THURSDAY, JUNE 12, 1873

JEREMIAH HORROX

I.

IF national glory can ever be connected with a natural phenomenon, the transit of Venus over the sun's disc may be said to bring peculiar distinction to England. It is in a manner inscribed upon one of the most brilliant pages of our naval history; it led to some of the most remarkable discoveries for which mankind is indebted to our geographical enterprise, and made the renown of our most famous navigator. A hundred and thirty years before Cook, the phenomenon itself was, for the first time in human history, accurately observed in a corner of England, by an English youth, self-taught, and provided with few of the appliances of scientific research. Now that the spectacle, so striking in itself, so sublime in the infrequent regularity of its recurrence, so important as the key to numerous astronomical problems, is again attracting the attention of civilised mankind, now that the expanse of ocean from Honolulu to Kerguelen's Land is about to be dotted with watchers from the other side of the earth, the occasion appears favourable for recalling the memory of the original observer, Jeremiah Horrox, curate of Hoole, near Preston, in his day one of the most insignificant of English hamlets.

The little that is known respecting Horrox's family and circumstances at least suffices to reveal the difficulties with which he had to contend. The place of his birth was Toxteth, near Liverpool. We cannot discover that the date usually assigned, 1619, rests on any good authority, while it is rendered improbable by the fact that in this case he must have been matriculated at thirteen, and ordained at twenty. The first letter of his that has been preserved, dated in the summer of 1636, indicates, moreover, a compass of astronomical knowledge, as well as a general maturity of mind, hardly conceivable in a youth of seventeen; while his references to the discouragements which, previous to his acquaintance with his sympathising correspondent, had almost induced him to renounce astronomical study, bespeak a more protracted period of investigation than would have been possible in such early years. The date 1616, though unauthenticated by any external testimony, may very well be correct. Notwithstanding a doubtful report which traces his family to Scotland, his thoroughly Lancastrian patronymic denotes a local origin. His father's profession is unknown; we suspect him to have been a schoolmaster. The family dwelling is usually identified with a house pulled down a few years since to make room for the railway station. The family was numerous, and although it cannot have been indigent, Jeremiah's matriculation as a sizar at Cambridge, and short stay at the University, prove that it was not rich. His entrance at Emmanuel College, then a stronghold of Puritanism, is conclusive as to the auspices which presided over his bringing-up. This matriculation took place on July 5, 1632; he certainly left the university without a degree, and the fact of his first-recorded astronomical observation, June 7, 1635, having been made at Toxteth, is an almost certain testimony of his recession having taken place before that date. Want of means, and the necessity for contributing to the support of his family, are the only assignable reasons for a step which must have thrown the young student on his own resources, as regarded books, instruments, and intellectual companionship. The first glimpse we obtain of him is from the above-mentioned letter to Crabtree, dated June 21, 1636. From this and subsequent letters we gather that he has been for at least a year an observer of the heavens; that his circumstances are narrow, and prevent him from obtaining the books and instruments he desires; some, however, of the books he incidentally mentions must have been expensive, and can hardly have been procured by him elsewhere than at Cambridge. A list of these in his own handwriting is preserved, and has been noticed by Prof. De Morgan, who ("Companion to the Almanac" 1837) points out that not one was the work of an English mathematician, or printed in this country. It further appears that his time was much engrossed by other pursuits, which no doubt bore reference to his preparation for orders, and to his exertions to support himself in the interim. He was, in all probability, engaged in tuition, to which land-surveying, or some similar occupation, may have been added. Thus three years passed by, at the end of which time we find him curate of Hoole, a village about five miles to the south of Preston, the church of which was at that period a chapel of ease to the adjoining parish of Croston. The patron was Sir Robert Thorall, the incumbent the Rev. James Hyatt. Horrox may be assumed to have been recommended to the latter by their common Puritanism, Mr. Hyatt having been one of the ousted ministers of 1662. He did not, however, retain his curacy much above a year; the cause of his resignation is unknown.

It is now time to treat more specifically of Horrox's correspondence with Crabtree, the source of almost all our information respecting him. Crabtree, a clothier of Broughton, near Manchester, was one of a small band of worthies by whom astronomy was cultivated in the northern counties in those days, some particulars respecting whom will be found in the notes to Sherburne's translation of Manilius. These letters survive in the Latin version of Prof. Wallis, who naturally omitted whatever had no immediate bearing on science. A re-examination of the originals, should these still be extant in the Bodleian Library or elsewhere, might probably result in the retrieval of some interesting biographical particulars. As it is, we obtain many glimpses of the scientific circumstances of the day. Errors were inevitable in the comparative infancy of astronomical science, and the mistakes of the master were naturally a snare to the pupil. Horrox was for a time not only misled, but induced to distrust the accuracy of his own observations by their incompatibility with those of Lansbergius. Crabtree opened his eyes to the errors of the latter, and thus indirectly rendered him the still higher service of leading him to recognise the greatness of Kepler, which Lansbergius had disparaged. His study of Kepler led, as we shall see, to his own great discovery: before entering upon this, however, it will be convenient to dispatch the minor matters of scientific interest contained in the correspondence. It is curious to learn that Horrox's telescope cost him only 2*s.* 6*d.*, and was nevertheless better than some more expensive ones which he had had an opportunity of examining. He did not obtain even this modest instrument until May 1638, about a year before Milton viewed the moon through "the optic glass" of "the Tuscan artist":—

"At evening from the top of Fesole,
Or from Valdarno, to descry new lands,
Rivers or mountains in her spotty globe."

The "mute inglorious Miltons" of Toxteth seem not to have been wholly incurious respecting the researches of their fellow villager, who speaks in another letter of having endeavoured to exhibit Venus in her crescent phase to "sundry bystanders," who however were unable to discern the phenomenon owing to their inexperience in the use of the instrument. The possession of a telescope may have stimulated his desire to become acquainted with the writings of its inventor. Four months later we find him possessed of Galileo's dialogue on the "System of the Universe," and anxious to procure his "Nuncius Sidereus," and treatise on the Solar Spots. He had previously speculated upon the exact period of the creation of the world, which he sought to determine by a combination of astronomical and scriptural data; and upon the origin of comets, which he supposed to be emitted from the sun. The phenomena of the planetary aphelion and perihelion had likewise engaged his attention, and elicited remarks which almost seem prophetic of the great discovery of Sir Isaac Newton. In observing the setting sun he had noticed a raggedness of the margin, which he rightly attributed to atmospheric conditions. During the last three months of his life, when unable to bestow time on astronomical research, he commenced an attentive study of the irregularities of the tides, from which he hoped to obtain a demonstration of the rotation of the earth. The Lancashire coast, where the recess of the tide is very considerable, is highly favourable to similar observations.

(*To be continued.*)

CARUS'S HISTORY OF ZOOLOGY

Geschichte der Zoologie bis auf Joh. Müller und Charles Darwin, von J. Victor Carus. Pp. 739. (München, 1872.)

TWO of the most characteristic qualities of the present time are scepticism and sympathy; and by a happy combination of the ability to investigate statements instead of taking them on trust, and the power of realising past states of knowledge and of feeling, a most important advance has been made in history. But the historical method is not confined to what is commonly so called. It has been applied to philology and philosophy, and has reformed both, while even in the physical sciences its importance is now fully recognised. It is true that a science like Zoology, which deals entirely with objective facts, is more independent of history than some others, and its history does not really begin till the seventeenth century. But as part of the history of the human mind, it will always be important to study the sciences of pre-scientific ages, and when we meet with such a master-mind as that of Aristotle, whatever he wrote becomes of the highest interest because it was his.

The work before us, by the son of the late eminent zoologist of the same name,* is one of the series undertaken by command of the late King of Bavaria, and published by a Historical Commission of the Royal Academy of Sciences in Munich. It embraces the history of the whole body of science in Germany, and the volumes which have already appeared have been written by men of high eminence in their several departments.

* The accomplished author himself is now lecturing in Edinburgh as Prof. W. Thomson's substitute.

Fortunately, however, Prof. Carus does not at all confine himself to Germany, so that the present work is an attempt at a complete history of zoology, from the earliest to the present time. It naturally divides itself into two parts, the first treating of what may be called pre-scientific zoology, which is only of general historical interest, the second tracing the development of zoology, as a science of observation and experiment, from its foundation by Ray and Linnæus. These two sections are handled on a very different scale, for the former occupies more than half the book, and is therefore sufficiently minute, while the whole history of modern zoology is compressed into three hundred pages. The consequence is that, while accurate as to facts, the latter part is often little but a list of names and dates.

We shall therefore simply direct the attention of zoologists to the second portion of Prof. Carus's history as convenient and well-arranged for reference, and dwell here on his detailed account of the less known progress made in ancient and mediæval times towards a knowledge of the varieties and structure of animals.

