

14. *Observations at Concepcion, Chili, (lat.* 36° 46′), *from the Mercurio of Valparaiso.*

An aurora was visible here on the nights of Sept. 1st and 2nd. It appeared at midnight in the south part of the horizon, and was visible until two o'clock in the morning. It had a movement of translation from east to west. In appearance it resembled a cloud of fire, or a large ignis fatuus, which threw out some flame or vapor, and spread a light like that of the moon. For more than an hour the city was brilliantly illuminated by this heavenly light.

15. *Observations at Santiago de Chili*, (*lat.* 33° 28′), *from the Mercurio of Valparaiso.*

On the morning of Sept. 2nd, about two o'clock, the sky to the south of Santiago was brilliantly illuminated by a light, composed of blue, red, and yellow colors, which remained visible for about three hours. This phenomenon is very rare in Chili. The aurora was also seen in Valparaiso (lat. 33° 6′).

15. *Observations at Kapunda, South Australia*, (*lat. about* 35°), *by* J. B. AUSTIN; *from the London Times of Nov.* 14, 1859.

On Monday evening, Aug. 29th, just after dark, the aurora appeared like a large and brilliant pink cloud, extending about 25° or 30° above the horizon, and 60° or 70° in length. It continued visible for about twenty minutes, during the last five of which, splendid streamers of pink and white light were shooting vertically through it. It was seen almost throughout these colonies at the same time, and on four nights in the same week; but I saw it only twice, once Aug. 29th, and again on Friday, Sept. 2nd, when the most gorgeously brilliant display took place. It commenced immediately after sunset, and increased in splendor during the evening. For several hours, little was to be seen but a deep rich pink light over the southern part of the sky; but by degrees it extended, and, about nine o'clock, a huge pillar of fire appeared in the west, where it remained until midnight. After the moon went down, the brilliancy of the aurora increased, and from about half-past eleven till past twelve, a beautiful pale, soft, greenish-blue light, like the dawn of morning, extended itself above the southern horizon for about 100° or 110°, and about 18° or 20° in height. From this, streamers or radii of red, white and blue light shot upward to beyond the zenith, fully half the sky being covered with this splendid illumination, the light from which equalled that of the full moon in England. These radii converged towards a point about 15° north of the zenith, but did not themselves extend more than half that distance beyond the zenith. This was its last appearance, and a splendid finale it was. The powerful electric excitement in the atmosphere had an extraordinary effect on the telegraph wires, agitating the instruments violently in some places, and quite interfering with the transmission of messages.

Art. XXXVI.—*Geographical Notices;* by Daniel C. Gilman, Yale College Library. No XII.

Reprint of a Tract, by Nicolaus Sillacius, (A. D. 1494), on the Second Voyage of Columbus.—Although the principal object of these "Geographical Notices" is to record the recent progress of our knowledge of the world, yet we cannot forbear to make mention of a remarkable publication which pertains to the discovery of the New World at the close of the fifteenth century.

Christopher Columbus, in his second voyage across the Atlantic, set sail from Cadiz, September 25, 1493. Soon after his return, Guglielmo Coma wrote from Spain to Nicolaus Sillacius in Pavia an account of the journey. These letters were translated by Sillacius into Latin, and such other information was added to them as could be gathered from current reports; and this whole account of the voyage of Columbus, was published under the title, "*De Insulis Meridiani atque Indici Maris nuper inventis.*" This curious tract has been almost forgotten for nearly four hundred years; and, at the present time, but two copies of the original edition are known to be in existence,—one belonging to the Marquis Trivulzio of Milan, and the other to James Lenox, Esq., of New York. The last named gentleman, with characteristic liberality, has made this pamphlet accessible to all scholars, by causing it to be carefully re-printed in the original Latin, with an English translation by Rev. James Mulligan, a biographical introduction, notes, and a bibliographical appendix, in which much important information is given in respect to the early printed account of the various voyages of Columbus. The whole work forms a quarto volume of about 180 pages, printed in a truly elegant style.

It cannot be expected that this tract will add very much to what is known from other sources of the great navigator and his voyages, but as a contemporaneous record of most important discoveries, the volume will always be prized not less by the geographer than by the historian and bibliographer.

Voyage around the World of the Austrian Frigate Novara.—The reference which has been made in a previous page of this number of the Journal to the Austrian circumnavigatory voyage, furnishes us with an appropriate occasion for speaking of that exploring expedition.

The Imperial frigate "Novara," under the command of Commodore von Wüllerstorf, set sail from Trieste April 30, 1857, and returned to the same port August 26, 1859, having successfully completed a voyage of scientific observation around the world, the first which was ever undertaken by the Austrian Navy. It is of course too early for the results of this expedition to be fully made public, but various accounts of the whole voyage, and of particular observations, have been given in the journal of the Geographical Society of Vienna, and in Peterman's *Mittheilungen. L'Institut* of Paris has also published a series of articles on the subject, communicated by M. Marschall of Vienna.

The "Novara" is a frigate of 1800 tons burthen, and 44 guns. It was manned by 354 men. The scientific corps, in addition to the commodore and other naval officers, consisted of the following naturalists, viz., Dr. Hochstetter, physicist and geologist; Frauenfeld and Zelebor, zoologists; Dr. Scherzer, ethnologist, having charge also of investigations in national economy, the botanist Jellinek and the artist Selleny.

Sailing, as we have stated, from Trieste, the expedition touched for longer or shorter periods at Gibraltar, (eleven days), Funchal, (nine days), Rio Janeiro, (three weeks), Table Bay, Cape of Good Hope, (twenty-four days), Island of St. Paul, (seventeen days), Point de Galle, (eight days), and Madras, (eleven days). Sailing from the last named port Feb. 10, 1858, from that time until August 11, a period of six months, the vessel was directed to various island and continental seaports of south-eastern Asia, including Nikobar, Singapore, Batavia, Hong Kong, Shanghai, etc. In September the island Puynipet was visited, and afterwards Sydney, (a month), Auckland, (seventeen days), Papeiti, on Tahiti, (eleven days), Valparaiso, (twenty-four days). Leaving the latter port May 11, 1859, the Novara reached Trieste at the end of the following August. The whole extent of the voyage was nearly forty thousand nautical miles.

Various letters and partial reports, submitted to the Academy of Sciences in Vienna, have already been printed, and a complete narrative of the voyage, and full reports of all the scientific observations which were made upon it, is soon to be prepared and printed.

Dr. Hayes's Proposed Arctic Journey.—A meeting of the American Geographical and Statistical Society was held in New York, March 22, for the purpose of encouraging Dr. Hayes in respect to his proposed voyage to the Northern ocean. Dr. Hayes was present, and, in addition to his statements, an eloquent exhibition of the importance of this expedition, together with an appeal for material aid, was made by Dr. Francis Lieber.

Hon. Geo. Folsom, E. H. Vielè, Esq. Profs. Mitchell and Silliman, and Dr. A. H. Stevens also took part in the meeting, and letters were read in approval of the undertaking from Profs. Bache, Henry, Guyot, Dr. Gould, etc.

The general purpose of Dr. Hayes has already been set forth in this journal, in a paper from his own pen, "On the Practicability of Reaching the North Pole," (vol. xxvi, p. 305–23, Nov., 1858.) At the recent meeting it was stated that ten thousand dollars had already been subscribed in aid of his enterprise, and at least ten thousand more are needed to insure the sending forth of the expedition.

The various weighty problems which are proposed for solution, especially the determination whether or not there be an open Polar Sea, present the strongest claims to the liberal contributions of all who are interested in the promotion of geographical discovery, or in the progress of physical science.

Journal of the American Geographical and Statistical Society.—We are informed that this Journal, which has heretofore been published monthly, will hereafter be issued quarterly each number comprising at least 128 royal octavo pages. The first number, announced for the month of April, will comprise nearly 150 pages, consisting in part of original articles by the following writers: Commander Matthew F. Maury, Prof. Alexander D. Bache, Prof. Arnold Guyot, E. George Squier, Paul B. Du Chaillu, Dr. David Livingstone, Joseph C. G. Kennedy, James Wyne, M.D., together with late geographical and statistical intelligence, and careful notices of new scientific works bearing upon the objects embraced in the Society's labors. The subscription price to those not members of the Society will be three dollars a year.

Letters in reference to any matters connected with the Journal should be addressed to Daniel Willard Fiske, General Secretary of the Society, New York.

Explorations in the Amoor Region.—We have already called attention to the great efforts which are making by the Russian government to ascertain the resources and characteristics of Eastern Siberia, and to bring the immense region drained by the Amoor and its tributaries into connection with the commerce of the world. To our own countrymen, these investigations are especially important, when we consider the probable effect which will be produced upon the commerce of the Pacific.

At a recent meeting of the Royal Geographical Society of London, a paper was read, presenting extracts (prepared and translated under the direction of Capt. R. Collinson) from various official Russian reports, respecting the districts adjacent to the Amoor river, These extracts from the writings of Messrs. Pescurof, Vasilief, Radde, Usoltzof, Paragchefski, etc., are printed in the Transactions of the Royal Geographical Society, vol. 28,—together with an original map compiled by J. Arrowsmith. As our space at the present time allows us to quote but one of these reports, we have selected that by M. G. Radde, upon the table lands east and southeast of the great Lake Baikal—or, as he terms it, the Dáuro-Mongolian Frontier of the Trans-Baikal region.

"If by the word 'Steppe' be understood an extensive, treeless and arid plain, without any considerable undulations, that term cannot, in its full sense at least, be applied to the tracts now under consideration. Scientifically, and with regard to the formation of its surface, this region should be described as an elevated extent of country, intersected by many bare mountain ranges; the valleys and low plains between which are in some places strongly impregnated with salt, and produce exclusively *chenopodeæ* whilst in others they receive the waters of many small springs and atmospheric moisture in the shape of snow and rain, giving rise to innumerable small, turbid, and muddy lakes, seldom containing water fit for use, but more often contaminated with saline and alkaline solutions. An ordinary observer, one who has not penetratrated into the external structure of the earth's surface, or, what is of greater importance, into the properties of the soil from which he derives his sustenance, would see here only a contrast of conditions, namely, the contrast of the wooded surface to the treeless and bare, inducing him to call such a country a steppe. Whether the latter surface be level, or high and undulated, it would equally by him be termed a steppe; and only perhaps in distinguishing two contiguous regions would the mountainous and desert zone be designated as the 'high' steppe.

The Dáur country on the Mongolian frontier cannot, both with relation to its absolute height and its topographical features, be even approximately compared to a regular steppe; nor can any parallel be drawn between the chemical properties of their vegetable strata. Whilst in many regions, as for instance in the extensive Orenburg, Taurida, and Bessarabian steppes, the chernozem, so favorable to cultivation, penetrates the surface to 2 and 3 feet, there is a total absence of organic matters in the woodless valleys of the Dáurian frontier table-lands; and the soil of that extensive region has not undergone any considerable change for many centuries, owing to all the elevations, and frequently the valleys, abounding in siliceous ("jasper and flint") formations, which either do not admit of precipitation at all, or with great difficulty; added to which, the decomposition of hard rocky masses is materially retarded by the dryness of the atmosphere, and the want of snow and rain.

A further exposition will show that, leaving aside the peculiar stamp which characterises the organic nature of this region, the material and moral condition of its scanty population have succumbed to the influence of the physical conditions above-mentioned. The greater part of this desert track, perfectly unsuitable for the production of grain, is apparently, like the inhabited regions of the Gobi desert on the south, destined by nature for the nomadic life of the wild and superstitious Mongol, who, spurning the ties of a fixed abode, scours the level plain on his fleet steed.

With respect to geographical position, the Dáurian frontier-steppes occupy a narrow zone between longitude 112° 30′ and 119° E.; their chief extension is from west to east, and they are only in a few places intersected by the parallel of 50° N. latitude. If the treeless elevations are alone to be denominated steppes, the boundary of the Russo-Dáurian steppes must be drawn southwards from Nijni-Ulhun frontier station, as the mountains on the banks of the Onon, extending farther west, are covered all over with dense forests; on the east, on the other hand, from the above station, and between Akshinsk and Mogoitu, along the right bank of the Onon, extends a forest of tall trees, the predominating family of which, the pines suddenly disappears a few versts east of Mogoitu, and is succeeded by a straggling wood of birch, as far as Kubuhai.

The steppe district thus only crosses the Onon at Niji-Ulhun, occupying also a small zone, well irrigated with numerous small streams, on its left bank.

In its easterly extension, parallel to the course of the Onon, the steppe is not bounded on the north by this river, but by a very thick forest extending between the Onon and the desert, in some places 10 miles in breadth. This forest is worthy of notice for its historic associations as the sojourn of Chingis-Khan, and also in a botanico-geographical respect, forming, as it does, a natural boundary between the river and the steppe, which is remarkable for its small breadth and its clearly defined limit on the south. The forest thins gradually towards the east, down the Onon, and terminates entirely at the place where the river bends abruptly to the north on meeting the western spurs of the Adoncholon mountains; farther in that direction, with a lesser fall, and often contracted between banks of granite, the river pursues its course as far as its confluence with the Shilka, through a wooded country more frequently overgrown only with bushes.

The frontier steppe, which has already a breadth of about 53 miles between the old Chindan fortifications and the Uldza river, extends towards the south along the confines of this pine forest, acquiring a greater width farther on. The Onon-Borza* rivulet, flowing from the northeast, and which likewise approaches the southern offshoots of the Adoncholon mountains at 116°, and after bending to the north unites after a course of twenty miles with the Onon at Ust-Borzinsk, belongs at its western middle course to the steppe region. In like manner, the more sloping southern

* This stream is called Onon-Borza in distinction to the three Borza rivulets which fall into the Argun.

declivities of the Adoncholon mountains, of which the summits alone are overgrown with stunted birch-trees (these are often, however, found in great density along the entire northern slope of that chain), are referable to the same region.* To the eastward, however, almost on the meridian of Tsagan-olu (116° 43′), two rows of woody elevations extend from east to north, intersecting each other at the most westerly lower range of the Buko-Hada, where the eastern branch terminates. The bare elevations running from this knot to the south expand the farther they extend, and form, near the frontier, the wooded table-land of Altangan, so called after one of its principal valleys.

The abovementioned mountains, which terminate in Buko-Hada, form first on their eastern, then on their northeastern extension, a water-shed between the affluents of the Onon and Argun; the Gazimur river takes its rise on its northern side. The Altangan table-land lies between two systems of saline waters; the lake of Tarei-nor is the largest representative of the western basins, whilst to the eastern belongs Ubudk, Tsagan-nor, Hara-nor, and many others. The culminating points of this region occur in the Steppe district, which here increases in width, being more than 67 miles in breadth between Tsagan-olu and Abagaitu. Having by barometrical measurement taken the height of Tsagan-olu at 2711 feet English, 500 more must be added for the mountain pass of Soktui. Only one valley, the largest and broadest of those occurring in the frontier steppes, on the Russian side, intersects the Altangan plateau from east to west, continuing on the other side of the mountains from their western slopes.† This is the valley of the Urulungui rivulet, which flows for 100–113 miles in a direction towards the Argun, and terminates there at Novo-Tsuruhaitui military station. At its lower course, the Urulungui flows gently along a winding channel, bordered at first occasionally by bushes of the willow, the precursors of a more luxuriant vegetation than that of the steppe. The region, however, between the Urulungui Argun, and Altangan plateau loses its vegetation more and more towards the south; on the frontier at Abagaitu it is intersected by parallel

* In Dáuria trees and bushes are only found on the northern slopes of mountains from two causes. The first is, that the southern slopes are much drier than the northern, which longer preserve the moisture of the soil, and so assist vegetation; whilst almost every plant withers in summer on the southern side of the mountain. The second cause is attributable to the circumstance that the fires which occur in the steppes in spring become sooner extinguished on the north side than on the opposite, where the snow leaves the ground earlier (being, in fact, perfectly dry by the end of February), thus offering no obstacle to the spreading of the fire. The limits of wood and bush vegetation are not governed in these regions by the rigor of the winter, but solely by the dryness of the soil and sultriness of the summer months.

† The Urulungui valley probably commences in the vicinity of the Chinese frontier, to the west of the Altangan plateau.

40° 35′ and is so unproductive and barren, that on that account alone, and without reference to its topographical features, it may be considered the extreme northeastern end of the Gobi desert, which extends to the lakes of Buir-nor and Dalai.

Broad, light-green, and low tracts, overgrown with reeds, and winding only along the very edge of the Argun, intersect the bare and rocky desert, the uniformity and character of whose vegetation is at last broken by the Urulungui rivulet, at Novo-Tsuruhaitui. Lower down from the mouth of that stream the valley of the Argun assumes another aspect; and the river itself, taking a bend to the northeast, visibly contracts and flows more rapidly. Here the chernozen soil of the valley with its diversified flora also makes its appearance, so that the Urulungui may not only be considered as the limit of the high Dáurian steppes, but also the sharply-defined natural boundary of their vegetation.

To the north of the Urulungui commences the district of the metalliferous deposits of the Nertchinsk mountain region, remarkable also for its vegetation, which, lower down in the valley of the Argun, is very rich in forms, particularly at Chalbuchi village. It is here that the Mongolian oak, the Corylus heterophylla and Betula dahurica, seen nowhere in Siberia, first occur. Lastly, possessing a sufficiently thick population, some portions of this region are highly favorable to the production of cereals; but it is less adapted to the depasturage of cattle than the steppes, on account of the many mountains by which it is intersected.

To describe in a few words the boundaries of the high Dáurian steppes, it suffices to say that their limit on the north is formed by a pine forest, extending along the right bank of the Onon, by the Onon-Borza rivulet and the Adoncholon mountains, together with the elevations at the upper courses of the Gazimur and Urulungui rivulets; on the southeast by the Argun; and on the south by the Chinese frontier laid down in 1727; the western extremity of the steppe being bounded by the forests on the right bank of the Onon,

The whole of this country, occupying an area of 380 square miles, attains an absolute height of 2200 (English) feet at its greatest depressions (namely, at Kulussutaefski military station at Bayrn-Tarei lake), and almost 3000 feet at its highest elevations. Numerous mountain chains, rarely however detaching isolated spurs, intersect it in various directions, forming broad valleys, with a saline soil, and which are often found to contain accumulations of precipitated Glauber salt and soda, but seldom any water-basins. Even where the latter occur, they never attain any considerable depth, and are mostly so shallow and level that after a snowless winter or hot summer they completely dry up and frequently remain in that state for many years. The most striking example of this is afforded by the great Baryn-Tarei lake, lying south of the Kulussutaefski frontier station, which was found dry by Pallas in 1772: since then it filled with water, which again entirely evaporated five years ago, so that it now only presents a dry saliferous and muddy bottom, cracked in numerous wide fissures by the burning rays of the sun. With the exception of a few rills, generally filled only by snow water in spring, and remaining perfectly dry during the greater part of the year, a small number of spring morasses are alone to be found there. Not unfrequently such morasses occur in the vicinity of saline lakes; but often, having no efflux, they drain themselves, when, owing to the pressure of water beneath, the surface around their swampy edges rises several fathoms in winter with its icy covering. The ice remains in such places until the middle of summer; and even so late as the month of June have I seen on a freshwater morass near Kulussutaefski, in the neighborhood of Tarei lake, blocks of ice one inch thick, capped, as it were, with a layer of earth of the same thickness, overgrown with reeds.

With such a scarcity of water and so great an elevation, it is conceivable that the atmosphere of this region must be very dry. To the south of this frontier zone, at the same time, extends an immense desert, and on the north, the rain-clouds, being attracted by a dense forest, and arrested by elevated ranges, discharge their waters to superfluity over the wooded district of Nertchinsk; whilst some 7 to 14 miles to the south not a drop of rain or dew will fall for months together. At the village of Tsagan-olu, I witnessed, at the beginning and latter part of the month of June, examples of such an unequal distribution of moisture; whilst the heaviest rains and storms, continually interfering with my excursions, prevailed at midday in the forests only 5 miles to the north, buckwheat was being scorched 3 miles to the south of the village, and no rain had fallen since the middle of May at the frontier stations of Soktuisk, (40 miles farther to the south), and Kluichefski and Chindan (33 miles more westerly). It is to be regretted that scarcely any observations on the moisture of the atmosphere of this elevated region have hitherto been made, as, together with a better knowledge of the chemical properties of the soil, they might have led to some definite conclusion on the greater or lesser fitness of the country for agriculture. At the same time we find that almost useless experiments on the growing of corn have for many years been repeated with great perseverance at the military settlements on the frontier. In none of the extensive and remote regions of Russia, in the same latitude, are there, probably, presented so many local conditions unfavorable to agriculture as in the frontier steppes of Dáuria; and it is very doubtful whether, even with increased labor, and the introduction of a better system of tillage, any regular or even moderate harvests can be obtained. Not only is there on one side the want of rain and snow, and the great elevation to influence the early autumnal frosts, but on the other the very properties of the soil offer still greater obstacles to cultivation; to be surmounted perhaps only by a Chinese density of population, and Chinese industry.

