circumstances no ship can be safe, how great soever her statical stability. All these principles have been known for some years through Mr. Froude's researches. The lecturer exhibited a machine he had contrived for illustrating them, in which the dynamical conditions of vessels of different degrees of stiffness and steadiness were approximately imitated by means of a peculiarly-constructed pendulum hanging from a pin, whose motions imitate those of a particle of water disturbed by waves.

SECTIONAL PROCEEDINGS

SECTION A.—MATHEMATICAL AND PHYSICAL SCIENCE

On the Mode of Action of Lightning on Telegraphs, and on a New Method of Constructing Telegraph Coils.—Mr. S. Alfred Varley. He remarked that when lightning storms occur in the neighbourhood of telegraph wires, although the wires may not be actually struck, powerful currents are induced in them which may be sufficiently strong to fuse the coils, but which more frequently simply demagnetise, and as often reverse the magnetism of the magnetic needles situated in the coils of needle telegraph instruments. Thus, not only is a considerable amount of damage done annually to telegraph instruments, but telegraphic communication is very liable to serious interruption. Mr. Varley mentioned a number of observations going to prove that an interval of dust separating two metallic conductors opposes practically a decreasing resistance to an increasing electrical tension, and that incandescent particles of carbon oppose about $\frac{1}{60}$th part of the resistance opposed by a needle telegraph coil. Reasoning upon these data, he has constructed an instrument, the main feature of which is what he terms a "lightning bridge." Two thick metal conductors, terminating in points, are inserted usually in a piece of wood. These points approach one another within about $\frac{1}{18}$th of an inch in a chamber cut in the middle of the wood. This bridge is placed in the electric circuit in the most direct course which the lightning can take, and the space separating the two points is filled loosely with powder, which is placed in the chamber, and surrounds and covers the extremities of the pointed conductors. The powder employed consists of carbon (a conductor) and a non-conducting substance in a minute state of division. When this instrument is used, therefore, lightning which strikes a circuit finds in its direct path not a space of air but a bridge of powder, consisting of particles of conducting matter in close proximity to one another. These the lightning connects under the influence of the discharge, and the particles are thrown into a highly incandescent state. The secondary current, developed by the demagnetisation, finds an easier passage across this heated matter than through the coils. These lightning bridges have been in use since January 1866, and at the present moment there are upwards of 1,000 doing duty in this country alone. Yet not a single case has occurred of a coil being fused when protected by them. The reason why a powder consisting entirely or chiefly of conducting matter cannot be safely employed is, that although it can oppose a practically infinite resistance to the passage of electricity of the tension of ordinary working currents, when a high tension discharge occurs, the particles under the influence of the discharge generally arrange themselves so closely as to make a conducting connection between the two points of the lightning bridge. In the course of his exposition, Mr. Varley endeavoured to prove that when telegraphic circuits protected by ordinary protectors are struck by lightning, it is to the secondary current and not to the main discharge that the fusion must be attributed. He also pointed out the defects of the protector, which consists of two silk wires wound side by side upon a bobbin.

Mr. Varley also read a second paper containing *A Description of the Electric Time Signal, at Port Elizabeth, Cape of Good Hope.* After an elaborate account of the Liverpool time ball, he proceeded to say that in the year 1859, Sir Thomas Maclear, the Astronomer Royal of the Cape of Good Hope, inspected the electrical time signals in this country, with a view to erecting time balls in connection with the Royal Observatory at Cape Town. Sir T. Maclear remarked the greater rapidity of action of the Liverpool trigger, and this led to Mr. Varley's afterwards designing and constructing at different times two triggers for use in the Cape. Both these triggers discharged more rapidly than the Liverpool trigger. In September 1864, he was requested to construct a trigger for discharging a time ball to be erected at Port Elizabeth. He considered the intervention of any relay or secondary apparatus to be objectionable. He therefore determined if possible to construct the trigger sensitive enough to be discharged by the batteries in the Cape Town Observatory, and in its construction he adopted a modification of a principle first introduced by Professor Hughes in his printing telegraph (described at the Newcastle meeting).

The trigger was constructed with a soft iron armature, rendered magnetic by induction from a compound bar magnet, and which strongly attracted the soft iron cores of an electro-magnet, but which was prevented from actually touching the poles of the electro-magnet.

A spiral spring attached to this armature was so adjusted that it nearly overcame the magnetic attraction induced by the bar magnets.

The time current polarised the electro-magnet in the opposite direction to that induced by the bar magnets, and as the attraction between the armature and the soft iron cores was already almost overcome by the spiral spring, a very small amount of polarisation in the opposite direction was necessary to release the armature, which was rapidly pulled away by the spiral spring, and the trigger discharged.

There were some other alterations made in the general mechanical construction of this trigger, but they may be regarded as matters of detail.

The rapidity of discharge was very great, $\frac{1}{20}$th part of a second only elapsed between the arrival of the time current and the falling of the ball.

From a report in the Port Elizabeth paper of August 29, 1865, giving an account of the inauguration of this time signal, and forwarded to Mr. Varley by Sir Thomas Maclear, it appears that the time elapsing between the time current leaving the Observatory at Cape Town and the receipt at Cape Town of the signal announcing the falling of the ball, is only $\frac{1}{10}$th of a second.

The time which elapsed between the Greenwich current reaching London and the falling of the ball at Liverpool was $\frac{1}{2}\frac{1}{0}$th of a second. In other words, the Algoa ball is discharged from a distance of 500 miles in less than $\frac{1}{7}$th of the time of that of the Liverpool ball.

What is being daily done in the Cape can, however, be best summed up by a short quotation from a letter received from Sir Thomas Maclear, giving an account of the successful inauguration of this time signal. After detailing the general arrangements, Sir Thomas Maclear goes on to state: "A few tentative signals having proved satisfactory, the 'preface' was issued from the Observatory at ten minutes before one o'clock, and at the instant of one o'clock, the Observatory time-ball clock closed the circuit discharging the Observatory ball, the Simon's Town ball, twenty-four miles distant; the Cape Town time gun, three miles distant; and the Port Elizabeth ball, distant 500 miles."