The first chapter treats of the earliest animals known to man, including those domesticated in prehistoric times. The names of the Ox, Sheep, Goat, Pig, Dog, Horse, and Goose, occur in allied forms in most of the Indo-European languages, and their bones are found among the dust-heaps of the earliest race of men known. The Cat (αἴλουρος), though domesticated in Egypt, was not a household animal till much later in Western Europe: the "cat" of the Greeks and Romans (γαλῆ) being almost certainly the whitebreasted beech-marten (*Martes foina*) a conclusion learnedly and perspicuously established by Prof. Rolleston in a paper published in the *Journal of Anatomy and Physiology*, for November 1867. But the Flea and the Louse appear to have been familiar from the earliest times, and Mice, Flies, and Worms are also among the first named by man. To the same primitive group belong the Bear, the Beaver, which lived in English rivers up to comparatively recent times, and the Wolf and Fox, the names of which (*vulpes*, Wolf) have evidently been confounded.

After a short account of the part taken by animals in early mythology and in the fables common to the Indo-European nations—a chapter which might have been with advantage enlarged from the pages of Grimm, Dasent, and Link—our author enumerates the domestic animals known in classical times, which include, beside those already mentioned, the Camel (confounded with the elephant during the Middle ages), the common Fowl (ὄρνις περσική Aristoph., Av. 485), which was introduced from the East between the date of Homer and Hesiod and that of Æschylus, the Chenalopex, probably identical with our sheldrake (*Tadorna vulpanser*), pigeons of various breeds, and birds of prey which were used for hawking. The list of wild animals was greatly increased by the games of the Roman circus, and many, like the Hippopotamus, Rhinoceros, and Giraffe were better known under the Empire than they have been until very recent times. Pliny mentions the occurrence of the Platinista in the Ganges, but no notice of the Hyrax, a form so familiar to the Hebrews, is to be found in Greek or Roman authors.

The next sections are occupied by a tolerably full account of the knowledge of anatomy and physiology possessed by Aristotle, and by his successors, Herophilus and Erasistratus, and of the attempts made towards a classification of the animal kingdom. The groups recognised by the first, and perhaps the greatest, of naturalists, are surprisingly near to what are now accepted. 1. Viviparous quadrupeds, clothed with hair (*ζωοτόκα τετράποδα*)—Mammalia, exclusive of Cetacea. 2. Birds (*ὄρνιθες*) exclusive of bats. 3. Oviparous quadrupeds, inclusive of snakes and frogs. 4. Cetacea (*κήτη*), with teats and milk (Hist. An., iii. 99). 5. Fishes (*ἰχθύες*). Those with (red) blood are distinguished from the remaining "bloodless" classes. 6. The Cephalopodous mollusks (*μαλάκια*). 7. The testaceous mollusks, including ascidians, cirripedia and echinidæ (*'οστρακόδερμα*). 8. Malacostraca—Crustacea. 9. Insecta (*ἔντομα*) including all air-breathing Arthropoda. Lastly, Starfishes, Sponges, and some other groups, are characterised as partaking of the nature of plants (Zoophyta).

On the whole, Aristotle's zoology is less imperfect than his anatomy. In spite of Prof. Carus's opinion, the well-known passage (Hist. An. i. 39) clearly states what is repeated in two other passages, that the back of the skull is empty, and his views of the position and functions of the heart, lungs, and nerves are scarcely more scientific than Plato's notions of hepatic triangles. Indeed it is difficult to believe that Aristotle can ever have completely dissected a single mammal. The digestive and reproductive systems he understood much better. But beside his wonderful industry in collecting facts, the acuteness and power of generalisation displayed by Aristotle in other branches of science are not wanting in natural history. Thus he remarks that insects with horny wings have no sting. "I have never seen an animal with solid hoofs and two horns." When horns are present there are no canine teeth. Quadrupeds which bring forth their young alive are clothed with hair, those which lay eggs, with scales. Insects with four wings have the sting behind, those with two, in front. Nor is it the least proof of Aristotle's greatness that he gave an impetus to biological science which produced the Alexandrian school of anatomy, and only ended at the beginning of the third century of our era with the death of Galen.

The contributions of Roman authors to zoology, such as those buried in the huge mass of crude and chiefly worthless material which Pliny called natural history, only mark the decay of the science. During the subsequent dark ages (the darkness of which is probably for the most part subjective) the most remarkable work on zoology is the famous "Physiologus," also called the "Bestiarius Theobaldi," of uncertain authorship and date, but known over the whole of Christendom from the eighth to the thirteenth century by translations into Syriac, Armenian, Arabic, Ethiopic, German, English, Icelandic, and French. The Greek text is probably the original, from which the Latin was taken. This long-forgotten book, like Pliny's, includes accounts of plants, stones, and other natural objects, and describes among more common-place animals, mermaids, unicorns, and onocentaurs. There are mentioned, of quadrupeds, the antelope (perhaps the Urus), beaver, elephant, hyæna, monkey, and lion, beside common European species; thirteen species of birds, including the ostrich; of reptiles, several kinds of lizards, and serpents, but only one invertebrate animal, the ant. The original plan appears to have included only the animals mentioned in the Bible, and the chief object of the book is to draw moral lessons from the habits of the creatures described. The "Physiologus," while of great historical interest, is, of course, devoid of even relative scientific value.

Passing over the Arabian naturalists, who added little original, we come to the three writers who represent the science of the Middle Ages when the writings of Aristotle became generally known and the systems of scholastic philosophy were founded—Thomas of Cantimpré, Albertus Magnus, bishop of Ratisbon, and Vincent of Beauvais. They were all Dominicans, and all belong to the thirteenth century, that remarkable era of revolution in philosophy, politics, and art. At this time knowledge of foreign animals was greatly increased by the travels of Marco Polo (1275-1292), who described the wild horses, musk deer, and yaks of Tartary, the camels and asses of Persia, and the rhinoceroses, elephants and tigers of India.

Museums only began to be formed in the sixteenth century when the discovery of America brought to light so many new animals and plants; but for a long time they were what museums still too often are, mere lumber rooms of "Dinge gantz seltzam und fremdt," as Duke Albert of Prussia wrote in 1559. All the earliest anatomical preparations, including the celebrated dissections of Harvey still preserved in the College of Physicians, are dry.

The *Lucidarius*, a medley of stories about animals, which represents in the Renaissance what the Physiologus does in the Middle Ages, appeared in 1479, and like the latter was translated into all the European languages.

The earliest attempt at a System of Zoology was by. Wotton in his *Differentiis Animalium*, published at London in 1550. It is little more than a reproduction of the doctrine of Aristotle. Conrad Gesner's *Historia Animalium* appeared in 1551. Like Wotton, he was a physician, and practised in Switzerland and South Germany. His work is chiefly remarkable for its illustrations, one of which, the figure of the Rhinoceros, was drawn by Albert Dürer. Passing over the names of Aldrovandi (1522-1603), Johnston (1603-1675), and Sperling (1603-1661), the next important work on zoology was Bockart's *Hierozoicon*, published in 1663. This work of the learned Norman Huguenot has been a quarry which succeeding biblical commentators have continually used, but its value is almost entirely literary: indeed it was written rather as a contribution to hermeneutics than to natural science. The figures in a work of Clusius, "Exotica," which belongs to the early part of the seventeenth century, show by those of the sloth, the manatee, the armadillo, humming-bird, cassowary, dodo, penguin, and molucca crab, how much the discoveries made in America, Madagascar, and New Holland, were increasing the list of known animals.

During the first half of the seventeenth century there also appeared the earliest monographs. Thus Nicholas Tulp, the anatomical lecturer in Rembrandt's famous painting at the Hague, gives a description and an admirable engraving on copper of what he calls an "orang-outang," evidently a chimpanzee from Africa; and in the same *Observationes Medicæ* (1641) figures a narwhal as "Unicornis marinus." The *Libellus de Canibus Britannicis* (dedicated to Gesner), of our countryman John Kay (Caius) was earlier than Tulp's papers. It was followed by monographs on the elephant by Lipsius and Caspar Horn, on the stag, with an account of its dissection, by Agricola, of the hippopotamus, from a specimen sent in brine from Damietta to Rome, by Columna, and of fishes in general by Salviani and Rondelet. In 1634 was published at London *Insectorum theatrum*, avowedly founded on the words of Wotton and Gesner, and on a compilation from both which had been begun by Thomas Penn, and interrupted by his death; the next editor was Thomas Mouffet, but he also died several years before it was published. This is a noble monograph, with woodcuts so accurate and characteristic as to compare with the best productions of modern skill. It is also remarkable for containing a full and correct account of the *Acarus scabiei*, which was afterwards so long forgotten. Beside insects (in the Linnæan sense of the word) it describes worms of various kinds, and among them what is apparently a *Bothriocephalus latus*. This species is still more distinctly figured by Tulpius (Obs. Med. tab. vii), but by some strange error it is represented with two heads. Spigelius (*de lumbrico lato*) gravely discusses whether it is an animal at all.

