THURSDAY, AUGUST 10, 1876

THE MOON

The Moon and the Condition and Configurations of its Surface. By Edmund Neison, F.R.A.S., &c. (London: Longmans, Green, and Co., 1876.)

FROM the earliest ages our satellite has attracted a great portion of the attention of astronomers of all nations, and from its proximity to us it is only right that it should still have a large amount of astronomical labour bestowed on it. This labour is divided into two kinds, that of ascertaining its motions in space, and that of inquiry into its physical constitution. It is of this latter research or the study of selenography, chiefly, that the present work treats. The author tells us that he has taken the "Mond" of Beer and Mädler as a basis, but that the greater portion of the material has been mainly derived from eight years constant selenographical observations, principally made with a 6-inch equatorial of fine definition and with a $9\frac{1}{3}$ inch With-Browning reflector. Also, use has been made of some hundred lunar sketches made of late years by different astronomers, and which from time to time have been sent to the author; the work, therefore, is as complete as our present knowledge enables it to be. The first chapter of the book treats of the motions, figures, and dimensions of the moon, and mention is made of the elliptic inequality, discovered by Hipparchus, evection, variation, and annual equation. The alteration in appearance of the lunar surface, due to *libration*, is a matter of the utmost importance in selenography, and the discussion of its effects, together with the formula for computing the same at the end of the book, will be useful to those interested in lunar observations. The author then proceeds to discuss the question of a lunar atmosphere, and his arguments in favour of the same, having a surface-density of $\frac{1}{300}$ that of our air, are extremely forcible. It has always seemed strange that the moon should have had neither air nor water, or almost none, and we are glad to see that it is not incompatible with appearances that a mass of air and water should have existed comparable to ours, when the relative mass of the moon and earth are considered.

The weather-beaten and ruined portions of the moon's surface are referred to as indications of the effects of these agents; and in favour of the existence of an atmosphere, Mr. Neison points out that "it may be reasonably supposed that the ratio of the mass of the primitive lunar atmosphere to the mass of the moon would be a similar ratio to that which obtains on the earth, considering the close connection between the two; but such are the conditions prevailing on the surface of the moon, that so far from the resulting atmosphere resembling in surface-density that of the earth, it would only be $\frac{1}{50}$ as dense, for not only is the surface of the moon as compared with its mass much greater, but the force of gravity at its surface is much less powerful, so that from these causes the atmosphere would occupy a much greater comparative volume, and consequently possess a very small density." One would, at first sight, fancy that $\frac{1}{50}$ was too small a probable estimate, but when we consider that the mass of the moon is $\frac{1}{81}$, that of the earth and its surface $\frac{1}{13}$, we get, per unit of area, $\frac{13}{81}$ of the mass of the terrestrial atmosphere.

The force of gravity on the moon's surface being about $\frac{1}{6}$ of that on the surface of the earth, the pressure of the atmosphere at the surface will be about $\frac{1}{37}$ of the pressure of our atmosphere; correcting this for the probable effects of temperature, we get somewhere about $\frac{1}{50}$ as the surface density. The author explains the absence of water and the disappearance of a great portion of the above small quantity of atmosphere by drawing a parallel between the surface of the moon and earth, and stating that "the joint effect of the action of the terrestrial surface oceans and atmosphere has been to form the present crust of the earth, where is to be found locked up an immense mass of water and of the constituents of our atmosphere which originally formed part of the early terrestrial oceans and atmosphere, and by this means probably a very considerable portion of these must have been by now removed. A similar action would have ensued on the moon with this important difference, that as, relatively to these masses the lunar surface is more than six times as great as the earth's, this absorption of the oceans and of the atmosphere would have been not only more rapid, but have been carried to six times the same extent under the same conditions." The present surface density, he therefore argues, may be now $\frac{1}{6}$ of its original state, or about $\frac{1}{300}$ of the density of that of the earth.

The estimated density of Bessel and others from refraction, of about $\frac{1}{1000}$ of that on the earth, is referred to, coupled with a remark that the temperature was assumed by him to be uniform and a factor depending on the difference of the form of gravity at the surface of the earth and moon omitted, and if correction is made for these, the result should be $\frac{1}{350}$ as the surface density. From observation of occultations it has long been known that a difference of some 2″ existed between the semi-diameter of the moon as determined by occultations and that determined by direct measurement; irradiation accounts for a part of this, leaving the rest to be accounted for by horizontal refraction, and this, we read, renders a surface density of $\frac{1}{200}$ of that of the earth's atmosphere possible, but from other considerations the author puts the probable density at $\frac{1}{300}$. The effect of such an atmosphere in mitigating the climate is shown, but it is not quite easy to see from the present evidence that such an atmosphere, having a pressure of about one terrestrial ounce to a square inch, will account for lunar appearances, or even if we take the pressure at one time to have been $\frac{1}{37}$ of that on the earth, or six ounces. This will require future observations to settle. We are inclined to think that the author is rather over-zealous in his cause when he states to the effect that the mass of atmosphere over a square mile in area must be estimated in millions of tons. This can scarcely be the case, judging from the above-estimated pressure. The occultations of stars, the blue halo occasionally seen around isolated craters, and Lord Rosse's experiments upon radiation from the surface of the moon are all discussed. The author, however, candidly acknowledges that no definite results can be obtained from them either one way or the other, but is convinced that the balance of evidence is in favour of an atmosphere of considerable magnitude, although of slight density; and "to neglect this is to render nugatory all attempts to explain the phenomena presented by the moon."

In treating of the physical condition of the lunar surfaces, it is pointed out that Beer and Mädler's frequent quotation, "The moon is indeed no copy of the earth, much less a colony of the same," is not so well founded as it would appear to be; for although the first impression gained from the general appearance of the surface is that it contains neither oceans, seas, nor river systems, with the accompanying formation, but a desert containing innumerable craters and surface irregularities, still on a closer investigation with adequate means, more points of resemblance become manifest. The more level regions of the moon, especially the shores, though known to have been long destitute of water, are pointed out as appearing to show many traces of its action, as the formation of diluvial deposits recognised by Sir John Herschel; whilst Prof. Phillips traced many analogies between the apparent volcanic formations of the earth and moon, and found many indications of the action of a disintegrating atmosphere.

The greater craters apparently existing on the moon when examined with powerful telescopes, the author tells us, appear less and less like volcanic orifices or craters; their inclosing walls lose their regularity of outline and form, and appear as confused masses of mountains broken by valleys, ravines, and depressions, crossed by passes, and surrounded by low plateaus and an irregularly broken surface; whilst the seemingly smooth floors generally appear as diversely interrupted as the environing surface. These formations are thus seen in their true character, not as craters, but as low-lying spaces surrounded by mountain regions or disturbed highlands.

The author appears to think that the *ring plains* and *wall-plains* are not volcanoes, in the ordinary sense of the term, but depressions surrounded by mountain ranges, and that the great number of apparently small craters are mere shallow hollows, such as are not uncommon on the earth.

The fact that gentle slopes and valleys, like many of our river valleys, would not, except under most favourable circumstances, be shown in relief, is a matter which may easily escape notice, and is here referred to; and further, any small abrupt feature may cast a shadow completely masking much more extensive formations. Attention is called to the fact that Mädler pointed out that formations possessing a north or south direction are much more easily seen upon the moon than those extending east and west, a peculiarity tending to give an imperfect idea of the true nature of the surface, and accounting in some measure for the general meridional direction of numbers of the smaller formations of the moon, such as the ridges, land-swells, and rills, as matter very noticeable on a glance at a lunar map.

The variation of the appearance of lunar formations during the course of a lunation is very forcibly described, as also is that due to libration. The effects of the changes in temperature are referred to as causing a physical variation of the surface, and the changes in the crator Linné, and the ring-plains of Messier are referred to as probable instances of physical change.

The various formations on the lunar surface are enumerated and described with considerable minuteness. With regard to the rills or clefts, Mr. Neison seems to incline to the belief that the majority of them are ancient river-beds, though at present their nature is purely conjectural.

Some thirty pages are devoted to an abstract of the work done upon the moon by various astronomers from the earliest times; but we find no mention of Nasmyth's and Carpenter's excellent book in this list, a work which surely deserves some notice.

The book, of 576 pages, is illustrated by five drawings of craters, and possesses no less than twenty-two maps, containing together the whole of the moon's surface, each of which is accompanied by a full explanation, taking up at least three-fourths of the book, the scale of the maps being 24 inches to the moon's diameter. Three of the craters—Gassendi, Maginus, and Theophilus, are drawn upon an enlarged scale. This work will, no doubt, be of considerable service to those who make our satellite their chief study, since, besides the objects enumerated by Beer, Mädler, and Schmidt, it contains a large amount of new work.

HOVELACQUE ON THE SCIENCE OF LANGUAGE

La Linguistique. By A. Hovelacque. (Paris: Reinwald and Cie., 1876.)

IN speaking lately of the Science of Language we alluded to the question that is still being debated among its students as to whether it ought to be classed with the physical or with the historical sciences. Its method is that of the physical sciences, while phonology, which forms so integral and fundamental a part of comparative philology, is purely physiological in character. On the other hand, since phonetic sounds do not become language until they have been made significant, the science of language may be regarded as a historical one. M. Hovelacque is a warm supporter of the first opinion, and his book is an attempt to treat the science of language as a physical science pure and simple. In this respect he is a follower of Schleicher, as he is also in applying the Darwinian hypothesis to the history of speech and in holding at the same time that the various languages of the world have branched off from a number of independent centres. His work is a valuable contribution to the literature of the subject.

M. Hovelacque starts with the assertion that man is man solely in virtue of language, or rather of the capability of language. Following M. Broca he holds that this capability is a function of the third frontal convolution of the left, more rarely of the right, hemisphere of the brain, and that it was first acquired by a primate which thereby became a man. A certain number of the same primates, "less favoured by circumstances, were checked in their development, and relapsed into a regressive change of character; their remains are to be recognised in the anthropoid apes, gorillas, chimpanzees, orangs, and gibbons." Those primates which by a process of natural selection acquired the capability of speech and with that the characteristics of man, gradually improved upon their new possession, wherever external circumstances were favourable, and with the development of speech came also the development of conceptual thought and a corresponding progress in culture and civilisation.

A morphologic investigation of language enables us to trace the several stages of its development, and by supplying intermediate forms furnishes an important verification of the Darwinian theory. Thus we begin with isolating languages and monosyllabic roots, and then pass on through the agglutinative to the inflectional family of speech, each family, together with the members of each family, gradually increasing in complexity of organism. The roots themselves can be shown to be of onomatopœic or interjectional origin, and the interval between them and the six distinct sounds emitted by the *cebus azaræ* of Paraguay is far less than that between the several stages of linguistic development. Linguistic development itself depends upon the changes brought about in the pronunciation of words by natural causes, and since the laws which regulate these changes fall ultimately under the province of physiology, the "historical life" of language is as much a subject of natural science as the more special phenomena of the physiologist.

The main objection which offers itself to this theory is the necessity it involves of explaining the development of speech by the accidents of phonetic decay. No doubt the meaning of words is largely influenced by the forms they may assume in pronunciation under the action of phonetic laws which ultimately go back to such controlling conditions as climate, food, and the like; but just as often it is the meaning which determines the form. After all, it is not the particular phonetic sound which constitutes language, but the signification put into it by the joint but unconscious action of a community. Without language, it is true, there can be no thought; but it is equally true that language without thought would be only the gibberish of a parrot.

Another objection which holds against the view of M. Hovelacque is the undue limitation which it imposes upon the science of language. M. Hovelacque's work is little more than a catalogue of the various languages of the world, classified morphologically and genealogically, with a description of the chief phonetic and grammatical peculiarities of each. No place is left for that inner life of language which stands nearer to psychology than to physiology, and the science of language is accordingly made almost synonymous with phonology alone. One misses an account of the nature of language and the causes of its change and growth; one misses equally any reference to comparative grammar and syntax, to the changes of signification undergone by words, and the light they throw upon the history of the human mind. In short, in M. Hovelacque's hands the science of language appears as a classified collection of existing phenomena, while the causes and complex history of these phenomena are left untouched. In assuming, too, that the inflectional languages have once been isolating, M. Hovelacque assumes much more than can be proved. The Indo-European tongues *may* once have resembled Chinese; but there is no proof of the fact, if fact it be, and the "Parent-Aryan," as restored by Schleicher and Fick, is as thoroughly inflectional as Sanskrit itself.

On the other hand, M. Hovelacque does good service in showing how fully all the evidence now at our disposal tells against the theory which would refer the manifold languages of the globe to only two or three original sources. On the contrary it would seem that the beginnings of speech were as numerous as the independent communities of primitive man. It is strange, however, that an author who hesitates about admitting the relationship of the Mongolian to the Finnic-Tatar group should yet accept without questioning the Indo-European affinities of Lycian and Etruscan.

To sum up, M. Hovelacque is a good scholar, and his book is a useful summary of the relationship and characteristics of the various languages of the world. It is also a valuable contribution on the side of those who hold that the science of language must be included among the physical sciences. But it exhibits the defects as well as the advantages of this view; and thus while it proves the difficulty of distinguishing between a physical and a historical science at least so far as the science of language is concerned, it yet shows that to regard the science of language as a merely physical one leads to an unsatisfactory inadequacy of treatment and an unjustifiable narrowness of view.

A. H. SAYCE

THE GERMAN NORTH SEA COMMISSION

Jahresbericht der Commission zur wissenschaftlichen Untersuchung der deutschen Meere in Kiel für die Jahre 1872, 1873. Im Auftrage des Königlich Preussischen Ministeriums für die landwirthschaftlichen Angelegenheiten, herausgegeben von Dr. H. A. Meyer, Dr. K. Möbius, Dr. G. Karsten, Dr. V. Hensen, Dr. C. Kupffer. (Berlin, 1875.)

THE second portion of the Report of the North Sea Expedition, just published, contains Article VI., Bryozoa, edited by Dr. Kirchenpauer. Like most of his countrymen, the author accepts Ehrenberg's name for this group, although there is no doubt that, as urged by Allman and Busk, Vaughan Thomson's name (Polyzoa) has the priority. The number of species met with is but small; we make it 55, the author 54, but perhaps he excludes *Pedicellina echinata.* A most interesting account is given of the Flustra of the Northern Sea, and we welcome the account of the geographical distribution which is appended to each species as a valuable addition to our knowledge. From the richness of Dr. Kirchenpauer's collections, he was peculiarly well able to give a long list of habitats. Among the very complete list of authors quoted, we miss a paper on New Zealand Polyzoa by Sir C. Wyville Thomson, published in the Natural History Review for 1858.

The Tunicata are described by Dr. C. Kupffer. Twenty-four species (not twenty-three) of Simple Ascidians are enumerated, belonging to the following genera:—Ciona, 3 sp.; Phallusia, 6 sp.; Corella, 1 sp.; Cynthia, 8 sp. (1 new); Molgula, 5 sp. (3 new); Pelonæa, 1 sp. The author describes as occurring in some species of Cynthia and Pelonæa certain nipple-shaped bodies met with in the water chamber. These are regarded as standing in close relationship with the circulatory system, and are called Endocarps. All of the species, except those for the first time described, are to be met with in Great Britain; some of them are among those recently described by Alder and Hancock from the West of Ireland, and five of them are represented in a coloured plate. Of the Mollusca, the Gymnobranchs are described by Dr. H. A. Meyer. The number of species met with is but twenty-three; there is not much that is noteworthy in the list, but that "singular and gaudy animal" of Montagu, *Thecacera pennigera*, so rare on the British coasts, was met with. The list of the Brachiopods, Lamellibranchs, and Gasteropods is a very elaborate one, drawn up quite after the fashion of our British Association Dredging Reports; the locality, depth in fathoms, and nature of the ground in which each species was found is given, and a sketch of its geographical distribution is added. The greatest depth reached was about 365 fathoms. *Crania anomala* and *Terebratulina caput-serpentis* appear to have been met with in quite shallow water; *Malletia* (*Yoldia*) *obtusa*, Sars., *Kelliella abyssicola*, Sars., and other deep-sea species were met with at depths of from 50 to 360 fathoms. The following species are described as new:—*Lacuna vestita*, off Yarmouth; *Laëocochlis pommeraniæ*, nov. gen. et sp.; *Fusus mœbii*, and *Lathyrus albellus*. These three latter species are figured.