On the present State of the Question relative to Lunar Activity or Quiescence.—W. R. Birt, F.R.A.S. From the time of Schröter, the question of change on the moon's surface has been more or less agitated. The *Selenotopographische Fragmente* contains numerous instances of what he considered to be changes of a temporary character, and a few of a more permanent nature, as the formation of new craters. It is, however, notorious that he failed to establish the fact of a decided change in any one instance; nor is this to be wondered at when we consider the paucity of the materials he had at his command. Notwithstanding the comparative neglect into which the observations recorded in the "Fragments" have fallen, and the judgments passed upon them by some of the best known selenographers, there can be no question that they embody the results of zealous and persevering attention to the moon's surface, and ought not to be passed over in the examination of any given spot, the history of which we are desirous of becoming acquainted with *during the earliest period of descriptive observational selenography.*

The labours of Schröter's successors, Lohrmann, and Beer and Mädler, have added greatly to the number of objects, either as delineated on their maps or referred to in their letter-press. Lohrmann appears to have carefully studied Schröter's results, as we find him quoting the measures obtained by Schröter in several instances. On examining the results of the two greatest selenographical works of the present century, and comparing the one with the other, we find precisely the same kind of phenomena presenting themselves which in a great measure perplexed Schröter; but as Lohrmann and Mädler worked independently of each other, and Mädler evidently had a very low idea of the value of the preceding labours of Schröter, these phenomena

passed unnoticed at the time. Upon consulting the three works for elucidating the history of any given object, such results as these are frequently obtained. An object is found in Schröter designated by a Greek or other character, and its appearance described in his text. This object may be altogether omitted by Lohrmann, but given on Beer and Mädler's map, and objects are by no means rare which may be found on Lohrmann, but omitted by Beer and Mädler, and *vice versa*.

Were the results of the labours of Julius Schmidt during a period of nearly thirty years given to the public, there can be no doubt that our knowledge of selenography would be greatly advanced. His chart must contain a large proportion of the objects previously recorded by Schröter, Lohrmann, and Beer and Mädler; and judging from the instances already alluded to, of *apparent omissions* by one or other of the above-named observers, it is highly probable that the number of such instances would be much increased. The value of his measures (4,000) of the altitudes of lunar mountains for comparison with or addition to those of Schröter and Mädler, cannot admit of a doubt. His published catalogue of rills is very valuable in this respect. It is to Schmidt that we are indebted for one of the most important announcements bearing on the subject of lunar activity, that of a change in the crater *Linné*, "which," says Mädler (Reports British Association, 1868, p. 517) "has hitherto offered the only authentic example of an admitted change." He had previously said (same report): "What has lately been observed in the crater *Linné* proves, at all events, that *there* real changes have taken place, and that, too, under circumstances even visible to us." Further on, however, the great selenographer remarks that on the 10th of May, 1867, his eye having undergone an operation for cataract, he attempted an observation of *Linné* in the heliometer of the Observatory at Bonn, and found it shaped exactly, and with the same throw of shadow as he remembered to have seen it in 1831. "The event," he says, "of whatever nature it might have been, must have passed away without leaving any trace obse·vable by me." The doubt still hanging over this object is well known, and it may be regarded as furnishing, at least, one of the instances of the present state of the question of activity. The uncertainty attaching to the question of change in this particular instance mainly arises from the difficulty of deciding upon the accuracy or otherwise of the delineations of Lohrmann and Beer and Mädler, although both describe it as showing a diameter of five or six English miles. Generally speaking, the observations between October 1866 and July 1870, all agree in its present appearance, differing greatly from that which it must have presented according to the delineations and descriptions of the two selenographers just named, also that no change of a physical character has taken place in it during the 3½ years it has been under observation. It has been supposed that photography would solve all such difficulties, and that photograms of the lunar surface taken under similar angles of illumination and visual ray would agree with each other; but here again precisely the same difficulties present themselves which perplexed Schröter, and which have been met with in comparing Lohrmann's and Beer and Mädler's works. Objects figured by the earlier selenographers occur on some photograms, but not on others, of about the same phase of illumination. There appears to be an agency capable of affecting the visibility of objects, rendering them indistinct or invisible on some occasions; while, on others, they are distinctly seen on the photograms. Whatever operations may have taken place in the crater *Linné*, producing phenomena the recurrence of which is *rare*, in all the examples above mentioned, from Schröter's time to the present, we have phenomena of a different character, exceedingly difficult of explanation, and constituting an important element in the solution of the question of present activity or quiescence; for unless it be fully proved that *all* these instances depend upon changes of visual and illuminating angles, a strong suspicion will exist of their being more immediately connected with the moon itself. To effect such a proof, however, is a matter of no small difficulty. Mädler alludes to the performance of calculations of the most varied kind as necessary for the delineation of lunar forms, and in the case before us the calculation of several elements for *each separate observation*, and they are very numerous, is absolutely essential for the purpose of referring the phenomena observed to changes of illumination and visual ray. Calculations of this kind have not been made to any great extent, and the consequence is, that the entire question remains involved in doubt. During the last seventeen months, as many as 1,227 observations of the spots on *Plato* alone have been made, and although the varying state of the earth's atmosphere affects in no slight degree the visibility of such delicate objects, phenomena are presenting themselves which call for a much more rigorous treatment than has yet been accorded to them. The affirmation of change on, or quiescence of, the moon's surface, must depend, not upon the accumulation of desultory and undiscussed observations, but upon such as are undertaken on a well-arranged system, and discussed with reference to every known agency capable of affecting them. The present state of the question is therefore one of *doubt*, one that calls for observation of the most vigorous character, and discussion of the most rigorous nature to settle it. Observation of late has been tending towards a registration of minute detail detected on the moon's surface, but discussion in various ways is behind the requirements of selenography, and until it can keep pace with observation the doubt alluded to above must remain.

Section B.—Chemical Science

On Artificial Alizarine.—Mr. W. H. Perkin, F.R.S. In introducing Mr. Perkin the President said that that gentleman might be regarded as the representative in England of artificial colouring matters, and that the subject to be treated of was one of great importance, both theoretically and in its practical aspects. The author referred to the use of madder and its preparation called garancine, in the production of Turkey red dye and then traced the history of the investigations of chemist regarding the chemical nature of the colouring matters contained in madder-root. About thirty-nine years ago those investigations commenced, and ever since that time they have been continued by many eminent chemists, among others by Graebe, Liebermann, Schunck, Strecker, Laurent, Anderson, and others, as also those of the author himself. Two colouring compounds had been obtained from madder known as alizarine and purpurine. The exact composition of alizarine had been the subject of much discussion among chemists. From that compound a hydrocarbon derivative had been obtained which is called anthracene, and then from anthracene, as an ingredient of coal-tar and mineral pitch, alizarine had been produced by the action of various chemical agents. Alizarine, thus artificially produced, yields with mordants the same colours on cotton goods as the natural alizarine from madder-root. Mr. Perkin performed a great variety of experiments in order to demonstrate the chemical identity of the artificial and natural alizarine when absolutely pure. He had also, during upwards of twelve months, been engaged in studying the properties of anthracene and its compounds, all of which are very markedly fluorescent. There were many difficulties in the way of obtaining artificial alizarine in large quantities, but they were gradually disappearing. In the discussion which followed, Dr. Schunck, F.R.S., referred to the alleged differences between the natural and the artificial alizarine, and said he had no doubt whatever that the two were identical, and he thought the confusion had arisen from persons examining impure products. The artificial product was generally supplied impure, but the impurities could be separated. He was quite satisfied as to the importance of alizarine, and that it was the only essential dye product of madder. Some years ago he had shown that the finest madder pinks contained nothing but alizarine.