The very soil of these regions is of a twofold nature: a great part of the steppes, and all the mountain-chains in particular, are as if sown with flint, jasper, and chalcedony, deeply buried in a hard argillaceous sand, and forming also the upper vegetable strata, which present no traces of fertility; whilst all the depressions of the surface are impregnated with salt, and therefore produce only a few saline plants. The climate is at the same time unfavorable to the growth of any plant. Severe snowless winters prevent the cultivation of winter wheat, while the early autumnal frosts are generally prejudicial to crops, and impede the fallow tillage. Spring wheat and buckwheat are consequently alone sown; and even these crops perish in great part from the droughts in May and June, no shade being afforded to their roots by their thin foliage and feeble growth of stem, which rises only one foot from the ground. As a rare exception, a snowy winter will sometimes follow a series of dry years; but this, although acting beneficially on the fields, is of great injury, by its long continuance, to the cattle, which are not unfrequently entirely destroyed by the want of fodder. Under ordinary climatic conditions, the want of snow is the chief impediment to their safely passing the winter; so that, on the freezing of the few fresh springs, the animals suffer much more from thirst than from hunger, and from the first half to the end of December are often so reduced that, even with a sufficiency of food, they are unable to survive the second half of that month.

Appreciating the advantages which Eastern Siberia derives from the opening the Amúr to commerce, Mr. Radde proceeds to consider the present agricultural wealth of the Dáurian Steppes, and its future influence and development. The first part of the paper has already shown the unproductiveness of the country, and the great obstacles presented by its climate. Cattle-breeding and sheep-farming in particular, would alone appear to admit of some development, as the lowlands and steppes afford good pasturage; and the prejudicial climatic conditions might, with perseverance, foresight, and industry, be rendered less unfavorable.

Wool is the only article which Mr. Radde adduces as an export, and he considers there will be no difficulty in finding a market for it in the United States. The frontier region of Dáuria and Mongolia is capable, the author thinks, of producing two millions of sheep; whilst cattle-rearing must for some years remain in its present state, owing to the scantiness of population, and the difficulty of making provision for the winter."

KHANIKOFF'S TRAVELS IN PERSIA.—Through the attentions of D. W. Fiske, Esq., General Secretary of the American Geographical Society, we have received the *Proces-Verbal* of the meetings of the Imperial Geographical Society of St. Petersburg, held Dec. 16, 1859, and Jan. 13, 1860.

At the latter sitting, M. Khanikoff presented an account of his researches in Persia, to which, in a former number of this Journal, a brief allusion was made. His remarks were chiefly directed to the Province of Khorassan, as will be seen from the following abstract which we translate from the *Proces-Verbal.*

The limits of this vast province, bounded on the north by a plateau which stretches in the direction of latitude from Hindou-Kousch to the southern extremity of the Caspian Sea, and toward the west by another plateau, making an angle of from 20 to 30 degrees with the meridian, are far less clearly defined toward the east. This traveller is of the opinion that Khorassan may be justly considered as bounded in this direction by the western slopes of Hindou-Kousch, which stretch from Hérat to Kandabar, as well as by the mountains which separate Séistan from Béloudchistan. The space thus enclosed presents four natural sub-divisions, to which M. Khanikoff gives the name terraces. The first embraces the salt desert lying between Kaschan, Koum, Bastam, Nichibour, and Tebbès. Its general inclination is directed from northeast to southwest, and its lowest point is on the line joining Bastam and Tebbès. The second comprehends the dry desert of Lut, and toward the north borders on the preceding; the mountains of Kirman are its southern limit; its general inclination is from north-northwest to south-southeast, and its lowest point is probably no more than 500 feet above the level of the sea. The lowest point of the third, which includes Séistan, is at the surface of Lake Hamoun, of which the waters are 1,545 feet above the level of the sea. Finally, the fourth terrace, which is the least extended, is bounded by the line which, on one side, joins Birdjand and Sebzar, and on the other stretches from the first of these villages to Toun, Haff, and Yezdoum; its general slope inclines from southwest to northeast. The limits of these four divisions of Khorassan are not everywhere well defined, but they are clearly indicated by the directions of the water courses and the inclinations of the ravines. The northern frontier of Khorassan coincides with the isothermal line of 12° Cent., a fact which gives plausibility to the conclusion, that over all the expanse of the northern plateau of central Asia, from Orenbourg to Meshed, over a space of 20 degrees in breadth, the annual temperature seldom falls to 6° Centigrade; at the southern limit of the first terraces described above, date trees grow and produce fruit in abundance, from which we must conclude that the annual temperature here is not below 18° Cent.; hence, in this direction and in moving toward the equator two geographic degrees only, the mean annual temperature acquires an increase equal to that gained to the north of Meshed by a progress of 20 degrees along the meridian. M. Khanikoff calls the attention of the Geographical Society to this point, that the rapid elevation of the degree of the annual temperature cannot be explained by the astronomical and hypsometrical coördinates alone, of the regions where this increase has been observed. He thinks that one of the essential causes to which it should be referred is the dryness of the air, which rapidly increases from the southern shore of the Caspian Sea to the frontier of Béloutchistan, so that in the desert of Lut the atmosphere contains only $\frac{13}{100}$ths of relative humidity.

After entering into detail relative to the probable limits of the highest temperature, determined from the softening observed in the stearine which was in the baggage of the members of the expedition, M. Khanikoff has described some of the most striking atmospheric phenomena which he has himself studied in Khorassan. He mentions, among others, waterspouts and whirlwinds of dust, the dry mist, the atmospheric fluctuations, the mirage, and finally, observations upon the zodiacal light, which was seen by the expedition while traversing the space between Anarderré and Kirman. In conclusion, M. Khanikoff presented to the assembly the whole trigonometrical network, by aid of which the sketches which he has traced were drawn, adding the details of the circumstances which accompanied the operations. For want of time he reserved to another meeting the enumeration of the ethnographical labors of the expedition.

Art. XXXVII.—*Correspondence of Prof. Jerome Nicklès, of Nancy, France, dated Feb. 26th*, 1860.

French Academy of Sciences, Public meeting and distribution of the prizes.—This meeting was held Jan. 30; it was concluded with the eulogy upon Thénard, pronounced by Flourens, one of the Perpetual Secretaries. The following is a summary of the principal prizes awarded.

Prize for Astronomy.—This prize was awarded to Robert Luther, for the discovery of *Mnemosyne*, the only new planet of the year 1859. Mnemosyne is the 57th of the group of telescopic planets between Mars and Jupiter, and the 8th of those which are due to Mr. Luther. The

Academy has already four times awarded the prize to this astronomer for the discovery of the five planets, Thetis, Proserpine, Bellona, Leucothea, and Fides.

The prize for Mechanics was awarded to Mr. Giffard for the invention of a new feeding apparatus for steam boilers, which he calls an "*automatic injector.*" The report, made by Combes, gives great praise to this apparatus. The injector very advantageously replaces the feeding pumps of steam boilers. In addition to the fact that it avoids all loss of heat, other than that which results from the cooling of the exterior of the tubes in which the steam and hot water circulate, the absence of any movable solid parts, exposed to wear and derangement, the extreme facility with which the quantity of water supplied can be regulated between limits sufficiently narrow, &c., render it very valuable for locomotive machines. Accordingly several great railroad companies have already applied it to machines of this kind.

The automatic injector takes its origin from an observation made by Savart in 1832, in his experiments upon the fall of liquid veins; a current let fall from a vessel where the level is maintained at a given height, penetrates directly and quite unbroken into a vessel where the surface is less elevated.

The observations of Savart; the phenomena, long known, of the communication of lateral motion to fluids by which is explained the action of those blowing machines called *trompes;* the forcible drawing in of air through the intervals which separate the bases of the tuyeres of high furnaces; the effects of the blast pipe of locomotive engines, &c., have given Mr. Giffard a hint which has led him to the invention for which the Academy have just awarded a prize.

Mr. Giffard, who is a person of very earnest spirit, is known in France by his attempts at guiding balloons. Some years since he obtained evident results in directing them, since he succeeded in making his balloon move *against* the *wind*, at the Hippodrome in Paris. This fact was stated in a report signed by intelligent men. Although without means to continue his researches upon this point, Giffard was not discouraged. Instead of making a show of his misery, and representing himself as persecuted by science, or as a martyr to an idea, he left balloons for the time and set about the construction of locomotive machines; he thought of his injector, and put off his researches upon ballooning until by his labors he should again be furnished with means to continue them; complete success crowned his investigations; he has now placed himself in a condition to take up again his favorite pursuits.

Physical sciences.—A prize has just been divided between Daubrée, Dean of the Faculty of Science at Strasbourg, and Delesse, mining engineer at Paris. The question proposed for competition was in respect to the *Metamorphism of Rocks.* The report of the committee will not convey to the readers any more knowledge than they have already obtained from this Journal which has often made mention of the labors of Messrs. Delesse and Daubrée.

The *Prize for experimental Physiology* has been awarded to Pasteur for his researches in regard to *fermentation.* They bear upon *alcoholic fermentation*, *lactic fermentation*, and *tartaric fermentation*, and of their isomeric compounds. The report of the committee has particular reference to the physiological side of the question; it was edited by Claude Bernard, an emimently distinguished physiologist. The following extract is from the report:

Following the example of Cagniard Latour, Pasteur considers the yeast of beer an organized body; he regards the modifications which it undergoes during alcoholic fermentation as of a nature essentially vital, and he shows that the chemical phenomena of fermentation are connected with a perpetual renewal of the yeast; whence it follows that, during the alcoholic fermentation, the sugar not only gives origin to chemical substances which disengage themselves or remain dissolved in the liquid, but at the same time, there is still a portion of the sugar which is taken up by the yeast in the form of cellulose, and another portion in the form of fatty matter, while the nitrogen of the old yeast serves to regenerate the new. Pasteur has made in this respect an experiment which, so to speak, reduces physiological conditions to the most simple relations which can connect living beings with mineral nature. He has shown, in fact, that the globules of yeast develop and multiply, and that the sugar ferments, when a quantity of the globules, so to speak, imponderable is sown in a medium composed at the same time of: 1st. A solution of pure candied sugar. 2d. An ammoniacal salt, the dextro-tartrate, for example. 3d. Mineral substances containing phosphates. The ammonia is seen to disappear and to be transformed into the complex albuminous matter of the yeast, while at the same time, the phosphates give up their mineral constituents to new globules. The carbon which is one of the constituent elements of the yeast, is evidently furnished by the sugar. Before Pasteur, the lactic yeast was generally considered as organic matter in process of alteration, but not as organized matter. Our author has discovered and pointed out the special character of a lactic yeast, which is much more minute than the yeast of beer. During the lactic fermentation this yeast buds and multiplies, behaving in the matter of reproduction very much like the yeast of beer. In regard to the fermentation of tartaric acid and its congeners, Pasteur has arrived at very unexpected results, having a high chemical and physiological interest. Putting into conditions of fermentation with albuminous matter, and at a suitable degree of heat, the racemate of ammonia, which is formed by the union of the right- and left-handed tartrates of ammonia, and which has no effect on polarized light, it was seen that the phenomena of fermentation finally manifested themselves, and that new chemical products were formed at the expense of the racemate of ammonia. But it is remarkable that only the elements of the *right*-handed tartrate separate or ferment, to give rise to the products of fermentation, while under the same conditions the *left*-handed tartrate remains unaltered in solution in the liquid, which then acts very energetically upon polarized light. In this fermentation there is produced a yeast peculiar to the right-handed tartaric acid, which developes itself in presenting the characteristics of a mycodermic vegetable.

This example proves, in the plainest manner, the influence of the molecular dissimilarity of organic bodies, in the phenomena of fermentation. It is, indeed, impossible otherwise to interpret the marked difference which, in this respect, the right and left tartaric acids exhibit, since both have exactly the same physical properties, the same chemical composition, and they differ only in the interior arrangement, which gives to their constituent parts a rotatory power equal, but in opposite directions, and which corresponds to the dissimilarity which is reproduced in their aptitude or inaptitude, to be influenced by ferments.

In short, Pasteur regards the chemical phenomena of fermentation as being always correlative to the vital phenomena of organization, and to the development which takes place, at the same time, in the organized yeasts which have the power to excite it.

The committee judged that the author, in thus pursuing the physiologic study of the yeasts, in the direction which he had chosen, would bring new light to bear upon a series of organic products, which are related to the phenomena of nutrition and histogeny.

Transplantation of the Periosteum.—Honorable mention was also made of Mr. Ollier, in reference to his interesting experiments on the transplantation of the periosteum, preserving its property of renewing the osseous tissue. The author showed, that if a strip of the periosteum is detached from the bone of a living animal, and is transplanted to the subcutaneous cellular tissue upon the same animal, or upon another individual of the same species, the fragment of periosteum becomes encrusted, and continues to live in such a manner that vessels are formed in its substance, and communicate with those in its vicinity, as can be proved by careful injection after death: Ollier has likewise proved that, several hours after death, the possibility of this transplantation of the periosteum still remains.

Prize relative to the unhealthy Arts.—This prize was awarded to the inventor of a lamp suitable for giving light to laborers at work beneath the surface of water. It is a lantern, consisting of a thick cylindrical covering of glass fixed between two iron plates. A reservoir, containing a mixture of alcohol and turpentine ("burning fluid") is placed in the interior. Where the apparatus is plunged into water, the air necessary to support combustion comes to the bottom of the lantern through two iron tubes opening up into the atmosphere. The products of combustion are likewise removed by means of a tube fitted to the centre of the upper plate, which is also prolonged so as to open into the atmosphere, and of which the section is double that of both the tubes, through which the external air is supplied.

The inventor of this apparatus was a simple workman, named Guigardot. With this lamp it is possible to work under water to the depth of twenty metres. It has been used with success at the works of the monumental bridge built over the Rhine, at Strasbourg. It illuminates a circle of $2^{m.}$ 50 radius, even in turbed water.

Medical Prize.—In the preceding years we gave much praise to the committee in charge of the medical prize. They sometimes expended even 90,000 francs in prizes of all kinds, awarded even to chemists whenever their labors were important to one of the branches of the healing art. This year the committee have been economical, one knows not why, inasmuch as important works are not wanting. They have awarded only honorable mention to physicians for the labors of their profession.

Prize for Organic Chemistry.—The opposite state of things appears in the department of chemistry, which for the first time has awarded a prize to living chemists. A sum of 6000 francs was divided between Messrs. Wurtz and Cahours; to Prof. Wurtz for his researches upon glycol and its derivatives, and upon the new bases containing oxygen recently discovered; to Prof. Cahours for his labors in reference to the organic radicals. Be it understood, there is no question as to who is the discoverer of the composite radicals (Liebig), nor of the organo-metallic radicals (Lœwig).

Bréant Prize.—This prize is in reference to cholera and contagious diseases. It has not been awarded. The pieces sent for this *concours* were mostly mere letters, containing medical formulæ, all, according to their authors, infallible for the cure of the cholera, and all wanting, both in practical observations in regard to this dangerous disease, and in rational deductions as to the nature of its attack, and the symptoms which accompany and constitute it. The following are the principal questions proposed for the *concours* of 1861 and 1862:—

1st question. "Discuss carefully and compare with theory, the observations upon the tides, made in the principal ports of France."

2nd quest. "To complete in some important point the geometrical theory of polyhedrons."

3rd quest. "Establish the general equations of the movement of the earth's atmosphere, taking into consideration the rotation of the earth, the calorific action of the sun, and the attractive forces of the sun and moon."

4th quest. "Study of any question, at the option of the candidates, relative to optical phenomena."

5th quest. "At different points of the thermo-electric scale and for a difference of temperature reduced to 1° C. to determine the directions and compare the relative intensities of the electric currents produced by different thermo-electric substances."

6th quest. "Determine by experiment the causes which influence the difference of position of the optical and photogenic foci."

7th quest. "Comparative anatomy of the nervous system of fishes."

8th quest. "Study of the hybrid vegetables, with respect to their fecundity, and the perpetuity or non-perpetuity of their character."

9th quest. "Study of the mode of formation and of the structure of spores and of the other organs which contribute to the reproduction of fungi, their physiological office, the germination of the spores, and particularly with reference to parasitic fungi, their mode of penetration and developement in other living organized bodies."

Each of these prizes consists of a medal of the value of 3000 fr.

10th quest. "Essay upon carefully made experiments, to throw new light upon the question of so-called spontaneous generation."

11th quest. "Experimental study of the modifications which can be effected in the developement of the embryo of a vertebrate animal by the action of external agents."

12th quest. "Study of the distribution of the vessels of the latex in the different organs of plants, with particular regard to their relation to or connection with the lymphatic or spiral vessels, as well as with the fibres of the liber."

13th quest. "Determine experimentally what influence insects may exercise upon the production of the diseases of plants."

Each of these prizes consists of a medal of the value of 2500 fr.

Besides these prizes there are others: especially the Bréant prize (100,000 fr.) is to be awarded "to the person who shall have discovered the means of curing the Asiatic cholera, or shall have discovered the causes of this terrible scourge."

Now that we are upon the subject of prizes we will also say that a prize of 6000 fr. is offered by the society of Pharmacy at Paris, for "the question of the artificial production of quinine, or in default of this, of a substitute possessing equivalent anti-febrile properties." The prize for artificial quinine has been open since 1849.

Time strictly limited to July, 1861.

These concours are open to all scientists without distinction of country.

Obituary. Death of Poinsot.—This illustrious geometer died on the 10th of December last, at the age of 83 years. Born in 1777, he carried on his studies at the Polytechnic School from which he went out in 1796 in the character of an engineer of bridges and highways. He was successively, Professor at a Lyceum in Paris, and member of the higher council of public instruction. He had been a member of the Institute since 1813 where he took the place of the mathematician Lagrange. We shall speak no further of the titles, the dignities, and the decorations of Poinsot, since all these objects of human pride are but a vapor, and have added nothing to his merits, whom mechanicians place, in the history of mechanics, immediately after Archimedes, Galileo, Huygens and Newton. His principal claim upon the memory of posterity is founded upon "*La Statique*," the "*Memoire sur l'équilibre et le mouvement des systèmes*, 1806," and the *Théorie nouvelle de la rotation* and the *Mémoire sur les cônes circulaires roulants;* the last memoirs were in 1853.

In the *Elémens de Statique*, he brought to light his beautiful theory of couples and its application to the conditions of the equilibrium of machines.

Poinsot had a truly philosophic mind; he knew how to render the most abstract matter accessible and to bring it down to the most elementary ideas; this power was one of the characteristic traits of his genius. It is equally recognized in his work upon the precession of the equinoxes, that remarkable phenomenon discovered by Hipparchus, and explained two thousand years afterwards by d'Alembert.

Poinsot retained his intellectual activity to the close of his life. He never experienced the trials which are often met by men of science in their pathway. While yet young he saw the most illustrious judges proclaim his rare talents, and his life has always been happy and honored; Poinsot was not only a great savant, but moreover a good man.

Discovery of an Intra-Mercurial Planet.—The year 1860 has been inaugurated in France by an astronomical discovery, all the more remarkable because made in unusual circumstances. It is the discovery of another planet between the earth and the sun, verifying the conclusion to which Leverrier had arrived by the power of computation, that there existed one or more planets within the orbit of Mercury. This conclusion was announced to the Academy at the session of September 12, 1859, in a letter from Leverrier to Mr. Faye. Very soon reports were spread that the discovery of such a planet was no new thing. Among these reports, the most persistent was this, that such a planet had been observed towards the commencement of the year 1859, by an amateur astronomer, a poor country doctor. Leverrier, resolving to trace this story to its source, went personally to Orgères, a village in the Department of Eure and Loir, where this doctor resided. Great was the emotion of Dr. Lescarbault on seeing before him the illustrious director of the Observatory of Paris. He had little difficulty in satisfying Leverrier that the observation had really been made. He said, "On the 26th of March last, (1859), about four o'clock, in pursuance of my regular habit, I was exploring the sun's disc with my telescope, when suddenly I noticed near the border a small, well defined, round black spot, having a sensible motion. It advanced farther and farther on the sun's disc. Unfortunately just then occurred a call from a patient. I went down below, gave my advice to the patient, reascended to my observatory, the black spot continued its journey, and I saw it reach the opposite border of the sun and disappear, after being about an hour and a half on the disc."