Article IX., by Dr. Möbius, describes the Copepoda and Cladocera. *Euchæta carinata*, sp. n., is described and figured. The remaining orders of Crustacea are described by Metzger. We note the appearance in the North Sea of an Erichthus form, thus indicating the presence of a Squilla. *Galathea Andrewsii*, Kin., is placed as a synonym of *G. intermedia*, Lilljb.; *Thia polita*, *Nika edulis*, *Bythocaris simplicirostris*, and other interesting forms, were met with. *Sergestes Meyeri*, *Byblis crassicornis*, and *Dulichia monocantha* are described and figured as new.

The list of fish taken is most meagre, containing but thirty-two species.

The meteorological investigations of Prof. Karsten are exceedingly interesting, and records are appended as to the temperatures of the sea at various depths.

Dr. Hensen appends a Report on the Fisheries of the German Coast, in which we find elaborate statistics of the number of fishing-stations, of the fishermen, and the amount of assistance given to them. The off-shore fishermen are distinguished from the deep-sea trawlers. The number of fishermen on the German coasts is 17,195, with say 8,130 boats; the number of English fishermen, is given as 134,000, with 36,000 boats. In France, the number is 73,757 men, with 16,819 boats; in Italy, 60,000 men and 18,000 boats; in Austria, 7,196 men and 1,852 boats. These numbers are based on reports dating between 1871 and 1874.

A portion of the Report is devoted to the subject of the possibility of estimating the take of fish. According to the official return of the German Treasury on the import and export of fish during 1873, it would appear that these equalled on—

	m.
River fish and cray-fish	342,000
Sea fish in general	3,150,000
Herrings	27,798,000
Shell fish	387,000
Caviar	973,000
Total ...	32,650,000

This portion of the Report of the North Sea Commission ought to be studied by all those interested in our own fisheries. E. P. W.

OUR BOOK SHELF

Eighth Annual Report of the Noxious, Beneficial, and other Insects of the State of Missouri. By Charles V. Riley, State Entomologist.

THE perusal of Mr. Riley's yearly reports is one of the pleasures to which the entomologist looks forward with undiminishing eagerness. Each succeeding volume throws open to the student of science fresh fields of discovery in the realms of both nature and art. Mr. Riley's ready appreciation of the practically useful in invention, accompanied by that quick discernment which enables him at once to reject or rectify what is useless or cumbersome, renders him especially fitted for the responsible position which he occupies.

The report now before us is devoted to the consideration of five noxious insects, and one innoxious—the Colorado Beetle, the Canker-worm, the Army-worm, the Rocky Mountain Locust, the Grape Phylloxera, and the Yucca-borer, the greater space being given to the third and fourth of the above-mentioned species, in consequence of the ravages which they have committed in Missouri during the past year.

In the chapter on the Canker-worm an illustrated description is given of a very simple and ingenious contrivance (p. 20) for arresting the progress of the insect at the time of oviposition; it consists of a circle of tin which surrounds the trunk of the imperilled tree at a few inches distance, and which is held in position by a circle of muslin attached to the tin at its lower edge, and drawn closely round the trunk, with a cord, at the top; the tin is then covered with a mixture of castor oil and kerosene on its inner surface, which forms an effectual barrier to the insects.

Other interesting inventions are described; and not only are careful figures prepared of the noxious species in all stages, but also of their natural enemies; so that it is the agriculturist's own fault if he fails to distinguish between his friends and foes.

The Report concludes with the life-history of the Yucca-borer (*Megathymus yuccæ*), an insect hitherto referred to the moths, but which Mr. Riley determines to be a butterfly. Judging by the figure of the adult larva it might be questioned whether the insect is not as nearly related to the moths; it has the aspect of a *Sphinx* larva with the wrinkled and (apparently) shining character and general coloration of a *Cossus*;[1] the pupa bears out the resemblance; the rapidity of its flight quite accords with what is notoriously the character of a Hawk-moth, and the form of its antennæ in no way militates against such an affinity; still it must in fairness be admitted that Mr. Riley adduces much evidence in favour of the Rhopulocerous character of the species, the value of which cannot be contradicted until we can bring forward proofs that some undoubted moth possesses the same structural peculiarities. A. G. B.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*]

Optical Experiments

1. Fold a sheet of writing-paper into a tube whose diameter is about 3 cm. Keeping both eyes open, look through[2] the tube with one eye, and look at the hand with the other, the hand being placed close by the tube. An extraordinary phenomenon will be observed. A hole the size of the tube will appear cut through the hand, through which objects are distinctly visible. That part of the tube between the eye and hand will appear

[1] Mr. Riley notes its resemblance to this genus (p. 177).

[2] It is necessary to focus the eye upon any object seen *through* the tube. —ED.

transparent, as though the hand were seen through it. This experiment is not new, but I have never seen it described. The explanation of it is quite evident.

2. Drop a blot of ink upon the palm of the hand, at the point where the hole appears to be, and again observe as before. Unless the attention be strongly concentrated upon objects seen through the tube, the ink-spot will be visible within the tube (apparently), but that part of the hand upon which it rests will be invisible, unless special attention be directed to the hand. Ordinarily the spot will appear opaque. By directing the tube upon brilliantly illuminated objects, it will, however, appear transparent, and may be made to disappear by proper effort. By concentrating the attention upon the hand, it may also be seen within the tube (especially if strongly illuminated), that part immediately surrounding the ink-spot appearing first.

3. Substitute for the hand a sheet of unruled paper, and for the ink-spot a small hole cut through the paper. The small hole will appear within the tube, distinguishing itself by its higher illumination, the paper immediately surrounding it being invisible. Many other curious experiments will suggest themselves. For example: if an ink-spot somewhat larger than the tube be observed, the lower end of the tube will appear to be blackened on the inside.

4. While making these experiments, an improvement upon the experiment described in NATURE, vol. xii., p. 502, was suggested, as follows:—Look through a paper tube with one eye at green paper, and through another tube with the other eye, at red paper. The paper should be illuminated by the direct solar ray. The two colours, at first vivid, are rapidly enfeebled. After half a minute, transfer both eyes to either one of the papers, say red. To the eye fatigued by green, the red colour is very brilliant, and the effect is the more striking on account of the simultaneous impressions now received by the two eyes.

Washington University, St. Louis

F. E. NIPHER

Antedated Books

THE evil practice of issuing antedated periodicals has long been a matter of complaint amongst naturalists. The editor of the *Journal für Ornithologie* is a well-known sinner in this respect—the quarterly number of that journal, although invariably dated on the first day of each quarter, being always several months in arrear. But a still more flagrant instance of this practice is now before me in the third number of the new edition of Layard's "Birds of South Africa," which, although only issued to the subscribers within these last few days, is dated on the cover "May, 1875!" As two new genera (*Aëthocichla* and *Neocichla*) are instituted herein, the result is to give these names an unjust priority of fifteen months over what they are legally entitled to. This seems to be a still easier method of gaining precedence than the American practice of publishing telegraphic bulletins of new discoveries, and will not, I trust, be persevered in, if attention is called to it. F.Z.S.

August 7

Protective Mimicry

I HAVE been reading over in the file of NATURE the controversy that arose out of Mr. Alfred Bennett's paper at the British Association in 1870, on "Natural Selection from a Mathematical Point of View," in which he attacked Darwin's theory on what seems to be one of its strongest points, namely, protective mimicry. I do not feel certain whether he is right or not in denying that natural selection is adequate to produce mimicry. The argument really depends on a question of fact, namely, whether the first variation could be great enough to be useful to its possessor; and from the great comparative variability of colour, I see no decided impossibility in this.

But the writers in that controversy neglected other facts of colour which it seems impossible for natural selection to produce, from the infinite improbability of a first variation ever occurring. One of these is the change of colour with the seasons in such animals as the ermine, which is brown in summer and white in winter. Had the ermine been either permanently brown or permanently white, there would have been nothing wonderful in it, but it seems impossible that the character of becoming white in the winter and brown in the summer could ever have originated in ordinary spontaneous variation, without a guiding intelligence.

Another case of at least equal difficulty is the case of change of colour for the purpose of protection, from moment to moment. The chameleon is the best known instance of this, but I believe there are many such cases among fishes. It seems utterly impossible for such a character to originate in spontaneous unguided variation. JOSEPH JOHN MURPHY

Old Forge, Dunmurry, Co. Antrim, July 20

A REMARKABLE instance of this phenomenon is shown in a small crustacean, of the genus "Rypton" (Mr. Spence Bate has not yet determined whether it be a new species or no). This very delicate little animal is found only in holes in the coral inhabited by the common "Echinus" of Mauritius; its colour is a deep purple, with four longitudinal stripes of a much lighter tint; and this is precisely the pattern of the spines of the said Echinus. WILMOT H. T. POWER

λ Ophiuchi

I AM going to undertake the calculation of elements of λ Ophiuchi, which you proposed to calculators in NATURE, vol. xiv. p. 29. I shall also within a short time give orbits of γ Coronæ, which has not been separated as far as I know since spring, 1867, when it was observed in Harvard College, and of ξ Libræ (Scorpii). About the latter binary star we know but very little. Mädler has given a circular orbit with a period of over 100 years, while Thiele gives a highly eccentric orbit with a period of about fifty years. It may very likely be found that the older determination is the most trustworthy, but the case deserves a thorough examination, which I am going to make. I have been engaged in a re-determination of elements of σ Coronæ, by which the long period has been re-ascertained.

There are different other double stars which with advantage might be inquired into, and thus prevent different investigators from confining themselves to the same objects, while others remain uncared for. I hope that you will be kind enough to publish the above remarks in your widely circulated paper.

Markree Observatory, Collooney, Ireland, July 17 WILLIAM DOBERCK

The Cuckoo

THE cuckoo is still singing in this part of the country. I may mention, as a point of some interest, that the note of this bird in South Germany is precisely the same in pitch as it is here, the observations in both cases having been made with a tuning-fork in the month of May.

Can any of your readers inform me whether the cuckoo *in all parts of the country* is in the habit of occasionally singing the *cuc* without the *koo?* GEORGE J. ROMANES

Ross-shire, July 24

THE FERMENTATION OF URINE AND THE GERM THEORY

CAN Bacteria or their germs live in liquor potassæ (Pharm. Brit.) when it is raised to the boiling-point (212° F.)? Such is now the simple issue to which certain great controversies have been reduced. If Bacteria germs cannot resist such an exposure, then, by M. Pasteur's own implicit admission, his exclusive germ-theory of fermentation must be considered to be overthrown by the broader physico-chemical theory. The truth or not of M. Pasteur's germ-theory is the central question in dispute, but standing on either side, or in close juxtaposition, are two dependent subjects of controversy whose importance for biological science and for medicine is even greater.

The question whether living matter can or cannot originate *de novo*, for example, depends upon the answer which is to be given to the question whether Bacteria and their germs are or are not killed in boiling liquor potassæ. This, also, is practically admitted by M. Pasteur in his comments (*Comptes Rendus*, July 17) upon my recent experimental evidence.

The other subordinate problem, the solution of which depends upon the same issue, is the truth or falsity of an exclusive germ-theory in explanation of the origin and spread of the communicable diseases. If the germ-theory of fertilisation can be proved to be untrue, and if living ferments can be proved to originate spontaneously, we should soon cease to hear much about an exclusive germ-theory of disease. This derivative doctrine would not long survive the death of its parents.

Thus M. Pasteur's theory of fermentation, the popular doctrine *omne vivum ex vivo*, and the germ-theory of disease, must all be simultaneously overthrown if it cannot be proved by M. Pasteur, or some of his followers, that Bacteria germs are not killed when they are immersed in strong liquor potassæ raised to 212° F. (100° C.). How matters have been brought to this desperate predicament may be explained in a very few words.

Since the year 1862, M. Pasteur has defended four main positions, on the strength of which he has based his germ-theory of fermentation, his repudiation of "spontaneous generation," and his support to the germ-theory of disease. In the year 1870 and subsequently, I have many times submitted these four positions to an independent criticism by means of experiment, and the result has been a confirmation of two of them, and a rejection of the remaining two—the rejection being necessitated rather on account of facts obtained by new methods than from any implied defect in the particular range of experiments from which so distinguished an investigator as M. Pasteur deduced his opinions. Our respective views on these four points may be thus tabulated :—

PASTEUR	BASTIAN
1. That *all* boiled organic infusions having an acid reaction will, when protected from contamination, invariably remain pure.	1. That *some* boiled organic infusions having an acid reaction will, when protected from contamination, ferment and swarm with Bacteria.
2. That all Bacteria and their germs are killed in such boiled acid fluids.	2. Do.
3. That *some* boiled organic infusions having a neutral, or slightly alkaline reaction, will not remain pure even when protected from contamination. They will, on the contrary, ferment and swarm with Bacteria.	3. Do.
4. That all Bacteria and their germs are not killed in such neutral or slightly alkaline fluids raised to 212° F. (100° C.).	4. That all Bacteria and their germs are killed in such neutral or slightly alkaline fluids raised to 212° F. (100° C.).

Omitting, for the present, all intermediate stages of the controversy which has now been carried on for several years between one or other of M. Pasteur's followers and myself, I will proceed to show how the questions between us have been affected by my latest researches.

The results obtained in these researches have been embodied in a memoir communicated to the Royal Society on June 15, of which an abstract was published in NATURE, vol. xiii., p. 220. A very short "Note" on the subject of these researches was also submitted to the *Académie des Sciences* on July 10, and subsequently published in the number of the *Comptes Rendus* bearing that date. M. Pasteur replied to this note at the next meeting of the Academy (*Comptes Rendus*, July 17), at a time when he would appear not to have seen the fuller abstract of my researches published in NATURE. This will account for an error into which he seems to have fallen in regard to one of the most important conditions prescribed for some of my experiments, to which I shall have occasion presently to refer. In the first place, however, I must call attention to a different part of the subject.

One of the most notable results of my recent work is this :—I have ascertained that a moderately acid urine will, after it has been boiled, remain pure when kept free from contamination at a temperature of 77°–86° F. (25°–30° C.), though the same specimen of "sterilised" urine will ferment and swarm with Bacteria in less than three days, if it is maintained at the higher temperature of 122° F. (50° C.). Many acid vegetal infusions will behave in precisely the same manner.

Here, then, is a ready means by which any careful experimenter may ascertain whether M. Pasteur is not wrong in maintaining his proposition No. 1. And if this is the case, then there is nothing for M. Pasteur to do but to renounce his exclusive germ-theory of fermentation, and to adopt the doctrine of "spontaneous generation," since he still declares that Bacteria and their germs are killed in acid fluids raised to 212° F. (100° C.). His words are (*Comptes Rendus*, July 17, p. 179) :—"J'ai prouvé directement qu'ils perissent dans un milieu acide à 100 degrés."

But there is another means of establishing the truth of my conclusions derived from these recent researches to which I will now allude. This is the point principally referred to in my "Note" to the Academy, and upon which M. Pasteur dwells in the above-mentioned communication.