Section C.—Geology

On the Extension of the Coal-fields beneath the newer Formations of England.—Mr. Edward Hull. Having referred to the paper by Sir R. I. Murchison, on the parts of England and Wales in which coal may or may not be looked for, the author expressed his gratification that his own views coincided in the main with those of his chief, especially as regarded the absence of coal in the eastern and portions of the midland counties, now overspread by mesozoic formations. The author showed that there was evidence for believing that the coal measures were originally deposited in two continuous sheets, one to the north, and the other to the south of a ridge of old land formed of Silurian rocks which stretched eastward from Shropshire to the south of the Dudley coal-field. This ridge, or barrier, had never been altogether submerged beneath the waters in which the coal measures were deposited. Towards the north, the boundaries of the coal formation were formed by the Cambro-Silurian rocks of North Wales, the Lake District, and portions of the southern uplands of Scotland. Over the region north of the barrier, the coal measures

were deposited in greatest thickness towards the north-west; while over that south of the barrier they were deposited in greatest force in a westerly direction.

At the close of the coal period, disturbances of the strata, resulting from lateral pressure acting in north or south directions, took place over the whole carboniferous area of the north of England, whereby the strata were thrown into a series of folds, the axis of which ranged along approximately east and west lines. These disturbances were accompanied and followed by enormous denudation, by which the coal measures were swept away over large tracts of the north of England, and the northern limits of the Lancashire and Yorkshire coal-fields were determined. As regards the tract south of the central barrier, it was inferred, on the ground of parallelism of direction with the east and west flexures of the north of England, that the northern and southern limits of the South Wales coal-field, the axis of the Mendip Hills, and the easterly bend of the culm-measures of Devonshire, were all referable to the same geological period, *i.e.*, that which intervened between the deposition of the carboniferous and the Permian rocks.

After the deposition of the Permian beds over the inclined and denuded surfaces of the carboniferous rocks, disturbances (accompanied by denundations) occurred along lines nearly at right angles to those of the preceding period, *i.e.*, along north and south lines (approximately). To this epoch the axis of the Permian chain, and all north and south trendings of the strata, were probably to be referred. Some of the results brought about by these movements were the disseverance of the Lancashire and Cheshire from the Yorkshire and Derbyshire coal-fields, the determination of the western limits of the Flintshire and Denbighshire coal-field, the disseverance of the Forest of Dean coal-field from that of South Wales, and the uptilting of the lower carboniferous rocks along the eastern margin of the Somersetshire coal-field beneath the Jurassic formations.

From these considerations it seemed clear to the author that to the intersection of these two systems of disturbances (*i.e.*, the E. & W. with the N. & S.) and the concomitant denudation, the basin-shaped form of nearly all the British coal-fields (sometimes partially concealed by newer formations) might be attributed.

The author then proceeded to show that over these carboniferous basins, the Permian and Triassic rocks were distributed according to a well-defined plan, the Triassic strata thinning away towards the south-east of England; and concluded by discussing the views of Sir R. I. Murchison, Professor Ramsay, and Mr. Godwin-Austen regarding the absence or presence of coal under the cretaceous and tertiary strata of the south of England.

On the History and Affinities of the British Coniferæ.—Mr. W. Carruthers. Having pointed out the great divisions of this natural order, the author traced their appearance and development in the stratified rocks. The *Araucarieæ*, now represented by fifteen species, all confined to the southern hemisphere, made their appearance in the carboniferous period, where at least eight species determined from the wood structure had been found. In the secondary rocks six species had been found based on the cones, and these showed an affinity to the group of modern Araucarias found in the Pacific Islands. The *Pineæ*, a large group chiefly confined to the northern hemisphere, appeared in the Old Red sandstone, as determined by H. Miller; a single species had been determined from wood in the coal; the species greatly increased in the secondary rocks, where several species of cedars had been detected. The *Taxodieæ*, represented among living plants by fifteen species, chiefly from the northern shores of the Pacific, made their appearance in the secondary rocks, one species being abundant in the Stonesfield slate, and were continued by species of Sequoia in cretaceous and Tertiary rocks. The two species from the Gault are associated with pines having the characters peculiar to the species associated with the existing mammoth trees of California. The *Cupresseæ*, represented by the cypresses, and in our native flora by the juniper alone, are known only in Tertiary strata by a few species of fruits and foliage. The *Taxineæ*, containing nearly 100 species, found all over the world and represented in Britain by the yew, made their appearance in the carboniferous rocks, as determined by a fruit described by Dr. Hooker, and shown by him to be nearly related to the living *Salisburia*. The supposed Taxineous wood from the North American Devonians, to which Principal Dawson gave the name of *Prototaxites*, was a remarkable *Alga* of enormous size. Several Taxineous fruits had been found in the Eocene strata at Sheppey.

Notes on Fossil Crustacea.—Mr. H. Woodward. A considerable number of new species was described which had been met with during the past years belonging to strata from the Silurian to the Tertiary. The author expounded the changes in the larva of the living King Crab, and showed the remarkable resemblances between its early condition and the palæozoic *Trilobites*. The earliest known King Crab occurs in the Upper Silurian, so that the pedigree of these two ancient forms dove-tailed into each other in Silurian times, and these contemporaneous forms approached much nearer to each other than would be expected from a comparison of the living King Crab with the Trilobite.

Report on Earthquakes in Scotland.—Dr. Bryce.

On the Tertiary Coal Fields of Southern Chili.—Mr. G. A. Lebour. This was a detailed description of the beds of coal, and those intercalated with them. The list of fossils appeared to Prof. Harkness and Mr. Carruthers to indicate a Secondary rather than a Tertiary age.