The measurements made by Dr. L., with very simple means of his own, enabled Leverrier to determine that the chord of the sun's disc traversed by the planet was 9′ 17″; and it would have required $4^h\ 26^m\ 48^s$ to traverse the diameter of the sun. He estimates the angular diameter of the planet at about a quarter that of the planet Mercury when in transit. If this estimate is correct, the new planet is insufficient to account for the anomaly in the motion of the perihelion of Mercury; and there probably is, as M. Leverrier announced, a group of small planets between Mercury and the sun. Observers should give special attention to this subject at the next total solar eclipse.

The new planet has not been officially named, but it is already designated as *Vulcan.* Its return is looked for at Paris at the end of March or the beginning of April.

This discovery was not a matter of mere chance. Dr. Lescarbault, having observed the transit of Mercury, on the 8th of May, 1845, conceived the idea that if there were between the earth and the sun any planetary body besides Venus and Mercury, it must also sometimes cross the sun; and that by frequently observing its disc, such body might be detected thereon as a black point.*

Without fortune and without means of observation, Dr. Lescarbault was unable to obtain a telescope until 1853; and it was not until 1858 that he commenced systematic work. He made most of his auxiliary apparatus for himself, and went to work in astronomy very much as Scheele did in chemistry.

* In 1836 and 1837, M. Pastorff of Buchholz observed, several times, a pair of small, round black spots, of unequal size, passing, in a few hours, across the sun's disc, and each time in a different path. These must have been a planet with a satellite. Messrs. E. C. Herrick and Francis Bradley of New Haven, in 1847, endeavored to re-discover those bodies, by observing the sun's disc, twice a day, with a large telescope, and by exploring the vicinity of the sun with the telescope, armed in front with a pasteboard tube, blackened within. These efforts proved unsuccessful. (See this Journal, Nov. 1859, vol. xxviii, pp. 445–6.) The hypothesis of an intra-mercurial planet was also proposed, many years since, by M. Buys-Ballot, in his researches on a maximum and minimum of the solar heat.

M. Leverrier further informs us that the explanations which M. Lescarbault gave him entirely satisfied him that the alleged observations were real, and are entitled to a place in science; that the long delay in making them public is due solely to the modest reserve of the observer, and the quietude of a residence remote from the excitement of great cities. In support of his assertions, M. Leverrier has exhibited the proofs which he found in the domicil of Dr. Lescarbault, viz. a paper on which the latter had marked, on the 26th of March, 1859, the appearance of the black point on the solar disc, and a pine board on which Dr. L. had chalked his computations and his drawings.

Hardly had these announcements been made, when several other similar observations were called to mind. Mr. Scott, Chamberlain of London, writes in Galignani's Messenger of January 14, 1860, that an Englishman, Mr. Lloft, observed, January 6, 1818, a black point traversing the sun's disc, and that he (Mr. S.) had made a like observation in the summer of 1847, had thence concluded the existence of a third inferior planet, and had so published. His son, five years old, standing by, was placed at the telescope, and confirmed the observation by crying out, "I see a little balloon on the sun."

M. Wolff of Zurich has published several ancient observations, which he thinks refers to the transit across the sun of an intra-mercurial planet, viz. by (1,) Scheutzer of Crefeld, June 6, 1761; (2,) Staudacher, towards the end of February, 1762; (3,) Lichtenberg, November 19, 1762; (4,) Hoffmann, at the beginning of May. 1764; (5) Dangos, January 18, 1798; (6,)Fritsch, October 10, 1801; (7,) Stark, October 9, 1819. All these observers saw a round well-defined point, of the apparent diameter of Mercury, crossing the solar disc in a short period, varying from two to three hours. Using only the observations 5, 6 and 7, Mr. Wolff finds that they accord with the supposition of a planet having a revolution around the sun in 19·25 days, a result surprisingly near to that (19·7 days) deduced by M. Leverrier, from the observation of Dr. Lescarbault.

In settling the question, who is entitled to the credit of the discovery, it is clear that Dr. Lescarbault's observations are the first which have the scientific seal, and are of such a nature as to be subject of computation.

New Members Elected.—

Fizeau was elected 2nd of January member of the section of Physics in place of *Cagniard de Latour*—deceased.

Plana was elected March 5, foreign associate in place of *Lejeune-Dirichlet* deceased.

At the session of the *Académie* held March 19, Mr. J. A. Serret was elected a member in the section of Geometry in place of *Poinsot*, deceased.

Dausey has lately (March 12) communicated to the Academy a memoir, setting forth the geographical and physical observations of Hommaire de Hell made in 1846–48 in Turkey and Persia, where he died at Ispahan in August, 1848. His manuscripts were taken to France by Jules Laurent the well-known artist who accompanied the expedition. Dausey reports the barometric and astronomical determinations of much value.

Hypnotism and Magnetism.—French society has been almost carried away with a kind of scientific epidemic which may be compared to the malady called "table tipping," which was so much in vogue a few years since. This epidemic is Hypnotism or nervous sleep. It was brought into notice by a French surgeon following a work of Dr. Braid (published 15 years since), in which this physician, describes under the name of Hypnotism, the nervous affection which is produced under the following circumstances:—When a brilliant object is placed directly before the face at a distance of from 8 to 15 inches, and the subject of the experiment is requested to fix his eyes steadily upon the object, in such a way as to produce a permanent contraction of the muscles of the eye and eyelid, there will be seen to come on, after the lapse of a few minutes, a state analogous to catalepsy. M. Broca, having successfully employed hypnotism in producing insensibility to pain, proposes it to surgeons as an anæsthetic agent capable, in many cases of being substituted in practice for ether or chloroform, "because," as he remarks to physicians, "this method, introducing as it does no substance into the system, appears to me absolutely harmless."

From the numerous experiments which have since been made, it results; 1st, that this kind of anæsthesia can be developed but rarely, and in persons whose nervous system is especially predisposed; 2nd, that hypnotism may give rise to attacks of epilepsy.

This kind of anæsthesia is therefore not less open to objection than the other. Its origin is more ancient than is supposed; it was established more than two centuries ago, under the name of *irradiation* or *phenomena of actinobolism*, by P. Kircher in his *Ars magna Lucis et Umbræ.*

It is concluded that in hypnotism is seen only a fact in the domain of animal magnetism, which is not yet a science. We will not enter further into the details.

Porous bodies.—Among the topics of scientific interest which awaken attention at present, is the research of Jamin, professor at the École Polytechnique, upon the equilibrium and movement of fluids in porous bodies. The new results at which he has arrived afford an explanation of the ascent of the sap in vegetables without the necessity of recourse to the vital force. It is apparently a question of capillarity only.

Jamin has applied the new facts which he has discovered to the construction of an apparatus composed entirely of inorganic materials, but showing in its structure a great analogy with vegetables. This apparatus has the property of raising water as trees do, to a height greater than that attained by means of atmospheric pressure, from a moist soil whence the water is constantly drawn to the factitious leaves where it is continually evaporated.

Reduced to its most simple form this apparatus is composed of a block of some well dried porous substance as chalk, lithographic stone, &c., or a porous battery cell filled with a powder well rammed in, white chalk for instance, oxyd of zinc, or even with earth. A manometer is imbedded in the interior of the mass, and the whole is plunged in a vessel full of water. The water immediately penetrates its pores and drives out the air, which collecting in the interior, exercises a pressure upon the manometer amounting with oxyd of zinc to *five* atmospheres and with starch it exceeds *six* atmospheres. This is not the limit of the greatest possible pressure; Jamin makes known the causes which diminish it in these cases and proves that the water is forced into porous bodies with a force which he calls π, and which is equal to that of a considerable number of atmospheres.

A tube 1·20 metres long filled with plaster and terminated at the summit by an evaporating surface is inserted by its base into a reservoir closed and filled with water; a vacuum is caused measured by 15 or 20 millimetres of mercury or by 200 or 270 millimetres of water; and the water appears even at the upper extremity of the tube, which proves *that porous bodies are able to raise water higher than can be done by atmospheric pressure.* These facts cannot be explained by the ordinary laws of capillary attraction, since these bodies are not formed of impermeable tubes, but of corpuscles in juxtaposition, separated by small empty spaces. Jamin has therefore submitted the problem to the calculus and has come to results, of which we mention the following:

If in a damp porous body, the water is compressed by a power of several atmospheres, it can congeal only at a temperature below 0° C.* Consequently the old wood is able to resist frost, while the young shoots being less dense are unable to do so.

Since water in filtering through a porous body is compressed as it enters and dilates again as it runs out, it should exhibit electric currents and many other phenomena.

The theory can not be applied to non-homogeneous porous bodies. In the extended memoir which he has prepared, Jamin discusses the complicated results which may be occasioned by irregularity of structure; he makes an application of it to wood, and shows that the interior pressure must be augmented in the denser tissues; that the air must come from the larger tubes, which cannot serve for the ascent of the sap.

It is plain that the evident tendency of all these experiments is to explain the ascent of the sap in vegetables by capillarity. The idea is not new, but it has not been hitherto fully admitted, notwithstanding the experiments which have been heretofore made.

Jamin gives it probability in showing by decisive experiments, that porous bodies exercise a capillary action superior to the pressure of the atmosphere; further, he gives the physical theory of capillarity in porous bodies and succeeds in calculating the phenomena of the movement of liquids in trees. This is thoroughly physiological. If the Academy of Sciences could award the great prize for physiology, for a work upon fermentation, when it is not yet known whether this purely chemical phenomena is the result of vital action as Cagniard de Latour maintains, or the manifestation of a mechanical effect as Liebig explains it, in his beautiful theory of fermentations,—if this work in chemistry deserved the great prize for experimental physiology for a stronger reason should they award this prize for the splendid physical researches of which we have just spoken.

Application of electric light in Medicine.—The ordinary processes of illumination are of very difficult application when the object is to employ artificial light in diagnoses or in certain cases in operative medicine, inasmuch as the illumination is insufficient, or the light is more or less colored, and is accompanied with heat. It is not the ordinary electric light here spoken of, but that produced by induced currents. The problem to be solved consists in finding a source of light with little or no heating effect, which can be compressed into tubes of small capacity and of forms adapted

* This fact has just been demonstrated by Mr. Sorby for water contained in capillary tubes of a small diameter.—J. N.

to circumstances, and which finally is of such whiteness as not visibly to alter the color of organic tissues illuminated by it. This problem has just been solved by Dr. Fonssagrive, physician of the naval school at Brest, with the coöperation of Messrs. Ruhmkorff and DuMoncel. The apparatus consists of an empty tube of Geissler, of very small diameter folded and turned upon itself after the manner of multipliers; this tube contains a gaseous mixture which gives a perfectly white light when it is traversed by an electric current produced by an induction coil.

Phosphorescence.—The electric lamp (*photophore*) of which we were just speaking calls to mind some observations upon phosphorescence which have just been made by Phipson; he has found that like cane sugar, the *sugar of milk* or *lactine* becomes luminous by concussion, and also by fracture. To evolve phosphorescence from the *nitrate of uranium*, it is sufficient to shake briskly a bottle containing a certain quantity of this salt in the crystalline state. The light is very vivid when the experiment is made with one or two kilograms of this substance. Calomel possesses the same property, although in a less degree.

Works of Arago.—The sixteenth and last volume of Arago's works has just appeared. It contains a great number of unpublished researches made by Arago during his career.

The sixteen triangles are here given which Messrs. Biot and Arago determined in the prolongation of the meridian of France to the island of Formentera. Arago alone, has measured besides, a seventeenth triangle having its summit at the enclosure of Galaro in the island of Majorca and resting in one direction upon Camprey in the island of Trizza, and the other upon the mole of Formentera, with the design of obtaining the length of an arc of the parallel nearly 3° from the extremity of the meridian, and to determine the curvature of that portion of the earth's surface. The results of these measurements, heretofore unpublished, are found in this volume.

Arago in the year 1853, the very year of his death, communicated to the Academy a memoir upon the figure and physical constitution of Mars, which is likewise contained in this volume, and is accompanied by more than 3000 micrometrical measurements of the diameters of Mars, Jupiter, Saturn and Uranus, which were taken from 1811–1847.

It is well known that Arago made numerous investigations in regard to the refractive power of atmospheric air, dry or humid, and of different gases and vapors. He labored in this department for nearly half a century, namely, in 1805 with Biot, in 1815 and 1816 with Petit, and in 1852 with Fizeau. Chemists and physicists will derive much advantage from the determination of the refractive powers not only of some simple gases, but also of compound gases such as oxyd of carbon, carburetted hydrogen, sulphuretted hydrogen, cyanogen, the vapors of sulphur and of carbon, of sulphuric ether and chlorohydric ether.

This volume also contains studies upon optics, atmospheric electricity, &c. United to the other fifteen volumes it forms a lofty monument to science as well as to one of its most noble representatives.

Bibliography.—The following works have just appeared at Paris.

Oeuvres d'Arago. Tom. xvi, 1859; chez Gide, 5 rue Bonaparte.—This volume is devoted to scientific notices bearing upon the personal labors of Arago, many of which have never before been published.

At the *Librarie centrale des sciences rue de Seine; Recherches sur le non-homogénéité de l'étincelle d'induction*, par M. Th. du Moncel, 1860.—M. du Moncel, who gives all his leisure to the study of electricity and its applications, presents in this brochure the result of his researches upon the electric spark, and especially the spark of induction, of which the non-homogeneity was discovered by him in 1855.

At Baillière Bros., Paris & New York. *Traité èlementaire de Physique expérimentale*, Tom. 1. 12°, 1860, par M. Forthomme, professor of Physics at the Lyceum in Nancy.—This work, even by the confession of the author, contains nothing new, but is distinguished by its method. The most difficult questions in regard to gravity, hydrostatics and heat are explained in the first volume with great clearness, and thus rendered intelligible to persons little versed in these matters, which are so important in our day and have so many useful applications.

By *Lacroix & Baudry*, Quai Malaquais.—*Grands hommes et grandes choses, notices scientifiques sur les inventions et sur les decouvertes modernes et sur les auteurs, par Victor Meunier.* 8°, 1860—This work appears by numbers, once a week; its author, of whom we have often spoken, has acquired in France a great reputation as a popularizer. He established the *Ami des Sciences*, a journal for scientific discussions, which he has directed for six years, and is remarkable for the independence of his opinions and judgments. M. V. Meunier in this new publication proposes all the great scientific questions of the day.

Art. XXXVIII.—*Description of an Equatorial recently erected at Hopefield Observatory, Haddenham, Bucks;* by the Rev. W. R. Dawes.

(*From the Monthly Notices of the Royal Astronomical Society.*)

My observatory was furnished, in May last, with an equatorially-mounted telescope by Messrs. Alvan Clark and Sons, of Boston, U. S., which in several important points differs from any other in this country; and I therefore hope that a brief description of it may not prove uninteresting to the Royal Astronomical Society.

The form combines great firmness and compactness with considerable elegance of design. The massive part of its structure is of cast-iron, the base of which is firmly-bolted down to a stone pier. The semicircular form of the upper part affords a secure position for most of the wheel-work of the driving-clock, of which the going-weight descends in a groove on the east side of the pier, and is not seen in the drawing. The space between the polar axis and the semicircular bed-piece is occupied at its lower part by the hour-circle. Immediately above this is a sector, which clamps on to the axis, and the wheel-work of the clock occupies the upper portion. The sector has a radius of rather more than 9 inches and an arc of 30°, or two hours of right ascension. This arc has a face of an inch and a half in breadth, between which and a cylinder 7 inches in circumference there is just room enough for two thin bands of sheet-brass, each of about three-fourths of an inch in width, to pass side by side. These bands are both keyed by the end into one groove in the cylinder, at such a distance that they cannot overlap or interfere with each other. They are then bent round the cylinder in opposite directions, the end of one being fastened to one extremity of the arc of the sector, and the end of the other at the other extremity of the arc to a piece of brass which is acted upon by a screw and nut, for giving to both the bands a due degree of tension. The sector and cylinder thus move together without friction, irregularity, or lost time.

Upon the same arbor with the cylinder is the wheel, 15 inches in circumference, in the racked edge of which the driving-screw works. This arrangement gives the screw about the same driving-power as if it acted on the edge of a wheel nearly 40 inches in diameter, fixed on the polar axis.

I have every reason to be satisfied with the going of the driving-clock; and the cylindrical bob of the pendulum being screwed on to its steel rod, the rate is capable of adjustment to the greatest nicety. Great care has been bestowed by the makers upon the accurate dividing of the wheel-work; and I have much pleasure in acknowledging that its performance fully bears out my expectations, founded on the character given by the Messrs. Bond of the clock-work applied by the same makers to the great Munich equatorial in Harvard Observatory, which has been so successfully employed for the purposes of telescopic photography. While the speed of the clock is regulated by the vibrations of the half-seconds pendulum, the action of the pendulum on the wheel-work is rendered smooth and equable by an ingenious application of Bond's *Spring-governor;* and so perfectly successful is this contrivance, that with the thread of the micrometer bisecting a star, and a power of 800 or 1000 on the telescope, no interruption or jerk from the escapement is perceivable.

For producing a slow motion in right ascension, the driving-screw is mounted on a brass frame, which, being carried by a fine screw under the observer's control, acts as a slipping-piece through nearly five minutes of time.

A firm clamp, close to the cradle of the telescope, fixes the declination-axis, and is accessible to the observer both at the eye-end, and also during the setting of the declination-circle. A slow-motion screw acts on an arm extending from the clamp to the bottom of the cradle to which the screw is attached.

To permit the adjustment of the polar-axis to the latitude and meridian of the place, the upper part of the cast-iron bed-piece is made with a groove which receives loosely a projecting keel on the portion bolted down to the pier. The form both of the groove and of the keel being semicircular, the upper portion is moved upon the lower by the stout screw which is seen in the drawing, and the polar-axis is thus easily raised to the required angle. The adjustment to the meridian is performed by the screws on each side of the groove in the upper piece pressing against the keel in the lower, which has play enough in the groove to allow of a moderate degree of azimuthal motion.

To facilitate the finding of objects in Mr. Clark's "Two-eye-piece Micrometer," when their distance exceeds the field of one of the eye-lenses, the finder is furnished at its eye-end with a small position-circle divided into degrees. The thick wires of the finder being placed in the direction of the objects to be measured, the reading of the position-circle indicates the approximate setting for the micrometer, whereby the two objects may be immediately found by their respective eye-lenses. The aperture of the finder being two inches, it will show a star of the 9¾ magnitude of Struve's scale.

The object-glass of the telescope has a clear aperture of 8¼ inches, and a focal length of about 110 inches. The materials were furnished by Chance and Co., of Birmingham. The figure is excellent to the circumference, and the dispersion but little over-corrected. Its performance fully supports the character of Mr. Alvan Clark's object-glasses, and I believe it to be capable of everything which can be performed by such an aperture. It clearly divides γ^2 *Andromedæ*, and shows the smallest companions among the stars of the Pulkova Catalogue.

Haddenham, near Thame, November, 1859.

SCIENTIFIC INTELLIGENCE.

I. CHEMISTRY AND PHYSICS.

1. *On Fraunhofer's Lines.*—Kirchhoff has communicated a preliminary notice of very remarkable investigations on the spectra of colored flames. These investigations have given an unexpected clue to the origin of Fraunhofer's lines, and justify some remarkable conclusions as to the constitution of the atmosphere of the sun and perhaps also of the more brilliant fixed stars.

Fraunhofer remarked that in the spectrum of the flame of a candle there are two bright lines which correspond with the two dark lines D of the solar spectrum. The same bright lines are obtained more easily and stronger from a flame into which common salt has been introduced. The author produced a solar spectrum and allowed the sun's rays before they fell upon the slit to pass through a powerful salt flame. When the sun-light was sufficiently weakened two bright lines appeared instead of the two dark lines D; when however the intensity of the sunlight exceeded a certain limit, the two dark lines D were seen with much greater distinctness than without the presence of the salt flame.

The spectrum of Drummond's light usually contains the two bright sodium lines, when the luminous portion of the lime cylinder has not been exposed to ignition for a long time; if the lime cylinder remains fixed, the lines become weaker and finally vanish. Under these circumstances, a salted alcohol flame placed between the lime cylinder and the slit, produces two dark lines of extraordinary sharpness and fineness, which exactly correspond to the lines D of the solar spectrum. In this manner these lines are artificially produced in a spectrum in which they do not actually occur.

If we introduce chlorid of lithium into the flame of Bunsen's gas lamp, the spectrum exhibits a very bright, sharply defined line, which lies in the middle between Fraunhofer's lines B and C. If we allow solar rays of moderate intensity to pass through the flame to the slit, we see the line bright upon a dark ground; but with a greater intensity of the sunlight a dark line occupies its place, which has precisely the character of Fraunhofer's lines. When the flame is removed, the line vanishes completely.