As regards the frequent fertility of boiled organic fluids having a neutral or faintly alkaline reaction (No. 3) it will be seen that M. Pasteur and myself are thoroughly agreed, notwithstanding Prof. Tyndall's representations to the contrary, made in the columns of this journal in the early part of this year. M. Pasteur now says (*Comptes Rendus*, July 17, p. 178) :—"Je m'empresse de déclarer que les expériences de M. le Dr. Bastian sont, en effet, très exactes ; elles donnent *le plus souvent* les résultats qu'il indique . . . Il n'y a donc entre M. Bastian et moi qu'une différence dans l'interprétation d'expériences qui nous sont maintenant communes." The difference of interpretation to which M. Pasteur alludes depends upon our difference of view in regard to position No. 4. It was specially with the hope of dissipating any doubt remaining upon this part of the question that one section of my new experiments was undertaken. I determined to submit M. Pasteur's interpretation to the test of direct experiments, conducted in a way likely to yield decisive results.

If the fertility of the boiled neutralised fluids or infusions were really due to the survival of germs, as M. Pasteur supposes, then the boiling of the fluid in its acid state (when its germs would by admission be destroyed), and the subsequent addition to it of a sufficient amount of boiled liquor potassæ, without extraneous contamination, should be attended by negative results—that is, the fluid should remain pure, according to M. Pasteur, if it were really germless.

But numerous experiments performed in this manner have shown me that sterilised urine, to which boiled liquor potassæ, in proper quantity, is added, will ferment and swarm with Bacteria in a few days—and all the more quickly if the experimental vessels and their fluids are maintained at a temperature of 122° F. (50° C.).

M. Pasteur, whilst admitting the facts, says that this addition of boiled liquor potassæ to sterilised urine causes the mixture to ferment because such added liquor potassæ contains germs which were not killed when this fluid was raised to 212° F. (100° C.).

This, truly, is an astounding hypothesis. My reply, however, is simple. It was an objection already anticipated and met by me, as any one may see by referring to the concluding portion of my abstract, as published in NATURE.

The answer is this :—If boiled liquor potassæ were a germ-containing medium, then one or two drops of it (as of other germ-containing media) would always be capable of contaminating many ounces, or even a gallon or more of sterilised acid urine. This, however, is never the case.

The boiled liquor potassæ is only capable of imitating fermentative changes, and of leading to the appearance of Bacteria when it is added in quantities strictly regulated by the quantity and degree of acidity of the specimens of urine with which experiment is being made.

Another fact, just as strikingly opposed to M. Pasteur's view that Bacteria germs can survive in boiled liquor potassæ has been revealed by my researches on the fermentation of urine. It is this :—A very slight excess of liquor potassæ over and above the quantity needed for exact neutralisation almost always yields negative results. This, of course, would be quite inexplicable if the liquor potassæ really acted as a mere germ-containing medium.

An error of procedure of this kind, unwittingly made by M. Pasteur, because he was not forewarned, was in all probability the reason of his obtaining negative results when he operated with solid potash raised to 110° C. or higher. M. Pasteur says (*loc. cit.*, p. 179) : The potash was dropped into the urine in quantity sufficient to render it "alkaline." The negative results obtained in these trials he attributes to the fact that the potash had been heated to 230° F. (110° C.), whilst I feel certain that they were rather due to the addition of an excess of potash, seeing that the addition, as he himself says, rendered the fluid "alkaline."

Briefly, then, M. Pasteur admits me to be correct in stating that boiled liquor potassæ, in proper quantity, will fertilise sterilised urine, and I prove that his interpretation of this fact is wrong by referring him to the totally different effects which would result from the addition of one or two drops, or of a slight excess of boiled liquor potassæ. These effects are wholly irreconcilable with the notion that living germs are capable of surviving after they have been boiled in strong liquor potassæ.

H. CHARLTON BASTIAN

OUR ASTRONOMICAL COLUMN

REISSIG'S COMET (?) OF 1803.—The following particulars of a stellar-looking object, with considerable retrograde motion, were communicated to Bode—at the time the centre of general astronomical correspondence—by Reissig, of Cassel, son of a well-known optician at that place. He stated to Bode that on the morning of Feb. 2, 1803, he perceived with a 30-inch comet-seeker, near the double-star 148 Ophiuchi B. (36 Ophiuchi Fl.), a star of from 5th to 6th magnitude, which he had not remarked on Jan. 28, with a 7-feet reflector. "The star or comet," under a power of 400, appeared without sensible nebulosity, and somewhat magnified. On the early morning of Feb. 4, the stranger appeared to have moved to the westward. The weather was not clear again till the morning of the 7th, when the object was faint from presence of the full moon, and it was difficult to fix its position. On the 9th it was found near 139 Scorpii B. (25 Scorpii Fl.) ; at 3.2 A.M. it occulted this star, and at 4.9 there was first perceived a space between them. Unfavourable weather following, further observation was prevented. Reissig sent Bode a small chart of the path of the object "between π Ophiuchi and Antares," and the four following places, from observations with a 3-feet Gregorian reflector and an annular micrometer.

Feb. 2 at 4h 51m A.M.	... R.A. 253° 48′	... Decl.	26° 19′ S.
,, 4 ,, 3 49 ,,	... ,, 252 4	... ,,	25 49
,, 8 ,, 4 4 ,,	... ,, 249 30	... ,,	25 12
,, 9 ,, 4 45 ,,	... ,, 248 51	... ,,	25 11

With regard to these places, Bode remarked that they do not lie in a regular curve, which may well be attributed to the observations (apparently rough). He observed, further, that the elongation of the object from the sun on Feb. 2 was 56° 34′ W., that as seen from the sun its motion must have been retrograde, and hence it was "a very distant comet."

On attempting to found a parabolic orbit upon the positions given by Reissig, taking, however, the place of 25 Scorpii for the place of the object at 3.2 A.M. on the 9th, it is soon apparent that the distance, instead of being very great, as Bode surmised, must have been very small, so small, indeed, that the earth's perturbations during the week's observations, might, and probably would, greatly distort the apparent track as deduced from the orbit. In fact, after a number of trials, in which, as was to be expected, the elements resulting therefrom differed but slightly and yet gave large differences in the geocentric places, we find that, assuming the elements to be—

Perihelion Passage, 1803, February 10·164 G.M.T.

Longitude of Perihelion	146° 15′
,, Ascending Node	307 45
Inclination...	0 55
Log. Perihelion Distance	9·98234

Heliocentric Motion—Direct,

the following apparent track of the comet results—

0h G.M.T.	Longitude.	Latitude.	Distance from the Earth.
1803, Jan. 25 ...	62° 35′ ...	+ 2° 17′ ...	0·0336
26 ...	60 52 ...	+ 2 6 ...	0·0258
27 ...	57 44 ...	+ 1 47 ...	0·0182
28 ...	49 56 ...	+ 0 57 ...	0·0106
29 ...	10 29 ...	− 3 1 ...	0·0040
30 ...	278 57 ...	− 5 18 ...	0·0065
31 ...	262 2 ...	− 4 6 ...	0·0137
Feb. 1 ...	256 2 ...	− 3 40 ...	0·0214

No further weight is to be attached to these inferences from calculation than as tending to render possible such positions of an object moving under the laws of gravitation, but duly regarding the rough character of Reissig's observations, his last place differing some ten minutes of arc from what we might judge it to have been, if 25 Scorpii were occulted an hour previous. If a comet were really moving in an orbit with elements resembling the above, it might have passed in twenty-four hours (January 29-30) from Pisces to Sagittarius, and the circumstance of the object not being found by Reissig near the place it occupied on February 2, with a much larger telescope on January 28, would be accounted for. We are met nevertheless by the difficulty, that for a body at so small a distance from the earth, to appear like a star of the fifth or sixth magnitude, devoid of nebulosity, it is necessary to assign it very small dimensions, while the appearance described is quite irreconcilable with the aspect presented by the few comets which have been seen in close proximity to the earth, particularly that of 1770, which at its perigee was upwards of two degrees in diameter according to Messier.

Reissig claimed to have discovered the comet of 1801 some twelve days before it was detected by Pons, but the account he sent Bode of his observations is a singularly lame one. (B. J., 1805, p. 129.)

It must be admitted that the examination of such observations as those of Reissig and of Huth, as treated in this column last week, is mainly a matter of curiosity, still if it be possible to show that the observations are not necessarily to be regarded as impositions upon the astronomical world, it will be granted that something is gained thereby.

SATELLITES OF SATURN.—Mr. Marth's elaborately constructed ephemerides of the satellites of Saturn appear in the *Astronomische Nachrichten*, Nos. 2,098–2,100, with some remarks on the advantage of careful estimations of conjunctions with the ends of the ring and the limbs of the ball over micrometrical measures during the next two or three years. The preparation of these ephemerides must involve an amount of labour and care of which few but those who have attempted such calculations can form any adequate idea, and their value is proportionally great.

THE MUSEUM OF NATIONAL ANTIQUITIES OF FRANCE

ALL the French national museums are located in Paris with the exception of the Museum of National Antiquities, which is at some distance from Paris, in a small town of the *banlieue*. Although the Château de Saint Germain, which has been allotted to that interesting and really national collection, is a very picturesque monument, and the forest round a favourite pleasure-ground for Parisian families, the site allotted to the museum about ten years ago was not selected with the view of giving an additional attraction to the place. But the very idea of collecting relics of prehistoric ages in order to demonstrate that our ancestors lived in the age of the so-called diluvian animals was opposed by a formidable number of influential people.

Napoleon III., personally a believer in the new theory, insisted upon the creation of the museum, but he assented to place it at St. Germain in order not to offend directly the prejudices of a formidable number of his supporters.

The St. Germain château was elegantly built in brickwork by Francis I., the king chevalier, who dedicated it to his fair dame, Diane de Poitiers. It was within its walls that Louis XIV. was born, and the government of the Mazarin was sitting in its elegant precincts when Paris was in the hands of the Fronde. Louis XIV. disregarded the building where his cradle had been surrounded by such dangers, and built Versailles with all its magnificence at a small distance of six miles. So St. Germain sank gradually from the dignity of a regal residence into the degrading condition of a prison for soldiers condemned to penal servitude by the Council of War of the First Military Division. The site was only famous as being the favourite spot where Alexander Dumas built his celebrated villa of Monte Christo, and the first place connected by a railway with Paris, as early as 1837.

The opening of the museum was the inauguration of a new era for the castle of St. Germain. Reparations and restorations were begun with activity, and are proceeding with such zeal that in the course of two years hence they will be completed. During the Franco-German war St. Germain was a stronghold of the German armies besieging Paris, but the museum remained unmolested, having been taken by the Emperor William under his special protection, and M. Gabriel de Mortillet, the *conservateur*, who had remained at his post, took advantage of his influence to protect the inhabitants of the city with much energy.

His superior, the then Director of the Museum, is M. Alexandre Bertrand, a brother to M. Joseph Bertrand, the present Perpetual Secretary of the Academy of Sciences. The museum is now placed under the control of the historical commission for constructing the Map of Gaul. This learned body is publishing a magnificent series of maps and engravings in order to illustrate the progress of the science of the prehistoric period, as well as of the Gallic, Roman, Gallo-Roman, and Merovingian. They are also manufacturing in the establishment models of the objects exhibited which cannot be sold for money, but are sent by the Government to the several provincial museums, or presented to learned men in consideration of objects given to the museum, so that they may be acquired by way of exchange. There is also in the establishment a special library in which have been collected by M. Gabriel de Mortillet all the books relating to prehistoric antiquities, and which is open free on certain days to the public. A carefully compiled catalogue has been prepared, and is to be published.

The establishment is in some respects connected with the Prehistoric Congress, M. Gabriel de Mortillet having originated the idea at La Spezzia, and M. Alexandre Bertrand or he having been delegated by the Government to all similar meetings which have taken place since that period. M. Alexandre Bertrand was delegated to Stockholm last year.

The objects collected in the galleries are very numerous, arranged in excellent order, and accompanied by inscriptions sufficient for the perfect understanding of their historical bearings. A catalogue has been issued, and is sold at a small price by the porters.

In the basement have been located casts from the Trojan column for showing the arms and manner of the Romans when practising warfare.

In the same part of the building are to be found the models of Roman arms which were tried in the Polygon of the forest before the members of the Congress of Geography, as mentioned in our "Notes."

These apparatus were constructed by a French officer in order to elucidate questions raised by the publication of "La Vie de César," edited by Napoleon III., who had secured the collaboration of a number of eminent members of the Académie des Sciences Morales et Politiques. Two volumes of that altogether interesting and well-written book (although the theories of Cæsarism cannot be said to have borne the severe test of facts) have been published by M. Plon, the editor of his Imperial Majesty. The first sold immensely, as Napoleon III. was then at the zenith of his power; but the circulation of the second, issued a few months before the Franco-German war broke out, was very limited indeed—so limited that the editor prosecuted the Emperor to recover the money spent by him; but the petition was discharged with costs.

It is for the publication of "La Vie de César" that the siege of Alesia, the crossing of the Rhine, &c., have been expeditiously and carefully executed. The building of bridges over powerful streams, encampments established, assaults given, cities defended, all the warlike operations of the Romans, can be understood by a visit paid to the Museum of St. Germain. All this would have remained a mystery for thousands of visitors, as the museum is fast becoming a place of resort, if Napoleon III. had not felt it necessary to justify by historical arguments his theories on the advantages of the government of societies by men with a special destiny.

The large hall in the second floor may be said to be the most essential part of the museum. It contains the famous Moulin Guignon jaw and other human fossils discovered by Boucher de Perthes. In a glass case have been exposed seriatim the celebrated bones embellished by prehistoric artists with sculptures of the then living animals.

A magnificent bust of Boucher de Perthes, and another of Christy, the famous English banker and amateur geologist, have been erected side by side in a conspicuous place. It is a justice paid to their joint labours in the foundation of prehistoric science. It was due to the moral courage displayed when resisting the authorities, of such men as Cuvier, Elie de Beaumont, Buckland, and a number of other official geologists, and to the ingenuity displayed in the demonstration of such important facts.

On the walls have been painted magnificent maps exhibiting the distribution of caves and places where stone or bronze implements have been discovered, and the limits of the several Gallic tribes in existence when Cæsar invaded Gaul. A number of pictures *al fresco* are exhibited showing the several phases of prehistoric life, principally in lake-dwellings.

No such institution is to be found in England, although cave-hunting is becoming an important pursuit in the country of Lubbock, Lyell, Huxley, and Dawkins. A visit to St. Germain is a very useful way of spending a holiday, especially if the visitor has previously written a note to M. Gabriel de Mortillet, who is always ready to give kindly personal explanation to foreign visitors.

W. DE FONVIELLE

THE BASKING SHARK

TO many it may be a quite new and strange fact that the Basking Shark, almost the largest fish now living, is to be commonly met with at certain seasons around the western part of the British Islands. The fine specimens recently added to the zoological collections of the British Museum and the Royal Dublin Society have excited some wonder; but the popular mind, while it associates sharks with tropical seas and coral reefs, seems as yet hardly to have taken in the fact that if it wants to see about the biggest of all sharks in small shoals, playfully gambolling, it need wander no farther than to the Atlantic coast of Ireland. There, towards the end of April, and often all through May, these Basking Sharks will be met with. They have even been counted off Tory Island in shoals of from sixty to a hundred, basking in the bright morning suns of June.

It is about 110 years ago since the esteemed Bishop Gunnerus (born 1718, died 1773) published an account of this big fish in the Trondhjem Society's Journal, and a great number of authors have written on the subject since then. Under many local names—Basking Shark, Sun-fish, Pelerin—it has been well known to fishermen; it reaches a length of 40 feet, although average-sized specimens do not measure more than between 20 and 30 feet in length; of large size, and shark though it be, it would appear, like many other big animals, to be of a gentle, mild, and placid disposition, to be fond of sunning itself on bright days, and to never interfere with mankind unless when they interfere with it; and yet with all these facts in its favour, the animal being, so to speak, common, having local names, being of a size not easily overlooked, and not being, like its cousin the Blue Shark, a man-eating devil, this *Selache maximus* was very little heard of and less known until the other day. Twelve months ago Dumeril, in his "Ichthyologie Générale," could with truth write about the specimen in the Museum at Paris: "Il semble être, jusqu'à présent, le seul représentant dans les Musées de l'Europe centrale de cette énorme espèce des Mers du Nord." To this moment nothing very exact is known as to its food. Pennant thought it fed on marine plants, Linnæus considered its food to be medusæ; some fishermen foolishly think it lives on herrings; and as to its times and seasons nothing is known. Why does it come from north to south, and why then go north again?