Meantime anatomy and physiology were making rapid progress. Vesalius (1514-1564), the father of modern anatomy, and his contemporary Eustachius, who ventured to oppose his own dissections to the authority of Galen, Fallopius, and his successor at Padua, Fabricius, and the still more illustrious pupil of Fabricius, William Harvey, form a succession of almost unequalled eminence. The dissections of our countryman Thomas Willis (1621-1675) were not confined to human subjects, and the earliest microscopical observations, by Malpighi, Leeuwenhoek, and Hooke, were also to a large extent zoological. After the middle of the seventeenth century the three most illustrious scientific societies were founded, the *Academia Naturæ Curiosorum* (1652) incorporated as the "Leopoldinisch-Carolinische Academie" in 1677, the Royal Society in 1662, and the Académie des Sciences four years later. In 1667 Ray was elected a Fellow of the Royal Society, and began the series of papers which mark the first steps of scientific zoology, and surely prepared the way for his greater successor Linnæus. P. H. PYE-SMITH

OUR BOOK SHELF

Lehrbuch der Physik. Von Dr. Paul Reis, Zweite Lieferung, Leipzig. (Quandt and Bäudel, 1873.)

THE second part of this useful handbook of physics opens with the explanation of Mariotte's Law and the various applications of atmospheric pressure. The next division is devoted to the study of wave motion, which is discussed far more fully than in the ordinary run of scientific text-books. This leads on to acoustics, and we are at once plunged rather abruptly into the subject of musical intervals. The theory of consonance, the cause of the intensity of sound and its mode of propagation make up the novel arrangement of this chapter. Optics occupies the sixth division, and is carefully treated. Especially noteworthy is the chapter on the theory of the absorption and dispersion of light, in which there is an excellent account of spectrum analysis. The part before us breaks off in the discussion of physiological optics, where Helmholtz's researches are in part developed. It is a pity that the engravings are not equal to those generally found in continental text-books.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. No notice is taken of anonymous communications.*]

Jacamar in Britain

I SEE, in your review of Mr. Cordeaux's "Birds of the Humber District," mention of a Jacamar—I presume a Galbula—having been shot by a keeper named S. Fox, near Gainsborough, in 1849. You and the author, Mr. Cordeaux, naturally remark on the "extraordinary" puzzle of the fact.

As one who has often seen the Jacamar in its own tropic forests, and watched its flight and its feeding, I must be allowed to suspect some mistake, unless the most "startling"—in every sense of the word—evidence of the authenticity of the specimen is given.

Ready to believe everything, in such a world of wonders, I might have believed in a Jacamar being blown to south-west Cornwall, Ireland, or Scotland. But in the eastern counties—"Qu'allait il faire dans ce galere là?"

Harrow, June 6 C. KINGSLEY

The Use of Wires in Correcting Echo

[The following letter has been forwarded to us by Mr. J. J. Murphy]:—

Palace, Cork, May 30, 1873

My Dear Mr. Murphy,

Having seen in the newspapers some notices of the use of wires for correcting the echo by breaking the waves of sound in churches and public buildings, we were anxious to try the experiment in the cathedral of St. Fin Barre, Cork, the nave of which is of great height, between 60 and 70 feet, and narrow in proportion to its height. We were unable to obtain any reliable information as to the placing of the wires, so that what we did was very much in the way of experiment. I should state that the desks for the officiating clergy and the choir are placed at the intersection of the transepts, nave, and chancel, so that this may be regarded as the point from which the sound starts. The organ is placed in a gallery at the west end, and the organist seated in this gallery has always heard much more distinctly than the people sitting about two-thirds down the nave, particularly those close to the pillars; but the echo seemed to render the sound indistinct, more especially in the transepts, the north and south walls of which presented a large flat surface, and appeared to us to be probably the source of the echo.

At first we tried the wires strained at a considerable height, the level of the triforium, but they produced comparatively little effect; we then strained a double course of wire at about a height of 12 or 15 feet round the large piers of the central tower, so as to encompass the choir, and other wires completely across the nave and side aisles, and the effect was certainly very good. There was a greater distinctness of sound throughout the building. Our organist, who is a very accomplished musician, did not know that the wires were put up, and remarked to me one day after service that he did not know what it was, but that everything seemed to him in better tune.

This encouraged us to make further experiments. We then strained three wires completely across from the south wall of the south transept to the north wall of the north transept, so as to pass over the heads of the choir, but the effect was quite too great, it seemed to kill the sound, every sound seemed to stop at once, all resonance was gone. These wires we had at once to take down, and I should add that, as regards the organist, the wires over the heads of the choir seemed to produce a much greater effect than those directly between the choir and his seat; it appeared to him as if he had a bad cold and could not hear distinctly.

These wires appeared to prevent the voices rising and filling the cathedral. It seems very difficult to determine where to place the wires so as to produce a really good effect; but that they have a very great effect far beyond what one would have supposed, *a priori*, is admitted by all who have taken an interest in the matter here. Several members of the congregation have remarked that they heard better in the cathedral now, without knowing the cause. We have used very thin wire; a stranger would not perceive it unless his attention were called to it. We hope to make some further experiments especially with regard to the transepts of the cathedral.

The inexpensive nature of the experiment and the important result likely to be obtained make this a matter of great importance, independently of the great interest it possesses in a scientific point of view.

I may add that when in Dublin I attended Divine service in St. Andrew's Church, and having officiated in the church at different times I am well aware of the difficulty of filling it in consequence of the echo, but the use of the wires appeared to have made a very great difference, as I heard most distinctly. It seemed to me, however, that a far greater number were used, than my experience in Cork would have led me to suppose were necessary.

I hope this subject will receive the attention which it deserves.

J. J. Murphy, Esq. ROBERT S. GREGG

Fertilisation of the Wild Pansy

THERE are two points in the structure of the heartsease (*Viola tricolor*) which are not mentioned in Mr. Bennett's interesting article on its fertilisation, but which, I think, deserve notice.* The first of these is the lip of the stigma, which closes the entrance to the spur and must be pushed back by an insect trying to reach the nectary, thereby bending down the head of the stigma, so as to sweep any pollen that may be adhering to those parts of the insect which come into contact with it into its receptacle; while, in withdrawing, the insect necessarily presses against the lower side of the lip, and raises up the whole stigma, thus rendering self-impregnation impossible, or at least highly improbable. Modifications of the same contrivance may be seen in many other flowers, *e.g. Pinguicula*, *Iris*, &c.; it reaches, perhaps, its greatest perfection in *Mimulus* and *Bignonia*,† where, to the usual mechanical disposition of the parts, there is added irritability of the stigmatic lobes, which close together spontaneously when touched, expanding again after a while, if not already pollenated.‡

The second point to which I have alluded is the close, hairy lining of the fore part of the spur, forming a narrow groove at the base of the lowest petal.§ This groove generally contains a quantity of pollen that has fallen from the overhanging anthers. There is also a small tuft of hairs at the base of each of the lateral petals, arching over the essential organs, and forcing an insect to approach the nectary from below. These lateral tufts are present, I believe, in all the violets, but *V. tricolor* (including therein several sub-species) is the only British species which has the spur lined with hairs, as well as the only one not known to bear self-fertile cleistogenous flowers.

Although the flowers of the wild heartsease are quite scentless to our blunt organs, does it follow that they are necessarily so to an insect's far more delicate sense?* Some of the cultivated pansies are very sweet, and I am not aware that this quality has ever been made an object for selection by florists. These large garden pansies are much frequented by *Bombus muscorum*, which may be watched while performing the act of pollenation, as described by Prof. Hildebrand. W. E. HART

Kilderry, Co. Donegal

P.S.—Mr. Farrer, in writing of *Lotus corniculatus* (NATURE, vol. vi. p. 499), says:—"Five of the stamens, viz., those of the inner whorl, are shorter than the others, and their filaments are dilated at the top." Here Mr. Farrer's usually accurate pen seems somehow to have made a slip. It is the long outer stamens, those opposite the calyx-teeth, which have their filaments thus curiously modified for the purpose there explained.

* Both these points have already been described by Prof. Hildebrand ("Die Geschlechter-Vertheilung bei den Pflanzen," p. 53). Unfortunately, I have not the works of Sprengel and Hermann Müller to refer to.

† I have had no opportunity of examining the latter, but from the published descriptions it seems to correspond in its main features with *Mimulus*, of the process of fertilisation of which a full account has been given by Mr. F. E. Kitchener in the *Journal of Botany* for April.

‡ When it becomes necessary to introduce a new word into the language it is always well to select the most appropriate that offers itself. Some time ago Mr. A. W. Bennett wrote to the *Journal of Botany* (vol. ix. p. 112), asking for suggestions for a better rendering of the German word *Bestäubung* than "be-pollenment" or "pollenization." I afterwards (*Journ. Bot.*, vol. x. p. 25) proposed the term "pollenation," which has since been accepted by Mr. Bennett. He, however, continues to use the verb to "pollenize." Now, if I might be allowed the space, I should like to state my reasons for objecting to this expression—(1) The root *pollen* is Latin, while the termination ιζω is Greek. Of course, this objection is over-ruled by common usage, and by itself would go for nothing. (2) The word "pollenize" does not in its structure convey the idea intended. *Bestäuben* means to "sprinkle with dust," to "dust with pollen." The termination "ize," on the other hand, gives the signification of change or conversion; thus to "pollenize" would naturally mean to "pulverize," to "turn to flour or pollen," and might be correctly applied to the processes going on in the substance of the anthers, but not, without violence to grammar, to the application of pollen to the stigma. Numerous precedents might be cited for the use of the word "pollen," unaltered, as a verb, from which would be derivable either "pollenation" or "pollenment," but this would be at the risk of offence to ears scientific. The same objection would apply with still greater force to the word "be-pollen." "Empollen" is more euphonious, but would convey a slightly different meaning. On the whole, the word that I have used in the text is the best that I can think of. Perhaps some of your more classical readers might give us their opinions.