The author concludes from these observations that colored flames in whose spectra bright sharp lines occur, diminish the intensity of rays of the color of these lines, when these pass through them, to such a degree, that in place of the bright lines, dark ones occur, whenever a source of light of sufficient intensity is placed behind the flame. He further concludes that the dark lines of the solar spectrum which are not produced by the earth's atmosphere result from the presence in the ignited sun's atmosphere of those substances which produce in the spectrum of a flame bright lines in the same place.

We may assume that the bright lines corresponding with D in the spectrum of a flame always arise from the presence of sodium; the dark lines D in the solar spectrum allow us therefore to conclude that sodium is present in the sun's atmosphere. Brewster has found in the spectrum of the flame of saltpetre bright lines in the position of Fraunhofer's lines A, *a*, B; these lines indicate the presence of potassium in the sun's atmosphere. From the author's observation that no dark line in the solar spectrum corresponds to the red lithium line, it would follow that lithium is either not present at all in the sun's atmosphere or is present only in very small quantity.

The investigation of the spectra of colored flames has in this way obtained a new and high degree of interest. The author promises to pursue the subject in connection with Bunsen, and states that they have already obtained results which render it possible to determine the qualitative constitution of complicated mixtures from the appearance of the spectrum of their blowpipe flames. In pursuing together the investigation of Kirchhoff's discovery of the influence of flames upon rays of light, a remarkable fact has appeared which promises to be of great importance. Drummond's light requires a salt-flame of low temperature in order that the lines D may appear dark. The flame of alcohol and water is adapted to this purpose, but the flame of Bunsen's gas lamp is not. In this last case, the smallest perceptible quantity of salt causes the bright lines to appear. The authors reserve the developement of the consequences of this remarkable fact.—*Pogg. Ann.*, cix, p. 148, *January*, 1860.

Note.—Professor Stokes, in a letter to the editors of the L. and E. Phil. Mag. directs attention to the fact that Foucault, in 1849, published a short paper in l'Institut containing observations exactly analogous to those of Kirchhoff. Foucault's experiments were made by transmitting solar light through the galvanic arc, and appear to have escaped attention until their second and independent discovery by Kirchhoff.

2. *On the direct conversion of lactic into propionic acid.*—The conversion of lactic into propionic acid was first observed by Ulrich who obtained chloropropionic acid by distilling lactic acid with perchlorid of phosphorus. Lautemann has succeeded in a more direct transformation of lactic acid by the agency of iodid of hydrogen. When concentrated lactic acid is diluted with half its volume of water and the cooled liquid is saturated with iodohydric acid gas, iodine is set free and if the liquid is heated in a closed tube to 140° C. iodine is separated in quantity. The filtered liquid neutralized with potash and then distilled with dilute sulphuric acid yields propionic acid mixed with some iodine and iodohydric acid which are easily separated by carbonate of silver. The reaction which yields propionic acid is expressed by the equation

$$\underset{\text{Lactic acid.}}{C_6H_5(HO_2)O_4} + 2HI = \underset{\text{Propionic acid.}}{C_6H_6O_4} + 2HO + I.$$

The same result may be still more easily obtained by distilling lactic acid with biniodid of phosphorus when propionic acid passes over colored by free iodine. Lautemann considers these results as confirming Kolbe's view that lactic acid is propionic acid in which one equivalent of hydrogen is replaced by one of the compound radical HO_2.—*Ann. der Chem. und Pharm.*, cxiii, 217.

3. *On the Formation of Alanin from Lactic Acid.*—Strecker's discovery that alanin may be converted into lactic acid by the action of nitrous acid is familiar to chemists. Kolbe has succeeded in effecting the reverse process, that is, in obtaining alanin from lactic acid. Lactate of lime is to be converted into chlorid of propionyl by distillation with perchlorid of phosphorus and this by treatment with absolute alcohol into chloropropionic ether. The ether is then to be treated with a concentrated solution of ammonia in a closed tube for several hours. The solution on evaporation gives a mixture of sal-ammoniac and alanin which are easily separated. The reaction in this case is expressed by the equation—

$$C_6H_4ClO_3,\ C_4H_5O + 2NH_3 + 2HO = C_6H_4(NH_2)O_3,\ HO + NH_4Cl + C_4H_5O,\ HO.$$

Ann. der Chem. u. Pharm., cxiii, 220.

4. *On the Constitution of Lactic Acid*—Kolbe has communicated a very interesting discussion of this subject, maintaining his view that lactic acid is monobasic and is to be regarded as oxypropionic acid. We must however refer to the original paper which does not admit of condensation.—*Ann. der Chem. und Pharm.*, cxiii, 223.

5 *Contributions to the Chemistry of the Platinum-metals.*—Claus has continued his investigations of this most difficult subject and has obtained many new and interesting results, the most important of which we shall here state.

In the present paper, the author confines his attention chiefly to ruthenium and its analogies with osmium. The hydrated deutoxyd of ruthenium has the formula RuO_2+5HO and may be obtained either by digesting the corresponding chlorid with carbonate of potash and washing the oxyd with boiling water, or by precipitating the sulphate of the oxyd with potash or soda. The sulphate in question may be prepared by oxydizing the sulphid with nitric acid. The oxyd when freshly precipitated is a slimy ochre-colored substance which dries to a rust colored mass resembling hydrated sesquioxyd of iron. It dissolves in acids with a bright yellow color, and gives on heating with chlorhydric acid, a beautiful red solution of the chlorid. When heated to 300 C. the oxyd loses a portion of its water, and when more strongly heated, explodes in consequence of the instantaneous separation of the remaining water.

The author describes in full the double salts $RuCl_2$, KCl and $RuCl_2$, NH_4Cl and gives their reactions with the usual reagents. Ruthenium forms, like osmium, an acid containing four equivalents of oxygen, as well as the ruthenic acid RuO_3 already described by Claus. To this acid Claus gives the name of hyper-ruthenic acid; it may be prepared by passing a current of chlorine into an alkaline solution of ruthenic acid, prepared by fusing metallic ruthenium with caustic potash and saltpetre.

The hyper-ruthenic acid, being volatile, distils over, and may be collected in a receiver. The new acid is a golden yellow crystalline and volatile substance. On gentle heating, it melts into golden yellow drops, which again solidify as a crystalline mass. The acid is very volatile and evaporates at ordinary temperatures; its gas has a golden yellow color, and the acid has a peculiar smell which resembles that of nitrous acid. The gas irritates the lungs and produces a cough, but does not attack the eyes like osmic acid. It has little taste, but is somewhat astringent, though not acid. Its boiling point lies not far above 100° C.

The acid is very easily reduced. In the moist state its solution is decomposed after a few hours, with formation of the sesquioxyd. Alcohol and the greater number of organic bodies easily reduce it. Potash dissolves the acid slowly, but the solution passes after a time into one of ruthenate of potash. The author gives the relations of this body to other reagents in detail, but for these we must refer to the original memoir.

When a solution of $RuCl_2$, NH_4Cl is evaporated to dryness with an excess of ammonia, the chlorid of a new base is formed, the formula of which, according to Claus, is $2NH_3 . RuCl+3HO$. The salt has an Isabel yellow color, and crystallizes in rhombic tables, which are soluble in water and yield on heating pure metallic ruthenium. When this chlorid is digested with an excess of freshly prepared oxyd of silver, it yields a solution of the oxyd. On evaporation, this solution gives a rather porous, yellow, crystalline mass; this base appears to be even more caustic than caustic potash itself. The author promises a more complete description of this interesting base, as well as additional contributions to the chemical history of iridium and rhodium.—*Journâl für prakt. Chemie*, vol. lxxix, 28, *January 31st.*

6. *Synthesis of new Bases containing Oxygen.*—WURTZ has observed the remarkable and important fact that oxyd of ethylene $C_4H_4O_2$ combines directly with ammonia, forming very well defined and powerful bases. When concentrated aqueous ammonia is mixed with oxyd of ethylene and the mixture is allowed to stand, combination ensues with a strong evolution of heat. On evaporation an alkaline syrup is obtained which gives with chlorohydric acid a solution from which colorless rhombohedra separate. These have the formula $C_{12}H_{15}NO_6+HCl$: the platinum salt is $C_{12}H_{15}NO_6, HCl+PtCl_2$. The mother liquor of these rhombohedra contains another chlorid, the platinum salt of which has the formula—

$C_8H_{11}NO_4$, $HCl+PtCl_2$. The formation of these bases is indicated by the equations—

$$3C_4H_4O_2+NH_3=(C_4H_4O_2)_3H_3N=C_{12}H_{15}NO_6$$
$$2C_4H_4O_2+NH_3=(C_4H_4O_2)_2H_3N=C_8H_{11}NO_4.$$

These results may perhaps lead to the discovery of the true constitution of the complex organic alkaloids containing oxygen. They appear to show that Berzelius' view that the alkaloids are conjugates of ammonia may be true in some cases at least.—*Comptes Rendus*, xlix, 898.

7. *On a new series of Alcohols.*—WURTZ has also found that oxyd of ethylene unites with water to form new alcohols which he terms diethylene and triethylene alcohols. The reactions involved are represented simply by the equations—

$$C_4H_4O_2+2HO=C_4H_6O_4. \quad \text{Glycol.}$$
$$C_4H_4O_2+2HO=C_8H_{10}O_6. \quad \text{Diethylene-alcohol.}$$
$$3C_4H_4O_2+2HO=C_{12}H_{14}O_8. \quad \text{Triethylene-alcohol.}$$

Oxyd of ethylene also unites directly with glycol so as to form the diethylene and triethylene alcohols. The equations are

$$2C_4H_4O_2+C_4H_6O_4=C_{12}H_{14}O_8.$$
$$C_4H_4O_2+C_4H_6O_4=C_8H_{10}O_6.$$

All these substances behave like alcohols. The diethylene alcohol was also discovered by Lourenço and called by him the intermediate ether of glycol.—*Comptes Rendus*, xlix, 813.

8. *Researches on the Platinum metals;* from a letter of Dr. WOLCOTT GIBBS to one of the editors.—"The completion of my researches on the platinum metals has been delayed much longer than I expected. This has arisen partly from the intrinsic difficulty of the subject and partly from its expansion in particular directions in a very unexpected manner. A brief sketch of the results already obtained—imperfect as they are—may perhaps interest you.

The osmium-base of which Dr. Genth and myself published a brief notice about two years since in the Journal, proves to be the type of a very extensive series of compounds which promise to be of much theoretic interest. You will remember that the chlorid of that base is produced when osmite of potash, KO, OsO_3, is added to a solution of salammoniac. I now find that new complex bases are formed when the osmite is added to solutions of the chlorids of narcotin-ammonium, cinchonin-ammonium, &c., &c.; in short, almost all the complex alkaloids which I have yet tried give analogous bases containing osmium in the radical. The new bases are very easily decomposed with evolution of osmic acid. They are more stable in the presence of an excess of chlorhydric acid and give crystalline double salts with the chlorids of gold and platinum.

These however are not the only or even the most remarkable basic compounds which I have discovered. Many of the ammonia-metal bases already described are capable of forming new bases into which osmium enters either as a conjugate body or as replacing hydrogen. When, for instance, osmite of potash is added to a solution of the chlorid of pallad-diamin, $2NH_3 . PdCl$, a yellowish brown solution is formed which on addition of chlorhydric acid gives a beautiful yellow crystalline precipitate insoluble in cold water and containing palladium, osmium and the elements of ammonia.

When osmite of potash is added to a solution of chlorid or sulphate of luteocobalt, $6NH_3 . Co_2Cl_3$ or $6NH_3 . Co_2O_3 . 3SO_3$, a buff-yellow precipitate is thrown down, which on addition of HCl gives a wine-yellow solution. This solution after a short time deposits beautiful crystals of the chlorid of a new base containing osmium, cobalt, and the elements of ammonia. The chlorid gives well crystallized salts with the chlorids of platinum, gold and mercury. Its solution is decomposed by gentle heating, osmic acid being evolved while a black powder is thrown down.

The other ammonia-cobalt bases give analogous compounds which however are decomposed almost as soon as formed. It is my intention if possible to examine the relations of osmite of potash to one or two of the arsenic and antimony bases, as for example to the chlorids having the formulas $(C_4H_5)_4AsCl$ and $(C_4H_5)_4SbCl$.

When ammonia is added to a solution of osmite of potash, the red color of the latter passes immediately to wine-yellow. Fremy supposes that an osmiamid is formed here having the formula $OsO_2 . H_2N$. I find that the product is a new osmium base, the chlorid of which is formed at once by neutralizing the yellow solution with HCl. This chlorid has probably the formula $\left.\begin{matrix} H_3 \\ Os \end{matrix}\right\} NCl$, though it may be $\left.\begin{matrix} H_3 \\ OsO_2 \end{matrix}\right\} NCl$. In like manner I am still doubtful whether the true formula of the other ammonia-osmium base is $2NH_3 . OsO_2 . O$ or $2NH_3 . OsO + 2HO$. Experiments now making will decide this point. Meantime I may say that I do not agree with Claus in considering the formula $2NH_3 . OsO_2 . O$ improbable since we have many analogous cases, as for instance in xanthocobalt and flavocobalt, the formulas of the chlorids of which are, you will remember,

$$NO_2 . 5NH_3 . Co_2O . Cl_2 \text{ and } 2NO_2 . 5NH_3 . Co_2O . Cl_2.$$

The chlorid $2NH_3 . OsO_2 . Cl$ or $2NH_3 . OsCl + 2HO$ is decomposed by boiling with chlorhydric acid, giving sal-ammoniac and a new chlorid which is perhaps identical with that just mentioned. This is easily explained by the equation

$$2NH_3 . OsCl + HCl = NH_3 . OsCl + NH_4Cl,$$

and the decomposition would then be exactly analogous to that of pallad-diamin under the same circumstances

$$2NH_3 . PdCl + HCl = NH_3 . PdCl + NH_4Cl.$$

I hope soon to decide these questions by experiment and analysis, but the analyses are very difficult and tedious, and I have to feel my way and find out new methods for almost every determination.

You will see from the above that osmium is likely to become one of the most interesting of the elements, and that it is capable of forming an extraordinary number and variety of compounds. I am also very busy with the remarkab e class of double nitrites which I described at the meeting of the Am. Association for the Advancement of Science in August last. There are still some difficulties to be overcome, but I am confident that I shall be able to effect a perfect separation of all the metals of the platinum group. The white iridium salt appears to have the formula $Ir_2O_3 . 2NO_3 + 3KO . NO_3$, but it contains a small quantity of chlorine which may be essential. Its stability and insolubility are very remarkable, and will I think prove of great value in separating iridium from the other metals. The corresponding salts of the other metals of the group appear to be all soluble. The ruthenium salt is soluble even in alcohol and ether, and gives with sulphid of ammonium a magnificent red solution. This is by far the most delicate test for ruthenium yet discovered as the reaction is peculiar to that metal. Claus' beautiful reactions with sulphocyanid of potassium and sulphydric acid are much inferior for qualitative purposes."

New York, March 30, 1860.

Technical Chemistry.

9. *Solution of Cellulose in Ammonio-oxyd of copper.*—Some time since Erdmann, in his *Journal für praktische Chemie* (lxxvi, 386) expressed the opinion that cellulose is not really dissolved by cuprate of ammonia, as stated by Schweizer (*ibid*, lxxii, 109), but only swollen to a sort of thin mucilage like the well known limpid "solution" of starch.

This view was based upon the fact that when a clear solution of cellulose in NH_3CuO is diluted with a large excess of water, the cellulose separates entirely in the course of a few days.

In defense of his original statement Schweizer now urges that the cellulose must be really dissolved by ammonio-oxyd of copper: since its fibres are unquestionably destroyed when this reagent comes in contact with them—as may be distinctly seen with the microscope; and since the cellulose precipitated from a solution of cotton in the above mentioned reagent no longer exhibits any trace of definite structure.

A solution of cotton in NH_3CuO may also be filtered perfectly clear—although this operation is somewhat difficult when large quantities of the liquid are operated upon. The solution is moreover capable of passing through the cell membranes of plants as shown by Cramer (*ibid*, lxxiii, 6).

The destruction of the solvent power of the cuprate of ammonia by dilution appears to depend upon alterations which this compound itself undergoes under certain circumstances. It often happens that a solution of ammonio-oxyd of copper which at first dissolved cotton with the greatest ease gradually loses this power even when kept in carefully closed vessels completely filled with the liquid. It is moreover well known to chemists that solutions of ammonio-oxyd of copper, and of the ammonio-copper salts, undergo decomposition when diluted with large quantities of water;—hydrate of copper being precipitated.

This decomposing influence which water exerts upon solutions of the compound of ammonia and oxyd of copper is in the opinion of Schweizer the cause of the gradual precipitation of cellulose from such solutions when these are largely diluted.—*Journal für praktische Chemie*, lxxviii, 370; compare also Cramer, *ibid.* lxxiii, 1, *et seq.*

10. *Decoloration of Indigo by Sesquioxyd of Iron.*—According to Kuhlmann, when a solution of blue indigo is acted upon at the temperature of 150° (C.) by hydrated oxyd of iron its color is, almost immediately, completely destroyed. The same thing occurs with a number of other coloring matters.—In noticing this fact Barreswil suggests that persul-

phate of iron may perhaps be applied in calico-printing as a discharge for indigo and also in bleaching blue rags for paper making.—*Répertoire de Chimie Appliquée*, Oct. 1859, p. 429.

[The observation that salts of the sesquioxyd of iron have the power of bleaching indigo and other organic coloring matters was first made by Prof. H. Wurtz of Washington and published two years since in this Journal (vols. xxv, 378, and xxvi, 52)—also in the Proc. of Amer. Assoc. 1858 and in several foreign journals—to him unquestionably belongs whatever credit may attach to the discovery—F. H. S.]

11. *Aluminum Leaf*—A Parisian gold beater, Degousse, has succeeded in obtaining leaves of aluminum as thin as those from gold and silver. The aluminum must be reheated repeatedly over a chafing dish during the process of beating. This leaf is less brilliant than that of silver but it is not so easily tarnished as the latter. It is easily combustible, taking fire when held in the flame of a candle and burning with an exceedingly intense white flame.

According to Fabian, (Dingler's polyt. Journal, cliv, 438,) the chemical lecturer will find aluminum leaf to be well adapted, for exhibiting the characteristic properties of the metal. It dissolves, for example, with surprising rapidity in a solution of caustic alkali.

[A specimen of this leaf accompanies the description of it in *Répertoire de Chimie Appliquée*, Oct. 1859, p. 435, also Nov., p. 488.]

12. *Critical and Experimental Contribution to the Theory of Dyeing.*—Under this title a somewhat extended treatise by Prof. Bolley of Zurich has appeared in the L. E. and D. Philosophical Mag. [4] xviii, 481, Supplement to Dec. 1859.

Two questions have long been agitated among chemists interested in the theory of dyeing. (1.) In what part of the colored fibre is the coloring matter situated? Does it merely adhere to the surface, or does it penetrate the entire substance of the cell-walls of such fibres as cotton and flax? Or lastly, in the case of such fibres is it stored up in the interior of the cells? (2.) What is the nature of the union between the dye and the fibre? Is it a chemical combination, or is it due to mere surface attraction? After comparing the various theories which have been advanced during the last century and discussing the merits of each, the author records the results of his own experiments, from which it appears that wool and silk in all cases where they have not been dyed with colors in a mere state of suspension* seem to be impregnated with the dye throughout their entire mass; while in the case of cotton, by far the larger portion of the coloring matter adheres to the surface of the fibre, the penetration of the cell-walls by the dye being either very slight or altogether wanting.

That the theory of W. Crum (L. E. and D. Phil. Mag., April, 1844,—compare this Journal, [2], xxviii, 125), in accordance with which the tubular form of the cotton fibres is an essential condition to their taking a dye, is unfounded, appears from the fact that the amorphous cotton-gelatine precipitated from its solution in cuprate of ammonia (see this Journal [2], xxvii, 118) may be mordanted and dyed like ordinary cotton. In like

* In which case the coloring matter only adheres as a crust to the surface of the fibre.

manner sulphate of baryta and other pulverulent mineral bodies may be mordanted and dyed with decoctions of dyewoods.

With regard to the nature of the force which binds the coloring matter to the fibre—whether or no it be chemical attraction? Bolley concludes that there is no sufficient reason for accepting the view, principally developed by Chevreul [and by Kuhlmann, Comptes Rendus. Tomes xlii, xliii *et* xliv], that dyeing is a direct consequence of chemical affinity. He believes that the power possessed by fibres of attracting certain bodies—whether salts or coloring matters or both—from their solutions, belongs to that class of phenomena which results from the action of finely divided mineral or organic bodies (charcoal or bone black for example,) on such solutions. The distinction between the action of charcoal and of fibres in thus removing saline matters, or dyes, from their solutions is one of *degree* only, the nature of the operation being identical in either case.