So little being known about its form and habits, it is not much to be wondered at that very little is known about its anatomy; and yet Sir Everard Home wrote an anatomical account of it, which is to be found in the *Philosophical Transactions* for 1809, in which he tells us that he found in the stomach of this fish structures showing a link in the gradation of animals between the whale tribe and the cartilaginous fishes. Why, to work out this idea alone ought to send the comparative anatomists off at once to Tory Island or Bofin.[1] We would, however, refer to another anatomical peculiarity, which, had it been known to Sir E. Home, would doubtless have clenched his argument, namely, the presence of rays or fringes of a whalebone-like substance along the gill-openings. It is true that Gunnerus in 1766 refers to these strange fringes; it is true that in the museum of that far north city of Trondhjem—and within view of the wondrous old cathedral where Gunnerus lies buried, and where to this day Norway's kings are crowned—there is to be seen a piece of one of them; that other Northern Museums, those of Christiania, Kiel, and Copenhagen also possess pieces, and equally true, that during all these days Gunnerus's statements had been overlooked, and these fringes were a puzzle to every-one who examined them. Prof. Hannover, indeed, in 1867, from their minute structure, described them, and thought they were planted on the outside of the fish's skin, like the long spines of certain rays.

Prof. Steenstrup, in whose charge the specimen we figure is, and to whose kindness we are indebted for the figure (1), having made up his mind that it did belong to the Basking Shark, proceeded to work out its history, and so came upon Gunnerus's description, which enabled him

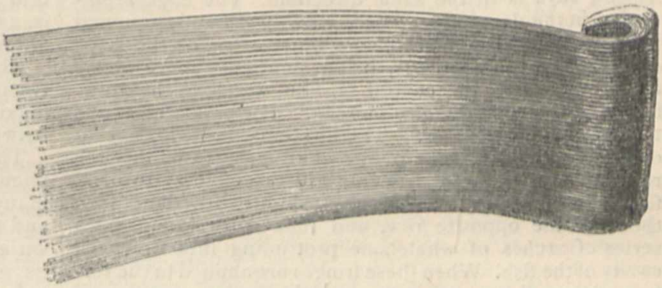


FIG. 1.

to suggest that this shark must have the interior of its mouth furnished with branchial fringes of a peculiar nature. He further argued that these must act as strainers; that the shark takes in whole volumes of minute food, catches it on these fringes, and then swallows it. He declares it to be a great mistake to call this fish a carnivore, that is, if he eats flesh at all, it is small flesh, not big flesh. He then objects to the writer of these lines, when describing a shark found in the Seychelles—"which is, the north whale excepted, the largest of living animals"—saying, "contrary to the habits of sharks, this one is not a carnivorous, but a herbivorous fish," as being too much on the other extreme. My excellent friend is right, and I have now no doubt that both these big, lubberly beasts—which in their mouth have scarcely more than the name of teeth—feed on all sorts of minute oceanic creatures, frequently taking in with them floating algæ. And he will be glad to know that, acting on the hint in his paper, when Mr. Cullen, the assistant in the Trinity College Dublin Museum, went down to Bofin in May of last year, to preserve for

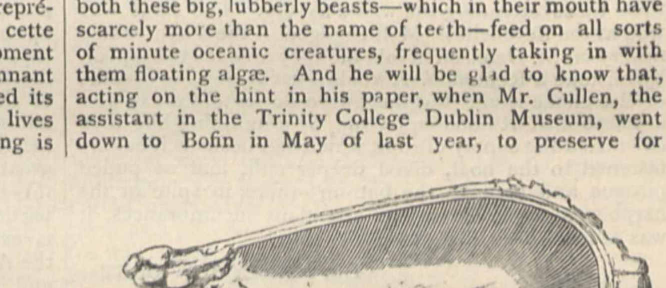


FIG. 2.

Dr. Carte the specimen now in the Dublin Museum, the first thing he did was to put his hand into the still quite fresh branchial openings, when he at once felt what Gunnerus had felt in 1766—the whalebone-like fringes. It is to be hoped that my colleague, Prof. Macalister, will ere long give an account of this specimen; in the meanwhile a description of the annexed (Fig. 2) drawing of these fringes—now for the first time figured *in situ*—will not be without some interest.

The gill-openings are five in number on each side of the neck. The first pair almost meeting on the top of the back. A thought here strikes us. As a rule these gill

[1] Islands off the west coast of Ireland—well known localities for this shark.

slits in the large sharks are small, here they are of immense size. Their function is to allow of sufficient water to flow in and over the gills to oxygenate the fish's blood; but in Selache they serve also as supports to the strainers; and as so big a body must require a great lot of food, the in-takings and out-puttings must be many, and might account for the gradual increase in the size of these slits until they reached their present immense proportions, where they have to subserve both the functions of nutrition and circulation. The convexity of the gill-openings is towards the shark's mouth, the concavity of these fringed rays is in the same direction. The edge represented in the drawing as jagged—an appearance assumed in drying—is attached to the inner edge of the flaps covering the gill-openings, being somewhat more firmly attached towards the central portion, which in the drawing is far too cartilaginous-looking. The gills are outside the whalebone fringes. There seems little reason to doubt but that the free points of the fringes of the one row can be so erected from its gill ray edge as to bend forwards and join, and perhaps slightly interlace with those of the opposite row, and thus there would be a series of arches of whalebone protruding into the neck cavity of the fish. When these fringes are applied to the surface of the gill rays, the water could flow without resistance. The gills were quite free from parasites, in this respect differing from the gills of the Rhinodon of the Seychelles. Although this is not the place to enter into minute details, there is little doubt that Dr. Fleming is wrong in stating that the skin seems smooth when the hand is passed from the head to the tail; and yet though the scales are, as described by Dr. J. E. Gray, armed with small curved points bent in all directions, so that the skin feels rough each way, the hand can be rubbed several times more easily from head to tail than from tail to head, indicating that a larger number of the curved points are directed towards the tail.

The oil from the liver of a medium-sized Basking Shark is worth nearly 40*l.* sterling; but the difficulties and danger of capturing these sharks seem altogether to be greater than those attending the whale-fishery. The same was true at the Seychelles. Men engaged at the sperm-whale fishery off St. Denis often told me they dreaded to harpoon by mistake a Rhinodon. A whale must come up to breathe or else choke itself. But there were stories told me of how a harpooned Rhinodon, having by a lightning-like dive exhausted the supply of rope, which had been accidentally fastened to the boat, dived deeper still, and so pulled pirogue and crew to the bottom—there, in spite of the harpoon in its neck and its attendant incumbrances, it was at home for a great length of time.

ED. PERCEVAL WRIGHT

ON THE PHYSICAL EXPLANATION OF THE INEQUALITY OF THE TWO SEMI-DIURNAL OSCILLATIONS OF BAROMETRIC PRESSURE [1]

THERE are, perhaps, few phenomena in the domain of terrestrial physics which have received more attention than the diurnal variation of barometric pressure, and on the causes and explanation of which, nevertheless, there is more diversity of opinion even at the present day. Dove, Sabine, Herschel, Espy, Lamont, Kreil, Broun, and many others have in turn engaged in the discussion of this vexed problem, and at the present time Mr. Alexander Buchan is publishing an elaborate and most valuable *résumé* of the existing data in the *Transactions* of the Royal Society of Edinburgh as a preliminary to a renewed investigation.

The general features of the diurnal variation of pressure are familiar enough to every one who has ever observed the rise and fall of the barometer for a few days in India, and most other tropical countries. From about 3 or 4 in the morning the pressure increases gradually towards sunrise, then more rapidly, and culminates generally between 9 and 10 A.M. A fall then sets in, which becomes rapid during the hottest hours of the day, and the pressure reaches its minimum generally between 4 and 5 P.M. The pressure then increases till about 10 P.M., but in general does not attain the same height as at the corresponding morning hour. Lastly, a second fall brings it to a second minimum between 3 and 4 A.M., which, except on mountain peaks and at such stations as Simla and Darjiling, is, as far as my own experience goes, never so low as the afternoon minimum.[1]

Thus, then, the pressure rises and falls twice in the twenty-four hours, attaining, in general, its absolute maximum about 9 or 9.30 A.M., and its absolute minimum between 4 and 5 P.M.

This may be taken as a general description of the phenomena as exhibited in the tropics; but it presents many striking variations at different places, and at one and the same place at different times of the year. These variations affect—the hour at which the pressure attains its maximum and minimum values, the absolute amplitude of the oscillations, and lastly, their relative amplitude. It is this phenomenon—the variation in the relative amplitude of the day and night oscillations—the probable physical explanation of which I have now to bring to notice.

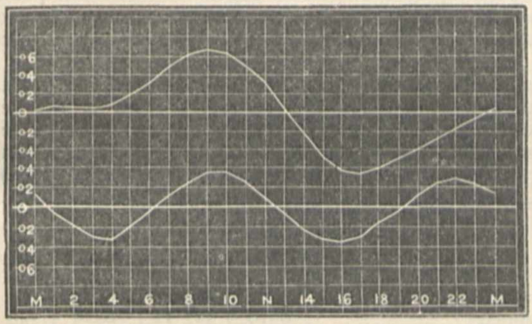
It was observed by Arago, apparently some years prior to 1841, that in Europe "the proximity of the sea has the effect of diminishing the amplitude of the interval during which the diurnal fall lasts, viz., that which occurs between 9 A.M. and 3 P.M.;" and considering the whole phenomenon as made up of a single and double oscillation, it may easily be shown that this interval is determined mainly by the relative amplitude of these two elements. The latest notice on the subject is given in the following extract from Mr. Buchan's Memoir, a copy of the first part of which (for which I am indebted to the author) has reached me only within the last week. In summing up the characteristics of the midday fall of pressure, he says:—"Whatever be the cause or causes on which the diurnal oscillations of the barometer depend, the influence of the relative distribution of land and water in determining the absolute amount of the oscillation in particular localities, as well as over extended regions, is very great. From the facts detailed above, it will be seen that this influence gives a strong local colouring to the results, particularly along the coasts, and that the same influence is extensively felt over the Channel, the Mediterranean, the Atlantic, and other sheets of water on the one hand, and on the other over the inland portions of Great Britain, Europe, and the other continents;" and farther on he adds: "While, as has been pointed out, numerous illustrations can be adduced showing a larger oscillation over the same region with a high temperature and a dry atmosphere than with a low temperature and a moist atmosphere, the small summer oscillation on the coasts of the Mediterranean and those of the Atlantic adjoining is in direct opposition to the idea that any such conclusion is general. For over those parts of the Mediterranean and Atlantic the temperature is hottest in summer and the air is driest—so dry, indeed, that no rain, or next to none, falls; and yet there the amplitude of the oscillation now contracts to its annual minimum. On the western coasts of the Atlantic, from the Bahamas northwards to Newfoundland, the temperature is at the annual maximum, but the air is not dry, being liberally supplied with moisture, and the rainfall is generous. But with these very different meteorological conditions there occurs, equally as in Southern Europe, a diminished oscillation during the summer months in the islands and near the

[1] Possibly some coasts may furnish an exception.

coasts of North America; and in the south of Europe the oscillation reaches its annual maximum just at the season when the annual minimum occurs near the sea-coasts, even although the general characteristics of the atmosphere be substantially the same in both cases."

I am not at present aware whether Mr. Buchan has been led by these observations to any definite conclusions as to the physical cause of the variation he so clearly summarises in the passages above quoted. In the part of his memoir which has reached me all theoretical discussion is deferred. But these passages afford such remarkable confirmation of an explanation at which I arrived some weeks since, on approaching the subject from an entirely different quarter, that I do not think it necessary to withhold longer the publication of my view. If Mr. Buchan's conclusions are the same as mine, the facts that I have to bring forward will seem to afford independent confirmation of that view.

Any person glancing over a series of curves illustrating the diurnal rise and fall of the barometer cannot fail to be struck with the characteristic difference of those of places with a continental and those with an insular climate. The case of the Mediterranean described by Mr. Buchan seems, perhaps, to be an exception; but, as I



1. Diurnal oscillation of barometer at Leh in Ladakh. 2. Do. Square 3 N. Atlantic.

shall presently show, it is an exception of such a kind as most strongly to confirm the rule. The accompanying curves are striking, perhaps extreme, examples of this characteristic difference. The first is that of Leh-in-Ladakh,[1] situated in the Indus valley (the observatory being 11,538 feet above the sea), and is for the month of September. The climate is characteristically dry and the summer heat excessive, notwithstanding the elevation. The curve for Yarkand and Kashgar, still further north, and only 4,000 feet above the sea, is of similar character but smaller amplitude. The second curve figured is that for the northern half of square 3, of North Atlantic, published by the London Meteorological Office. In the former the double oscillation has almost disappeared, the nocturnal fall of pressure being represented by little more than a halt for some hours between two periods of rising pressure; and nearly the whole fall of the day takes place between 9 A.M. and 5 P.M. In the case of the Atlantic curve the day and night oscillations are almost exactly alike, the night oscillation being only slightly less than that of the day. These characteristic differences are perhaps best expressed by the ratio of the constant coefficients U' and U'' in Bessell's interpolation formula—

$$x = M + U' \sin (n\theta + u') + U'' \sin (n2\theta + u'') +, \&c.,$$

since the magnitude of U' determines the inequality, and that of U'', though variable under different conditions of climate, is so to a much less extent than the former term, and chiefly depends on the latitude. The following are the values of U' and U'' in English inches, and their ratios for the mean diurnal curves of a few stations (chiefly Asiatic). The arcs u' u'' corresponding thereto are also given :—

c.	U'	u'	U''	u''	$U' : U''$
Yarkand (9 months) ...	·0348	4° 33′	·0215	161° 59′	1·6 : 1
Leh (September) ...	·0517	343 9	·0254	143 19	2 : 1
Lucknow (year)	·0265	341 30	·0355	168 53	0·75 : 1
Hazambagh, do.... ...	·0193	349 46	·0343	145 45	0·56 : 1
Calcutta, do.	·0265	341 24	·0391	151 7	0·68 : 1
Bombay, do.	·0179	337 17	·0385	157 13	0·46 : 1
Batavia, do....	·0240	24 7	·0369	159 34	0·65 : 1
Square 3, Atlantic, do.	·0055	354 51	·0319	159 26	0·17 : 1

As a general rule the more humid the station and the smaller the range of temperature, the smaller is the value of U', and hence it has sometimes been spoken of as the temperature element of the oscillations; the double oscillation which is superimposed on it being referred by Dove, Sabine, and Herschel to the varying tension of water vapour, by Lamont and Broun to some solar influence other than heat; and by Espy and Kreil to the oscillation of pressure produced by heat in an elastic fluid expanding and contracting under the influence of gravity. To me it seems that there can hardly be a doubt that the last explanation is the true one, and that this has not been generally recognised I attribute to the fact that the consequences of the theory as a purely physical problem have never yet been traced out and verified by such a mass of facts as Mr. Buchan is now bringing together. So long as the *whole* phenomenon is not satisfactorily accounted for, some doubt may reasonably attach to the explanation offered of one only of its elements.