§ Morphologically speaking, this is the uppermost petal, which, by the bending of the peduncle and consequent inversion of the flower, is made to assume the position best fitted to afford a convenient landing-place for insects.

* The flowers of *V. palustris*, which are nearly unicolorous with a few dark lines pointing to the nectary, are apparently scentless; but after standing for a short time in water in a warm room, they become quite sweet.

Fertilisation of Orchids

MR. DARWIN, in his "Fertilisation of Orchids," speaks of a Madagascar orchid (*Angræcum sesquipedale*) with nectaries 11½ inches long, and supposes that these plants must be fertilised by the efforts of huge moths, with probosces capable of such expansion, to obtain the last drops of the nectar which is secreted in the lower part of these whip-like nectaries. Can any of your readers tell me whether moths of such a size are known to inhabit Madagascar? They would probably be Sphingidæ of some kind, as no other moths would combine sufficient size and length of proboscis. W. A. FORBES

Culverlea, Winchester, June 2

Ground Ivy

I HAVE this spring found, in many different places, specimens of ground ivy, having flowers with undeveloped stamens. They seem generally, though not always, to be on different plants from those bearing perfect flowers, and below the average in size, the tube being more slender. Also, in nearly all my specimens, the stigmas diverge in a more or less horizontal direction (across the flower) instead of remaining open in the usual vertical one. Is this second form of the flower common? and if so, may not the greater tendency to horizontal divergence compensate for the want of stamens, by bringing the stigmas into the position most favourable for receiving from an insect any pollen which a previous visit to a perfect flower may have left on its head or back? S. S. D.

Hail Storm

DURING the passage across us this afternoon of a thunderstorm moving at so great a distance above the earth that the thunder was very feeble and the lightning very faint, we had a great hail storm, which commenced with conical-shaped opaque stones of the size of peas, at $4^h\ 27^m$ (only lasting one minute), beginning again at $4^h\ 29^m$ with circular transparent stones having a small opaque nucleus (again only lasting one minute), followed at $4^h\ 33^m$ with flattened stones of the form of common acid drops, transparent, except a thin opaque envelope (which soon melted), and having *externally* in the centre a small rugged piece of ice. The size varied from two to three inches in circumference, and the force with which they fell cut off the leaves from the trees and broke 200 panes of glass in my greenhouses. These stones continued to fall for seven minutes with very heavy rain.

Twelve hailstones were gathered after the storm was over, and on being melted yielded 0·060 inch of water when measured in the glass of an eight-inch gauge, and the amount caught within an eight-inch hoop measured 0·750 of an inch, and this added to the rain, gave 1·430 inches as the amount fallen during the storm. E. J. LOWE

Highfield House Observ., Nottingham, June 3

THERMO-ELECTRICITY *

II.

GUIDED by considerations of Dissipation of Energy, I was led some years ago to the hypothesis that specific heat of electricity must be, like thermal and electric resistance, directly proportional to the absolute temperature. If this were the case, the lines in the diagram would be straight for all metals; and parabolas would be the graphic representation not only of electromotive force, but of the Peltier effect, in terms of the temperature of a junction. And I found by actual measurement of curves plotted from experiment, that, within the range of mercury thermometers, the curves of electromotive force for junctions of any two of iron, cadmium, zinc, copper, silver, gold, lead, and some other metals, are parabolas with their axes vertical; the differences from parabolas being in no case greater than the inevitable errors of experiment and the deviation of mercury thermometers from absolute temperature. If, then, the line for any one of these metals be straight within these limits of temperature, so are those of all the others. This makes the tracing of the diagram within these limits a very simple matter indeed. And an easy verification is furnished by the fact that from the parabolas for metals A and B, and A and C, we can draw the lines for B and C, assuming any line for A; and we can then compare the temperature of the intersection of these lines with that of the neutral point of B and C as found directly. Another verification is supplied by the tangents of the angles at which these parabolas cut the axis of abscissæ, for the sum of two of them ought in every case to be equal to the third.

In fact, if we assume, in accordance with what has been said above,

$$\sigma_1 = k_1 t, \quad \sigma_2 = k_2 t,$$

where k_1 and k_2 are constants, Thomson's formulæ give at once

$$\frac{\Pi}{t} = -\int (k_1 - k_2) dt,$$

or

$$\Pi = (k_1 - k_2)(T_{1,2} - t)t$$

where $T_{1,2}$ (the constant of integration) is obviously the temperature of the neutral point.

Also

$$E = J\int \frac{\Pi}{t} dt = J(k_1 - k_2)\int (T_{1,2} - t)dt$$

$$= J(k_1 - k_2)(t - t_0)(T_{1,2} - \frac{t + t_0}{2})$$

where t_0 is the temperature of the cold junction. This is the parabolic formula already mentioned.

Comparing with the parabola as given by observation we get the values of $k_1 - k_2$ and $T_{1,2}$. Similarly we obtain $k_1 - k_3$ and $T_{1,3}$. Hence we may calculate $k_2 - k_3$, and (by the second equation above) the value of $T_{2,3}$ from the relation

$$(k_1 - k_2)T_{1,2} + (k_2 - k_3)T_{2,3} + (k_3 - k_1)T_{1,3} = 0.$$

Thus we have the means of verification above alluded to—for the equation just written expresses the relation between the tangents of the angles at which the three parabolas cut the axis of abscissæ.

[It is to be remarked that if the circuit consist of one and the same metal, we have

$$k_1 = k_2, \ T = \infty, \ (k_1 - k_2)T = \tau \text{ suppose},$$

whence

$$\Pi = \tau t,$$

which shows that the electric convection of heat may be regarded as an infinitesimal case of Peltier effect between adjacent portions of the same metal at infinitesimally different temperatures.

Also, on the same hypothesis, we have

$$E = J\tau(t - t_0)$$

which seems to accord with the result of some experiments made for me by Mr. Durham, in which the deflection due to the contact of the hot and cold ends of the same wire was shown to be proportional to the difference of temperatures and independent of the actual temperature of either.]

Endeavouring to extend the investigation to temperatures beyond the reach of mercury thermometers, I worked for a long time with a small air-thermometer, of which the principle was suggested to me by Dr. Joule. But this involved very great experimental difficulties, due mainly to chemical action at high temperatures; and, after much unsatisfactory work, I resolved to make one thermo-electric junction play the part of thermometer in observing the indications of another. In fact, an exceedingly elegant result follows at once from the preceding formulæ, if we suppose the specific heat of electricity to be proportional to the absolute temperature in each of four metals, and then draw a curve whose ordinate and abscissa are the simultaneous galvanometric indications of pairs of these metals, with their hot and cold junctions respectively at the same temperatures. For if τ be the difference of absolute temperature of the junctions, we have

$$x = A\tau + B\tau^2$$
$$y = C\tau + D\tau^2$$

where the four constants depend upon the nature of the metals and upon the absolute temperature of the cold junction. These equations give

$$(Dx - By)^2 = (CB - AD)(Cx - Ay)$$

which is the equation of another parabola, also passing through the origin, but with its axis no longer vertical.

A simple proof of this theorem is furnished by the motion of projectiles in vacuo. Suppose a particle to move under gravity, and subject, besides, to another constant force parallel to a given horizontal line—its path would have both ordinate and abscissa parabolic functions of the time. But its path might also be found by compounding into one the two accelerations, and as each of these is constant in direction and magnitude, their resultant will have the same property, and thus the resultant path is a parabola. Tried in this way through ranges of temperature up to a red heat, I found that while some pairs of circuits gave excellent parabolas, others were far from doing so, sometimes in fact giving curves with points of contrary flexure. I was on the point of recurring to the air-thermometer, when I noticed that in nearly every case in which the curve was not a parabola, iron was one of the metals employed; and, by the help of some alloys of platinum, I was enabled to get an idea of the true cause of the anomaly, and afterwards to verify it by an independent method. The cause is this, that while, as Thomson discovered, the specific heat of electricity in iron is *negative* at ordinary temperatures, it becomes *positive* at some temperature near low red heat; and remains positive till near the melting point of iron, where it appears possible, from some of my experiments, that it may again change sign. Thus the line for iron, straight at ordinary temperatures, passes downwards from the first quadrant to the fourth, and thence rises into the first again.