A given weight of well prepared animal charcoal can, as a rule, deprive a larger quantity of liquid of its color than an equal weight of wool or silk. Neither wool or silk can remove *all* the color from a solution as charcoal can, their effect extending only to a certain degree of dilution beyond which the particles of coloring matter resist their attraction. Dyes which may have been taken up without a mordant by wool or, especially, by silk may be removed again by long washing in water, a fact which is not true in the case of charcoal, or only to a very slight extent. The attraction of coloring matters for water is therefore more completely overcome by charcoal than by animal fibre; but even the cleanest vegetable fibres, as, unmordanted and completely bleached cotton, possess a certain power of attracting coloring matter. That cotton should have less effect in this matter than wool or silk is not surprising in view of the great difference in the structure of cotton fibre as compared with that of the two substances last mentioned. It is well known that wool and silk in consequence of their physical constitution belong to the class of strongly absorbent or hygroscopic substances, *i. e.* in consequence of a certain porosity or looseness of their particles they swell up when moist and become easily penetrated by a liquid throughout their entire mass; on the other hand the cell-walls of cotton fibres are denser, less penetrable and at the same time thinner and therefore unable to contain the same quantity of liquid.

It has been often urged that since fibres, especially those of animal origin, not only exert an attraction for salts &c. but also possess the power of decomposing some of them, their action must be chemical But in this respect the behavior of charcoal is similar to that of the fibres. So too with regard to the increased attraction for color exhibited by mordanted cotton which is on a par with the fact observed by Stenhouse that the decolorizing power of wood charcoal is considerably increased by precipitating alumina upon it.

According to the Author mordants act by producing insoluble colors (lakes). Their behavior towards coloring matters in solution must be ascribed to chemical affinity, with which however the fibres themselves have nothing to do.

The so-called substantive dyes become insoluble from some other cause than the addition of a mordant, for example oxydation of protoxyd of iron, or of white indigo.

That common alum with which wool or silk has been impregnated is able to attract coloring matter from solutions and precipitate it on the fibres depends not upon the strength of the chemical affinity of these fibres for the coloring matter, but upon the fact (experimentally proved by Bolley) that they become saturated with the alum which cotton does not.

13. *Cellulose Digested by Sheep.*—The researches of several German chemists* have proved that the cellulose of plants is by no means so indigestible a substance as was at one time supposed, but that on the contrary it is digested in considerable quantities, by the ruminants at least, especially when a portion of the food of the animal consists of some snbstance rich in oil.

In order to ascertain to what extent the digestibility of cellulose may depend upon its state of aggregation, SUSSDORF and A. STŒCKHARDT have undertaken a series of experiments, of which only a very brief abstract can be here given. From their results it is evident that even the most compact kinds of cellulose can be in great measure digested by sheep. The experiments, commenced in July, 1859, were upon two wethers respectively five and six years old. These were fed: 1st, upon hay alone; 2d, upon hay and rye straw; 3d, hay and poplar wood sawdust which had been exhausted with lye; in order that the sheep should eat the sawdust it was found necessary to add to it some rye-bran and a small quantity of salt; 4th, hay and sawdust from pine wood mixed with bran and salt; 5th, hay, spruce sawdust, bran and salt; 6th, hay, paper-maker's pulp from linen rags and bran; after several unsuccessful attempts to induce the sheep to partake of the pulp when mixed with dry fodder it was at last given to them in a sort of paste or pap prepared by mixing bran with water. The experiments were continued until November, with the exception of a short intermission during which the animals were put to pasture in order that they might recover from the injurious effects—probably due to the resinous matters of the spruce wood,—of the fifth series of experiments.

The animals, as well as their food, drink and excrements were weighed every day. The amount of cellulose in the excrements was also daily determined by analysis. The composition of the food ingested having been previously ascertained.

It appeared that when the animals were fed: (1.) with hay (35 lbs. per week), 60 to 70 per cent of the cellulose contained therein was digested, *i. e.* it did not appear as such in the solid excrements. In this experiment the animals gained $7\frac{1}{2}$ lbs. in 18 days. (2.) With hay 14 lbs., and straw 7 lbs. (per week), 40 to 50 per cent of the cellulose of the straw was digested. The animals having lost $2\frac{1}{2}$ lbs. in 11 days. (3.) With hay $10\frac{1}{2}$ lbs., poplar saw-dust $5\frac{1}{4}$ lbs., bran 7 lbs. (per week), 45 to 50 per cent of the cellulose of the poplar wood was digested. The animals having gained $2\frac{1}{2}$ lbs. in 13 days. (4.) With hay $10\frac{1}{2}$ lbs., pine wood saw-dust 7 lbs., bran $10\frac{1}{2}$ lbs. (per week), 30 to 40 per cent of the cellulose of the pine wood was digested. The animals having gained 10 lbs. in 24 days. (5.) With hay $9\frac{1}{3}$ lbs., paper-maker's pulp 7 lbs., bran 14 lbs. (per week),

* For a portion of these interesting results, see: *Agriculturchemische Untersuchungen und deren Ergebnisse angestellt u. gesammelt bei der landwirthschaftlichen Versuchstation in Mœckern.* Leipzig, WIGAND, 1852–57; also *Die landwirthschaftlichen Versuchs-Stationen.* Dresden, WERNER, 1858–59.—F. H. S.

80 per cent of the cellulose of the paper pulp was digested. The animals having gained 7 lbs. in as many days.

These experiments are to be continued, and more particularly with a view of ascertaining whether any nourishing effect is to be attributed to the cellulose.—STŒCKHARDT'S *Chemischer Ackersman*, 1860, No. 1, p. 51.

II. GEOLOGY.

1. *Notes on the Geology of Nebraska and Utah Territory*, (in a letter to one of the Editors from Dr. F. V. HAYDEN, dated Fort Laramie, March 3d, 1860.)—It will be seen by referring to the several memoirs, published in connection with my associate, Mr. Meek, and the second edition of a geological map of Nebraska and Kansas, that the great Lignite Tertiary Basin covers a vast area in the northwest. We find by personal observation that it occupies the greater portion of the country bordering on the upper Missouri, Yellow Stone and Big Horn rivers, that it extends far up into the Wind river valley and west along the North Platte road to the Sweet Water mountains, the Cretaceous rocks being exposed here and there by local upheavals, only except along the base of the mountains.

The lignite beds, which are well developed south of Fort Laramie extending along the base of the Laramie mountains to the Arkansas and southward, furnishing the coal or lignite in the vicinity of Denver City and probably forming a part of the same basin.

I have, in a former paper, suggested that fresh-water deposits near Fort Bridger are probably on a parallel with the estuary beds of Judith river, which at that time were not positively known to be Tertiary. The facts now in my possession show, with a good deal of certainty, that they form the lower portion of the great Lignite Basin. These estuary deposits, which occur in a number of localities in the west and northwest, as along the Grand and Cannon Ball rivers, at the mouth of the Judith on the Missouri, near the mouth of the Big Horn on the Yellow Stone, seem to have ushered in the tertiary epoch of the West, which had already been foreshadowed in Cretaceous formation No. 5,* by the Tertiary character of the Mollusca. We have already, in a former paper, noted the fact that a large portion of the fossils peculiar to the Cretaceous formation No. 5, are closely similar to true Tertiary types and in most of the localities the transition from No. 5 to the estuary beds is scarcely perceptible. On the North Platte, especially at Deer Creek, No. 5, which is very largely developed in this region, is not unfrequently thrust up through the overlying lignite beds, charged with its characteristic fossils. Along the bluff banks of the stream, where the beds are but slightly disturbed, the order of sequence of the strata is so perfect that I would not have been in doubt where to draw the line of separation until we came to the first seam of lignite, and even then I would have considered several beds of the Lignite formation as the upper portion of No. 5 had I not found in these lower lignite beds *Unios* and other fresh-water shells, together with impressions of leaves identical with those occurring so abundantly in the Upper Missouri and Yellow Stone Tertiary strata, and furthermore these beds on the

* The Cretaceous series of Nebraska has been divided into five formations, which for convenience have been numbered from the base in ascending order, 1, 2, 3, &c.

North Platte have now been traced continuously over the intervening country from the mouth of the Yellow Stone river to the Platte. I have ascertained the fact that the lignite beds along the North Platte are a continuation of those on the Upper Missouri, and that they extend in their full developement far up into the Wind river valley and along the Platte road to the Sweet Water mountains. As yet I have seen no indications of lignite in any of the divisions of the Cretaceous period except in formation No. 1 near the Big Sioux river on the Missouri and in a series of sandstones and shales near Fort Benton which we have referred to the same rock. As we proceed south and southwest in this region No. 1 seems to disappear gradually, and along the Laramie mountains I cannot determine its existence at all.

The geographical extension of the great Lignite Basin seems to me to be one of the most interesting questions in the geology of the West at the present time. Very little is known as yet of its limits and from the interesting facts collected by Dr. Engelmann and from other sources it must occupy a large area to the southward and westward from this point, and we already know that it extends far northward into the Hudson's Bay vicinity.

In regard to the White river Tertiary Basin its boundaries have been published with a good degree of accuracy. Its limits north of the Platte river are now well known, and as I have already stated in a former paper, one of the upper members of that basin is revealed along this river, and these, in their southern and southwestern extension, pass by a gradual transition into the Yellow Marl or superficial deposits of the Quaternary period. That the White river Tertiary beds are of later date than those of the Lignite Basin, is clearly shown by the former having been observed resting conformably upon the latter in several localities.

2. *Note on Prof. Newberry's criticisms of Prof. Heer's determination of species of North American Fossil Plants*, in a letter to Prof. Asa Gray, Cambridge.—*Dear Sir:* When I offered for publication in this Journal, the translation of part of a letter from Prof. O. Heer, concerning some fossil plants of the Tertiary, I was far from supposing that any of the statements of my learned friend would not appear satisfactory to every one interested in the study of our American Palæontology. Much less could I foresee that those statements would be construed in a manner that I do not think quite justifiable. As Prof. Heer's letter was published without his knowledge and sanction, I am forced, much to my regret, to defend his position against Dr. Newberry, a personal friend also, and a true and faithful pioneer in the field of our botanical palæontology.

I know nothing about the discussion on the Cretaceous formations and fossils, except what has been published in this Journal. And although last year, during my connection with the State Geol. Survey of Arkansas, I had the opportunity of examining well exposed strata at different stages of the Cretaceous, I was unable to find there any fossil plants, and therefore I have never seen as yet an American Cretaceous plant. Thus I can take my arguments only from the statements of Dr. Newberry himself. It is unnecessary to recall the five points in discussion.

The two first statements are, even from the assent of Dr. Newberry, satisfactorily explained by the insufficiency for exact determination of sketches made from incomplete specimens. Nevertheless, it is but right to remark in favor of the opinion of Dr. Heer that *Populus Leuce* has not the leaves toothed as Dr. Newberry says, but only *subdenticulate*, following Unger's figure and description. Moreover, species of *Populus* bear toothed and entire leaves on the same branches.

The third assertion, about the value of the genus *Ettingshausenia*, concerns only the author of the genus and has nothing to do with the determination of the species. There was no call for a critique upon Prof. Heer on this account.

The fourth statement is the only essential one; it is: "that *excepting the so-called Credneria and Ettingshausenia, all the genera enumerated in Dr. Newberry's letter are Tertiary and not Cretaceous.*"

Nobody will consider as identical the genera *Salicites* and *Salix*, *Alnites* and *Alnus*. Then, from the eleven genera enumerated by Dr. Newberry, two only are found in the list of Hiehle, containing seventeen genera. These two genera are *Populus* and *Acer*, the true characteristic genera of the tertiary of Europe. The first is represented already in that formation by thirty-three species; the second by forty-five or more, and except the species admitted by Hiehle, not a single one has been found in the Cretaceous. From the nine other genera of Dr. Newberry which are not mentioned as Cretaceous by Hiehle, not one has been found by anybody else in that formation. Hence Prof. Heer was right to say that, except the two which he named, all the genera enumerated in the letter of Dr. Newberry were of the Tertiary and not of the Cretaceous. Every botanist without exception would have come to the same conclusion.

Therefore I cannot understand how the accusation of ignorance or of partiality could be brought against the celebrated Professor of Zurich. How it could be thought that a naturalist who has spent his life, without regard to personal interest, in the constant pursuit of his favorite science (the Botanical Palæontology of all the formations), could be supposed to ignore not only the fossil flora of the Cretaceous, but what Hiehle has published in the Palæontographia. This work is in every library and Prof. Heer has quoted it all along in the three volumes of his admirable Fossil-flora of the Tertiary! And how a man of such high moral standing as Prof. Heer, respected everywhere for his faithfulness and devotion to science, could be accused of giving his opinion or rates of determining the plants from a *judgment biased by erroneous oral testimony*, is still more inconceivable to me.

In the letter of Prof. Heer, there is not a word which could be construed as censurable or offensive to anybody. When the most learned Botanists take again and again species and genera for re-examination, new determinations, for a constant changing of names, of relations, of affinity, even with specimens of living plants; when such Palæontologists as Heer, Corda, Unger, Göppert, Braun, &c., who have on hand the largest libraries, numerous specimens, and every facility for comparing them are forced every day, either by new discoveries or by a more careful study, to acknowledge mistakes and to change their nomenclature; when the same fossil leaf is for different authors a *Populus*, a *Salix*, a *Laurus*, a *Ficus*, a *Quercus!* it cannot be expected that in America, where the science of Botanical Palæontology is scarcely born, where the age of the strata from which the fossil plants are taken is mostly uncertain, where we have no possibility of comparing specimens, and not a single library where we can find all that has been published on palæontology, we can come at once by some kind of divination to the correct determination of a fossil species. And most commonly the examination has to be made on pieces of broken specimens of leaves, of which the general outline and the details of nervation are obliterated. We have thus to begin and to break the way, and the only means of doing it with advantage to our successors is to publish our fossil species as fast as we can get them, figuring them carefully and determining the species as well as we can without caring for any foreign opinion.

About the fifth statement of Prof. Heer, I can not admit, as Dr. Newberry appears to do, that the fossil flora of the American Cretaceous ought to be closely related to the European. The Tertiary of North America, of which numerous specimens have recently been collected in Mississippi, Tennessee and Kentucky, has some of the genera of Europe, *Sabal*, *Phœnicetes*, *Laurus*, &c., represented by peculiar species different from those of Europe, with a large number of *Terminalia*, *Magnolia*, two genera scarcely represented in the European tertiary. But this question cannot be examined now.

Contrary to the supposition of Dr. Newberry, it is certain that the fossil plants obtained from three different stages of the Tertiary in Mississippi, Tennessee and Kentucky, indicate a warmer climate than that of the same latitude at our time. And it is certain also that already two marine fossiliferous beds (very rich indeed) have been discovered, the one near Cairo in Illinois, by Prof. A. H. Northen; the other near Oxford, by Prof. Hilgard, and perhaps a third one in Tennessee by Prof. Safford,—three State Geologists of the three different States. I have spent some time last fall in the examination of the formations; and besides the specimens of fossil leaves which I have collected, I have now for examination the rich collection of the Mississippi University of Oxford, and the private collections of fossil plants of both Prof. Hilgard and Prof. Safford. It is from these materials partly examined that I have taken the above conclusions.

Respectfully Yours, LÉO LESQUEREUX.

Columbus, Ohio, April 3, 1860.

III. BOTANY AND ZOOLOGY.

1. *Florula Ajanensis*, by REGEL and TILING. Moscow, pp. 128, 4to, 1858. (Extr. Trans. Imp. Soc. Mosc.)

Primitæ Flora Amurensis, by C. J. MAXIMOWIEZ. St. Petersburg, 1859. (Extr. Mem. present. Acad. Im. Sci. St. Petersb.), pp. 504, tab. 10, 4to.—The first-named work, with the *Florula Ochotensis* of Trautvetter and Meyer in Middendorf's Journey, gives an account of the botany of the confines of Northeastern Siberia bordering on the Ochotsk Sea. The latter, a bulky volume, contains the first fruits of the botanical explorations consequent upon the colonization of the lower part of the Amoor River by the Russians. These districts nearly abut upon the northern part of Japan, and therefore possess for us a peculiar botanical interest. They share notably, though not as largely as Japan, in those North American types which present so curious a problem in geographical distribution. Having been supplied with an almost complete suite of the specimens upon which these works are based, through the generosity of the directors of the Imperial Botanic Garden and of the Museum of the Imperial Academy of Sciences of St. Petersburg, we wish to collate the various representative forms both with our Japanese materials and with their American relatives before we offer any definite remarks. Suffice it now to say, that the Amoor flora offers several additional species identical with peculiarly Eastern North American ones: e.g. *Acer spicatum, Pilea pumila, Asplenium thelypteroides,* and *Symplocarpus fœtidus!* Also several closely representative ones; such as *Caulophyllum robustum,* doubtless the same as the Japan plant, which in fruit answers perfectly to our *C. thalictroides,* and I still suspect not distinct from it; and *Maximowiczia Chinensis,* Rupr. (to which evidently belongs *Sphærostema Japonicum,* Gray), a close counterpart of our *Schizandra; Acer tegmentosum,* very nearly our *A. Pennsylvanicum; Hylomecon vernalis* which seems very close to our *Stylopnorum diphyllum;* and *Plagiorhegma dubium,* which has the look of a monstrous dicarpellary *Jeffersonia.* Indeed, good flowers are still wanting to make it at all certain it is not a *Jeffersonia!*

Very remarkable indeed is this division of monotypic or nearly monotypic genera or groups between Northeastern Asia and Northeastern America,—of which so extended a list can now be given,—and very suggestive is it (at least where the species are identical or nearly so) of a comparatively recent communication between the two countries. A. G.

2. *Harvey's Thesaurus Capensis,* No 2, has come to hand. The plates (26–50) are better printed and do more justice to Dr. Harvey's facile pencil than those of the first part. Among the illustrations is a new *Crotalaria,* "a sort of first fruits of the botany of Lake Ngami;" an interesting new Bixaceous genus (*Rawsonia*), with petaloid scales "evidently homologous with the crown of *Passiflora,* and with the inner stamens hypogynous, the outer perigynous"; also a fine new Scuphularineous genus, *Bowkeria.*—No. 3, just received, continues the work to plate 75. One of the plates illustrates a new genus of the Passion-flower family, with blossoms no larger than chickweed. A. G.

3. *Hooker's species Filicum, being Descriptions of all known Ferns,* illustrated with Plates. Part ix, or vol. iii, part 1. pp. 64, tab. 141–170.—This portion of this important and standard work contains the genera *Lomaria* and *Blechnum,* and excellent figures of 23 species. The most interesting Fern to us, here illustrated, is the *Blechnum doodiodes* of Hooker, known only from two specimens gathered by Douglas somewhere in Northwestern America, "probably up Frazer's River": the question is whether the plant may not prove to be a remarkable variation of *Lomaria Spicant,* which inhabits that region, and is the only Lomarioid Fern of so northern a latitude. A. G.

Part x, or vol. iii, part 2 of this important work has just come to hand. It comprises the first part of the very large and very difficult genus *Asplenium,* the reduction of which to order will be a great boon to botanists.

4. *Journal of the Proceedings of the Linnæan Society,* No. 14: Botany.—This part concludes Prof. Andersson's account of the *Salices* of the East Indies, 34 species, of which many are new. Mr. Spruce has a short article on the Piassaba Palm (*Leopoldinia Piassaba*) of the Rio Negro of the Am-

azon, the long fibrous beard of whose petioles (vestiges of the membrane which envelopes the frond in vernation) is so important for cordage, &c. On young palms this pendant beard is four feet or more in length, and reaches to the ground. Besides this useful fibre, the pulp of the ripe fruit of this palm is said to yield the most delicious of all palm-drinks, having great resemblance to cream both in color and taste. The remaining article is one by Mr. Mitten, upon various new *Musci* of New Zealand, Tasmania, and various parts of the Southern hemisphere.

No. 15, contains Observations on the Growth and time of Appearance of some of the Marine *Algæ*, by Dr. Cocks; and another of the Præcursores ad Floram Indicam, by Drs. Hooker and Thomson. The present paper is devoted to the *Balsamineæ*, amounting to about one hundred species, notwithstanding a large reduction. The great mass of the order belongs to India; the extra-Indian Balsams known being about eight in more oriental parts of Asia and Maylasia; about as many more in Africa and Madagascar, three in Europe and Siberia, and two in North America. The Indian species are "so universally and excessively prone to vary" that they have given their monographer immense trouble and small satisfaction. We may remark that our two American species and *I. Nolitangere* of the Old World seem to run together; at least, intermediate forms occasionally appear which strongly suggest a common origin. A. G.