My own attention was first drawn to the subject of the explanation which I am about to give by a paper of Mr. F. Chambers in the *Phil. Trans.* for 1873, in which that gentleman showed, as the result of an analysis of the diurnal variation of the winds at Bombay, that one element of this variation is a double rotation of the wind direction of such a character that the southerly components attain their maximum value at the epoch of the most rapid semi-diurnal rise of pressure, the easterly components at the epoch of maximum, the northerly with the most rapid fall, and the westerly with the epoch of minimum. On these facts Mr. Chambers based a suggested explanation of the barometric tides; regarding them as a phenomenon of static pressure; and assumed (as now appears, on insufficient grounds) that the phenomenon in the northern hemisphere is generally of the same type as at Bombay. There was indeed one feature in his explanation, which it seems difficult to reconcile with mechanical laws, since he supposed air to flow from both east and west towards a region where the pressure is rising above the mean, and by its accumulation to produce a maximum of static pressure. But apart from this, the discovery was an important one, and since it clearly showed that a regular horizontal transfer of air corresponded to the oscillations of pressure, it held out a promise that further steps in the same path might clear up what appeared to be anomalous, and possibly lead to a complete explanation of the diurnal oscillation.

Some time before this paper reached me, the Rev. M. Lafont had placed in my hands four years traces of a Secchi anemograph, erected on St. Xavier's College, Calcutta, and these having been measured off, tabulated, and reduced, I was interested to find that the diurnal wind variation at Calcutta showed the double diurnal oscillation quite as distinctly, and relatively even more prominently than that of Bombay. But one important difference presented itself. The north and south elements of the oscillation, while agreeing in epoch with those of

[1] This is computed from the hourly observations, recorded during six days, by Capt. E. Trotter, R.E., and of one day by Dr. J. Scully, together with six days' observations by the latter at the hours 4 and 10 A.M. and P.M.

Bombay, were reversed in direction and taken together with the latter, showed a tendency to a cyclonic circulation of the atmosphere around the Peninsula during falling pressure, and an anticyclonic circulation with rising pressure. Moreover, the east and west components agreed almost exactly in epoch with the north and south components, the result being a movement of air from the north-west, with falling pressure, and from the south-east with rising pressure. These facts, taken in conjunction with the positions of Bombay and Calcutta, on opposite sides of the Peninsula, seemed to point to the differential conditions of land and water being probably concerned in the phenomenon. Another and not less important fact connecting the winds with the diurnal oscillation of the barometer appeared at the same time. When the wind variation was analysed by Bessel's method, there appeared an east and west oscillation of considerable magnitude, corresponding in epoch with the barometric inequality expressed by the first periodical term of the barometric formula. This was easily distinguished from the oscillation of the sea and land winds, since the latter is nearly north and south at Calcutta. At Bombay where the sun and land-breezes are nearly east and west, such an oscillation would be undistinguishable, even if it really exists.

The east and west oscillation of diurnal period indicates an outflow of air to the eastward during the daytime, an inflow from the east during the night, and the former phase of it evidently corresponds to the hot winds of the Gangetic plain and northern India, and indeed to the day-winds of the dry months of the greater part of India. They blow towards the sea from the eastward, only in the western portion of the Dakhan, Mysore, &c. This system of day-winds consists of an outflow of air from the Peninsula towards the sea on both coasts, the westerly direction greatly predominating.

The next step in the inquiry was to ascertain what general cause would operate to produce this efflux and influx of air; and the obvious suggestion was that it must consist in the differential action of the sun's heat on dry air and water.

Let V be any volume of dry air at pressure P, and absolute temperature T, and let τ units of heat be communicated to it, raising its temperature from T to $T + t$, while the volume remains constant. The pressure will be thereby increased from P to $P + p$, wherein

$$p = P\left(\frac{T+t}{T} - 1\right) = P\frac{t}{T} \qquad (1)$$

Also

$$\tau = V\varsigma \frac{P}{P}\frac{T_0}{T} tc, \qquad (2)$$

wherein ς is the density of air at the standard pressure P and temperature T_0, and c its specific heat at constant volume, compared with water as unity.

If now the same quantity of heat τ be employed in evaporating water at temperature T (the whole being consumed as latent heat), and filling the volume of air V with vapour at pressure p', the total pressure will become $P + p'$, and

$$\tau = V\varsigma'\frac{p'}{P}\frac{T_0}{T}\lambda$$

where ς' is the hypothetical density of water vapour at P and T_0, and λ its latent heat at temperature T. Substituting for ς' its approximate equivalent $\frac{5}{8}\varsigma$

$$\tau = V\tfrac{5}{8}\varsigma\frac{p'}{P}\frac{T^{\circ}}{T}\lambda \qquad (3)$$

and equating (2) and (3) and eliminating common factors,

$$Ptc = p'\tfrac{5}{8}\lambda$$

$$p' = P\frac{tc}{\frac{5}{8}\lambda} \qquad (4)$$

From (1) and (4)

$$p : p' = P\frac{t}{T} : P\frac{tc}{\frac{5}{8}\lambda}$$

$$p = p'\frac{\frac{5}{8}\lambda}{Tc} \qquad (5)$$

which gives the ratio of the increase of pressure produced by the same quantity of heat, employed in the one case simply in heating dry air, and in the other in charging it with vapour. At a temperature of 80° Fahr. = $T = 541$,

$$p = 7{\cdot}36\,p';$$

that is to say, when a given quantity of heat is employed in heating dry air at the temperature of 80° it raises its pressure more than seven times as much as when it simply charges it with vapour without altering the temperature. With lower values of T the difference will be still greater.

This great difference is no doubt much reduced in nature by the effects of radiation; and while some evaporation is effected on the land surface, there is some increase of temperature over the sea, but it may be expected that some part of this difference will manifest itself in the greater intensity of the forenoon pressure in the lower strata of the atmosphere on the land as compared with the sea, and in fine clear weather as compared with cloudy weather, when banks of clouds present an evaporating surface. With regard to this latter point, it has been shown by Lamont and Kreil's investigations, that between clear and cloudy days, there is a difference of this kind, and that it is manifested not only in the greater magnitude of the diurnal co-efficient U', but also, although to a much less degree, in that of the semi-diurnal co-efficient U'' of the barometric formula. Further evidence of the same kind is afforded by the values of these co-efficients for the several months at Calcutta.

	U'	u'	U''	u''
		° ′		° ′
January	·0287	330 18	·0415	151 34
February	·0319	327 12	·0423	146 48
March	·0343	329 27	·0437	146 44
April	·0361	336 53	·0425	146 38
May	·0325	344 43	·0385	148 13
June	·0218	357 28	·0336	146 23
July	·0192	2 6	·0396	150 30
August	·0218	0 5	·0372	144 29
September	·0232	354 41	·0400	151 25
October...	·0234	343 12	·0393	160 59
November	·0250	337 38	·0399	164 22
December	·0270	335 18	·0411	158 55

The driest months in Northern India being March and April, while July is the wettest and most cloudy.

On Espy and Kreil's hypothesis of the cause of the double oscillation, there is no apparent reason why the evening maximum, arising from contraction and dynamic pressure, should be equal to the morning maximum, which seems unquestionably due to the increased tension of the lower atmosphere in consequence of heating and the introduction of vapour; and any inequality will, of course, appear in the value of U', or of the co-efficients of other terms of odd periodicity. But the fact established by the anemometer that an outflow of air from a heated land area takes place during the day-time, at once assigns a cause for the greater part of the equality, viz., an alteration of the static pressure. This is not an overflow in the upper regions of the atmosphere, but an outflow of the lower strata, or a tendency in that direction. It does not, of course, follow that to produce a reduction in the mass of air over a continent, there should be an actual motion of the air outwards in all directions. The very small forces in action will be manifested even more in retarding in-flowing currents than in accelerating efflux; and it is only in very dry and highly-heated regions, such as India, that they produce well-marked diurnal surface winds, blowing outwards towards the sea;

winds of elastic expansion, such as are the hot winds of India and Australia; winds which are distinct from convection currents, though, it may be, coexisting with and accelerating them. The relations of these winds to the barometric tides are very marked, but it does not seem that the differences of tidal pressure would suffice to generate them, were there not a movement of the air in the same direction arising from more persistent differences of pressure. They probably also depend much on local and irregular differences of pressure.

The air thus removed in the day-time from continental areas must, of course, collect over the nearest areas of evaporation, with the effect of diminishing the mid-day fall of pressure over those tracts; and thus seems to be explained those apparent anomalies in the magnitude of the mid-day semi-oscillation of the barometer to which, in the passages quoted from Mr. Buchan's memoir, he has drawn attention, viz., in the case of the Mediterranean area and the Atlantic coast of North America.

The direction in which this movement of the air takes place will, of course, vary with the locality, but there will always be, on an average, a greater diurnal movement towards east coasts than towards those facing to the west. This may be illustrated by the case of Calcutta and Bombay, and it is more extensively illustrated by the predominant westerly direction of the land-winds of India, and the cold westerly diurnal winds[1] that blow across the high plains (17,000 to 19,000 feet) of the Changchenmo and Rupshu in Western Tibet. The reason is sufficiently obvious. As the great waves of pressure advance from east to west, the local barometric gradient of any place (in so far as it is determined by the diurnal oscillation) will be expressed by a tangent to the existing phase of the wave. During the hottest part of the day, viz., from 9 or half-past 9 to half-past 4 or 5, this gradient (which is the steepest and most prolonged of the four) inclines to the eastward, and increases the declivity towards east coasts arising from the excess of pressure over the land. In the opposite direction, viz., towards west coasts, it goes to diminish that declivity. At night the case is reversed. The west to east barometric gradient from 10 P.M. to half-past 3 or 4 A.M. is in the same direction as that tending to produce an influx of air from the sea towards the land on west coasts; this, however, is opposed to the land wind of the coast line, which is a true convection current, and arises from quite different causes; and, although traceable in the wind variation at Bombay, it there manifests itself only by decreasing the velocity of the former. There are, moreover, independent grounds for the inference that this compensating in-flow chiefly affects the higher strata of the atmosphere, while the day wind is chiefly produced in the lower and more heated strata. At Calcutta the easterly (or negative westerly) tendency of the wind at night is very prominently exhibited in the curve of diurnal variation, but although of longer duration it is at no time so intense as the westerly tendency in the early afternoon hours.

In like manner may be explained the difference of epoch of the corresponding phases of the semi-diurnal east and west variation at Calcutta and Bombay. The gradient of pressure, in so far as it depends on the semi-diurnal oscillation, will, of course, be to the west with a rising pressure, and to the east with a falling pressure, and this normal tidal gradient is affected by the small difference of amplitude over land and sea, in such manner that its changes will be accelerated as affecting east coasts, and retarded as affecting west coasts. Now if we suppose that the acceleration in the one case and the retardation in the other amount to an hour or an hour and a half, and that the interval between the change in the direction of the gradients, and their effects on the wind, as manifested by the anemometer, is also about an hour and a half, we should roughly reproduce the conditions shown to exist at Calcutta and Bombay respectively.

According to this view, the local static pressure of the atmosphere, apart from irregular movements, is shown by the height of the barometer at the hours of minimum pressure, and the difference of these expresses the weight of the atmosphere removed and restored by the oscillatory movements chiefly between land and sea.

I should add, in conclusion, that this communication is merely a *résumé* of some of the more salient topics discussed in two papers, "On the Winds of Calcutta," and "The Theory of Winds of Elastic Expansion," which will shortly be published *in extenso* elsewhere.

H. F. BLANFORD

[1] This I state on the authority of Dr. Cayley, who assures me that on the high plains these afternoon winds are always from the west.

CARBONIFEROUS LAND SHELLS

IN a recent visit to the South Loggius, in Nova Scotia, in which I was assisted in the examination of the cliff by Mr. Albert J. Hill, Manager of the Cumberland Coal Mine, we found a number of well-preserved shells of *Pupa vetusta*, in the indurated clay, filling an erect sigillaria, in a bed considerably higher than those in which the shell was previously known. It is nearly in the middle of group xxvi. of my section of the South Loggius, 222 feet above the main coal-seam, 842 feet above the bed in which the species was first recognised by Sir C. Lyell and myself, and about 2,000 feet above the lowest bed in which I have yet found it. It thus appears that this little pulmonate continued to flourish in the carboniferous swamps, after its remote ancestors had been covered with 2,000 feet of sediment, including many beds of coal, and nearly the whole thickness of the productive coal-measures. *Conulus priscus*, the only other land-snail found in this section, on the other hand occurs only, so far as known, in the lowest of the beds above-mentioned. Two other carboniferous land-shells, *Pupa vermilionensis*, Bradley, and *Dawsonella Meeki*, Bradley, have been found in the coal-field of Illinois; and it is worthy of remark that, according to Dr. P. P. Carpenter, all the four species belong to distinct generic or sub-generic forms, and that all these forms are still represented on the American Continent.

On the same visit, we were so fortunate as to find another large sigillarium stump, rich in reptilian remains, which it is hoped may, on examination, afford new species and further information on those already known.

J. W. DAWSON

THE BIRDS OF KERGUELEN'S LAND[1]

AS regards the publication of results achieved by the naturalists accompanying the recent Transit expedition, our American friends appear to be getting the start of us. While we are engaged in issuing "preliminary reports," they have already arranged and classified their collections, and are beginning to publish their discoveries. The specimens of birds obtained by Dr. Kidder, surgeon and naturalist attached to the astronomical party at Kerguelen's Land, or Desolation Island, have been placed for determination in the hands of Dr. E. Coues —one of the most competent zoologists in the United States—and the result has been the very interesting memoir now before us. We knew already that Kerguelen's Land was not an inviting place of residence for the more highly organised animals, and that few birds were to be found there. We know now what those few are, and have full particulars about most of them, their lives, and habits. According to Dr. Coues' determination, Dr.

[1] "Bulletin of the United States National Museum," No. 2. Contributions to the Natural History of Kerguelen Island, made in connection with the American Transit of Venus Expedition, 1874-75. By J. H. Kidder, M.D. I. *Ornithology*. Edited by Dr. Elliott Coues, U.S.A., 8vo. 52 pp. (Washington, 1875.)

Kidder's collection contains examples of twenty-one species of this class, belonging to six families, namely, eleven Petrels, four Penguins, three Gulls, a Cormorant, a Duck, and a Sheath-bill. Of these, the two last-named are "the only partial vegetable feeders observed, all the other birds feeding exclusively on flesh, fish, or marine invertebrates." Of the *Chionis*, or Sheath-bills, a singular abnormal form related to the Plovers, of which there are (or were lately) living specimens in the Zoological Society's Gardens, Dr. Kidder might well have sung, in the words of the old song, "their tameness is shocking to me." "They would scarcely get out of my way," says the Doctor, "and seemed greatly interested in my movements. When I sat on a stone, keeping perfectly still, the whole party, twelve in all, came up to examine the intruder. They walked all around me, coming almost within reach; others flying up from more distant rocks to join them, and finally stopped, almost in a semi-circle, for a good stare. After watching the birds for a time, I shot four specimens, not without compunction, on account of killing such trustful acquaintances. When I walked off to get a sufficient distance away for a shot, the whole troop started to follow me, making little runs and stopping, as if filled with curiosity. I shot all four without moving from the spot, reloading for each, the birds not all flying out of range even after the gun had been fired. On subsequent occasions, various members of the party captured specimens by hand; all that was necessary to attract them within reach being to remain perfectly still. After one had been caught it served as a lure for others. When taken home alive they still showed no fear, but when let loose in the house took food readily."

Another curious fact observed is that in the absence of true birds of prey in Kerguelen's Land, the Skua of the Southern Seas (which Dr. Coues, widely departing from the ordinary binomial system designates as "*Buphagus skua antarcticus* (Les.), Coues"), appears to have taken upon itself all the habits and practices of a Buzzard or Kite. "It was at first taken for a hawk by all of us; its manner of flight, watchfulness of the ground over which it flew, and habit of perching on spots commanding a wide view, all suggested this impression. It was, indeed, difficult to believe the evidence of my own senses when I found a web-footed bird avoiding the water and preying solely, so far as my observations extended, upon other birds. When any of the party went out shooting he was pretty sure to be followed by one or two 'sea-hens,' as the sealers call them, and had often to be very prompt to secure his game before it should be carried off in his very presence."