To recur to our analogy, an income represented by the iron line is one which for a number of years steadily diminishes, reaches a minimum, and then steadily increases. If this be associated with a steady expenditure, the fluctuations of capital will depend upon the comparative values of the expenditure and the minimum income. If the expenditure be less than the minimum income, the capital will go on increasing slower and slower to a certain point, then faster and faster; there will be no stationary point, but there will be a point of contrary flexure. If the expenditure be just equal to the minimum income, the point of contrary flexure will be also a stationary point. If the expenditure be greater than the minimum income there will be a maximum of capital, then a point of

* Abstract of the Rede Lecture, concluded from p. 88.

contrary flexure, and then a minimum; the maximum and minimum being the stationary points corresponding to the two occasions on which the expenditure equals the income. The maximum and minimum will obviously be farther apart, and smaller, the larger is the expenditure compared with the minimum income.

The latter part of these statements is well exhibited by the behaviour of circuits of iron, and various alloys of platinum with Iridium, Nickel, and Copper.

[Some of these, involving two, and in one case three, neutral points, were shown.]

In each of these cases there are obviously two neutral points, at least. Now suppose the two junctions raised to the temperatures of these two neutral points respectively, and we have a thermo-electric current maintained *entirely* by the specific heat of electricity, as there is obviously neither absorption nor evolution of heat at either junction. Still further, suppose (as is *very nearly* the case with one of the alloys I have just used) that the specific heat of electricity is *null* in the metal associated with iron, and we have the very remarkable fact of *a current maintained in a circuit, without absorption or evolution of heat at either junction or in one of the metals, but with evolution of heat in one part of the second metal and absorption in another part.* This suggests immediately the idea that iron becomes, as it were, a different metal on being raised above a certain temperature. This may possibly have some connection with the Ferricum and Ferrosum of the chemists; with the change of magnetic properties of iron, and of its electric resistance, at high temperatures. Dr. Russell has kindly enabled me to verify these properties in a specimen of pure iron prepared by Matthiessen. I find similar effects with Nickel at a much lower temperature. The method of control which I employed to satisfy myself that these peculiarities are due to iron and not to the platinum alloys, requires a little explanation. It depends upon the fact that by the help of two metals made into a double arc (wires of the two being stretched side by side, without contact except at the ends) we can explore any portion of the field between the lines for these two metals by simply altering the ratio of the resistances in the two parts of the double arc. Such a complex arrangement gives a line passing through the intersection of the lines of the two constituents, and depending for its position on their relative resistances. I shall not, at this stage of my lecture, trouble you with the formula which gives the line for the double arc in terms of the resistances of the two metals and their lines, but simply show the experiments with the help of a gold and a palladium wire, the one having the specific heat of electricity positive, the other negative; while their neutral point is considerably below the temperature of the room. Between their lines is included the peculiar portion of the iron line, and by making shots at it, as it were, in various directions from the neutral point of gold and palladium, we shall be able to study its bearings.

[Several of these experiments were shown, till finally the gold wire was melted.]

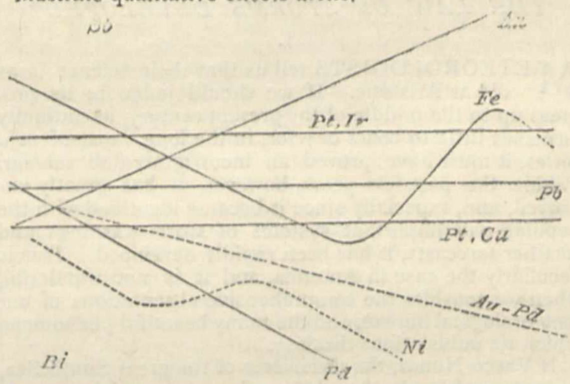
I have here wires of iron, gold, and palladium, bound together at one end, which is to be the hot junction. One end of the galvanometer coil is connected with the free end of the iron wire, the other slides along a long copper wire which connects the free ends of the gold and palladium wires. By sliding it towards either I diminish the resistance of that branch of the double arc and increase that in the other—*i.e.* I give that branch of the double arc the greater importance in the combination.

Throwing the greater part of the resistance into the palladium branch, I find a neutral point at a moderate temperature, but I cannot reach a second without melting the gold. Throw more resistance into the gold, the first neutral point occurs at a higher temperature than before, but a second is attainable. By still further increasing the resistance in the gold the two neutral points gradually approach one another, one rising in temperature the other descending, until at last we reach a maximum-minimum, the result of the confluence of the two points. The line for the double arc is now such as to *touch* the iron line. Still further increase the resistance of the gold, and we find a mere point of inflexion, the galvanometer indications having constantly *risen*, though at a retarded and then accelerated rate, during the heating of the junction.

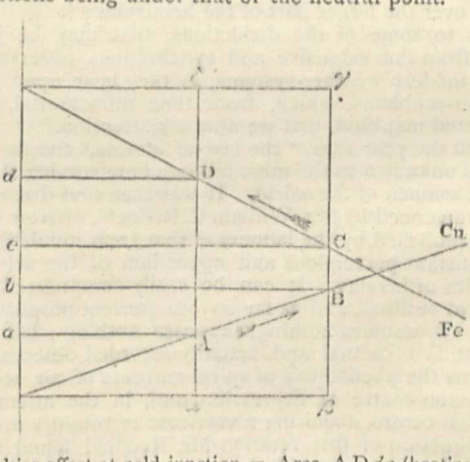
Two of the platinum alloys which I employed with iron seem to give lines almost exactly parallel to the lead line —*i.e.* in them the specific heat of electricity is practically *nil.* When a circuit is formed of these alloys the current therefore depends upon the Peltier effects at the junctions alone, and is sensibly proportional to the difference of their absolute temperatures, thus furnishing a very convenient thermometer for the approximate estimation of high temperatures. I am at present engaged in drawing the thermo-electric diagram in terms of temperatures as given by this combination, and the reduction to absolute temperatures will finally be effected by a comparison of this temporary but very convenient standard with an air-thermometer.

P. G. TAIT

NOTE.—The following rude sketch of a part of the thermo-electric diagram will perhaps render some of the preceding remarks more intelligible. It is drawn to illustrate qualitative effects alone.



The following diagram exhibits the amount of the Thomson and Peltier effects, and of the electromotive force, in a copper-iron circuit, the temperatures of both junctions being under that of the neutral point.



Peltier effect at cold junction	=	Area	ADda	(heating)
,, ,, hot ,,	=	,,	BCcb	(cooling)
Thomson effect in Copper	=	,,	AB$\beta\alpha$	,,
,, ,, Iron	=	,,	DC$\gamma\delta$	,,
Electromotive Force	=	,,	ABCD	

The arrows show the direction of the current; and Euclid's proposition as to parallelograms about the diagonal of a parallelogram shows at once the application of the first law of Thermodynamics to the figure, as the Electromotive force together with the Peltier effect at the cold junction obviously amount to the sum of the two Thomson effects and the Peltier effect at the hot junction.

Also, if we suppose the lines A D, B C, to be very close to one another, since we have always $AD = \frac{\Pi}{t}$ we get $(BC - AD)t = t\delta\left(\frac{\Pi}{t}\right) = -(\sigma_1 - \sigma_2)\delta t$, whose application to the second law is obvious. The reader may easily construct for himself diagrams for other cases of relation of the temperatures of the junctions to that of the neutral point.

Thomson's original paper will be found in the *Transactions of the Royal Society of Edinburgh*, and farther details of my experimental work in recent numbers of the *Proceedings* of the same society. I may avail myself of this opportunity of asking assistance from men of science in procuring wires or foil of the more infusible metals, such as Cobalt, Chromium, Tungsten, &c.

P. G. TAIT

*THE LAW OF STORMS DEVELOPED**

I.

METEOROLOGISTS tell us that their science is as old as Aristotle. If we should judge by its progress up to the middle of the present century, its antiquity furnishes little to boast of; for, in the long lapse of centuries, it must have proved an incorrigibly dull scholar. Within the past few years, however, it has greatly improved, and, especially since it became identified with the popular and important systems of storm-warnings and weather-forecasts, it has been rapidly developed. This is peculiarly the case in America, and it is not wonderful, when we consider the comprehensive observations of our meteorological bureau, and the many beautiful phenomena which its publications disclose.

If Vasco Nunez, the discoverer of the great South Sea, was so awed by the grandeur and expanse of its waters, as seen with the naked eye, how much more may we be impressed as telegraphic meteorology enables us to discover, at a glance, the tossings and undulations of the aërial ocean over the larger part of the hemisphere!

It is to some of the deductions, that may be justly made from the extensive and synchronous observations of the modern weather-systems, as they bear upon those weather-problems, which, from time immemorial, have interested mankind, that we now ask attention.