Section D.—Biology

Mr. Edward Atkinson, of Leeds, read a paper on the *Osteology of Chlamydophorus truncatus*—a fine male specimen of which had been presented to the Philosophical and Literary Society of Leeds. First glancing at the bibliography of this little quadruped, the author proceeded to draw attention to some points in the structure of its skeleton. The general conformation of the head is very remarkable, differing from all other Edentates in its relative dimensions, excelling its congeners both in altitude and in breadth as compared with length. He also alluded in detail to the structure of the lower jaw, the ear, the scapula, sternum, and pelvis. With regard to the dentition, his observations were not quite in accord with those of Harlan or Hyrtl. *C. truncatus* is a true monodont with eight grinders on either side of both maxilla and mandible. Those of the lower jaw perforate the whole depth of the bone, dimpling the inferior margin. The first tooth of the lower jaw has no opponent, and therefore no masticatory surface. The eighth upper tooth is also without an antagonist, but its analogue in front has a double facet.

Mr. R. McAndrew, F.R.S., presented a report on the *Marine Mollusca of the Gulf of Suez.* This report gives the general result of a dredging excursion to the Gulf of Suez in February and March 1869. Mr. E. Fielding accompanied the author. Leaving Suez on the 10th February in a boat of about twelve tons burthen, with one of about five tons for dredging, and a small boat for landing, the party reached Tur in about three weeks' time. Their crew consisted of Maltese and Neapolitans, an Arab, who proved an excellent diver, and a native of Tur, who acted as pilot. From Tur they crossed over to the Point of Zeite and the desolate islands situated towards the western side of the Straits of Jubal. After working about a week among these, and finding it a very rich collecting ground, they bore away to Ras Mahommed, where they ended their labours, proceeding from this to Tur, from whence they went by land to Suez. The number of species obtained (not including the Nudibranchiates) was 818. Of these 619 have been identified, the remaining being still undetermined. About 355 have not previously been recorded as from the Red Sea. Of these 53 species, including three genera, are new to Science, and have been described by Messrs. H. and A. Adams. Professor Issel, of Genoa, records 640 species as from the Red Sea, and his list includes 100 new species. Some of these were figured but not described in Savigny's "Description de l'Egypt." Mr. McAndrew dwelt on the extraordinary dissimilarity between the Fauna of the Red Sea and that of the Mediterranean; the number of species common to Japan, the Philippines, Australia, and to the Red Sea, is worthy of further observation. In addition to the Mollusca, a collection of Echinoderms, Crustacea, and Corals, was made and divided among the British, Edinburgh, and Liverpool Museums. The sponges collected were sent to Dr. Bowerbank, except one, which had been described by Mr. Carter as a new genus under the name of *Grayella.*

Professor Wyville Thomson, F.R.S., read a report on *Some of the Echinoderms of the Expedition of H.M.S. Porcupine.* The impression was very general that through the exertions of Forbes, McAndrew, Jeffreys, and others, the marine fauna of the British Islands was now pretty well known. It was also thought that below a depth of some 300 or 400 fathoms animal life became extinct. Through the investigations of Dr. Car-

penter and the author in H.M.S. *Lightning*, and since then by investigations carried on in H.M.S. *Porcupine*, with the additional help of Mr. Jeffreys, not only had the number of new species found been very great, but animal life had been found abundant to the enormous depth of upwards of a mile. Confining himself now to the Echinoderms, he might say that the fauna became not so much a local fauna as one of depth and temperature. All the well-known Scandinavian forms were met with in the "cold area"—such as *Pteraster*, *Euryale*, &c.; while in the "warm area," such wonderful genera as *Pourtalesia* and *Brissingæ*, having possibly its nearest ally in forms found in the Ludlow rock, but also a new soft-bodied genus belonging to the Diademidæ, were met with. All the new forms, embracing both new genera and species, would be described in full in the forthcoming report.

Dr. M'Intosh, F.L.S., read a preliminary report on *Certain Annelids dredged in the expedition of H.M.S. Porcupine.* The specimens were chiefly procured from water under 500 fathoms off the coast of Ireland. They are on the whole of a northern type, many of the rarer having been previously procured by Mr. Jeffreys off the Shetland Islands, and well known in the northern seas generally. There were several new and most interesting species, including a *Sthenelais*—a form allied to *Leanira Malmgreni*, but probably requiring a new genus for its reception; a *Eunice*, *Nothria* and *Chætozone*, the *Antinoe Sarsi* of Kinberg, and the *Petta pusilla* of Malmgren were, besides, added to our fauna. The author tendered his thanks to Professors Carpenter and Wyville Thomson, and more especially to Mr. Gwyn Jeffreys, for their kindness in securing the collection.

Dr. G. W. Child read a Paper on *Protoplasm and the Germ Theory*. Mr. Samuelson read a Paper *On the Controversy on Spontaneous Generation, with new experiments.* In the interesting discussion which followed, the President, Dr. Hooker, Mr. G. Bentham, and Mr. Crace Calvert took part,

Mr. P. L. Sclater read a Paper on *Certain Principles to be observed in the Establishment of a National Museum of Natural History*. [This Paper will be found *in extenso* in another column, with a woodcut. The following is an epitome of the interesting discussion which followed.]

Mr. Wallace entirely agreed with all the main principles advocated by Dr. Sclater, such as the separate government of the Natural History Museum, the association of Palæontology with Zoology, and the separation of the collections into a "typical and a scientific series," both of which should be at all times available; but he differed from him on a point which he considered to be no less important than any of these, viz., as to the mode of arrangement of the specimens which would be most efficient for all the purposes such a museum should fulfil. In a national institution, if any part of it was set apart for the elevation, instruction, and amusement of the public, these purposes should be carried out in the most efficient manner, and this could not be done by the system of wall-cases advocated by Dr. Sclater, and which he (Mr. Wallace) believed to be radically wrong. The objections to these wall-cases were numerous:—

1. They admit of any object being seen by the smallest number of persons at once, so that any one person studying an object, almost necessarily monopolises it, and prevents others from approaching it, an inconvenience that reaches its maximum in the recessed cases exhibited in Dr. Sclater's plan.

2. Objects in wall-cases can be seen only on *one* side, which, as *all* sides of natural objects require to be seen, would necessitate many specimens to do the duty of one.

3. The observer on the one side, from which alone he can see an object, will generally stand in his own light, and will often have distinct vision further impaired by reflection from the glass.

4. When small objects occur alternately with large ones, a great waste of space occurs, and the attention is distracted from the less conspicuous object.