Many details are likewise given respecting the habits of the other nineteen species observed, and great credit is due to Dr. Kidder and Dr. Coues for the speedy manner in which they have put together this interesting memoir. But what Mr. Eaton, the English naturalist at Kerguelen, and Mr. Sharp, who, we believe, has been, or is working out his birds, will say to it, we cannot tell. We fancy they will not be very much pleased at being thus forestalled.

MAYER'S RECENT ACOUSTICAL RESEARCHES[1]

THIS communication is merely a preliminary note, to be followed by an elaborate paper on the above subjects. For conciseness and clearness, I present the few facts I have now to offer in the form of notes of experiments:—

Experimental Observations on the Obliteration of one Sound by another.—Several feet from the ear I placed one of those loud-ticking spring-balance American clocks, which make four beats in a second. Then I brought quite close to my ear a watch (made by Lange, of Dresden) ticking five times in the second. In this position I heard all the ticks of the watch, even those which coincided with every fourth tick of the clock. Let us call the fifth tick of the watch which coincided with one of the ticks of the clock, its fifth tick. I now gradually removed the watch from the ear, and perceived that the fifth tick became fainter and fainter, till at a certain distance it entirely vanished, and was, so to speak, "stamped out" of the watch.[1]

Similar and more striking experiments were made with an old silver watch, beating four times to the second, by causing this watch to gain about thirty seconds an hour on the clock, so that at every two minutes the ticks of the watch and clock exactly coincided. When the watch was held near the ear, every one of its ticks was heard distinctly; but on gradually removing it from the ear, the ticks of the watch became fainter and fainter at the coincidences, and when the watch had been removed to a distance of nine inches from the ear, the ticks of the watch were utterly obliterated during *three* whole seconds of its ticks about the time of coincidence. On removing the watch to a distance of twenty-four inches, I found that I lost its ticks during *nine* seconds about the time of coincidence. It is here important to remark that the ticks of the clock are *longer* in duration, as well as *lower* in pitch, than those of the watches. With the watch remaining at the distance of twenty-four inches from the ear, I listened with all my attention, as tick by tick the watch approached the time of coincidence. Since the ticks of the watch are shorter in duration than those of the clock, they are *overlapped* by the other about the time of coincidence. Hence as, so to speak, the short ticks of the watch glided, tick after tick, under the long ticks of the clock, I perceived that more and more of the duration of each successive watch-tick became extinguished by the tick of the clock, until only the *tail* end of the short tick of the watch was left audible, and at last even this also crept under the long tick of the clock, and the whole of the ticks of the clock were rendered inaudible for *nine* seconds, at the end of which time the front or *head* of the watch-tick, as we may call it, protruded beyond the clock-tick, and then slowly grew up into a complete watch-tick as before. In this succession of events the tick of the old silver watch (made by Tobias) disappears with a sharp *chirp*, like a cricket's, and re-appears with a sound like that made by a boy's marble falling upon others in his pocket. By this experiment, therefore, a gradual analysis is made of the effect of the tick of the clock on the tick of the watch, affording a beautiful illustration of the fact that one sonorous sensation may overcome and obliterate another.

Experiments to determine the relative intensity of the Clock-ticks which obliterate three Watch-ticks.—The clock was placed on a post in the middle of an open level field in the country, on nights when the air was calm and noiseless. The ticks of the clock became just inaudible when my ear was removed to a distance of 350 feet. The ticks of the watch became just inaudible at a distance of twenty feet. The ratio of the squares of these numbers makes the ticks of the clock about 300 times more intense than those of the watch. On the same nights that I made the above determinations I also put the clock on the post, and placing against my zygomatic process a slender stick graduated to inches and tenths, I stood with my ear at distances from the clock of from eight to sixteen feet, and then slid the watch above and along the stick (taking care that it did not touch it) until it reached such a distance from the ear that its fifth tick just disappeared. Knowing the relative intensities of the ticks of clock and watch when placed at the same distance from the ear, the law of the reciprocals of the squares gives the relative intensities when the clock and watch are at the several distances obtained in the above experiments. Large numbers of such experiments have been made, and the results agree perfectly well when we take into consideration first, the difficulty

1 "On the Obliteration of one Sonorous Sensation by the simultaneous action of another more intense and lower Sound, and on the discovery of the remarkable fact that a Sound, even when very intense, cannot obliterate the sensations of another Sound Lower than it in Pitch; with Applications of these Discoveries to Measures of the Intensities of Sounds, and to the Proper Method of Conducting Orchestral Music." By Alfred M. Mayer, Ph.D., Member of the National (American) Academy of Sciences, and Professor of Physics in the Stevens Institute of Technology, Hoboken, New Jersey, U.S. America. Read before the National (American) Academy of Sciences, in Washington, April 20, 1876, and now first printed from the manuscript sent through Mr. Alex. J. Ellis, F.R.S.

1 The precise number of ticks in a second here mentioned are not necessary for roughly observing and understanding these phenomena. I observed them by a common American pendulum clock placed on a table, which increased the power of its half-second ticks, and a watch beating five times in two seconds. Rev. Mr. Haweis informs me that he has often noticed a similar effect at night with ordinary watches. The sensation produced by the obliteration of the tick, when the proper distance of the watch from the ear has been attained, and the consequent sudden division of the ticks into periods separated by silences, is very peculiar. It is difficult not to believe that some accident has not suddenly interfered with the action of the watch, instead of merely with our own sensations.—A. J. E.

thrown in the path of the determinations by the *gradual* fading away of the watch-ticks as they approach coincidence with the clock-ticks; and, secondly, the impossibility of arriving at *any* result at all, if the slightest noise (the rustle of a gentle breeze, the piping of frogs, the bark of a distant dog) should fall on the ear of the observer when engaged in making an experiment. The general result of the numerous experiments thus made shows that the sensation of the watch-tick is obliterated by a coincident tick of the clock, when the intensity of the clock-tick is *three times* that of the watch-tick. This result, however, must be regarded as merely approximative, not only from the manner in which it was obtained, but from the *complexity* of the sounds on which the experiments were made. It is interesting, however, both as being, I believe, the first determination of this kind that has ever been made, and as having opened out a new and important field of research in physiological acoustics.

Experiments on Musical Sounds.—Reserving the further development of my discoveries to a future paper, I will now briefly describe some of the more prominent and simple phenomena, which I discovered in experimenting with *musical sounds*. At the outset I will remove an objection always made by those versed in acoustics, but unacquainted with these new phenomena. It is as follows:—"You say that one sound may obliterate the sensation of another; but are you sure that the real fact is not an alteration of the *quality* of the more intense sound by the action of the concurrent feebler vibration?" I exclude this objection by experimenting as follows:—An open or closed organ-pipe is sounded forcibly, and at a few feet from it is placed the instrument emitting the sound to be obliterated, which may be either a tuning-fork on its resonance box, or a closed organ-pipe communicating with a separate bellows. Suppose that in the following experiment both tuning-fork and closed organ-pipe produce a note higher in pitch than the more intense or extinguishing sound of the open organ-pipe. Now sound the fork alone strongly, and alternately shut and open its resonance box with the hand. We can thus obtain the sound of the fork in a *regular measure of time.* When the ear has well apprehended the intervals of silence and of sound thus produced, begin the experiment by sounding the open pipe and tuning-fork simultaneously. Now, if any change is thus effected in the quality of sound emitted by the open pipe, this change cannot occur except when the pipe is sounding, and hence, if it occurs at all, it must occur in the regular measure in which the fork is sounded. The following are the facts really observed. At first every time that the mouth of the box is open, the sound of the fork is distinctly heard, and changes the quality of the note of the open pipe. But as the vibrations of the fork run down in amplitude, the sensations of its effect become less and less, till they soon entirely vanish, and not the slightest change can be observed in the quality or intensity of the note of the open organ-pipe, whether the resonance box of the fork be open or closed. Indeed at this stage of the experiment the vibrations of the fork may be suddenly and totally stopped without the ear being able to detect the fact. But if instead of stopping the fork when it becomes inaudible, we stop the sound of the open organ-pipe, it is impossible not to feel surprised at the strong sound of the fork which the open pipe had smothered and had rendered powerless to affect the ear. If we replace the tuning-fork by a closed organ-pipe of the same pitch, the results will be the same, but in this case I adjust the intensity of the higher closed pipe to the point of extinction by regulating the flow of air from the bellows, by a valve worked with a screw. The alternation of sound and silence is obtained by closing and opening the mouth of the closed pipe by the hand.

High Sounds cannot obliterate Low Sounds.—A new and remarkable fact was now discovered. No sound, even when very intense, can in the slightest degree diminish or obliterate the sensation of a concurrent sound which is lower in pitch. This was proved by experiments similar to the last, but differing in having the more intense sound higher (instead of lower) in pitch. In this case, when the ear decides that the sound of the (lower and feebler) tuning-fork is just extinguished, it is generally discovered on stopping the higher sound, that *the fork*, which should produce the lower sound, *has ceased to vibrate.* This surprising experiment must be made in order to be appreciated. I will only remark that very many similar experiments have been made, ranging through four octaves, and have been observed by a score of different ears, with the same invariable result. It is important to understand that this phenomenon depends solely on the *difference* of pitch, and not at all on the absolute pitch of the notes. Thus a feeble c''' (1024 double vibrations) is heard as distinctly through an intense e''' (1280 double vibrations) as a feeble c (128 double vibrations) is heard through an intense g (192 double vibrations) or an intense c' (256 double vibrations).

The development of the applications and of the further illustrations of these discoveries would occupy too much space; I must therefore restrict myself to mentioning some of the most interesting. Let a man read a sentence over and over again with the same tone and modulation of voice, and while he is so doing forcibly sound a c pipe (256 double vibrations). A remarkable effect is produced, which varies somewhat with the voice experimented on, but the ordinary result is as follows. It appears as though two persons were reading together, one with a grave voice (which is found by the combination of all the real reader's vocal sounds below c in pitch, or having less than 256 double vibrations), the other with a high-pitched voice, generally squeaky and nasal, and, I need not add, very disagreeable. Of course the aspirates come out with a distressing prominence. I have observed many curious illustrations of this change in the quality of the tone of the voice, caused by the entire or partial obliteration of certain vocal components, while listening to persons talking during the sound of a steam-whistle, or in one of our long, resonant American railway carriages. Experiments similar to those on the human voice, can be made, with endless modifications, on other composite sounds, as those of reed-pipes, of stringed instruments, of running water, &c. With one of my c (128 double vibrations) free Grenié reeds, I get very marked results. Using as a concurrent sound an intense c' (256 double vibrations) I perceive the prime or fundamental simple tone c to be unaffected in intensity, while all the other partial tones (higher harmonies or overtones, as they are sometimes called) are almost obliterated, except the fifth partial (or fourth upper partial) e'', of 640 double vibrations, and the sixth partial (or fifth upper partial) g'' (of 768 double vibrations), which come out with wonderful distinctness. The fact that the lowest, or prime partial tone in the majority of ordinary compound musical tones is the strongest, is due (among other reasons) to the fact that the sensation of each partial tone of which the whole musical tone is composed, is diminished by the action on the ear of all the components or partial tones, *below* it in pitch. Thus the higher the pitch of any component or partial tone, the greater the number of lower components which tend to obliterate it. But the prime, or lowest component partial tone, is not affected by any other. Another illustration I cannot resist giving. At the end of the street in New York, in which I now reside, there is a large fire-alarm bell, the residual sound of which, after its higher components have disappeared, is a deep simple tone. This bass sound holds its own with total indifference to the clatter of horses, or to any sounds *above* it in pitch. It dies out with a smooth gradient, generally without the slightest indentation or break produced by the other sounds of the street. Indeed in this case, as in all others where one sound remains unaffected by intense higher notes, the observer feels as though he had a special sense for the perception of the graver sound—an organ entirely distinct from that which receives the impress of the higher tones.

That one sonorous sensation cannot interfere with another which is lower in pitch, is a remarkable physiological discovery, and next after the demonstration of the fact that the ear is capable of analysing compound musical sounds into their constituent or partial simple tones, is probably the most important addition yet made to our knowledge of the nature of hearing. It cannot fail to introduce profound modifications into the hypotheses heretofore framed respecting the mechanism and functions of the ear.

Application to Orchestral Performances.—We have seen how an intense sound may obliterate, entirely or in part, the sensations of certain partial tones or components of any musical tone, and thus produce a profound change in its quality. In a large orchestra I have repeatedly witnessed the entire obliteration of all sounds from violins, by the deeper and more intense sounds of the wind instruments, the double-basses alone holding their own. I have also observed the sounds of the clarinets lose their peculiar quality of tone and consequent charm from the same cause. No doubt the conductor of the orchestra heard all his violins, ranged as they always are close around him, and did not perceive that his clarinets had lost that quality of tone on which *the composer* had relied for producing a special character of expression.

The function of the conductor of an orchestra seems to be threefold. First, to regulate and fix the time. Secondly, to regulate the intensity of the sounds produced by the individual instruments, for the purpose of expression. Thirdly, to give the proper quality of tone or *feeling* to the whole sound of his orchestra, considered as a single instrument, by regulating *the relative intensities* of the sounds produced by the various classes of instruments employed. Now this third function, the regulation of relative intensities, has hitherto been discharged through the judgment of the ears of a conductor who is placed in the most disadvantageous position for judging by his ears. Surely he is not conducting for his own personal gratification, but for the gratification of his audience, whose ears stand in very different relations from his own in respect to their distance from the various instruments in action. Is it not time that he should pay more attention to his third function and place himself in the position occupied by an average hearer? This position would be elevated, and somewhere in the midst of the audience. The exact determination of its place would depend on various conditions which cannot now be considered. That the position at present occupied by the conductor of an orchestra has often allowed him to deprive his audience of some of the most delicate and touching qualities of orchestral and concerted vocal music I have no doubt, and I firmly believe that when he changes his position in the manner now proposed the audience will have some of that enjoyment which he has too long kept to himself. During the past winter, in the Academy of Music at New York, I fully confirmed all the foregoing surmises, by placing myself in different parts of the house to observe the different results, and my opinions were fully shared by others who have a more delicate musical organisation than I can lay claim to.

In large orchestras, these interferences of sonorous sensations are so multiplied and various as to be beyond our mental conception. By taking them up in detail, some general laws may, however, be evolved. But it will be impossible to formulate such laws until, firstly, we are in possession of a *quantitative* analysis of the compound tones of all musical instruments (that is, until we know the relative loudness of the partial tones of which they are composed at all parts of their compass), and secondly, we have determined throughout the musical scale the relative intensities of the sounds (of simple tones) when obliteration of the sensations of higher (simple) tones supervenes. The powerlessness of one sound to affect the sensation due to another sound lower than itself in pitch greatly simplifies this problem.

Quantitative analysis of the compound tones of musical instruments is now the great desideratum of the composer. It is only after we know the relative intensities of the components of typical musical tones used in orchestral performances, that we can so regulate their intensities as to give those qualities of sound which the composer desires to be heard. Thus, it at once becomes evident that the instruments used in orchestral music should be very differently constructed from those used for solos or quartets. In orchestral instruments certain *characteristic* upper partials (overtones, harmonics) should predominate, in order to find expression in the midst of other and graver sounds. Such orchestral instruments will therefore have exaggerated peculiarities in their qualities of tone, which will render them unfit to be played on alone, and uninfluenced by other orchestral notes. It is surely not hopeless to anticipate that empirical rules may be attained, which will guide the musical instrument-maker to the production of those special qualities of tone required in orchestral instruments. It is fortunate that the very phenomena of the interferences of sonorous sensations will assist in the much desired solution of the problem of measuring the intensity of a sound (simple tone), either when existing alone or as component of an ordinary musical (compound) tone. On this subject I am now engaged. It is evident (by way of illustration), that so far as concerns the measure of the relative intensities of sounds *of the same pitch*, this problem has already received the simplest solution by merely placing these sounds at various distances, and obliterating the sensations they excite by means of a constant and standard sound of a lower pitch. But I reserve a description of this work for a more formal publication.