Until the year 1821, "the law of storms," simple as it is, was unknown to the most profound meteorologists and expert seamen of the world. It was then first discovered and announced by Mr. William C. Redfield, of New York, and established by the labours of that great mind, against the constant perversions and opposition of the scientific empirics of his day. It can be easily comprehended in its great outlines, and as far as our present purposes require. It assumes nothing, supposes nothing; but, from thousands of actual and actually recorded observations, presents the phenomena of spiral currents of air seeking a common centre of depression, and, in the attempt to find that centre, acquiring a vorticose or rotatory motion. The direction of this rotation Mr. Redfield found to be uniformly, in our hemisphere, contrary to that of the hands of a watch, with its face turned upward; and, in the Southern Hemisphere, the rotation is with those hands, or with the sun in its diurnal round. It is easy to see that, if the atmospheric column, resting over any given area of the earth's surface, should, from any cause, be suddenly diminished, or its pressure and intensity be reduced, the gaseous fluid would rush in from all surrounding regions to restore the disturbed equilibrium; and if the earth was not whirling around on its axis, every particle of the centre-seeking air would endeavour to move on the shortest, or the straight line. It is known, from the principles of mechanics, that this endeavour can never strictly be executed, because the axial rotation of the globe incessantly so acts as to throw every body, while in motion, in our hemisphere, to the *right* of the line on which it is moving, no matter whether that line be from east to west, north to south, or at any conceivable angle with the meridians or the equator. Obeying, in part, this tangential impulse, every particle of wind must take up a resultant motion. If it begins to blow toward the depressed centre of the storm as a north wind, it trends to the west, and is felt as a northeaster; if it begins as a south wind, it diverges as a southwester; if as an east wind, it becomes a southeaster; and, if as a west wind, it soon changes into the boreal northwest wind.

It has often been asked whether the storms of our latitudes attain the immense size formerly attributed to them; and many eminent writers have denied the possibility of their reaching a diameter of more than two or three hundred miles. Mr. J. K. Laughton, in his recently-published "Physical Geography," would have us believe that cyclones "do not attain the enormous magnitudes which have been assigned them." But this opinion rests merely upon conjecture, not yet upon a correct physical theory.

It is a well-known fact that the monsoons generated on the central plateau north of the Himalaya Mountains, and the whole system of Asiatic wet monsoons, may be regarded as an immense and prolonged cyclone; extend their "backing" influence into the Indian Ocean, and reach far to the south, through more than forty degrees of latitude (a radius of 2,500 geographical miles), and from the 60th to the 140th meridian of east longitude, far out into the Pacific, beyond the Bonin and Ladrone Islands, southeast of Japan. The whole system of wet monsoons may also be justly regarded as a grand cyclone, whose centre is stationary over the heated plains of Central Asia, whose intro-moving winds, bearing the evaporations of the Asiatic seas and oceans, feed it with meteoric fuel for six months in the year, and whose periphery may be regarded as embracing nearly one-third of the entire eastern hemisphere. Analogy, therefore, warrants the idea of a great cyclone. But, apart from all this, actual observations in different parts of the globe prove the frequency of storms of enormous magnitude. Thus, in the celebrated Gulf-stream storm of 1839, as Sir David Brewster long ago pointed out, several staunch merchantmen were foundering off the coast of Georgia, near Savannah, in the very heart of the gale, at the same hour that the winds in its north-west quadrant were taking the roofs off houses in New York and Boston, more than 800 miles distant—clearly revealing a cyclone whose formation was symmetrical, and whose diameter must have been nearly 1,300 miles. But, not to go back to old data, the West-Indian storm of August 18, 1871, before its centre had moved north of Florida, had begun to draw upon the regions of high barometer in the Northern States, had exerted its influence as far north as New London, Connecticut, and gave us the north-easterly cyclonic winds in the north-west quadrant of the whirl, on the entire Atlantic coast. The more furious cyclone of August 24, 1871, discovered to be then south-east of Florida, and telegraphically fore-announced as likely to endanger the coasts of the Southern

* From the *Popular Science Monthly*. Communicated by the author, Prof. Thompson B. Maury, of the Signal Office, Washington.

States in less than forty-eight hours, appeared on the 26th in full force in Northern Florida, but not until some eight or ten hours after it had set the atmosphere all around it (as far north as Boston) in cyclonic motion, and had caused the storm-cloud to spread itself over the entire region of the United States on the eastern slopes of the Alleghanies, and as far westward as Knoxville, Tennessee. It is no uncommon thing, as Redfield, Espy, Henry, Loomis, and others, long ago showed, for an area of depression on the upper lakes to make itself simultaneously felt as far south as the Gulf of Mexico, and as far east as New England.

If it fell within the scope of the design of this paper to consider the final cause of storms, it would be easy to show that, unless the law of storms ordained a large area, and a far extended path for the meteor, in some degree commensurate with the area of our immense continent, the meteor could not fulfil its office in the terrestrial economy—an office which, apparently, imposes upon it the task of gathering to its centre, through the agency of its intro-moving winds, the idle and inappreciable moisture scattered over the surface of the earth, condensing it into rain and snow, and diffusing it in these forms over immense districts of country.

It is of incalculable importance to observe, and carefully digest the fact, that when a storm-centre or area of low barometer is once formed, it is the nucleus for a vast aggregation and marshalling of meteoric forces. No matter how small at first, under favourable atmospheric conditions, the *courant ascendant* is formed, condensation aloft sets in, and the precipitation only serves to add "fuel to the flame" of the cyclonic engine. This process widens in geographical area, and after a few hours have elapsed, the storm may so develop as to cover a continent with its portentous canopy of cloud, while simultaneously strewing an ocean with wrecks, and throwing out in the upper sky, more than a thousand miles in its front, the fine filaments of the premonitory cirrus and cirronus.

In close connection with the size and magnitude of cyclones must be considered the distance over which they pass from their initial point. Much has been said on this part of our subject, and not a few writers have accepted the doctrine of Admiral Fitzroy, that they progress over but comparatively short distances. For such a view, however, it is impossible to find, either in the nature or physical office of the cyclone, any support whatever. The storm once engendered, no matter in what part of the world, may be stationary or progressive. There are well-authenticated instances of almost stationary cyclones and almost stationary typhoons, of which latter will be remembered the famous gale of the ship *Charles Heddle*—an Indiaman, carried round and round the storm-centre for five days—which progressed not more than 90 miles a day. Indeed we may, as has been said, regard every wet-monsoon region as a stationary and semi-perennial cyclone. Such a meteor has been shown to resemble an eddy moving in the current of a rapid river. The latter may be large or small, while it does not determine, but is determined by, the course of the on-flowing stream. It is true the centre of an eddy or water-hollow may soon be filled up and the whirl disappear; but it is because the depression is not maintained. If the depression could be maintained, it is easy to see that the eddy would continue, and pursue its way, as long as the current in which it is embodied continues to flow; it might be through the length of an Amazon or a Mississippi River. In the case of a cyclonic eddy or whirl, we know the atmospheric depression is maintained as long as the centre moves in a region sufficiently supplied with aqueous vapour to feed it. It is a physical impossibility, as has been often shown, that any storm, however vast or however violent, can prolong its advance or sustain its fury over a dry and desiccated surface. The most extended typhoons of the East, upon entering the dry and rainless continental regions, dwindle into the well-known and diminutive dust-whirlwind, such as Sir S. W. Baker describes as witnessed in Nubia, and as here illustrated, from the admirable pages of Mr. Buchan. The Sahara is a more formidable barrier to the passage of a storm than the majestic mountain wall of the Alps, and the simoom is, notwithstanding the stories of travellers and the legend of swallowing up the army of Cambyses on the African desert, a wasted and worn-out cyclone. In his "Desert World," Mangin, compiling the more accurate observations of the phenomenon, says: "It never prevails over any considerable area, and beyond its limits the atmosphere remains serene and calm; the phenomenon is of brief duration, the atmospheric equilibrium is speedily restored; the heavens recover their serenity; the atmosphere grows clear, and the sand-columns, falling in upon themselves, form a number of little hills or cones, apparently constructed with great care, like those mimic edifices of sand made by children in their pastime." The same writer also mentions a severe simoom which was "over in a couple of hours."

Embedded in the great aërial currents, however, and supplied with abundance of moisture, there is nothing to arrest either the rotatory or progressive movements of the storm. Like the drift-bottles cast upon the current of the ocean, and found after months to have been carried thousands of miles, from the equatorial to the polar parallels, there is every reason to suppose the tropic-cradled gale, and the minor storms also, are borne in the great atmospheric currents through quite as great distances. There is an authentic and well-attested account of a Japanese junk, lost or deserted off Osaka, drifting through the immense arc of the Kuro Siwo's recurvation, and encountered (in latitude 37°, by the brig *Forrester*, March 24, 1815) off the coast of California. That tiny craft must have followed in the bands of westerly winds and warm waters for seventeen months. Why, upon theoretical grounds, should we reject the hypothesis which represents the movement of storm-areas as prolonged for many thousands of leagues, or indeed that which represents them perpetually in motion around given centres of cyclonic or anti-cyclonic areas, keeping pace with the great winds in their eternal circuit?

As a striking corroboration of all this we find—what might have been assumed on theoretical grounds—that the logs and special observations of the Cunard steamships show that a vessel bound from Liverpool westward encounters frequent advancing areas of low pressure, indicating a number of rapidly succeeding barometric hollows or depressions, "each with its own cyclonic wind-system, moving across the Atlantic as eddies chasing each other down a river-current."