5. The use of wall-cases on one side of a gallery for an entire museum, is an expensive and wasteful mode of arrangement.

Objections (1) (2) and (3) are of the greatest importance. A public national museum must accommodate the thousands who throng to it on holidays, when alone the working classes can reap its benefits; and they should be invited and induced to examine and study, not merely to gaze and pass on. Teachers and parents should be able to give information as to the groups exhibited without interfering with other visitors, none of which things are possible with a range of wall-cases. The system advocated by Mr. Wallace was that of detached cases on tables or on the floor, of various sizes, and each exhibiting one typical object or group of objects, capable of being seen on *all sides*, and admitting of convenient examination in the best light by the *greatest number* of persons at once. The system had been adopted in a new museum at the India House, and at South Kensington, and was advocated by Dr. Gray, and partially exemplified in the great gorilla case, the groups of birds of paradise, and other detached cases in the British Museum. The numerous and very great advantages of this system should not be lost for the sake of an infinitesimal increase of convenience to scientific men. The great majority of specimens exhibited in the public galleries would consist of *common species*, of which an ample series of specimens would be preserved in the scientific collection for study. Of the few rare species which it might be advisable to exhibit to the public, perhaps not more than one a week would be required for scientific examination, and all such might be so mounted as to be easily brought into the students' room, adjacent to the gallery, when required. The man of science would thus *lose nothing*, while the public would gain incalculably; and so greatly was Mr. Wallace impressed with the educational superiority of one mode of arrangement over the other, that he believed it would be better to have the very rare and unique species represented by drawings or models only in the public department, rather than have the whole collection arranged in wall-cases, for the one purpose of allowing the scientific man to get them out more easily on the rare occasions when he required them.

Prof. Archer, of Edinburgh, said: However some of us may differ from Dr. Sclater in his opinions about the arrangements of the contemplated National Museum of Natural History, none of us will, in the slightest degree, differ from him in his belief that this is a subject of paramount importance. I am compelled to say that I do not agree with him as to his arrangement of wall-cases and back entrances, for some considerable experience has convinced me that unless under some peculiar circumstances, as in narrow galleries where there is too little space for detached cases, wall-cases are entirely a mistake. In this respect my own personal experience perfectly coincides with the opinions of Mr. Wallace, but Mr. Wallace has even underrated the advantages of the system he advocates, for he has only indicated by his diagrammatic illustrations a series of cases similar in size, placed at equal distances. But at South Kensington, where the question of constructing cases best adapted for the display of objects in a Museum, has received a greater amount of intelligent attention than in any other museum, they have shown that you can make cases which will admit of a perfectly symmetrical arrangement, and yet be of various sizes, so that small objects as well as large ones may be so exhibited as to permit of their being examined from all sides, instead of from only one point of view as in wall cases. Wall space is valuable for illustrations, especially pictorial ones, but when you arrange groups of animals in them, it is certain that if they are tolerably suitable for the exhibition of large specimens they cannot be equally fitted for small ones. There is one other point in which I cannot agree with the author of the paper, and that is, in the line he draws between the requirements of a Public Museum and one for the use of students in natural history. My own views are to exhibit as much as you can without injury to the specimens, because you never know what portion of your visitors are earnest students or pleasure-seeking idlers; and still further, you do not know how soon this class may be converted into the former.

Prof. Newton thought that being connected with a museum which was emphatically "national," he should be wanting in his duty if he did not express his general agreement with the principles laid down in Mr. Sclater's paper. What might be called the "structural" part of this very important question had been dwelt upon by previous speakers, but there was another part on which they had scarcely touched. This was the constitution of the governing body and officials of the New Museum. First it had been stated in the paper (and the statement was true) that of the fifty trustees of the British Museum only two or three were scientific men. That the museum was what it was, reflected, then, the greatest credit on the energy of those two or three. But care must be taken that the museum of the future, whether sent to South Kensington or kept in Bloomsbury, should be relieved of the burden of the Trustees; it was essential that their authority should cease, and that scientific authority alone should be supreme. Secondly, with regard to the mode of appointment of the officials—that was a matter for great deliberation. He believed that the system adopted a few years

ago had not yet had time to produce all the mischievous results which would follow if it were persevered in; but it was clear to him that in future they should have nothing to do with competitive examinations and Civil Service commissioners, in appointing assistants to the different departments, and he would prefer, as was suggested in the paper, that appointments should be made by the mild despotism of the director or superintendent of the museum.

Department of Zoology and Botany

Dr. B. W. Richardson read the *Report on Methyl Compounds*. He commenced his report by giving a review of some results of his previous reports, describing at length the action of nitrite of amyl and hydrate of chloral, both of which had proved of the greatest service in the treatment of disease. The former had been applied most usefully in the treatment of tetanus; the latter had been so largely applied as a narcotic, that since the discovery of its narcotic properties by Liebreich, more than a million persons had been successfully subjected to its influence. After his review of the past, the author brought forward new matter of research, introducing detailed accounts of the action of ethylate of sodium, ethylate of potassium, sulphur alcohol, sulphide of ethyl, bromide of ethyl, and triethylic ether. The facts respecting the action of these substances were all rich in interest, but two may be named specially, viz., in relation to the ethylates of sodium and potassium, and to triethylic ether. The first, when brought into contact with the surface of the body, acts as the most potent of known caustics, and promises to be rendered painless as well as caustic. The second is a new volatile anæsthetic, the sleep produced by which is deep, gentle, and apparently free from danger. In a final part of his report, Dr. Richardson dwelt on some general physiological observations, which attracted considerable attention. He showed that by the action of some of his anæsthetics, he could induce insensibility to pain without fully destroying consciousness; and he explained that in time this progressive step would be entirely realised. He described the effect produced by repeating applications of volatile agents upon the external nerves' expanses; and on the results of direct experiment, he explained that certain agents, such as nitrite of amyl, act immediately through the nervous system without any absorption of them by the blood. At the close of his report, Dr. Richardson showed how the elementary modification of the bodies of an organic series influences the physiological action of each compound, and expressed a hope that, by continued research, physiologists, moving with the chemists, would speedily bring the subject of the action of medicinal agents into the ranks of positive science.