No. 16. contains a paper by Mr. Babington on the forms or species compared with *Fumaria capreolata*, L.; another on a new Butter-tree of Southeastern Africa by T. Caruel of Florence,—the tree which Bertoloni supposed to be the shea-tree mentioned by Mungo Park, and which he described as the type not only of a new genus but of a new order. Mr. Caruel shows that the present tree is a *Combretum*, and doubtless not the shea-tree, the only grounds for considering them the same being that both belong to Africa, and both produce a kind of vegetable butter. Mr. Oliver, a young English botanist of promise, describes some new South American species of *Utricularia* (two of which are figured), with notes on *Polypompholyx* and *Akentra*, showing that Benjamin established the latter in a mistaken observation, and that the plant is an *Utricularia*. Mr. Spruce gives a very interesting account of a visit to the *Cinchona* forests of the Quitenian Andes. The *Cascarilla roja* was said to have a milky juice, a remarkable circumstance for a Rubiaceous plant. It appears that the juice as it flows from a wound is colorless, but that it turns white the instant it is exposed to the air, and in a few minutes changes to red. Mr. Moore announces the discovery in England of *Lastrea remota* (*Apidium remotum*, Braun), a form which has been confounded with *A. rigidum* and which appears to be as intermediate between *A. spinulosum* and *A. Filix-mas*, as *A. rigidum* is between the former and *A. cristatum*. Mr. Oliver continues an account of the British Herbarium now forming for the Linnæan Society; and Mr. Hogg, a note on the *Rosa rubella* of Winch., giving evidence that it is a mere variety of *R. spinosissima*. A. G.

5. *Martius, Flora Brasiliensis.*—This great work is conducted with such spirit,—now that the distinguished editor is able to devote his whole attention to it,—that three fasciculi reach us at the same moment, viz.:

Fasc. 18, part 3, is a supplement to the *Myrtaceæ* of Brazil, by Berg, of Berlin. This young botanist is thought to have elaborated the American forms of this great order in an able manner. Of the many new species and varieties described in this supplement, the greater part are from the Brazilian collection of Riedel, belonging to the Imperial Botanic Garden of St. Petersburg, and furnished to Dr. Berg by Dr. Regel, the accomplished curator of that establishment. To this is appended a tabular view of the geographical distribution of all the known American *Myrtaceæ*, now amounting to 1726 species, of which 1008 are nearly described by Berg (in the Fl. Brazil., and in the Linnæa), and 696 are indigenous to the Brazilian empire. The uses which the plants of the order are known to subserve in Brazil are likewise enumerated. Of four species the root, or its bark, is used medicinally: of three species the fibrous bark is turned to account, among which, from that of a *Couratari* the natives of the Uapes make clothing; the timber of *Couratari legalis*,—one of the largest trees of tropical Brazil—is so highly prized for a variety of purposes that its wasteful destruction is forbidden by law, whence the name *paos de ley*, i. e. *ligna legalia:* the leaves of several species are used medicinally for their aromatic properties combined with astringency: the flower-buds of a *Calyptranthes* are used as a substitute for the true Clove, which also is cultivated successfully in Brazil: the berries of no less than 55 species are enumerated as edible, or some of them medicinal; the most important being the *Guayavas;* five species of *Lecythis* and the famous *Bertholletia excelsa* furnish amygdaloid seeds of great richness and pleasantness (Brazil-nuts, &c.). Recent extrabrazilan genera are briefly noted. *Luma* (Bot. U. S. Expl. Exped.) is altogether overlooked, and in strictness may claim restoration, when the genera come to be revised and considerably reduced, as they probably will be. The three parts of fasc. 18, composing the *Myrtographia Brasiliensis* compose an entire volume, of 655 pages, with many plates.

Fasc. 23, is a continuation of the first fascicle (*Musci* and *Lycopodiaceæ*), and comprises the *Ophioglosseæ*, *Marattiaceæ*, *Osmundaceæ*, *Schizæaceæ*, *Gleicheniaceæ*, and *Hymenophylleæ*, by Dr. Sturm, of Nuremberg, with ten plates. One of these is a fine illustration of the rare and curious *Ophioglossum palmatum*,—a species which, by the way, Mr. Wright has lately collected abundantly in the eastern part of Cuba. Under the name of *Osmunda palustris*, Schrad., our North American *O. spectabilis*, a form of the European *O. regalis*, is figured: it appears to be rather common in the southern part of Brazil. It is interesting to remark that both this and two other more specially North American species, viz. *O. cinnamomea* and *Botrychium Virginicum*, occur on the one hand in Brazil, on the other in the Himalayas. The illustrations of the *Hymenophylleæ* are nature-printed, by the Vienna process. This does better for the fronds—so delicate in this tribe—than for the fructification.

Fasc. 24, of 215 pages, with 56 plates, contains the first part of Mr. Bentham's elaboration of the Brazilian *Leguminosæ*, including all the *Papilionaceæ* except the *Dalbergieæ* and the *Sophoreæ*. A double plate, filled with details, illustrates the structure of *Arachis*, the generic character of which, as well as of its allies, is remodelled, conformably to the observations of Dr. Niesler, as recorded in this Journal, several years ago. We perceive that the North American species referred to *Chæto-*

calyx (the fruit of the original species being unknown), belong to the allied and older genus *Nissolia*. *Chætocalyx Wislizeni*, Gray, is the same as *Nissolia platycarpa*, Benth. *C. Schottii* must take the name of *Nissolia Schotti*. A. G.

6. *J. D. Hooker's Flora Tasmania*,—the third and concluding part of the *Botany of the Antarctic Voyage* under Capt. Ross,—is now finished in parts 10 and 11, issued by the enterprising publisher, Lovell Reeve, in February last. The work makes two large quarto volumes, with 200 colored plates. Part 10 concludes the *Algæ* (by Dr. Harvey) and contains the *Lichenes* (the foliaceous ones by Mr. Babington, the crustaceous by Mr. Mitten), also a series of additions and corrections to the earlier part of the work. The letter-press of part 11 is entirely occupied, excepting the key to the genera, by the *Introductory Essay* upon the Botany of Tasmania and of Australia in general, of 128 pages, which admirable production may claim to be regarded as the most profound and far-reaching discussion of the abstruse theoretical questions bearing upon the origin and distribution of species which has ever been attempted. Although the literary composition bears some marks of haste, the subject-matter has been elaborated with great care, and in a manner at once bold, independent, and conscientious, opening new views and propounding new problems of the widest interest and, we may add, of the utmost difficulty. The more generally interesting portions of this essay are reproduced in the present volume of this Journal. A. G.

7. *Poison of Plants by Arsenic.*—It may be recollected that Professor Davy of Dublin last year reported to the Gardener's Chronicle (whence extracts were transferred to our pages*) the results of his experiments which went to show that some plants might with impunity be watered even with a saturated aqueous solution of arsenious acid; that the plants took up this arsenic and accumulated it in their tissues, to such an extent that traces of this metal were discoverable in the bodies of animals fed upon vegetables so treated. These astonishing results naturally excited enquiry. They have now been contradicted in a late number of the Pharmaceutical Journal (as we learn from the Gardener's Chronicle for March 10) by Mr. Ogston, an analytical and agricultural chemist, formerly a pupil of Prof. Graham. Mr. Ogston finds that, on watering the ground around the roots of some vigorous Cabbage plants, some months old, with a saturated solution of arsenious acid, in every trial, after two doses at intervals of three days, the plants died within the week. The same occurred with Scotch Kale, the only other plant subjected to the experiment. On testing the dead plants arsenic was detected only in the portion of the stem close to the roots, and which showed in its darkened color the marks of disease. In no case was any of the poison found in the leaves, or in the stem at more than five inches above the ground. Prof. Davy also startled the English agriculturists and medical jurists by calling attention to the fact that arsenic exists in the commercial superphosphate of lime, at least in certain kinds, coming from the iron pyrites used in the manufacture of the sulphuric acid employed in the production of the superphosphate, which arsenic, if plants may accumulate it in their tissues, would be conveyed

* Vol. xxviii, p. 443, 1859.

to the flesh of animals fed with turnips manured with such superphosphate, and so conveyed to the human system,—if not in quantity sufficient to poison, yet enough to account for the presence of arsenic in cases of death from supposed poisoning. Mr. Ogston now considers the question as to how much arsenic an agricultural crop (say of turnips) can obtain from an ordinary dressing of the superphosphate so prepared. "Take a very bad sample of pyrites to contain ·30 per cent of arsenic, and consider, as is the case, that in the manufacture of oil of vitriol, one-half of this is stopped by condensation in the flues; ·15 per cent will remain in relation to the pyrites, or about ·10 in relation to the manufactured oil of vitriol. Now suppose the superphosphate made from this acid to contain 20 per cent of it as a constituent, and that 3 cwt. are used as a dressing per acre, there will be added to this acre ·07 of a pound of arsenic, and this is to be distributed among from 20 to 25 tons of roots, giving a percentage infinitely small, and in my opinion relieving us from the necessity of the smallest anxiety on the subject. If, however, even this quantity is objectionable, the use of the Belgian pyrites, in which I have never found a trace of arsenic, would obviate all difficulty." A. G.

8. *Botanical Necrology for the year* 1859.—The following are the principal names upon this obituary record:—Some of them we have named before more briefly.

C. A. Agardh, Professor of Botany in the University of Lund, Sweden, from 1812 to 1834, when he became bishop of Carlstad; a voluminous writer upon botanical and other subjects, especially upon *Algæ*, and a distinguished and remarkable man. His earliest publication, a thesis upon the *Carices* of Scania, was published in the year 1806. He died on the 28th of January, 1859 at the age of 75 years. He was succeeded in his professorship by his son C. A. Agardh, the distinguished algologist.

Arthur Henfrey, Professor of Botany in King's College, London. The death, after a short illness, on the 7th of September last, of this amiable man and excellent vegetable anatomist, at the early age of 39 years, has has already been recorded in this Journal, (vol. 28, p. 443). In his field of research and in his knowledge of the literature of the subject, especially that of the Germans, he had no rival in Great Britain, and his death is deeply felt.

Dr. Thomas Horsfield, born in Pennsylvania, after completing his medical studies in Philadelphia he passed sixteen years in Java and the adjacent islands, in the service of the government, devoting much of his time to botanical and zoological researches; and the long remainder in a responsible position at the India House, in London. A selection only of his botanical collections was published by Mr. Brown and Mr. Bennett, under the title of *Plantæ Javanicæ Rariores*, etc.—a most important work. Dr. Horsfield died, on the 14th of July last, in the 86th year of his age.

A. L. S. Lejeune, a venerable Belgian botanist, died at Verviers, at the very close of the year 1858, in the 80th year of his age.

Thomas Nuttall, born at Settle, in the West Riding of Yorkshire, in the year 1784, may yet be reckoned as one of our own American botanists, since he came to the United States when only 22 years of age, and made here his whole scientific career. He returned indeed to his native land in the year 1842, and took possession of a handsome estate bequeathed by his uncle, where he indulged his fondness for horticulture; but his only botanical publication in these later years was made at Philadelphia, and elaborated during a visit to this country in 1852. His writings are intimately connected with the history of North American Botany, and his personal biography is very interesting. A full account of these is given by Mr. Durand, in an excellent address delivered to the Philadelphia Academy of Natural Sciences, and in an article by Mr. Meehan in his Gardener's Monthly for January, 1860. A critical estimate of Mr. Nuttall's contributions to science must be deferred to another occasion. He died at his residence, Nutgrove, near Preston, Lancashire, on the 10th of September last, aged 75 years. A. G.

ZOOLOGICAL NOTICES.—

1. *A trip to Beaufort, N. Carolina;* by WM. STIMPSON, M.D.—The vicinity of Cape Hatteras, the most projecting point of our coast south of New York, has a peculiar interest for the student of zoology. This cape, which divides the Areniferous region* into two nearly equal parts, the Virginian and Carolinian provinces, is remarkable for the exhibition of a fauna more tropical in character than that of either of these provinces, as will be shown below. This is an evident result of its proximity to the Gulf Stream, the warm waters of which are even said to be deflected directly upon the Cape after violent southeast gales.

Beaufort, N. C., which lies several miles WSW from the Cape, in latitude 35° N. is the only convenient point of departure for explorations in the waters of the vicinity. Some account of the zoological richness of this locality was kindly communicated to me by Capt. J. D. Kurtz, U.S.A., and influenced by a desire of completing a catalogue of the shells of our Atlantic shores, and particularly of procuring and examining a species of *Lingula* said to be found on the southern coast, I undertook its exploration in the month of March last, in company with my friend Mr. Theodore Gill. The harbor of Beaufort is situated at one of the southern outlets of Pamlico Sound, where it joins Bogue Sound. It is shallow and much obstructed by extensive shoals. Centre-board boats only can be used, except in the deeper channels, which are mostly narrow. The bottom is generally sandy, but that of the deep channels is shelly, and that of the shallower channels often muddy. Outside the harbor and off the coast, the depth never exceeds eight fathoms within a few miles of the land, with a variable bottom, sometimes "sticky" or clayey. These bottoms were all pretty thoroughly raked with the dredges. The beaches were also examined for those bivalve shells which perforate the sand to a

* The eastern coast of the United States may be conveniently divided into three regions, viz.—the Rupiferous, Areniferous, and Coralliferous, named from the character of the shores. The Rupiferous or rocky region extends from our northern limit to Cape Cod, or Long Island. The Areniferous or sandy region,—in which there are no rocks whatever and scarce even a pebble except where human agency has been at work—reaches from Long Island to North Florida. The Coralliferous region is characterised by the presence of reef-coral, and includes the peninsular of Florida. Each of these regions has its peculiar fauna, the distinctness of which, enhanced by the two great causes of difference of latitude and diversity of ground, is too well known to require further comment here.

depth beyond the reach of the dredge and are only exposed by the eroding action of the breakers during heavy storms. In following these beaches we observed a decided increase of the tropical character in the shells as we proceeded eastward toward the Cape.

In order to show the character of the fauna and the results of our explorations I have given below a catalogue of the Mollusca and higher Crustacea which occurred during our short stay, which seems to be the most concise and satisfactory method. A few prefatory remarks of a general character will not be out of place. Geologists will be interested to notice the occrrence of several species hitherto known only as Tertiary fossils, such as species of *Axinaea*, *Lucina*, *Astarte*, *Amphidesma*, *Tellidora*, *Myalina*, *Panopaea*, *Entalis*, and *Columbella*. These were found either alive or in such condition as showed them to be recent shells, which would doubtless have been found alive upon further search. The occurrence of *Myalina subovata* is interesting, although our specimens of this species, as of *Amphimesma constricta*, are not certainly recent, being only single valves. Of the beautiful *Tellidora lunulata* we obtained several living examples, some attaining a length of nearly two inches. Among the shells of a tropical character several species will be noticed which have not hitherto been found north of the West Indies, and do not exist upon the South Carolina coast. The large *Cassis* to which we have applied the name *C. cameo* is identical with the common cameo-shell of the Bahamas, which usually figures in collections under the name of *C. madagascarensis*. Of this we obtained several fine specimens on the beaches, none living however.

In the following catalogues all species of which the identifications is at all doubtful are indicated by the mark of interrogation.

Mollusca.

Cynthia vittata.
Molgula sp.
Ascidia sp.
Lingula pyramidata.
Anomia ephippium.
Ostrea virginiana.
" equestris.
Plicatula depressa.
" sp.
Lima scabra ?
" sp.
Pecten dislocatus.
" concentricus.
" nodosus.
Axinæa charlestonensis.
" sp.
Arca Holmesii.
" americana.
" cælata.
" lienosa.
" noæ ?
" ponderosa.
" incongrua.
Nucula proxima.
Yoldia limatula.
Leda acuta.
Pinna squamosissima.
" carolinensis.
Avicula atlantica.
Modiolaria lateralis.
Modiola plicatula.
" americana.
" tulipa ?
Mytilus edulis.
" cubitus.
Chama arcinella.
" macrophylla.
Cardium magnum.
" isocardia.
" muricatum.
Liocardium serratum.
" Mortoni.
Lucina crenulata.
" cribraria ?
" edentula.
" strigilla.
Felania sp.
Diplodonta ? punctata.
Lepton lepidum.
Astarte lunulata.
" undulata ?
Cardita tridentata.
Mercenaria violacea.
" Mortoni.
" notata.
Trigona sp.
Venus rugosa ?
Chione cancellata.
" pygmæa ?
Callista gigantea.
" maculata.
Dosinia discus.
Lucinopsis sp.
" n. sp.
Petricola pholadiformis.
Raëta canaliculata.
" lineata.
Mactra oblonga.
" similis.
" lateralis.
Donax variabilis.
Cumingia tellinoides.
Semele orbiculata.
" reticulata ?
Amphidesma constricta.
Abra æqualis.
Tellina alternata.
" fausta ?

Tellina polita.
" tenera.
" versicolor?
" tenta.
" iris.
" constricta.
" sp.
" n. sp.
Strigilla carnaria.
" flexuosa.
Tellidora lunulata.
Solen ensis?
" viridis.
Siliquaria gibba.
" bidens.
Solenomya velum.
Mya arenaria.
Corbula contracta.
Myalina subovata, *Con.*
Panopæa americana.
Saxicava distorta.
Gastrochæna sp.
Lyonsia hyalina.
" sp.
Pandora trilineata.
Pholas costata.
" truncata.
Pholadidea cuneiformis.
Teredo sp.
Polycera sp.
Actinodoris? sp.
Utriculus canaliculatus.
Bulla solitaria.
Tornutella puncto-striata.
Chiton apiculatus.
Entalis pliocena.
Dentalium sp.
Crepidula unguiformis.
" fornicata.
" convexa.
Clypidella pustula.
Fissurella alternata.
Zizyphinus sp.
Turbo crenulatus.
Littorina irrorata.
Scalaria Humphreysii.
" turbinata.
" lineata.
" novangliæ?
" multistriata.
Solarium granulatum.
Vermetus radicula.
Cerithium sp.
Bittium nigrum?
" Greenii.
" sp.
Triforis nigrocinctus.
Odostomia seminuda.
" impressa.
Turbonilla interrupta.
" sp.
" sp.
Obeliscus crenulatus.
Eulima oleacea.
Catinus perspectivus.
Natica pusilla.
Neverita duplicata.
Volva uniplicata.
Pleurotoma cerina.
Mangelia rubella.
" filiformis, *Holmes.*
Oliva litterata.
Olivella mutica.
Columbella avara.
" ornata.
" lunata.
" n. sp.
Dolium galea.
Semicassis granulosa.
Cassis cameo, *Stm.*
Purpura floridana.
Murex spinicostatus.
Nassa obsoleta.
" trivittata.
" vibex.
" ambigua?
Cerithiopsis terebralis.
" ? n. sp.
Acus dislocatus.
" concavus.
Busycon pyrum.
" canaliculatum.
" carica.
" perversum.
Cancellaria reticulata.
Fasciolaria gigantea.
" tulipa.
" distans.
Ranella caudata.
Strombus alatus.

Crustacea Decapoda.

Libinia canaliculata.
Pelia mutica.
Leptopodia calcarata.
Cryptopodia granulata.
Cancer irroratus.
Menippe mercenaria.
Panopeus Herbstii.
Pilumnus aculeatus.
Platyonichus ocellatus.
Lupa hastata.
" Gibbesii.
" spinimana.
Gelasimus pugilator.
Ocypoda arenaria.
Pinnotheres ostreum.
" maculatus.
Pinnixa cylindrica.
" sayana.
" chætopterana.
Persephona punctata.
Lithadia cariosa.
Hepatus decorus.
Calappa marmorata.
Porcellana ocellata.
" sociata.
Euceramus prælongus.
Hippa talpoida.
Lepidops scutellata.
Eupagurus pollicaris.
" longicarpus.
" annulipes.
Callianassa major.
Crangon septemspinosus.
Alpheus intermedius.
Virbius pleuracanthus.
Palæmonopsis carolinus.
Penæus brasiliensis.
" constrictus.

In the collection made at Beaufort and now deposited in the Smithsonian Institution a considerable number of new genera and species occur. We add descriptions of two of the most interesting.

Lingula pyramidata. Shell greenish-white, elongated-ovate, convex, regularly tapering from the middle to the summit with an outline very slightly convex; also a little tapering toward the extremity, which is less than two-thirds as wide as the middle, and subtruncate with broadly rounded corners. Surface smooth and glossy; lines of increment inconspicuous, but sufficiently distinct near the margins; two or three of them however at irregular intervals sometimes projecting more strongly, indicating epochs in the growth of the shell. Marginal setæ of the mantle well developed, those on either side at the extremity longer than the rest, equalling in length one-third the width of the shell. There are two black spots on the margin of the mantle at the extremity. Peduncle in life three times as long as the shell, thick, (one-third width of the shell,) at its point of attachment, but rapidly tapering and becoming very slender and hyaline, with an opaque axis or central cord; extremity glutinous and covered with adhering sand. Length of the animal 3·5; length of shell, 0·92; width of shell at the middle 0·35; at extremity, 0·21; half way between middle and summit, 0·26 inch.