The word *cyclone* has frequently, but incorrectly, been used as significant of an enormous or very violent meteor, as if its application was to be confined to the devastating hurricane of the West Indies or the terrific typhoon of the China seas. It simply means a storm which acts in a circular direction, and whose winds converge by radials or sinuous spirals, toward a centre, moving in our hemisphere in the opposite direction to that of the hands of a clock, and in the Southern Hemisphere in a contrary direction. Taking this as the definition of a cyclone, it seems clear, from observation alone, that all storms are to be regarded as cyclonic. Volumes have been written to prove that this is not the case. But we have only to examine a few series of weather-maps from week to week to see that, wherever you have an area of low barometer, into its central hollow the exterior atmosphere from all sides will pour, and that in so doing a rotatory spiral or vorticose storm is generated. The tornado, the simooms, the dust-whirlwind, the fire-storm, even the slow and sluggish storm which moves on our western plains as the labouring wheel of the steamship buried in a heavy sea, all attest that a body cannot move on the earth's surface in a straight line. It is not more true with us that the Gulf Stream turns to the eastward, the Polar Stream to the westward, and the equatorial currents to the northward, than that every air-current, in obedience to the same law, should turn to the *right* of the line along which from any cause it is called to move. The meteorist has therefore only to ascertain by observation where the barometer is lowest, to know at once the direction of the winds from the circumjacent districts, far and near, or at least to test the mathematical law by a grand experiment.

The tangential and centripetal forces, acting at the same time on any particle of air in the storm, may be equal or very unequal, and the cyclonic character of the gale may be well marked or partly concealed. In the tornado, with a diameter of only a few hundred feet, the tangential force may not be appreciable to an observer,

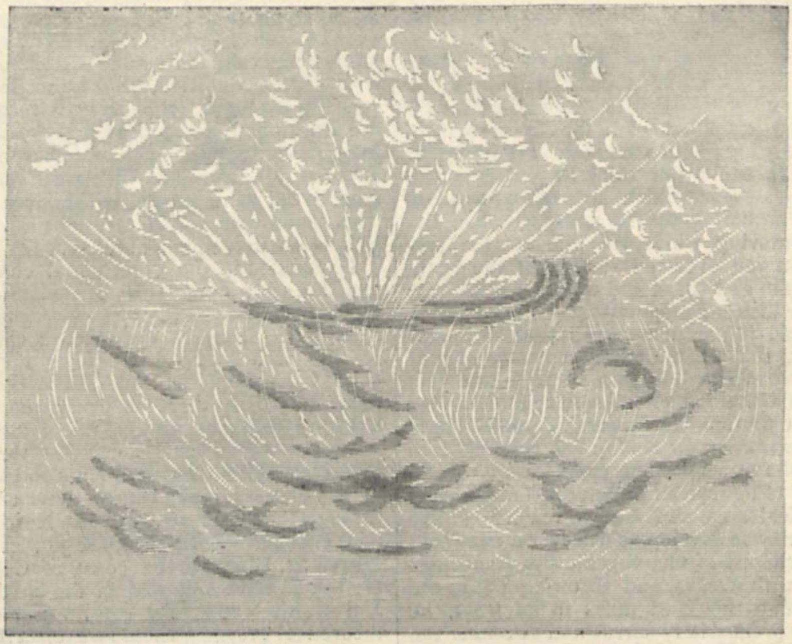


FIG. 1.—Cirrus and Cirronus Clouds.

but it is present, and intensely assists in communicating vorticose motion to the storm, whose roar is heard with awe by the stoutest heart, as it crashes through the forest and even ploughs up the soil of the earth. If the cyclonic or spiral feature should fail to manifest itself in any storm, we ought to look for such failure in the tornado. It is

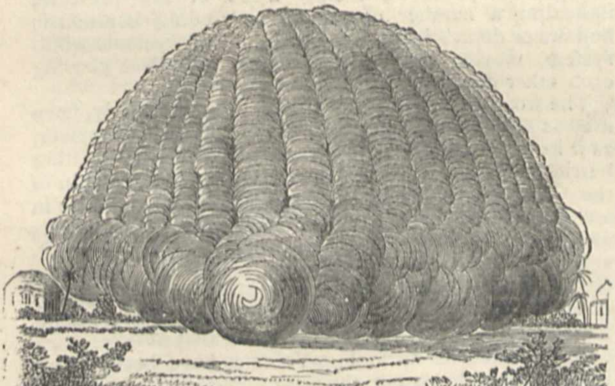


FIG. 2.—The Dust Whirlwind.

true that no barometric readings have ever been taken in the narrow heart of a tornado, but abundant evidence exists of the fearful rarefaction in the centre. While the meteor, once set in motion, may move forward with great velocity and destructiveness, the danger is clearly due to the intro-rushing and gyratory winds. There is not an instance, it is believed, recorded in which a tornado moved as much as 100 miles an hour; probably one-half that velocity would be too high an estimate for its usual and ordinary motion. But the wind, moving straightforward at the rate of 60 or 80 miles an hour, never worked anything like the disaster of a tornado. In the West-Indian hurricane, blowing at the rate of 100 miles an hour, houses have been blown down, ships innumerable stranded; but this is all mere child's-play compared to the suction and whirl of the tornado. The conclusion forced upon us is, that the ravages of the latter are due, not to the weight of the atmosphere, moving as a river-torrent in a straight line, nor to the rush of air behind the travelling vacuum, but to the torsive, racking motion—imparted to every object in its path—due to its gyration. To prove that this gyration is *always* from right to left, or against the hands of a watch, is, of course, practically impossible; but such a direction has often been observed in tornadoes.

It may, therefore, be safely concluded that, for all processes of meteorologic calculation, the disturbance, if not such at first, will soon become cyclonic. All daily weather-charts demonstrate this, not by a laboratory or lecture-room experiment, but on an infinitely wider and grander scale, and in a manner far more conclusive than any merely manual experiment could possibly make to appear. As Mr. Laughton has happily said, "Nature makes no distinction between small and great; the drop of mist that lights gently down on a delicate flower, and the avalanche that sweeps away a village, fall in obedience to one universal law."

(*To be continued.*)

THE CORONAL ATMOSPHERE OF THE SUN*

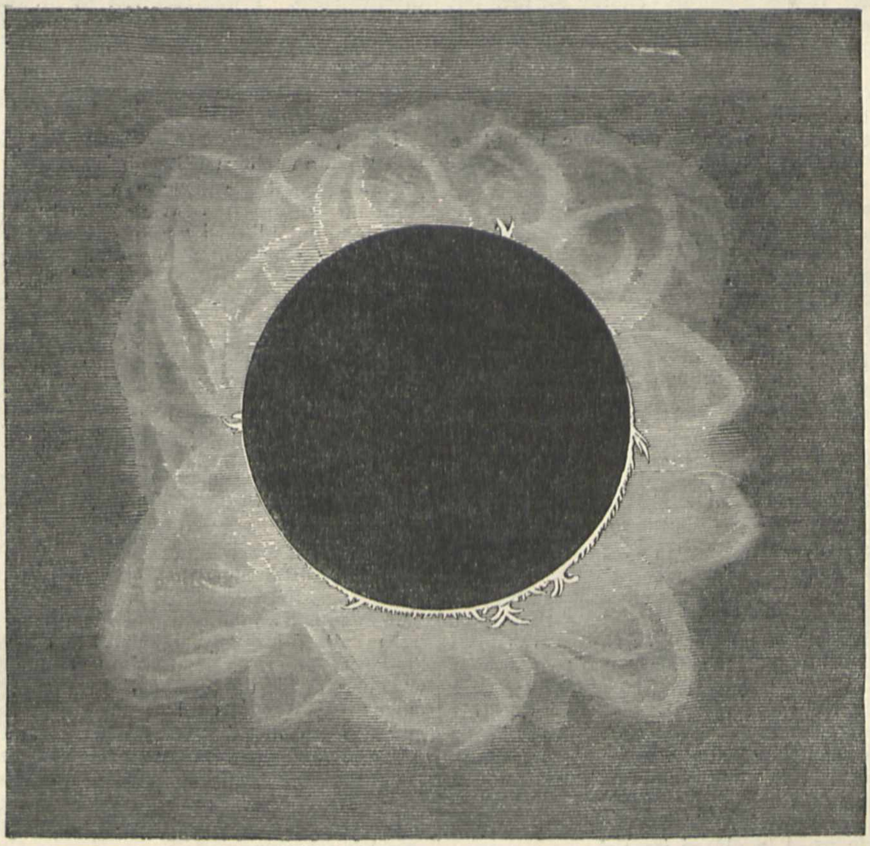
I.

I PROPOSE to bring before you rapidly the principal results obtained by me during the last total eclipse of the sun which I observed in Hindostan, at a point not very far distant from the place where I observed the great eclipse of 1868, which opened up such new horizons with regard to the constitution of the sun.

The last eclipse took place on December 12, 1871. The chief interest of the phenomenon is connected with the problem of the luminous corona which surrounds the sun during total eclipses. When that body is eclipsed by the interposition of the moon, you know that independently of those jets and luminous expansions which are known as protuberances, there is seen around the dark disc of our satellite a magnificent luminous phenomenon, resembling a glory or crown, which extends to 8′, 12′, 15′, and more from the lunar limb, and the frequent strange forms of which are variable at each eclipse. The observation of the eclipse which now occupies our attention, had for its object to definitely fix for us the nature of this singular phenomenon.