NOTES

PROF. HUXLEY, who has recently left for America, has accepted an invitation from Prof. W. B. Rogers to attend the Buffalo meeting of the Association for the Advancement of Science, and also to deliver a course of lectures before the Johns Hopkins University. His stay, however, in the country will be but short.

THE Academy of Sciences of the Institute of Bologna announces an open competition for the Aldini Medal, to be awarded to the author of the memoir of greatest experimental and scientific value in galvanism. The medal is of gold, of the value of 1,000 liras, and is open to all works which profess to have extended our knowledge in any department of galvanism, and which may be sent to the Academy expressly for the competition, during the two years comprised between June 1, 1876, and May 30, 1878. Memoirs must be written in Italian, Latin, or French. The usual conditions of such competitions are to be observed, and memoirs should be sent in before the last-mentioned date, addressed "Al Segretario perpetuo dell' Accademia delle Scienze dell' Istituto di Bologna."

WE notice in the *Revue Scientifique* further particulars regarding the meeting of the French Association for the Advancement of Science, to be held at Clermont-Ferrand on the 18th inst. A list of the papers to be read is also given. This is a very useful arrangement for those who may anticipate taking part in the proceedings, and others, and might with advantage, we think, be copied in this country. In the group of physics and chemistry we note the following among the subjects to be treated:—Diffraction in optical instruments; new volumetric determinations of arsenic; new salts of bismuth; experiments made to determine if the ether is ponderable; observations in celestial and terrestrial physics in Japan and Siam (by M. Janssen); thermo-diffusive properties of cast-iron; the idea of unity in chemical and cosmic phenomena; the radiometer, &c. In the group of natural sciences:—Vichy waters, from a physiological and hygienic point of view; recent prehistoric discoveries in Medoc; animal heat; influence of the want of air and light in the streets and houses on health; functions of leaves and roots of plants in tropical countries; cure of paralysis by continuous currents; operations for cataract; the bite of vipers; ophthalmia in the North of Africa; proof of the existence of ferment-germs in the organism as in the air; a new æsthesiometer; production of phenomena of synthesis in plants; sporadic and endemic goître in Puy-de-Dôme; on measles in beef and inermous tænia; resources of France as regards war-horses; various points in local archæology, geology, palæontology, &c. In the group of economical sciences:—Teaching of living languages, from the economical point of view; remedies for phylloxera; depopulation of the country and emigration to America; workmen's dwellings and morality of France; economical consequences of the war indemnity, &c., &c.

THE storm of August 3 will be long remembered not only as being about the heaviest summer gale that has occurred for many years, but also as having been most disastrous to life and property among the fishing population. It broke out on the fishermen on the east coast just when their nets had been shot for the night at distances of twenty miles, and upwards, out at sea. The value of the nets lost at Aberdeen alone is estimated at 4,000*l*. The rate of the fall of the barometer being nearly an inch in twenty-four hours, the point to which it fell being about 29·0 inches at sea-level over a wide district in the north, the time during which it remained low, and the large and comparatively rapid rise which followed are rather the characteristics of our more marked winter storms. A storm of this nature is, therefore, deserving of a very careful investigation, chiefly with the view of ascertaining how far it might have been possible to have given the fishermen some intimation beforehand of its peculiarly destructive character.

IN the *Bulletin International* of August 3, M. de Tastes relates some interesting particulars of a waterspout (*trombe*) which was observed near Tours, on May 25, 1876. It first appeared as a mass of whitish vapour against a background of dark-coloured clouds, which gradually assumed the form of an inverted cone pointing to the ground, and terminating in a long sinuous band. A whitish sinuous column soon appeared suddenly between it and the ground, and rapidly enlarged upwards, the whole phenomenon soon assuming the appearance of two cones united at their summits. The lower cone, at first lightish-coloured and in a certain degree transparent, gradually assumed a darker shade, which was propagated from the base towards the summit. When passing over the right bank of the Loire, a dense mass of sand, mud, and fine gravel, was observed drawn towards it; in crossing the river a *jet d'eau* broken into spray appeared in the form of a cone ascending the waterspout, with its base resting on the water, the spray on all sides being drawn inwards towards the axis in spires. It is said that an undefined glimmering appearance preceded the column of ascending spray. On reaching the extensive sandy shore of the left bank, clouds of sand were drawn violently in upon it, just as happened with the spray of the river. From the value of several of these points in the theory of waterspouts and other aërial movements, it is desirable, as opportunity offers that they be tested by observations made with the greatest accuracy and skill.

MR. F. E. NIPHER writes to the *American Journal of Science and Arts*, that not long since, while writing logarithms that were being read to him, he observed that the probability of error in writing the numbers appeared to be much less at the extremities of the number than in the middle. This he investigated at length in numbers of from five to ten digits. It was found that the probability of error is in all cases expressed by the terms of the expanded binomial $(a + b)^n$, where n is a function of the number of digits. a and b were, so far, always unequal with all the persons that had been experimented on. The probability of error is greatest just after the middle of the number. This led to an interesting investigation on the power of memory. Allowing definite intervals (t) of time to elapse between the giving and the writing of the number, it is evident that the number of errors will increase with the value of t. In order to aid the experimenter in abstaining from mentally repeating the number which he is to write, he is allowed to determine the value of t by counting the beats of a seconds pendulum. The investigation is yet in progress, but enough has been done to develop the fact that the relation between the number of figures (per 100) written correctly, and the values t, is a logarithmic one. It is the same as the function expressing the decrease in the amplitude of the beats of a pendulum in time, as due to a resisting medium.

WE learn from the same journal that the trustees of the Massachusetts Society for promoting Agriculture have offered some very handsome prizes for special plantations within the State of Massachusetts. In the first place, for the best plantation of not less than five acres of larch, or on the Cape, &c., of Scotch or Corsican pine, originally of not less than 2,700 trees to the acre, on poor, worn-out, or otherwise agriculturally worthless land, a prize of $1,000. For the next best, a prize of $600; for the third best, $400. Next, for the best plantation of the same extent with American white ash, not less than 5,000 trees to the acre, a prize of $600; for the next best, $400. Intending competitors must notify the Secretary of the Society, E. W. Perkins, Jamaica Plain, Boston, as early as December 1, 1876, and plant in the spring of 1877. Special directions, not only for planting and caring for, but also for procuring trees for the purpose, are given in a recently-published pamphlet by Prof. Sargent, of Harvard, "A Few Suggestions on Tree-planting," which the Society has reprinted for gratuitous distribution; and a citizen of Boston patriotically offers to look after the importation of the seedling trees, which, in such quantities, and for next year's planting, would have to be obtained mainly in Europe, at least the pines and larches. The ashes, probably, would have to be raised from seed; and the time, if need be, would doubtless be extended. The prizes are to be awarded in the summer of 1877.

AMONG various experiments with the radiometer which have lately been described to the French Academy, is one in which M. Govi inclosed a very sensitive instrument (the vanes of which were of polished aluminium on the one side and blackened mica on the other) in a glass cylinder, into which was continuously passed steam from boiling water. The radiometer began quickly to rotate (the aluminium face first) immediately the steam commenced to raise the temperature of the inclosure. Ere long, however, the temperature becoming invariable, the rotation diminished, and after a few minutes ceased altogether. On stopping the entrance of steam, the instrument rotated anew, but in the opposite direction, and did so for a long time before stopping. Every motionless radiometer, M. Govi points out, is like the instrument stopped at 100° in the above experiment. To make it turn in the inverse direction, you have merely to put it in a vessel of cold water; the black faces then move first, and the instrument only stops after a new state of thermal equilibrium has been established On being brought out of the cold water it turns as though it were struck by light, although it may be in complete darkness. A radiometer motionless in the inclosure at 100°, or at zero, will turn anew if the light of a bright flame be directed on the blackened face of its vanes; "because in both cases the light absorbed by the blackened face then becomes heat, which is added to that which the vanes possess already, and may consequently further liberate gas from them." In an experiment described by M. Ducretet, ether is poured on the envelope of a radiometer which moves with *direct* rotation (black surfaces repelled) in moderate daylight. The motion is arrested and changed to that in the *inverse* direction. This reaction presently ceases, and the vanes resume the original *direct* motion, notwithstanding the evaporation maintained on the envelope by a light sprinkling of ether. The rotation now becomes more rapid than it was at first, the evaporation apparently acting as a source of heat, and yet the lowering of temperature through evaporation is very perceptible. When the sprinkling with ether ceases, the motion resumes its normal velocity and remains *direct.* M. Ducretet also tried the effect of phosphorescent powders on the radiometer, but got no motion.

THE number of visitors to the Loan Collection of Scientific Apparatus during the week ending August 5 was as follows:—Monday, 2,951; Tuesday, 3,377; Wednesday, 488; Thursday, 441; Friday, 441; Saturday, 3,422; total, 11,120.

AN interesting contribution to the study of the eye affection known as neuro-paralytic keratitis, by Dr. Decker, has just appeared in the *Archives des Sciences.* He arrives at the following conclusions:—(1) It is not an ordinary traumatic keratitis. (2) It results from the combined action of two orders of things, *a.* determining causes, which are the exterior modifying agents; *b.* a predisposing cause, consisting in diminution of the resistance of the eye, the most exposed parts of which (cornea), become easily altered by the determining causes. (3) This vulnerability is the result of lesion of nerve fibres in the internal side of the trigeminus. 4. These are neither sensitive nor vasomotor nerves. 5. The hypothesis that they are trophic nerves best accounts for the facts observed. 6. Anatomically, neuroparalytic keratitis consists of a primary necrosis of the central part of the cornea (if the latter be left open), followed in a short time by a secondary inflammation of the peripheric parts, and of the conjunctiva.

MM. BECQUEREL give a brief notice in the *Bulletin Hebdomadaire*, No. 456, of the Scientific Association of France, of the observations of temperature made at the Museum of

Natural History, during 1875, with electric thermometers placed in the air, and in soils covered with grass and soils cleared of vegetation. From the results of the last four years, it is shown that the mean annual temperature of the two soils, at a depth of 39 inches, and that of the air, is nearly alike; that at depths of from 4 to 24 inches the influence of vegetation is to raise the annual mean 0°·7 above that of soils clear of vegetation; and that during these four years the temperature of soils covered with grass or any other vegetation has not fallen to freezing (32°), a fact of no little importance to horticulture.

EXPERIMENTS were made at Paris recently, before M. Baron, Director of the Electric Telegraph, on a new system for dividing the electric light. A single generator has fed with an admirable regularity not less than eighteen lamps, having each a power equal to 100 gas-jets. The effect was wonderful, and the apparatus will be tried shortly at the Lyons railway terminus. The principle is very simple, and was discovered by a working shoemaker. The current derived from a Gramme machine, slightly modified, is sent to a second machine, which rotates before forty-eight electro-magnets, four of these electro-magnets having a force sufficient to give a light equivalent to 100 gas-jets. Twelve electric lamps can be fed at any distance. By a very simple commutator any number of these twelve lamps can be grouped together, so that one, two, or more can be set in the same apparatus. Twelve working on the same point give a real burning sun. The force required for working both machines (the prime mover and the distributor) is derived from a 4 horse-power steam-engine. The experiments at the Lyons railway will be tried with sixteen lamps and an engine of from 6 to 7 horse-power. The light will be equal to 1,600 gas-jets.

THE French Society of Agriculture and Insectology will, as usual, hold its bi-annual exhibition at the Orangerie des Tuileries in September. The exhibition being universal, some contributions are expected from England. The last having been a success, left a large surplus in the hands of the Society, which will enlarge the scale of its operations.

SOME details regarding the malacological fauna of the Island of Saint Paul have been furnished by M. Velain, in a note to the French Academy, and will doubtless interest zoologists. Little was previously known of this fauna. The island, it is known, is more than 500 leagues distant from any continents, and the tranquil lake in the old crater of the volcano seemed likely to favour the development of embryos brought by oceanic currents. The list of Gasteropoda and Lamellibranchia comprises forty species, distributed in twenty-nine genera, five of which are new. This fauna, notwithstanding the small latitude of the island, is remarkable for its austral forms. The species are mostly of small size, rarely exceeding 3 mm.; among them appear as a giant the Ranella described by Frauenfeld, which sometimes reaches 8 cm. in height. The island may be said to have two distinct fauna, that of the interior of the crater and that of the exterior; the latter is less rich; the abrupt sides, environed with reefs on which the sea incessantly breaks with violence, being hardly favourable to the thriving of marine molluscs. The species here have short, rounded forms, with thick shells. Within the crater the littoral zone is extraordinarily rich in individuals, though not in species. The conditions are: a rocky bottom exposed to the light, weak pressure, temperature kept nearly constant by thermal springs (13° to 14° C.), agitation almost *nil*, marine vegetation extremely abundant. As for deep fauna, there is none of it; the abundant liberation of carbonic acid gas at the bottom of the crater prevents life being manifested below 20 to 25 metres. The deep fauna of the exterior, on the other hand, is very rich, as indicated by the shells thrown up on the beach. The fauna of Amsterdam Island is identical with that of the exterior of Saint Paul, only the proportion of the different species varies. There is, however, a gasteropod of the genus *Helix*, which is peculiar to the island.

IT was proved, a short time ago, that several kinds of seeds will germinate between pieces of ice. A full investigation of the lower limit of temperature at which plants may germinate has recently been made by M. Haberlandt (*Centralblatt für Agricultur chemie*). The experiments were upon wheat, rye, barley, red beet, rape, lucerne, poppy, and many other seeds. Several hundred seeds were employed of each species, and every fourteen days the seeds were taken out of the ice-chest, whose temperature was kept constant between 0° and 1°, and examined in a space whose temperature was under freezing-point. In forty-five days a decided beginning of germination was observable in eight different species (which are named). In four months it had continued to progress in a minority of these; the rest had stopped. In fourteen species there was no germination. M. Haberlandt is of opinion that those seeds which can germinate at a lower temperature than others of the same species, will give plants that require a less amount of heat for their complete development than the others, and thus by artificial sowing in cold spaces a means is to hand of obtaining species soon ripe and needing little heat. Of all the seeds which had remained for four months in the ice-case, only a few were found capable of development when brought into a warmer temperature of 16° C.

A UNIVERSAL CONGRESS for hygienic purposes and salvage will be held at Brussels on the occasion of the Exhibition. The Congress will meet from Sept. 27 to Oct. 4. A French committee has been formed of M. Claude Bernard, Admiral Paris, and others. A programme of the questions that are to come before the meeting will be found in the *Sanitary Record* for August 5.

THE *Meteorologische Beobachtungen* made at the hydrographic office of the Austrian navy at Pola during June last have been received. They are interesting from the position of Pola being near the southern extremity of the peninsula at the head of the Adriatic. The hourly observations show a strongly-pronounced maximum of wind force from 11 A.M. to 6 P.M., when it is nearly double the force registered from 9 P.M. to 6 A.M. The daily variation in the direction of the wind is equally well marked. Starting from a point east of south at 5 A.M., it gradually veers to westward, the most westerly point (nearly due west) being reached at 5—6 P.M., after which it gradually shifts back to its starting-point in the morning. The most interesting point in the diurnal curve of the barometer is the occurrence of the morning maximum at noon, being the time when this phase of the pressure occurs at places situated close to the sea-shore. The maximum temperature occurs as early as from noon to 1 P.M.