Dr. Brown-Sequard read two papers on the *Apparent transmission of abnormal conditions due to accidental causes, and on various alterations of Nutrition due to Nervous Influence.*

The President of the Association (Professor Huxley) said: The great theoretical problem they had now to determine was what effect artificial modifications and external conditions had upon living organisms—whether they produced changes which, being transmitted hereditarily, became the basis of new races. Referring to a resolution which had been brought forward at a former meeting, which endeavoured to pledge the Association to abstain from making grants of money to persons engaged in experiments which involved vivisection, he said they had before them that day one of the most experienced physiologists and vivisectors of his day, and he had only to ask the audience to form their own judgment as to whether Dr. Brown-Sequard was likely to inflict one particle of pain upon any creature whatever without having a plain and definite purpose in view. For himself he might say that nothing was more grievous to him than to think of the existence of pain in anything whatever. He hated to see it inflicted upon animals, and he carried his objection to its infliction so far that he disliked even to see a man beating his wife. Neither Dr. Brown-Sequard nor himself were indifferent to pain, and he hoped that in no sense were they cruel. He thought that the gentleman who brought forward the resolution to which he had referred, hardly knew what he was dealing with. If his friend Dr. Brown-Sequard would pardon his referring to a matter personal to him, he would remind the meeting that that great experimental physiologist, and that accomplished vivisector, who had, be supposed, performed as many vivisections as any man in the world, some years ago thought it advisable to turn the vast knowledge of the diagnosis of disease which he had obtained by this means into actual practice, and he (Professor Huxley) could assure them, from what he knew, that before long his wonderful mastery over symptoms caused his consulting rooms to be absolutely crowded by human beings suffering under multiform varieties of nervous disorders, who sought at his hands and from his knowledge that relief which they could not obtain elsewhere. The prevention of cruelty to animals, when understood in its proper sense, was as good an object as men could devote themselves to, but when they confounded the brutal violence of the carter or the wife-beater with an experiment carried out by a man of science, gently and for the purpose of relieving misery, the enthusiasts in that cause should change their name, and convert themselves into a society for the promotion of cruelty to mankind. If that question came before the Association again, and he hoped it would, he trusted they would recollect that the order of nature was such that certain kinds of truth were only attainable by experiments upon living animals, and that when they might result to the welfare of thousands and thousands of untold human beings who might otherwise be suffering unimaginable misery, those experiments were perfectly justifiable.

Dr. R. McDonnell, F.R.S., of Dublin, said that the President of the Association had viewed the admirable communication of Dr. Brown-Sequard from the Darwinian point of view, one of the greatest interest. He, like Professor Humphry of Cambridge, regarded such communications rather in their practical bearings, but first he might be allowed to say, how entirely he concurred with the President in his observations on the subject of experimental researches conducted upon animals. Indeed Dr. Richardson's report was in itself the most unanswerable argument that such experiments are undertaken with the hope of diminishing human suffering, and whosoever would oppose such an important and indeed successful means of attaining this end must be prepared to submit to the imputation of desiring that pain should remain unalleviated. Dr. McDonnell then alluded to the subject referred to both by Dr. Richardson and Dr. Brown-Sequard in speaking of the transmission along the nerves of certain sensations, and their being intercepted. He said that he had long felt some difficulty about adopting the hypothesis of Dr. Brown-Sequard that there existed distinct conductors for various sensations, as those of heat, pain, tickling, contact, &c. In explanation of the remarkable cases sometimes met with in which an individual who felt perfectly the contact of one's hand yet could not distinguish heat or cold, he proposed another hypothesis than that of distinct conductors, and he was indeed happy, on this occasion, to have an opportunity of submitting this hypothesis to the section and to Dr. Brown-Sequard for consideration. His (Dr. McDonnell's) hypothesis was, in fact, an application of the undulatory hypothesis to the propagation of nervous sensation—he supposed that sensations such as those of heat, pain, contact, as well as those of various colours, of form, of sound, were waves of different wave-lengths; and that, under certain circumstances, some waves were absorbed or intercepted while others passed on to the sensorium. He, in fact, drew an analogy or illustration of his hypothesis from Prof. Tyndall's well-known experiments on the absorption of radiant heat by vapours or scents passed into the air filling a glass tube. The glass tube in this experiment represented the nerve tubule, the slight change effected in the air contained within it produced by the introduction of the minutest quantity of scent causes an absorption *in transitu* of some waves of heat, others pass; thus, according to his supposition, might be explained the effect on vision of santonine. The experiment of seeing the complementary colour upon gazing at a white ground after looking upon a coloured disc, might be explained thus: A slight chemical change is effected in the nerve tubule by gazing at the coloured disc; when the white ground is looked upon, all undulations pass through *save* those which are absorbed, viz., those of the colour previously looked at. This, of course, gives the complementary colour. Many phenomena connected with sensation, Dr. McDonnell conceived, would find in this hypothesis a simpler explanation than in that of distinct conductors.

Department of Ethnology and Anthropology

On the Anthropology of Lancashire.—Dr. Beddoe, President of the Anthropological Society of London. The author drew a marked distinction between the inhabitants of North and South Lancashire, both as to their ethnological history and their present physical characteristics. In the former, he believed the Norse element to preponderate, having been introduced, probably, by colonisation from the Isle of

Man and even from Dublin. The people were still tall and fair, and often strikingly Scandinavian in aspect. The remaining British element might be partly Gaelic. In the south of the county, immigration and physical degeneration connected with the great development of the cotton manufacture, had been, and were, effecting changes in the prevailing physical type, which had previously been more Anglian and British, while the Norse element had been comparatively weak. The paper was partly based on numerical data.

On the Ottoman Turks.—Dr. Beddoe. This paper mainly consisted of a minute physical description of the Ottomans of Anatolia, with notices of certain tribes of Yuruks and Turkomans scattered about Asia Minor. The physical type, which for brevity's sake, he called Turanian, was much more prevalent among the former than was generally supposed. It was doubtful whether there was any need for invoking the influence of climate or other *media* to account for the elevation that had occurred in the Ottoman physique. Inter-marriage with the women of subjected races soon after the conquest, and absorption of foreign elements, might sufficiently account for it, and as these had been most prevalent in Rumelia, and in the large towns, it was there that the original Turanian type had been most obscured.