This animal was found imbedded in the weedy sand at low water mark, on the occasion of one of the extraordinarily low tides which occur at the equinox. It lives in a perpendicular position with its peduncle deeply penetrating the sand, and its shell scarcely projecting above the surface at its extremity. When drawn out and placed in a vessel of sea-water it showed its uneasiness by snake-like gyrations of the peduncle, which, far from being a simple stem for attachment, is a powerful muscular organ, filling the function of the foot of the Lamellibranchiata. Mr. Peale informs me that this part in the *Lingula anatina* forms a favorite article of food among the Fiji Islanders. Our Lingula appears to be not uncommon near Beaufort, as several specimens were found during a single retreat of the tide. It is interesting as being the first species* of this most ancient genus described from the Atlantic ocean. The other recent species, ten in number, are all inhabitants of the Pacific.

Euceramus, nov. gen. fam. *Porcellanidae.* Body subcylindrical. Carapax elongate-subrectangular, twice as long as broad; sides parallel. Front prominent, tridentate. Eyes minute, longitudinal, projecting a little beyond the orbits which are very incomplete, consisting only of the concave superior margin. Antennulæ placed immediately beneath the eyes; peduncle anteriorly bidentate. Antennæ large; mobile part nearly as long as the carapax and arising from the inner or superior side of the small coxal joint, thus being in contact with the eye at base. The outer maxillipeds are of the form usual in Porcellana, but the sternal piece to which they are attached is very large, nearly as long as broad, triangular in front, and truncate behind. Chelipeds small, subcylindrical, much shorter than the carapax; hands weak. Ambulatory feet subcylindrical; dactyli curved, setose, nearly as long as penult joint. Abdomen narrow, particularly in the males; appendages as in Porcellana.

This aberrant type should be referred to the Porcellanidea, notwithstanding its greatly elongated form, which would lead one to refer it at first sight to the Hippidæ or Raninidea.

Euceramus prælongus. Carapax regularly curved like a segment of a cylinder, above glabrous, and minutely striated transversely; striæ curved forward at the sides. Inter-orbital front one-third the width of the carapax, tridendate, teeth slender, pointed, median longest. Hands externally scabrous, or small-tuberculose and setose; fingers as long as the palm and not gaping. Ambulatory feet of the second and third pairs as long as the chelipeds; those of the first pair smaller. Length about three-fourths of an inch. Dredged on shelly ground in 4 to 8 fathoms.

* We understand that there is a specimen of Lingula from the coast of South Carolina in possession of Prof. Agassiz. Not having access to this specimen, we are unable to say whether it be identical with ours or not.

IV. METEOROLOGY AND ASTRONOMY.

1. *Abstract of Meteorological Observations, made during the year* 1859 *—with the average of seven years—at Sacramento, Cal.*, lat. 38° 34′ 41″, long. 121° 29′ 44″; by THOMAS M. LOGAN, M.D.

1859.	January.	Feb'y.	March.	April.	May.	June.	July.	August.	Sept.	Oct.	Nov.	Dec.	Mean.	7 years average.
Barometer.	In.	In.	In.	In.	In.	In.	In.	In.	In.	In.	In.	In.	In.	In.
Maxima,	30·491	30·332	30·385	30·371	30·171	30·211	30·066	30·080	30·126	30·270	30·326	30·368	30·266	30·243
Minima,	29·438	29·626	29·771	29·698	29·744	29·605	29·747	29·731	29·725	29·694	29·791	29·788	29·697	29·706
Mean,	30·211	30·026	30·151	30·004	29 976	29·836	29·917	29·892	29·927	29·943	30·040	30·167	30·008	30·004
Thermometer.	°	°	°	°	°	°	°	°	°	°	°	°	°	°
Maxima,	57·00	62·00	64·00	76·00	80·00	96·00	87·00	85·00	82·00	83·00	68·00	53·00	74·42	75·80
Minima,	34·00	39·00	40·00	40·00	53·00	61·00	60·00	58·00	56·00	49·00	42·00	34·00	47·17	46·70
Mean,	44·87	50·49	51·47	57·11	63·03	74·85	69·07	67·16	65·89	63·28	54·05	43·52	58·73	59·36
Thermometrograph.	°	°	°	°	°	°	°	°	°	°	°	°	°	°
Maxima,	58·00	63·00	66·00	78·00	84·00	102·00	92·00	89·00	85·00	87·00	70·00	54·00	77·33	77·99
Minima,	30·00	34·00	36·00	36·00	45·00	51·00	51·00	52·00	50·00	43·00	37·00	28·00	41·08	41·46
Range,	28·00	29·00	30·00	42·00	39·00	51·00	41·00	37·00	35·00	44·00	33·00	26·00	36·25	36·53
Force of Vapor.	In.	In.	In.	In.	In.	In.	In.	In.	In.	In.	In.	In.	In.	In.
Maxima,	·362	·436	·410	·524	·522	·942	·677	·564	·581	·524	·529	·334	·534	·529
Minima,	·120	·173	·078	·133	·136	·296	·338	·350	·226	·186	·189	·087	·193	·197
Mean,	·231	·289	·257	·321	·363	·503	·469	·451	·410	·367	·347	·230	·353	·357
Relative Humidity.	Per ct.	Per ct.	Per ct.	Per ct.	Per ct.	Per ct.	Per ct.	Per ct.	Per ct.	Per ct.	Per ct.	Per ct.	Per ct.	Per ct.
Maxima,	93·00	94·00	92·00	87·00	82·00	82·00	77·00	82·00	84·00	85·00	94·00	92·00	87·00	87·81
Minima,	32·00	60·00	19·00	30·00	19·00	18·00	36·00	29·00	25·00	32·00	51·00	30·00	31·75	31·26
Mean,	78·05	78·73	68·32	68·90	64·81	59·51	66·95	68·68	65·61	64·41	82·32	80·30	70·55	69·95
Number of clear days,	7	3⅓	11	10⅓	14⅔	20⅔	22⅔	25⅔	16⅓	16⅓	4	10⅓	162⅓	176 7–15
No. cloudy and foggy d'ys	17	8⅔	6	14⅔	12⅓	9⅓	7⅓	5⅓	11⅔	14⅔	10	15⅔	132⅔	126 1–15
No. rainy days,	7	16	14	5	4	0	1	....	2	....	16	5	70	63 1–15
Quantity of clouds,	6·3	5·5	3·7	2·9	2·2	0·5	0·7	0·4	1·4	1·3	5·7	4·1	2·9	3·3
Quantity of rain and fog,	0·964	3·906	1·637	0·981	1·037	0·000	0·030	0·000	0·025	0·000	6·485	1·834	16·899	16·927
1st dys & 2d force N. wind	11⅓ 2·3	4⅓ 1·8	8⅔ 3·2	5⅓ 2·5	6 2·9	3 1·0	1 0·6	1⅔ 0·6	7⅓ 2·2	10⅔ 1·4	7⅓ 1·4	14⅔ 1·4	81⅔ 1·7	
" " " N.E. "	6 2·0	1 1·7	1 1·2	2⅓ 2·0	1⅓ 1·1	1⅓ 0·4	0 0·0	⅓ 0·3	0 0·0	2 1·2	1⅓ 1·2	5 1·7	21⅓ 1·1	
" " " E. "	1⅓ 1·3	⅔ 1·0	1 0·8	1 2·0	⅔ 1·7	⅔ 0·3	⅓ 0·7	0 0·0	1⅓ 0·5	2⅔ 1·4	2 1·7	2 1·8	13⅔ 1·1	
" " " S.E. "	6⅓ 2·8	8 3·1	5 3·2	3⅔ 1·9	4 1·8	2⅔ 1·1	4⅓ 2·2	5⅔ 2·3	4 0·8	3⅔ 1·6	8⅔ 2·7	7 2·1	63 2·1	
" " " S. "	2 3·2	8⅓ 3·1	4 2·6	10⅓ 2·9	9⅓ 2·6	11 2·2	18 2·5	15 2·5	7⅓ 2·5	5⅓ 2·0	4 3·0	1 2·0	95⅔ 2·6	
" " " S.W. "	1⅓ 2·5	3⅔ 1·8	4⅔ 3·2	3 3·0	3⅔ 2·3	6⅔ 1·7	4 2·3	4⅓ 2·2	5⅓ 2·1	2⅓ 1·0	2 1·7	0 0·0	41 1·9	
" " " W. "	0 0·0	2 1·8	4 2·1	3 2·7	2⅓ 1·5	21⅔ 1·3	3 2·6	3 1·6	3 1·5	1 0·8	1⅔ 1·0	⅓ 0·7	26 1·4	
" " " N.W. "	2⅔ 3·1	0 0·0	2⅔ 3·6	1⅓ 3·0	3⅔ 2·6	2 0·7	⅓ 0·7	1 0·7	1⅔ 1·8	3⅓ 3·3	3 2·1	1 2·8	22⅔ 1·8	

General Remark.—The climatic feature of predominant interest in California lies in the rains of winter, which, although they fell in the early part of the present season in ample abundance for agricultural purposes, nevertheless, in their subsequent diminution, confirm the opinion expressed by us in former remarks, that the cultivator of the earth cannot depend with any certainty upon them alone, but must be prepared to supply their deficiency, whenever it occurs, by irrigation—for which expedient no other country, perhaps, is better adapted, both as regards soil and climate, as well as facilities of commanding water.

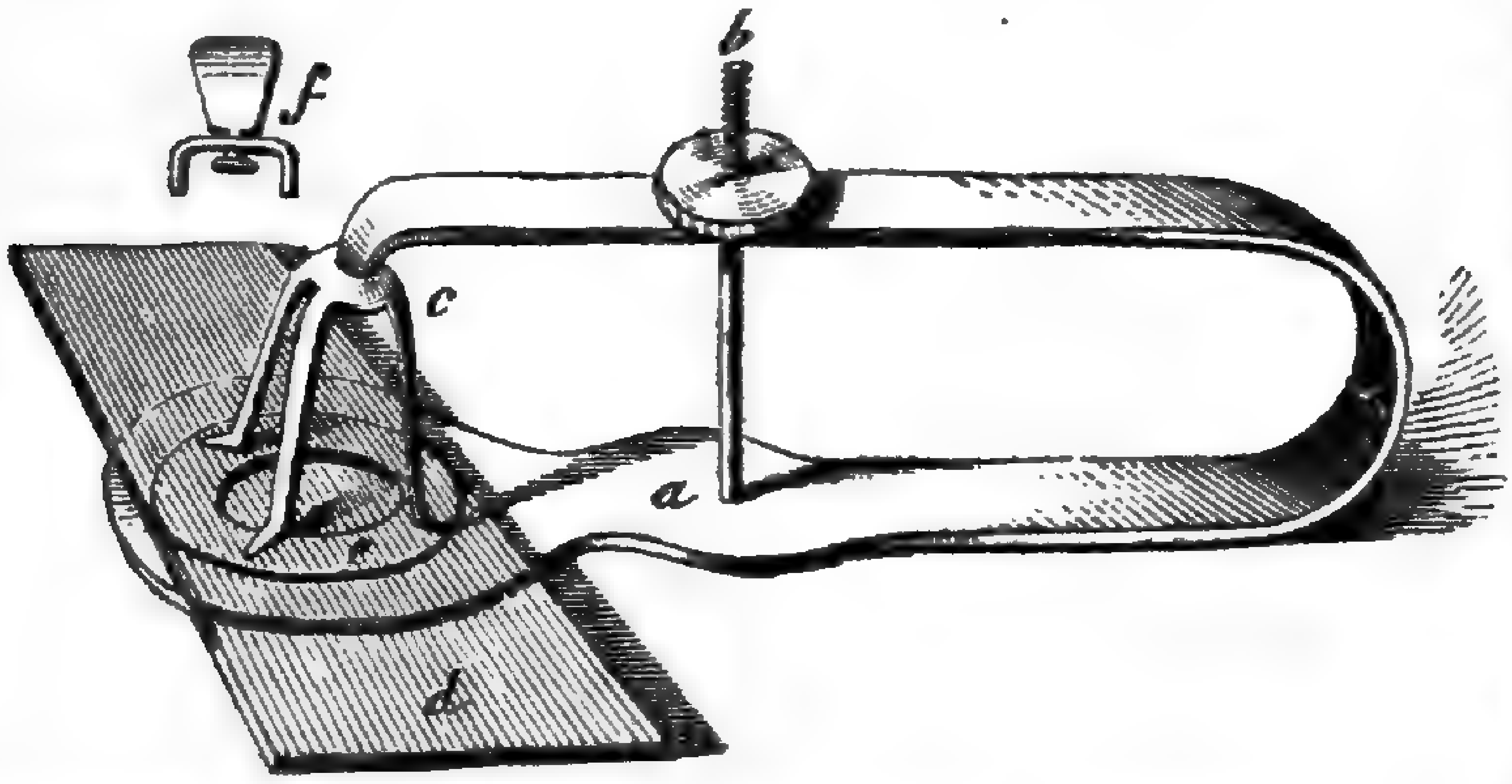
2. *Daylight Meteor of Nov. 15th*, 1859.—Of this remarkable body Professor Loomis has already published an interesting account at p. 137 and 298 of this volume. The meteor being one of the most brilliant on record deserves the fullest possible investigation, and we are glad to find in the Journal of the Franklin Institute of Philadelphia, for March and April, a valuable paper thereon by Mr. Benj. V. Marsh of that city, giving an extensive series of observations which he has collected, together with his deductions therefrom.

The meteor was seen in full sunshine, as a large ball of fire, from Salem, Mass., to Petersburg, Va. Its path was probably inclined to the vertical about 35°, and the direction of its motion was nearly west. Its velocity was very great, perhaps full 30 miles per second, and the meteor appears to have become luminous when more than 100 miles above the earth. During its brief passage of two or three seconds, it exploded several times, with reports which were loud and violent. These reports or detonations made two series, the whole occupying only half a second of time, the individual sounds being distinguishable because of the different distances they had to travel to reach the ear. The column of smoke resulting from the explosions was nearly a thousand feet in diameter, and its base was vertical about four miles north of Dennisville, N. J. The immense volume of smoke or substance of the meteor, dissipated by its excessive heat, shows that the body was of very considerable magnitude. The meteor's path would strike the earth near Hughesville, on the northwestern boundary of Cape May County, N. J., in which vicinity, or a few miles further west, it is probable that fragments may yet be found. Will nobody look for them?

V. MISCELLANEOUS SCIENTIFIC INTELLIGENCE.

1. *Probable Origin of Flint Nodules in Chalk.*—Dr. G. C. Wallich, Surgeon in the Indian army, has published (in the Quart. Jour. Microscopical Sci., No. xxx, p. 36) an interesting paper on the siliceous organisms found in the digestive cavities of Salpæ—embracing under this head the whole molluscoid tribes that frequent the open sea in shoals and live upon the microscopic organisms it contains. These creatures are in their turn the food of whales. In the digestive cavities of the Salpæ the siliceous shields of Diatomaceæ, &c., are freed of all or nearly all their soft portions, and these minute organisms aggregate into masses which in the whales are further aggregated and in the form of coprolites fall in vast numbers upon the pulpy cretaceous strata of Foraminiferæ, &c., now known to form the bottom of the ocean in many places, imbedding themselves there as nodules similar to, certainly, if not identical with, the flint nodules in the Chalk.

2. *New form of Compressor for use with the Microscope,* (in a letter to Prof. G. C. SCHÆFFER).—*My Dear Sir,*—At your request I enclose a sketch of the little instrument that I use as a substitute for the ordinary compressor, in mounting objects for the microscope.



The frame *a*, is made from one piece of hard brass, possessing sufficient elasticity for the purpose,—held in place and adjusted by the milled nut on the screw *b*. This screw is firmly riveted in the frame at *a*, and plays freely in a slot at *b*. The swinging tripod, (also cut from one piece,) is loosely riveted to the bent end of the frame as shown in section at *f*, having sufficient play in the collar to adjust itself to any inequality in the slide or the cover.

The centre of the frame, at *c*, under the tripod, is pierced with a hole of sufficient size to see the object to be mounted, so that the pressure from the screw can be adjusted without injury to the object. The sketch shows a slide, *d*, and cover, *e*, in place.

For mounting objects dry, or for covering cells I find it useful as it enables me to hold the cover securely while I have access to its entire edge, and can turn it in every direction to apply the cement. For mounting objects in balsam which require very thin covers, say ·01 to ·005 of an inch, such as the silicious epidermis of plants and other test objects, I add to the pierced hole in the frame a circular shield of glass, a little less in diameter than the cover, and of the thickness of an ordinary slide, imbedding but a small portion of its thickness in the brass. Then, after placing the cover on the balsam, and spreading it by heat, I put the slide cover down in the frame, and apply a pressure to drive off the superfluous balsam. The raised surface of the glass shield, keeps the exuding balsam from the frame, and at the same time prevents any bulging of the thin glass at the centre. Dried in this way, under pressure, it is easily taken from the frame and cleaned. I have found it a very simple and satisfactory way of perfecting the mounting of difficult test objects.

Other modifications of this little instrument will readily sugest themselves to you. It is, to me, a great convenience to have a dozen of them at hand, of various sizes, as I can thus get my slides thoroughly seasoned under a perfectly uniform pressure—and I can have twenty of them made for the cost of one English compressor.

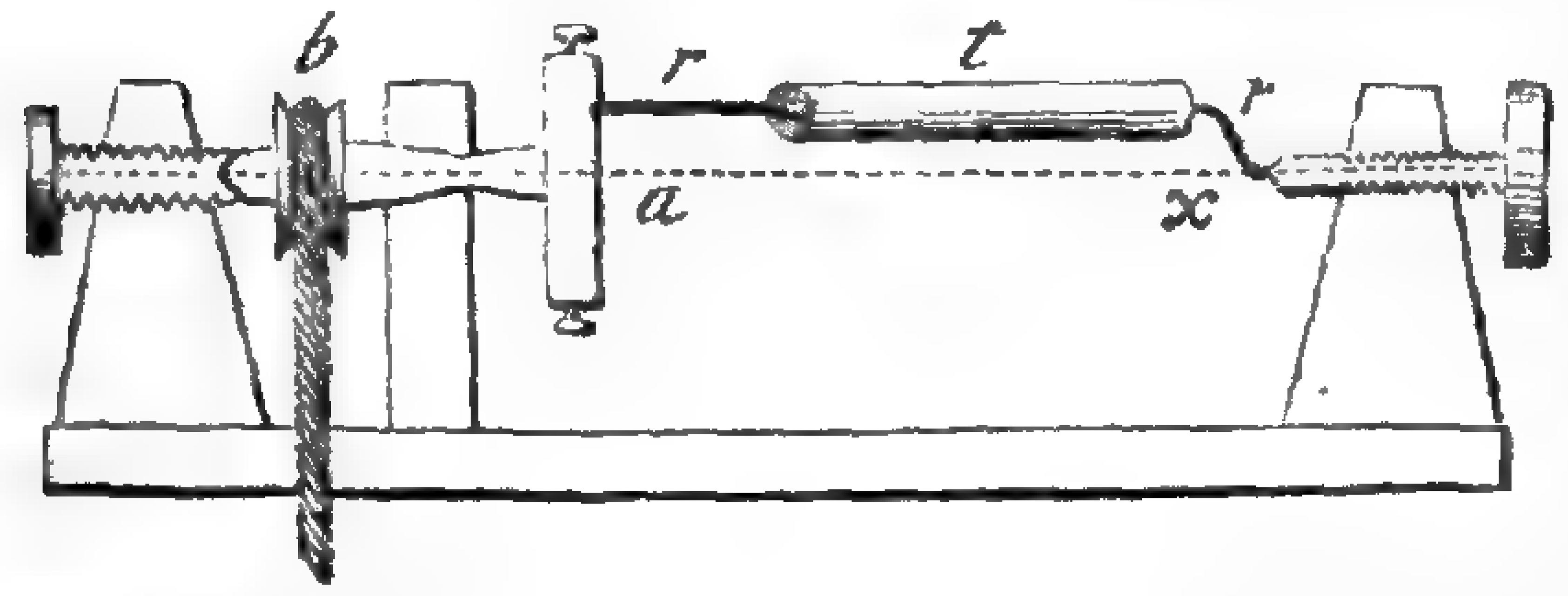
My friend, Mr. McAllister, Optician, 728 Chestnut St., Philadelphia, made them for me from a drawing, very neatly and accurately, at seven dollars and fifty cents per dozen (\$$7\frac{50}{100}$).