MRS. GRIESBACH has presented to the Lord President of the Council, for the proposed scientific museum, a valuable collection of acoustical apparatus, invented and made by her late husband, John Henry Griesbach. This apparatus is now exhibited in the Loan Collection of Scientific Apparatus.

IN a supplement to the *Gardener's Chronicle* for Aug. 5, is given a well-illustrated description of the Royal Botanic Gardens at Kew, including views in the centre of the palm-stove, the succulent house, the temperate house, &c.

THE additions to the Zoological Society's Gardens during the past week include a Racoon-like Dog (*Nyctereutes procyonides*) from Eastern Asia, presented by Capt. W. H. Bingoym; seven Common Guillemots (*Uria troïle*) and a Kittiwake Gull (*Rissa tridactyla*), British, presented by Sir H. Dalrymple, Bart.; a Brown Coati (*Nasua nasica*) from South America, presented by Mr. R. C. Corfield; two Hairy Armadillos (*Dasypus villosus*), born in the Gardens.

SCIENTIFIC SERIALS

Zeitschrift der Oesterreichischen Gesellschaft für Meteorologie, April.—This number contains an article by Signor Denza, director of the Observatory at Moncalieri, on an inspection by him of observatories of the second order, according to the recommendations of the Leipzig and Vienna Conferences, for the correction of barometers. He has found that a safe verification can only be made when all the rules and precautions are observed, and recommends that the barometer taken in travelling from place to place should not be too narrow in bore, and should be carefully compared with the standard before and after the journey.—The next article is by Dr. Hann, on the results of observations made by the Swedish Arctic Expedition of 1872 in Spitzbergen and East Greenland, published in Stockholm. The observations are of great value, and deserve the full notice here given them by Dr. Hann.

Reale Istituto Lombardo di Scienze e Lettere. Rendiconti. Vol. IX. Nos. 1, 2, 3 (1876). Among the papers contained in these numbers we note the following :—Singular structure of the leaves in the Empetroceæ, by M. Gibelli.—Sketch of Dr. Cantor's recent studies on the history of land-surveying, by M. Schiaparelli.—Researches on the action of oxygen, at the ordinary temperature on sulphur, on alkaline and terralkaline sulphides, and on hyposulphite of calcium, by M. Pellogio.—Report on the vine-disease of Phylloxera, by a Committee of the Institute.—On a new disease of chestnuts, by M. Gibelli.—On the constitution of veratric acid and veratrol, by M. Körner.—On the temperature of flames, by M. Ferrini.

Gazzetta Chimica Italiana, anno vi., 1876, fasc. iv.—E. Paternò and G. Briosi contribute a paper on hesperidin. These two investigators studied hesperidin derived from the common orange (*Citrus aurantium*, Risso). About 4,000 ripe oranges were found to yield 180 grammes of impure hesperidin. They experienced much difficulty in their endeavours to purify this substance.—G. Pisati contributes the only two original papers in addition to the one we have already noticed. His first paper details some experimental researches made by the author on electro-static induction. The second treats of the elasticity of metals at different temperatures.—The remainder of this number is filled up by summaries of the contents of foreign chemical journals, and a review of a book by T. Schutzenberger, "On Fermentation."

IN the *Zeitschrift für Wissenschaftliche Zoologie*, vol. xxvi., part 2 (December, 1875), W. Repiachoff continues his contributions on the Chilostomous Bryozoa, giving many interesting particulars about the development of the amphiblastic ovum of Lepralia and Tendra.—Ludwig Graff describes the anatomy of the Sipunculoid *Chætoderma nitidulum.*—Dr. Hubert Ludwig writes on the interesting Gastrotrichous Rotifers, established as a separate order by Metschnikoff.

SOCIETIES AND ACADEMIES

LONDON

Royal Society, May 18—"The Calculus of Chemical Operations.—Part II. On the Analysis of Chemical Events," by Sir B. C. Brodie, Bart., F.R.S., late Professor of Chemistry in the University of Oxford.

Introduction.—An account is here given of the origin of our views of the constitution of ponderable matter, regarded as constituted of units compounded of "simple weights." These considerations lead to two systems, and two only, in which the unit of hydrogen is respectively expressed by the symbols α and α^2. Between the systems we have no absolute means of selection, but a preference is here given to the system α as immediately leading to the law of even numbers.

The exception presented by the binoxide of nitrogen is then considered, and a hypothesis suggested to account for this anomaly.

The object of the work is then defined—namely, given a chemical event, how are we to determine the events of which it is compounded?

Section I.—The *Question of the Multiplication and Division of Chemical Equations* is here considered. It is shown that we may multiply and divide a chemical equation of the form $u = 0$, by any chemical function, if the sum of the numerical coefficients in that equation is equal to zero, but otherwise not. A method is given by which every chemical equation may be brought under this form. Such an equation is termed a "normal" chemical equation, for it is an equation on which we may operate by the rules of elementary algebra.

It is then shown that every chemical expression of the form $A(x-a)\,y-b$, and also $A(x-a)(y-b)(z-c)\ldots$ (that is, the continued product of any number of such factors more than one), necessarily $= 0$.

As regards the interpretation of normal chemical equations. Normal equations express the identity of the two members of the equation, not only as regards matter, but as regards matter and space also. Thus the equation $1 + 2\alpha\xi = 2\alpha + \xi^2$ asserts not only that the matter of two units of water is identical with the matter of two units of hydrogen and a unit of oxygen, but also that an empty unit of space and the space occupied by two units of water are identical with the space occupied by two units of hydrogen and a unit of oxygen.

It is further shown that in any chemical equation any one of the prime factors of the equation may be substituted for another, and the equation will still be true.

Section II.—Our knowledge of the identity of matter is derived from chemical transmutations or events ; and every chemical equation may be regarded as the record of such an event or some number of such events. Chemical events may be regarded as compound or simple. A compound event is defined as an event which is regarded in the system of events under our consideration as constituted of two or more events. A simple event is an event which is not so regarded. Thus, for example, take the system of the four events :—

$$
\begin{array}{llll}
(1) & \alpha^2\upsilon + \alpha^3\kappa^2\omega & = \alpha\omega & + \alpha^4\kappa^2\upsilon, \\
(2) & \alpha^4\kappa^2\upsilon + \alpha^3\kappa^2\omega & = \alpha\omega & + \alpha^6\kappa^4\upsilon, \\
(3) & \alpha^6\kappa^4\upsilon + \alpha^3\kappa^2\omega & = \alpha\omega & + \alpha^8\kappa^4\upsilon, \\
(4) & \alpha^2\upsilon + 3\alpha^3\kappa^2\omega & = 3\alpha\omega & + \alpha^8\kappa^6\upsilon.
\end{array}
$$

The event 4 is a compound event, being the aggregate of the events 1, 2, 3 ; whereas the events 1, 2, 3 are in that system simple events, being incapable of such a construction.

Section III.—*On the Causes of Events.*—The cause of an event is given when the operations are defined by the agency of which the event occurs. *Def.* If in any chemical event the change in the arrangement of the symbols, by which the composition of the units of matter before and after the event respectively is symbolised, be of such a nature that where in the arrangement before the event the symbol x appears, the symbol a appears after the event, and where a appears before x appears after, so that the two arrangements differ in this respect and this respect alone, then the event occurs by the substitution of a for x, which is the "cause" of the event. Hence the same event may arise from more than one cause. Thus, for example, the event

$$Ayx + Aab = Aya + Axb$$

occurs by the substitution of a for x and of b for y, for these symbols satisfy the condition given in the above definition.

It is similarly shown that the event

$$Axyz + Aabz + Aayc + Axbc = Axyc + Aabc + Aayz + Axbz$$

occurs by the substitution of a for x, b for y, z for c ; and, further, that if the equation to any chemical event be of the form $A(x-a)(y-b)(z-c)(v-d)(w-e)\ldots = 0$, that event occurs by the substitutions of a for x, b for y, c for z, d for v, e for w . . . .

If in these substitutions any symbol, say "a" $= 1$, the event occurs by the transference of the simple weight thus symbolised.

The following event occurs in three ways by the substitution of ξ for χ, the hydride of propyl, $\alpha^4\kappa^3$, being constant,

$$\alpha^4\kappa^3\chi^3 + 3\alpha^4\kappa^3\xi^2\chi = \alpha^4\kappa^3\xi^3 + 3\alpha^4\kappa^3\xi^2\chi,$$

the equation being of the form

$$\alpha^4\kappa^3(\chi - \xi)^3 = 0.$$

Similarly the event

$$\alpha^2\kappa\chi^3 + 3\alpha^2\kappa\chi = 3\alpha^2\kappa\chi^2 + \alpha^2\kappa$$

is an event occurring in three ways by the transference of χ, the equation being of the form

$$\alpha^2\kappa(\chi - 1)^3 = 0.$$

I submit the following equation to the consideration of the reader :—

$$\alpha^4\kappa^3\xi(\beta - \xi)(\alpha\kappa^2\xi - 1) = 0.$$

Section IV. *Elementary Analysis of Events.*—If the equation to a chemical event be capable of expression as the continued product of rational factors of the form previously given $(x-a)$, x and a being prime factors of the equation, the event is a simple event incapable of further resolution; but occasionally the equations to events may be expressed by rational factors, although not of this form. In this case they admit of an easy analysis into other events of which they are the aggregates. Take, for example, the equation

$$\alpha\chi^2 + 2\alpha\omega = 2\alpha\chi + \alpha\omega^2,$$

which may be written thus,

$$\alpha(\chi-1)(\chi-\omega) + \alpha(\omega-1)(\chi-\omega) = 0,$$

whence

$$\alpha(\chi-1)(\chi-\omega) = 0,$$
$$\alpha(\omega-1)(\chi-\omega) = 0,$$

the constituents being

$$\alpha\chi^2 + \alpha\omega = \alpha\chi + \alpha\omega\chi,$$
$$\alpha\omega\chi + \alpha\omega = \alpha\omega^2 + \alpha\chi,$$

Again, the following event is the action of chlorosulphuric acid upon water:—

$$\alpha\theta\xi^2\chi^2 + 2\alpha\xi = \alpha\theta\xi^4 + 2\alpha\chi.$$

This equation is of the form

$$\alpha(\theta\xi^2 + \theta\xi^3 - 2)(\chi-\xi),$$

whence

$$\alpha(\theta\xi^2\chi - 1)(\chi-\xi) + \alpha(\theta\xi^3 - 1)(\chi-\xi),$$

the constituents being

$$\alpha\theta\xi^2\chi^2 + \alpha\xi = \alpha\theta\xi^3\chi + \alpha\chi,$$
$$\alpha\theta\xi^3\chi + \alpha\xi = \alpha\theta\xi^4 + \alpha\chi,$$

The analysis of these two phenomena here indicated has actually been effected by experiment.

Section V.—In this section the doctrine of *Chemical Congruity* is discussed, two chemical functions being said to be congruous to one another in reference to a special substitution if they assume the same value when that substitution is respectively effected in them.

Further, a method is given for the *Development of Chemical Functions*, and for the complete theoretical analysis of any chemical event whatsoever—the theoretical analysis of a chemical event occurring by any number of specified substitutions, namely, of *a* for *x*, *b* for *y*, *c* for *z*, ..., being here said to be effected when all the different chemical events occurring in any way whatever by these substitutions are enumerated, the aggregate of which constitutes the event in question.

PARIS

Academy of Sciences, July 31.—Vice-Admiral Paris in the chair.—The following papers were read:—On the carpellary theory according to the Loaseæ (first part: Mentzelia), by M. Trécul.—Oscillations of temperature of half May, half June, and half July, 1876; non-synchronic parallelism of the barometric pressure and the temperature, by M. Sainte-Claire Deville. A minimum of temperature on the 10th, 11th, and 12th respectively; and maxima, on either hand, about the 7th and the 16th.—Fifth note on electrical transmissions through the ground, by M. du Moncel. The conductivity of hard stones, as also, doubtless, that of the ground, with regard to plates buried in it, is far from being uniform throughout their mass. The metals used as electrodes with his silex may be ranked thus as to electromotive force, each being electro-negative to those which follow—platina, copper, brass, iron, tin, lead, zinc.—Researches on the development of the chestnut, by M. Baillon. These throw some light on the disputed point of development of the cupule in the acorn.—On the disease called diarrhœa of Cochin China, by M. Normand. It has wrought great havoc among the troops there. It is caused by a parasitical worm in the tissues of the intestine. Milk has been the best remedy hitherto.—On the general theory of regulators, by M. Wischnegradski.—On globular lightning, by M. Planté. He describes a case of it at Paris, July 24. He thinks it due to spherical aggregation of air and steam through suction and rarefaction by the electric fluid, and condensation of positive electricity in this envelope of rarefied matter.—Radiometer with vanes formed of a metal and of unblackened mica, by MM. Alvergniat Frères. On heating and exhausting to a certain point, it became very little sensitive to light; would only turn with sunlight; but it had great sensibility to obscure heat.—Observations on vines having galls in large quantities, by M. Laliman.—New confirmation of phylloxerian migrations, by M. Lichtenstein.—Nebulæ discovered and observed at Marseilles Observatory, by M. Stephan. Twenty-three are described.—Note on dissociation of the vapour of calomel, by M. Debray. Calomel undergoes a commencement of decomposition at 440°. He heated it in a platinum tube, and held in the vapour a U-tube of gilt silver, through which circulated cold water. There was a greyish deposit containing a little mercury so divided in a fine powder of calomel that it could not attack the gold.—On the laws of compressibility and the coefficients of dilatation of some vapours.—Action of hydracids on tellurous acid, by M. Ditte.—On new salts of bismuth and their use in testing for potash, by M. Carnot. These are distinguished by their complete solubility in water. They are double hyposulphites of bismuth and of alkalies.—On the isomery of rotatory power in the camphols, by M. de Montgolfier.—On a case of spontaneous alteration of anhydrous hydrocyanic acid, and a new case of total transformation of this acid, by M. de Girard.—On the decomposition of cyanide of potassium, cyanide of zinc, and formiate of potash into carbonic acid, air, and pure hydrogen, by MM. Naudin and Montholon.—On two new sulphurised ureas, by MM. de Clermont and Wehrlin.—On the industrial employment of vanadium in manufacture of aniline black, by M. Witz. This proves a simpler and more economical mode of preparation.—On the manufacture of dynamite, by M. Sobrero. He recommends moulding the siliceous matter, after moistening with water, into blocks, then drying, then dipping slowly in the liquid. He experimented thus with fossil meal of Santa Fiora and olive oil, and was convinced that dynamite with 75 per cent. of explosive matter could be made by this method; danger from friction is avoided.—On the agronomic map of the Arrondissement of Rethel (Ardennes), by MM. Meugy and Nevoit.—On fermentation of fruits placed in carbonic acid, by MM. Joubert and Chamberland.—Cellulosic fermentation by means of vegetal organs, and probable utilisation of the sugar in the vegetation for the formation of cellulose, by M. Durin.—On the microzymes of germinated barley and sweet almonds as producers of diastase and synaptase (*àpropos* of note by MM. Pasteur and Joubert), by M. Bechamp.—Rectification in a former note on panification in the United States, and the properties of hops as ferment, by M. Sacc.—On the fermentation of urine, *àpropos* of a communication of M. Pasteur, by Dr. Bastian. Boiled solution of potash can fertilise sterile urine only when used in a proportion corresponding to the acidity and quantity of the liquid. The author asks M. Pasteur for direct proof that germs of Bacteria can survive in a liquid as caustic as the solution of potash made in the pharmaceutical proportions, when it is raised even for only a few instants, to 100°.—Observations on opinions attributed by Prof. Bastian to Prof. Tyndall, *àpropos* of the doctrine of spontaneous generation; extracts from two letters from Prof. Tyndall. He expresses surprise at being cited as guaranteeing the exactness of Dr. Bastian's experiments, and his entire concurrence with M. Pasteur.—On metallic powders in the atmosphere, by M. Phipson.

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