Mr. John S. Phené read a paper on *A recent examination of British Tumuli and Monuments in the Hebrides, and on the western coast of Scotland.*

On the Builders of the Megalithic Monuments in Britain.—Mr. A. S. Lewis. The author divided the inhabitants of Britain into three leading types, the Kymric, long-headed, dark-haired, and light-eyed; the Iberian, dark-eyed and dark-haired; and the Teutonic, round-headed, light-haired, and light-eyed. He controverted the idea now gaining ground that the Iberians represented the aboriginal race, and that they exclusively were the builders of megalithic monuments. He attributed these monuments to Iberians and Kymry indifferently, and believed the latter race to have come to Britain before the former. These views he supported by, among other arguments, a careful consideration of the statistics of the physical characteristics of the inhabitants of Great Britain, collected by Dr. Beddoe, President of the Anthropological Society of London, from which he showed that the Iberians were found in the largest numbers in the southern part of the island, while the monuments were found throughout it, and this distribution of races seemed also to show that the Iberians were a later arrival than the Kymry. Mr. Lewis stated, however, that the statistics were not sufficiently numerous to be absolutely conclusive, and appealed to the members of the Association to assist in collecting further statistics of the physical characteristics of the inhabitants of their own districts.

On the Massagetæ and Sacæ.—Mr. H. H. Howorth. Relying upon the Chinese authorities translated by Stanislas Julien and V. St. Martin, the author identified the Massagetæ with the Ta Yuetchi or Great Yuetchi, and the Sacæ with the Sse or Szu of the Chinese authors. A close criticism of all the information about the Massagetæ and Sacæ furnished by the Greeks, enabled us to say that they were two names for one race, or at most for two branches of one race, Massagetæ being probably the native form, and Sacæ its Persian equivalent. Western writers throw little light on what this race was. The Chinese authors prove that it was a branch of the Thibetan race, called by them the Khiang, which was predominant in Central Asia before the aggrandisement of the Turks in the sixth century. The same authors enable us to connect the Massagetæ and Sacæ with the Indo-Scyths who overthrew Bactria and the Greek civilisation of Asia in the second century, B.C. Sacæ is equivalent to the Sah and Saka of the Indian Epics, and to the more Western Scyth, and in the cuneiform inscriptions is the Aryan substitute for the Semitic Gimiri, the Cimmerii of Herodotus. These facts enable us to destroy the old nonsense about the Sacæ and Saxons, the Massagetæ and Goths, the Cimmerian and Welsh, having been related to one another. Sacæ, Massagetæ, and Cimmerii were all Turanians and in fact Thibetans.

Mr. Dendy read a paper on *Shadows of Genius.*

On the Racial Aspects of Music.—Mr. Kaines. The author drew attention to the settled melancholy which pervaded the music of the north of Europe—a characteristic not observable in the music of the south of Europe, or of the other people of the globe. He endeavoured to account for this physically and practically, and showed there were vast differences in the temperaments in the peoples in the north and south of Europe. Of the one it might be said "melancholy marked it for her own," while cheeriness and brightness marked the other. The first seemed saddened by the mysteries of life, death, Cold, and immortality. Mr. Kaines noticed briefly the great religious revolution which had taken place in Europe, and how it had (probably) powerfully influenced its music. Protestantism broke the spell under which the human intellect was bound by Roman Catholicism, and enlarged the sphere of man's knowledge only to show him how much there was that he could never know. Catholicism, in engaging to answer all the intellectual and moral needs of man, took from him responsibility, and gave him a restfulness to which Protestantism is a stranger. The change from the old to the new (or rather revised) faith, had not been without its effect on music, and the emotional cravings and wild unrest which characterised the music of our times, might be attributable to this cause.

A long and interesting discussion ensued, in which Mr. James Smith, Dr. O'Callaghan, the Rev. Mr. Owen, Dr. Evans, Dr. Hitchman, and others took part.

Section E.—Geography

Mr. Winwood Reade read a paper on the *Upper Waters of the Niger*, and as we understand that he will shortly read a similar communication before the Geographical Society, a brief abstract of his paper will be sufficient for the present. Last year Mr. Reade made an exploring journey from Sierra Leone to the Niger, and visited the gold mines of Bouré, a country mentioned by many travellers, but which he has been the first to reach. Leaving Sierra Leone on January, he went to Falaba, as Major Laing had done before him fifty years ago, though by a different route. Like Major Laing, he was detained at Falaba, and not permitted to pass that important town. He returned to Sierra Leone, bringing with him messengers from the King of Falaba to the Governor of Sierra Leone, and these, grateful for the kindness and liberality with which they had been treated, promised Mr. Reade that he should be allowed to pass Falaba if ever he should visit them again. He determined to go back with them at once. The promise was kept; Falaba is only fifty miles distant from the Niger or Toliba (great river), and within a month after leaving Sierra Leone he reached that river, which has now, in its western course, been touched by explorers at three distinct points: by Mungo Park, at Segou, in 1796; by Caillié, at Couroussa, in 1828; and in 1869 by Readin, at Farabana, where the river is only a hundred yards broad. The author of the paper claims to have discovered the most direce and the shortest route to the Western Niger. Without presuming to compare himself with such giants in travel as Park and Caillié, he pointed out that while Caillié had not been able to reach the Niger under two months, nor Park (nor subsequently the followers in his footsteps, Dochard in '21 and Mage in '64) under four months, he had reached it in one month. Mr. Reade expressed his thanks to Mr. Swanzy, who had borne the expenses of his two years' African travel; to Mr. Heddle, a merchant at Sierra Leone; and to the Governor-in-Chief, Sir A. Kennedy.

Sir H. Barkly, K.C.B., who was in the chair, having thanked Mr. Reade for his paper, Mr. F. Galton made some interesting remarks on the Niger, and said that the discovery of its being only 250 miles from Sierra Leone would, without doubt, have an important influence on the political future of that colony. Lord Houghton asked why nothing had been done by the Sierra Leone Government during the last fifty years to explore the country lying interior of their colony. Mr. Reade said that he was unable to answer that question, but perhaps the extreme difficulty of getting through the coast tribes had something to do with it; as he had explained in his papers it cost him two journeys to make the insignificant distance of 250 miles. We may explain to those who follow these abstracts with their maps that the position of Falaba is correct; and that Bouré or Buri (which is a country, not a town) is approximately correct, as laid down by Caillié, who passed near it. But the tract of country between Falaba and Caillié's first position on the Niger (Couroussa) must be mapped afresh. Mr. Reade does not intend to alter the position of the Niger's source, as laid down by Major Laing from native information obtained by him at Falaba. He was prevented by the wars constantly prevailing in that region from visiting the source, but the information which he collected respecting its position confirms in all essential particulars that obtained in 1822 by Major Laing.