Very truly, your friend, S. MORTON CLARK.

Washington, Jan. 4th, 1860.

3. *On Contraction of the Muscles, induced by Contact with Bodies in Vibration;* by O. N. Rood, Professor of Chemistry in Troy University.—Some time since, when grinding a slide for microscopic purposes, as the strip from time to time accidentally came into vibration, I experienced, in the hand holding it, a numbness, and, at times, an absolute inability to relax the grasp. It seemed as though an involuntary contraction of the muscles had been effected by the vibratory action.

For the examination of this matter, the apparatus seen in the woodcut was devised: *b* is made to revolve at a rate of from 50 to 60 revolutions per second; *r r* is a rod of iron placed excentrically and so that the distance *r a* is equal to $\frac{1}{8}$th of an inch. To protect the hand from blisters, the brass tube *t* encloses the rod, fitting it very loosely.



When the hand is laid on this sheath, the rate of rotation being between 40 and 60 revolutions per second, a feeling of numbness is first perceived; the muscles involuntarily contract with considerable force, and the hand grasps the sheath tightly. As long as this rate of revolution is kept up, it is almost impossible, by an effort of the will, to relax the grasp, just as is the case with the electro-magnetic machine employed for medical purposes. The sensations, indeed, resemble those occasioned by the use of this apparatus, and usually extended as high as the elbows. At the termination of these experiments no particular inconvenience was experienced, although the sensations produced by the higher rates of vibration were painful.

Experiments were made on different parts of the arm and hand. The results obtained differed in degree rather than in kind.

The resemblance of the symptoms and sensations produced by electricity and mechanical vibration, is at least singular, and may eventually throw some light on the method in which electricity causes contractions in the muscles.

Troy University, Feb. 3d, 1860.

4. *Large Object-Glass.*—Messrs. Alvan Clark & Sons, of Boston, have completed on their own account, an object-glass with a focal distance of sixteen feet, and clear aperture of *twelve inches.* It has a nice defining power, and Mimas, the nearer satellite of Saturn, was seen with it Feb. 14th and March 2d and 4th.

5. *Boyden Premium.*—Uriah A. Boyden, Esq., of Boston, Mass., has deposited with the Franklin Institute, Philad., the sum of one thousand dollars, to be awarded as a Premium to "any resident of North America, who shall determine by experiment whether all rays of light, and other physical rays, are, or are not transmitted with the same velocity."

The conditions are given in an advertisement at the end of this Number.

6. *Geological Survey of California.*—We learn that a bill for securing the geological survey of California is now under consideration and if not so already, is likely soon to become a Law. So important an act must meet with the approval of every one interested in the material prosperity of the golden state: while science has much to expect every way from the proper discharge of such a commission.

Book Notices.—

7. *Elements of Chemical Physics;* by Josiah P. Cooke, Jr., Erving Professor of Chemistry and Mineralogy in Harvard University. 8vo. pp. 739. Boston, Little, Brown and Company, 1860.—We cannot state the nature and scope of this important contribution to our scientific literature better than by quoting the following paragraphs from the Author's preface.

"The history of Chemistry as an exact science may be said to date from Lavoisier, who first used the balance in investigating chemical phenomena, and the progress of the science since his time has been owing, in a great measure, to the improvements which have been made in the processes of weighing and measuring small quantities of matter. These processes are now the chief instruments in the hands of the chemical investigator, and it is evidently essential that he should be familiar with the causes of error to which they are liable, and should be able to determine the degree of accuracy of which they are capable. All this, however, requires a theoretical knowledge of the principles which the processes involve, and a chemical investigator who, without it, relies on mere empirical rules, will be exposed to constant error. This volume is intended to furnish a full development of these principles, and it is hoped that it will serve to advance the study of chemistry in the colleges of this country. In order to adapt the work to the purposes of instruction, it has been prepared on a strictly inductive method throughout; and a student who has acquired an elementary knowledge of mathematics will be able to follow the course of reasoning without difficulty. So much of the subject-matter of mechanics has been given at the beginning of this volume as was necessary to secure this object; and for the same reason, each chapter is followed by a large number of problems, which are calculated, not only to test the knowledge of the student, but also to extend and apply the principles discussed in the work. Regarding a knowledge of methods and principles as the primary object in a course of scientific instruction, the author has developed several of the subjects to a greater extent than is usual in elementary works, solely for the purpose of illustrating the processes and the logic of physical research. Thus, the means of measuring temperature and the defects of the mercurial thermometer have been described at length, in order to show how rapidly the difficulties multiply when we attempt to push scientific observations beyond a limited degree of accuracy; so also the history of Mariotte's law has been given in detail, for the purpose of illustrating the nature of a physical law, and the limitations to which all laws are more or less liable; the condition of salts when in solution, and the nature of supersaturated solutions, have in like manner been fully discussed as examples of scientific theories; and, lastly, the method of representing physical phenomena by empirical formulas and curves, which are the preliminary substitutes for laws, has been illustrated in connection with Regnault's experiments on the tension of aqueous vapor."

After advising the student to study the details of science from original memoirs rather than from digests, and enumerating the chief sources from which he has drawn his facts and illustrations, the Author announces his design of following the present volume with two others.

"Although the present volume is a complete treatise in itself of the principles involved in the processes of weighing and measuring, it is also intended as the first volume of an extended work on the Philosophy of Chemistry. The arrangement of the chapters and sections has been adopted with this view, and the inductive method begun in this volume will be extended through the whole work. The second volume will treat of the theory of Light in its relation to Crystallography (including Mathematical Crystallography), and also of

Electricity in its relations to Chemistry. The third and last volume will be on Stoichiometry and the principles of Chemical Classification. This volume is now in preparation, and will be published next."

We have read, or studied, the larger portion of Prof. Cooke's present volume, with care, and, are happy to add, with much satisfaction. It is a more elaborate and thorough discussion of the subjects on which it treats than has before appeared in any text-book. All the important propositions are mathematically demonstrated in a simple but thorough manner.

The volume demands and must receive exact and searching study, and any chemical teacher who intends to employ it as his class book will find it capable of the same treatment which he has been accustomed to regard as peculiar to mathematical text-books. The French units of weight and measure are employed exclusively, and a collection of tables (21 in number) is added for scientific reference and for the convenience of the student and teacher in solving the problems (420 in number) which are appended at the close of each principal subdivision of the subject. The whole subject matter of the volume is treated under five chapters, viz., I. Introduction, II. General Properties of Matter, III. The Three States of Matter, IV. Heat, and, V. Weighing and Measuring.

The mechanical execution of the work is beautiful, and the press seems to have been very carefully supervised.

One circumstance in connection with this work cannot fail to attract the attention of all teachers of Chemistry in American Colleges, namely, that a revision of our whole scientific curriculum is demanded in most of our higher Institutions in order to admit of the expansion demanded by the introduction of such a treatise as Cooke's Chemical Physics into the course of study. Such a change Prof. Cooke has been able to effect since his appointment at Cambridge, and now his chemical teachings fill a course of recitations and lectures commencing in the Sophomore year and covering two or three years. This is a great change in the policy of a college where this subject was formerly a by-word, and it offers every encouragement for efforts to secure a similar change in other leading colleges. For this reform, as for the high scientific character of his present work, Prof. Cooke will receive the hearty thanks and esteem of all teachers in this department of science.

8. *Smithsonian Miscellaneous Collections.*—Catalogue of the Publications of Societies and of the periodical works in the Library of the Smithsonian Institution, July 1, 1858. *Foreign works.* Washington, 1859. pp. 259, 8vo, with a Supplement.—The arrangement of this catalogue is geographical, commencing with Scandinavia and ending in Europe with Great Britain. It is followed by an alphabetical Index to Learned Societies, and another of miscellaneous publications, chiefly Journals. The Smithsonian Library possesses, as appears from this list, the Transactions in full or in part of 501 Institutions and Learned Societies, and series, more or less complete, of 254 Scientific Journals exclusively foreign. The domestic publications form the subject of another catalogue. It is a most valuable aid to the student in ferreting out the often enigmatical references constantly found in books of science, and for determining the probability of being able to verify such references by a visit to Washington, or by correspondence. Only those who have undertaken researches can appreciate the value of such an aid.

9. *The New American Cyclopædia.* Vol. IX. New York, D. Appleton and Co., 1860.—This volume contains some fifty articles on zoölogical subjects. Some of them are of considerable length and show marks of much care and labor. The sections on Herpetology and Ichthyology are especially to be commended for the plan on which they are written. Instead of entering into trivial details the author has confined himself to giving the ways in which the best authors have arranged the divisions of Reptiles and of Fishes. The subject is thus properly kept within scientific limits. The more popular treatment is reserved for particular animals or for the minor zoölogical divisions. Thus, there is a very interesting and acceptable article on the Horse, and another on Insects. Some of the definitions are not quite precise, and terms are, in one or two instances, somewhat loosely applied: *e. g.*, speaking of the nervous system of an insect as a "brain and spinal cord." But these are of little consequence compared with the general good judgment and accuracy displayed. Nor is it too much to say that these contributions are far better than those commonly met with in Cyclopædias. It is understood that the zoölogical articles are from the pen of Dr. Kneeland, Secretary of the Boston Society of Natural History.

Other articles on scientific topics in this volume worthy of particular mention are Iron and the Iron manufacture by Mr. Hodge, and that on Isomerism by Dr. F. H. Storer, who is also the author of an excellent article on Chemistry in a former volume.

Among the living scientists of whom biographies are given we notice Herschel, T. S. Hunt, and C. T. Jackson.

10. *Cavendish Socy's. Ed. of Gmelin's Hand Book of Chemistry,* vol. XIII.—This is vol. VII of Organic Chemistry embracing organic compounds containing sixteen and eighteen atoms of carbon. The Cavendish Society announce that they will give only one volume for 1857, '58, and '59. This action of the Society is not well calculated to content their subscribers or to invite new recruits. But we can well imagine that the Council have found Gmelin a heavy load and that they are anxious to close it before undertaking any new publication; meanwhile let us hope that "Rose's Analytical Chemistry," long since undertaken, will not be unnecessarily delayed.

11. *Lieber: Geology of South Carolina.* Report IV. 1859. 8vo, pp. 194. Columbia, S. C. Apr. 2, 1859.—We have already announced (this vol., p. 287) the unfortunate discontinuance of the South Carolina Survey from an unwise withholding of the requisite appropriation of money. Dr. Leiber in this Report sums up his results and presents some general discussions on Metamorphism, a subject certainly of much importance in both a scientific and economic view in South Carolina. The Geognostic maps of Anderson and Abbeville districts and the "Industrial map" of the State, are very neatly printed in colors by Colton of New York.

Some of the most important subjects of this Report have already been laid before our readers, *e. g.*, the evidence of a change of level on the coast of South Carolina and the mineralogical details.

12. *Fundamental Ideas of Mechanics and Experimental Data,* by A. Morin. Revised, translated and reduced to English Measure, by Joseph Bennett, Civil Engineer. New York, Appleton and Co., 1860.—This

work is designed to illustrate the practical application of mechanics to the construction of machinery, draught vehicles, ship building, &c., and contains experimental data of great value in calculating the resistance to be overcome in the form of friction, vibration and wear and tear of machinery and other structures. The work is of great value to practical engineers and machinists and we rejoice to see it presented to the public in an English dress.

13. *Gangstudien, oder Beiträge zur Kenntniss der Erzgänge*, Dritter Band, drittes und viertes Heft.—The continuation of this valuable work, edited by Prof. Cotta and Herrmann Müller, contains a very important paper by Müller, on the relation between *mineral springs* and *mineral veins* in Northern Bohemia and Saxony; also, an extended article on *itacolumite*, its associates, and the metal bearing characters of the same, by Oscar M. Leiber, late State Geologist of South Carolina.

Ferrel, Wm., A.M.: The motions of fluids and solids relative to the Earth's surface; comprising applications to the winds and currents of the Ocean. From the Math. Monthly, Vols. I. and II. New York, Ivison and Phinney. 1860. pp. 72. [A very important discussion an abstract of which, prepared by the Author, we shall present at an early day].

Wilkes, Capt. Chas, U.S.N.: On the circulation of the Oceans. Phila., 1859. pp. 24, 8vo.

Hennessy, Henry, F.R.S., M.R.I.A.: A Discourse on the Study of Science and its relations to individuals and to society. 2d edition, Dublin, 1859. 8vo., pp. 54.

Kirkby, J. W.: On Permian Entomostraca from the shell limestone of Durham, with notes by T. R. Jones, F.G.S. XI plates. 8vo. pp. 17.

Parker, W. K. and T. R. Jones: On the Nomenclature of the Foraminifera, (from Ann. and Mag. Nat. Hist., Nov., 1859). 8vo, pp. 17.

Bellardi Luigi: Saggio di Ditterologia Messicana. Parte 1A, Torino Della Stamperia Reale. 1859. 4te, pp. 77. Tav II.

Logan, Thos. M., M.D.: Report on the Medical Topography and Epidemics of California. Phila., 1860. pp. 58, with maps.

Stevenson, D. and T.: Answer to Sir David Brewster's Reply to Messrs. Stevenson, on Sir D. B.'s memorial to the Treasury. Wm. Blackwood and Sons, 1859. 8vo, pp. 16.

Forget, A.M., M.D., etc.: Dental Anomalies and their influence upon the production of diseases of the maxillary bones. (Prize essay), translated from the French. Jones and White. Philadelphia, 1860, pp. 34, with vi plates.

Durand, Elias: Memoir of the late Thomas Nuttall, (from the proceedings of the Am. Phil. Soc.). 8vo, pp. 19. 1860, Phila.

Royal Society, London, Proceedings, Nos. 35, 36 and 37, from May 26, 1859, to January 12, 1860. 8vo, pp. 1–234.

Hall, James: Contributions to the Palæontology of New York, being some of the results of investigations made during the years 1855–1858, being part of the 12th Annual Report of the Regents of the University of the State of New York. March 15th, 1859. Albany, 8vo, pp. 110.

Harris, Elijah P.: The Chemical Constitution and Chronological Arrangement of Meteorites (an inaugural dissertation). Göttingen, University Press. 1859. pp. 131.

Isaac Lea: Observations on the genus Unio, &c., Vol. 7, Part II, with 25 plates, 4to, read before the Acad. Nat. Science. Philadelphia, 1859.

Haidinger (Wm.): Ansprache gehalten am Schlusse des Ersten decenniums der K. K. Geolg. Reichanstalt in Wein, 22 Nov., 1859. 8vo, pp. 38. 3 plates.

(The same). *Die Rutilkrystalle von* Graves Mount in Georgia. 5 January, 1860.

J. B. Schneider und Ernst Hartig: Untersuchungen über dir Heizkraft der Steinkohlen sachsens: Leipzig, W. Engelmann. 1860. 4to, pp. 509. Taf. IV. This is the third and concluding part of the great government research on the Coals of Saxony. Dr. Geinitz prepared the volume on their Geology and Prof. W. Stein is the author of the Chemical volume. The three parts form by far the most complete research on this important subject hitherto published.

Dr. J. Lamont: Magnetische unterschungen in Nord-Deutschland, Belgien, Holland, Dänemark, München. 1859, 4to, pp. xlv, Tab. ix.

(The same.) Untersuchungen über die Richtung und Stärke des Erdmagnetismus, &c., München, 1858, 4to, pp. cxv.

Ludwig Seidel: Die Theorie der Lichterscheisung des Saturn München, 1859, 4to, pp. 102, 1 chart.

Von Martius (Dr. Carl): Eine Rede zur feifer des Akademischen Saccularfestes am, 29 März, 1859. München. 4to pp. 74.

Feistmantel (Carl): Die Porphyre im Silurgebirge von Mittelböhmen, 4to, pp. 42. 2 Tafeln.

Daguein, P. A.: Traité de Physique, tome iii, 8vo, pp. 1007, avec Table alphabetique et analytique. Toulouse et Paris, 1860. This volume contains the close of Electricity, and Optics.

Proceedings Boston Soc. Nat. Hist., 1859.—p. 135, On the recent eruption of Mauna Loa; *H. M. Lyman.*—p. 138, Descriptions of new shells brought home by the North Pacific Expedition; *A. A. Gould.*—p. 142, List of Plants collected by E. Samuels in California; *A. Gray.*—p. 145, List of Plants collected by J. Xantus at Fort Tejon (5 new species); *A. Gray.*—p. 150, Natural History, etc., of Mohawk, Herkimer Co., N. Y.; *Whittemore.*—p. 153, On vibrating dams; *W. Edwards.*—p. 155, Experiments with the Japan Wax-plant, Rhus succedaneum; *E. S. Rand.*—p. 159, Habits of Esox fasciatus; *G. Curtis.*—p. 160, Green feldspar from Mt. Desert, Me.; *C. T. Jackson.*—p. 161, New shells of the North Pacific Expedition (of the genera Patella, Acmæa, Scutellina, Emarginula, Rimula, Chiton, and Dentalium); *A. A. Gould*—p. 166, On reversed bivalve shells; *L. Agassiz.*—p. 167, On Odontaspis griseus; *D. H. Storer.*—On the shedding of the antlers of the American red deer; *J. Wyman.*—p. 168, On the Carboniferous rocks of Nova Scotia and the United States; *W. B. Rogers.*—p. 174, On larvæ vomited from the human stomach, and on a new Gordius, G. trifurcatus; *White.*—p. 175, On a great meteorite recently discovered in Oregon; *Evans* and *Jackson.*—p 176, On the geology of the shores of the Bay of Fundy; *W. B. Rogers.*—p. 176, On the meteor of August 11th, 1859; *D. A. Wells.*—p. 179. On the transformations of the Cecidomyiæ, a posthumous paper by the late *Dr. T. W. Harris.*—p. 184, On Diatomaceæ prepared by Mr. Samuels; *Stodder.*—p. 185, On Cicindela Hentzii; *Harris.*—p. 189, Synonymy of three North American butterflies; *Harris.*—p. 191, On the Arrangement of Zoological Museums; *L. Agassiz.*—p. 192, Experiments on frogs; *B. J. Jeffries.*—p. 193, Descriptions of new Ophiuridæ belonging to the Smithsonian Institution and to the Museum at Cambridge; *T. Lyman.*—1860. p. 208, On flattened pebbles found in the conglomerate of Vermont; *E. Hitchcock, Jr.*—p. 209, On a mineral substance found in the medullary cavity of trees at the Sandwich Islands by Dr. Winslow; *A. A. Hayes.*—p. 211, On the Gorilla; *J. Wyman.*—p. 213, Chronological Index to the Entomological papers of T. W. Harris, M.D.—p. 222, On the lower jaw and a tooth of Physeter macrocephalus; *J. C. White.* s.

Proceedings Philadelphia Acad. Nat. Sci., 1860.—p. 3, Number of species of American Unionidæ; *I. Lea.*—Mosasaurus bones from New Jersey; *J. H. Slack.*—p. 4, Contributions to North American Lepidopterology, No. 3; *B. Clemens.*—p. 15, Appendix to paper on new genera and species of North American Tipulidæ with short palpi, etc.; *R. Osten Sacken.*—p. 17, Catalogue of the Mollusks of Mohawk, N. Y.; *J. Lewis.*—p. 19, Notes on the nomenclature of North American fishes; *T. Gill.*—p. 22, Prodromus descriptionis animalium evertebratorum, etc. Pars VIII, Crustacea Macrura; *W. Stimpson.*—p. 47, Mexican Humming birds; *R. M. de Oca.*—p. 49, Geographical distribution of Coleoptera; *J. L. Leconte.*—Geographical distribution of the Helices of North America; *W. G. Binney.*—p. 51, Reversed Unionidæ; *I. Lea.*—p. 55, Illustrations of fossils; *Conrad* and *Gabb.*—p. 55, Descriptions of new species of American fluviatile gasteropods; *J. G. Anthony.*—p. 72, Supplement to Catalogue of Venomous Serpents; *E. D. Cope.*—p. 74, Catalogue of the Calamarinæ in the Museum of the Academy; *E. D. Cope.*—p. 80, Descriptions of new species of Cyrena and Corbicula; *T. Prime.*—Mexican Humming-birds; *R. M. de Oca.* s.

INDEX TO VOLUME XXIX.

Erratum, in part of edition—p. 454, line 13, for "DAGURIN," read "DAGUIN."

NOTICE.

At the close of Volume XXX, Nov. 1860, an alphabetical and analytical Index will be prepared to the TEN Volumes of this Journal completed by that number. This Index will be issued to all subscribers with the number for January, 1861, and may also be ordered separately.

New Haven, May 1, 1860.