SECTION F.—ECONOMIC SCIENCE AND STATISTICS

On the Aptitude of North American Indians for Agriculture.—James Heywood, M.A., F.R.S. Indian Reservations in Canada are under the control of the Secretary of State at Ottawa. Mr. W. Spragge, Deputy-Superintendent of Indian Affairs, presents annually to the Secretary of State a report on the Canadian settlements of Indians. The Six Nations Indians in the Tuscarora reserve, near Brantford, on Grand River, in the province of Ontario, form the most important settlement of aborigines in Canada. Their reservation comprises 55,000 acres, surrounded on all sides by thriving communities of white settlers. The Indian population of this reserve amounts to about 3,000 persons, including 2,800 of the Six Nations, and about 200 of the Mississaguas, or Ojibbeways, located near the river New Credit, at the southern extremity of the Tuscarora reserve. According to a report of Commissioners, appointed by Sir Edmund Head, Governor-General of Canada, in 1856, the Six Nations Indians were settled in the Tuscarora reserve, by Mr. Thorburn, the Commissioner, in "farm lots, averaging 100 acres each by actual survey." The total clearing of the Tuscarora reserve "amounted in 1856, to 7348 acres, more than half of which had been done by the Indians themselves, the remainder having been chopped by squatters, who had been removed from the land." "Most of these squatters were compensated for their improvements to the amount of more than 8,000*l.*, paid from the funds of the Six Nations Indians." The Commissioners of 1856 report that the Six Nations Indians cultivate on their reserve "separate farms, and each is secure in his possession from the other Indians on the lot he occupies. His heirs inherit his improvements, but the soil belongs to the Six Nations in common. The Indian has no right of transferring his portion of land to another. The revenue of the Six Nations Indians amounts to 39,489 dollars annually." Besides the two Schools in the New Credit district, maintained by the Indian bands of that locality, there are in the portion of the Tuscarora reserve inhabited by the Six Nations, eight Schools, principally supported by the New England Company, a London corporation, formed under the Commonwealth, whose funds are devoted to the extension of civilisation and Christianity among the aborigines in British Colonies, and especially in Canada. Mr. Henry Lister, a member of the New England Company, visited the Tuscarora reserve in 1868, and reported of the Six Nations Indians that their chief crops were "wheat, Indian corn, oats, and hay." Most of the Indian houses in this reserve, Mr. Lister described as "cottages of one or two rooms, built of boards or logs, and usually heated by a stove. There is not a single village," Mr. Lister remarks, "on the reserve; each house stands in its own lot of about 50 acres." An agricultural society was formed in 1868, among the Six Nations Indians of the Grand River, at an annual subscription of one dollar (about four shillings), for each member, and their first show was held on the 15th of October, 1868, on a farm within the reserve. The policy hitherto pursued in Canada, with regard to Indians, has been to induce them by means of small annuities to remain, to a great extent, as residents in the Indian reservations of the Dominion to which their lands or settlements may respectively belong. According to the Rev. Edward R. Roberts, missionary to the New England Company at Chemong, near Peterborough, in Canada, the province of Ontario was "divided into districts, with reference to the Indians. The land of each district was valued at a certain rate per acre, and the interest of the aggregate sum was paid half-yearly to the Indians included in that district, which constituted their annuity. And, in addition, each band of Indians had a reserve of land in a particular locality for their settlement. The aggregate annuity of the several bands," Mr. Roberts observes, "remains the same, whatever changes by death, birth, or emigration may take place. If a band of Indians becomes less in number, those who remain receive proportionably more annuity. While, however, an individual Indian (or family) ceases to receive his annuity from the fund appropriated to the band he leaves, he may be received into another band, by application, and a vote of the people; but as such an accession to their numbers diminishes their individual annuity by allowing others to share it, an application of this sort is seldom acceded to, as might be expected."

SECTION G.—MECHANICAL SCIENCE

On the Extent to which existing Works and Practice militate against the profitable Utilisation of Sewage.—Mr. J. Bailey Denton, M. Inst. C.E. The author stated that, notwithstanding the great amount of attention devoted by chemists and other scientific persons during the last twenty-five years to the treatment of sewage, the general opinion arrived at now is that the refuse of towns can only be made to give up its fertilising elements by transporting it direct to the land either by the agency of matter or earth. In support of this view he made two quotations from the reports prepared by the Rivers' Pollution Committee of Inquiry. It is generally admitted that wherever people are congregated, and a number of dwellings exist together, it is not possible to provide for the largely increasing use of water, by a population doubling itself within the period of fifty years, without underground conduits for the discharge of liquid sewage. In nearly all our cities and large towns systematic sewage already exists. In the midland and southern towns water-closets are comparatively numerous, though privies with cess-pools still predominate, but in the northern towns water-closets are comparatively few, and the middens nearly universal. After mentioning various instances in which there is infiltration of subsoil water into the sewers, doing mischief in a variety of ways, the author called attention to the evil of indiscriminately admitting a largely disproportionate quantity of water into the sewers, without any power to regulate the time and extent of dilution. Assuming, with the Rivers Pollution Commissioners, that sewage must be utilised upon the land by the process of irrigation, Mr. Denton proceeded at some length to consider the conditions which should be observed in order to obtain the maximum amount of benefit from sewage farms. He concluded by saying, "With a sewage farm naturally or artificially drained, and the surface sloped so as to make the absorption and filtration of sewage certain; intermittent filtration may be practised by itself at any time when it is desirable to resort to it independently of irrigation. At seasons when the sewage may be applied profitably to vegetation, of course the two processes will proceed together; but it will only be by operations admitting alike of combined or separate action that purification and profit may be secured free from all chance of malaria. With the prospect of applying the sewage of towns extensively to land by way of irrigation, it is most desirable that the proper preparation of land to receive it should be indisputably understood and acted upon.

SOCIETIES AND ACADEMIES

PARIS

Academy of Sciences, Sept. 12.—M. Faye communicated a note on the mode of observing the approaching transit of Venus, in which, after giving some account of Mr. Newcomb's memoir on the same subject, he suggested an application of photography by means of electrical apparatus. M. Faye also presented a note on the chemical agents to be employed in opposition to miasmatic infection, in which he remarks upon the application of the phenic compounds to this purpose.—M. Dumas and M. Chevreul made some observations on the subject of this paper.—A letter was read from M. Sédillot on the surgical indications and the consequences of amputations in connection with wounds.—M. C. Bernard presented a note by M. Rabuteau, on the means of annulling the effects of insufficient alimentation. The author described the effects produced by Coffee in diminishing the waste of material in vital operations, and maintains that by the free use of coffee life may be supported in full activity with much less than the theoretical amount of nourishment. Cocoa and Tea partake of the same qualities.

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