The corona is the luminous manifestation which is predominant during a total eclipse, and thus it must, at all times, attract the attention of observers. We possess, indeed, descriptions by Plantade, by Halley, by Louiville, and by others, which go back to the commencement of the 18th century; of course these observers did not indicate the cause of the phenomenon.

Arago and his school form a period in the history of the attempts which have been made to discover the nature of the corona. Our great physical astronomer applied the polariscopic methods to these investigations, but he as well as his successors were baffled. In the "Astronomie Populaire," published in 1856 (tome iii. p. 604), we read the following conclusion upon this



subject: "I regret to say that the disagreement which has been found to exist between the observations made in different places by astronomers equally competent, on the luminous corona, in one and the same eclipse, has covered the question with such obscurities, that it is in the meantime impossible to arrive at any certain conclusion on the cause of the phenomenon."

By means of spectrum analysis the question has entered on a new phase. In 1868, while the nature of the protuberances was discovered, the spectrum of the corona was also obtained; it is true the observers found it continuous,† not an exact observation according to me, which retarded the solution of the question.

In the following year the Americans took up the matter.* They still found the continuous spectrum, but they established the existence of that celebrated green line (1474 in Kirchhoff's scale) which is the prevailing manifestation in the spectrum of the corona, and the meaning of which has yet to be discovered. We owe, moreover, to the Americans some very beautiful photographs of the protuberances, which show also the actinic power of the coronal light.

The eclipse of 1870 was marred by the bad weather. The few observations which could be made confirmed in general the observations of 1869.†

Thus, in 1871, we already possessed some very important data on the corona. Unfortunately these data were as yet incomplete, and above all inconsistent: for

* Translation of a paper read by M. Janssen at the Bordeaux meeting of the French Association for the Advancement of Science.

† Let us mention the observation of M. Rayet, who found luminous prolongation on the principal lines of the spectrum of a protuberance.

* The total eclipse of August 7, 1869, visible in N. America.

† We should mention, nevertheless, the beautiful observations of Mr. Young on the reversion of the lines at the base of the chromosphere.

example, the continuity of the coronal spectrum, on the one hand, was inconsistent with the observations of polarisation of the corona, and on the other hand, led to the scarcely admissible conclusion of a corona formed of solid or liquid incandescent bodies. Thus the new eclipse, which presented a new opportunity of attacking this great question, the calculation of which, it was felt, must now be near, excited a general rivalry.

England took the most considerable share in these observations. The [British Association, the] Royal Society, the Royal Astronomical Society, the Indian Government, worked harmoniously together. Among the noted men of science sent out, we shall mention specially Mr. Norman Lockyer, Colonel Tennant, Lieut. Herschel, Mr. Pogson, Capt. Fyers, &c. Italy was represented by M. Respighi, who was destined to make, on this occasion, some very beautiful observations; Holland by M. Oudemans, &c. At the request of the Academy and the Bureau des Longitudes, I was appointed by the French Government to represent France. It was a glorious charge for me, but at the same time a heavy one, which made me regret that circumstances did not permit of my having any French rivals.

The voyage being decided, it remained for me to settle the plan of my observations, the plan on which to set about to choose instruments, and to choose the place of observation. These points were of prime importance.

With regard to the plan of investigation, I knew very well that, coming after so many able men, I could not hope to solve the problem by simply adding to the numerous observations already made, a few similar observations. It was necessary to study the collection of known facts, to fix the obscure or contradictory points, and to secure a number of rapid observations (the totality would last only about two minutes in India) which should enable us to correct what was inaccurate, to complete what was insufficient, and to form, along with previous observations, a collection of data from which to deduce the true nature of the phenomenon. For example, I had no doubt, in spite of contrary observations, that the spectrum of the corona was not really discontinuous. I was persuaded that it must present, as a dominant characteristic, that of a spectrum of gas, and I found an explanation of the contrary appearances recorded in the feebleness of the light of the corona which did not admit spectra to be obtained, sufficiently luminous for discerning their true constitution. Thus, my intention was to bring my efforts to bear upon this chief point, to some extent the knot of the problem. The point was to obtain a spectrum much more luminous than those of my predecessors. For this purpose I constructed a special telescope having a mirror 37 centimetres in diameter, and a focus of $1^{m}\cdot43$, which gave spectra about 16 times more luminous than those of an ordinary astronomical telescope.

I attached also great importance to seeing the corona at the same time as I analysed its light. A special arrangement of the finder enabled me to attain this end.

Finally, a polarising telescope placed upon the large telescope enabled me to join the polariscopic indications to the other data, and to judge of their agreement. Such were my instrumental arrangements.

The choice of a station was of no less importance. At the point at which we had arrived, our investigations bore upon phenomena so delicate that a sky was required of absolute purity, if I may be permitted the expression. Let us say a few words as to where I sought to realise this second condition.

The eclipse was to be total in the south of Hindostan, at Ceylon, Java, and Australia. Australia was too far away. Java is, in December, subject to the rainy monsoon. There yet remained India and Ceylon, which represented for the line of totality a very considerable exentt, and offered a very great variety of stations from which to choose. To make this choice, I resolved not to trust to the general indications which we possess in Europe as to the climate of India, but to set out early, to visit all the stations, and to decide only after visiting the places, and collecting information on the spot.

I was at Ceylon by the beginning of November, nearly six weeks before the time of the eclipse, which would take place on December 12. On this island I was greatly assisted by the families Laggard and Ferguson, to whom I here beg to express my thanks. The information gathered in the north of the island, where the phenomenon would take place, was not so satisfactory as I desired, and it was agreed to seek for better fortune on the coast of Malabar. I then left Ceylon for Malabar, doubling Cape Comorin. On my way I made some magnetic determinations, and I had the good fortune to find that the magnetic equator, for the dip, passes quite close to Cochin. It was at Telecherry, an English post situated near the line of totality and the French colony of Mahé, that we disembarked. I was received by M. Baudry, a French merchant, who gave me a most gracious welcome and the most active assistance. Mahé was very valuable to me; our governor, M. Liotant, procured for me interpreters who spoke French and the dialects of the districts I was to traverse.

I had, meantime, to choose between the coast proper, the plain, and the stations of the Ghauts and the Neilgherries. As the eclipse was drawing near I could not think of sojourning at each station to make a lengthened investigation. I decided to utilise the telegraph and the railway* for making a simultaneous inquiry as to these stations. M. Baudry, whom I had instructed in observations to make every morning at the hour of the eclipse on the purity of the atmosphere at the coast, sent me these every day by telegraph. I had a similar station on the plain. The baggage had been taken to Coimbatoor, at the centre of the railway, ready to be conveyed speedily to the station selected. I myself visited the Neilgherries, and to gain time, I surveyed these mountains by utilising the night. The mass of information thus collected indicated the great superiority of the Neilgherries. A very careful investigation of this massive mountain-range induced me to locate my station in the north-west, where I had in fact much finer weather than in Dodabetta, one of the highest peaks, where Colonel Tennant and Lieuenant Herschel were afterwards established.

It was upon a mountain near Shoolor, an Indian village, lat. 11° 27′ 8″ N., long. 74° 22′ 5″ E. of Paris, that I fixed my observatory. The instruments were forwarded from Coimbatoor (at the foot of the Neilgherries) to Ootamacund in ox waggons. From Ootamacund to Shoolor the country consisted only of mountain and forest, without carriage roads, and the cases had to be carried on men's shoulders, the many difficulties attending which were happily overcome. Three days before the eclipse the observatory was erected, the instrument in place and ready for observation.

The observation at Shoolor was favoured by a sky of wonderful purity. As I have already indicated, my plan was to examine the corona from the triple point of view of its figure, its spectrum, and its phenomena of polarisation.

I first examined the corona in the telescope; the phenomenon was seen in all its splendour. The general form was that of a curvilinear square (*carré curviligne*), of which the outlines were irregular, but clearly defined. At its greatest height, the corona extended to about 14′ or 16′ from the lunar limb, and only to about half that distance at its narrowest parts. No diagonal was in the direction of the solar equator. All around the limb of the moon were seen trains of light which united towards the highest parts of the corona, and which gave to the entire pheno-

* There is a railway from Madras to the Malabar coast. I found it almost follow the direction of the line of totality.

menon the appearance of a luminous and gigantic dahlia, the centre of which was occupied by the black disc of the moon.

The corona did not present any essential differences of structure at the point of contact and the opposite point. The motion of the moon did not appear to produce any change in the structure. These facts completely convinced me that the corona is a real object, situated beyond the moon, the gradual motion of which body reveals its various parts. Having finished this investigation, I turned my attention to the luminous elements of the phenomenon. My view being yet as distinct as ever, I commenced by examining the spectrum of the highest and least luminous parts of the corona. I placed the slit of the spectroscope at a point two-thirds of a radius from the moon's limb (*environ du bord lunaire*). The spectrum was seen much more vividly than I expected at that distance, a result evidently due to the luminous powers of the telescope and to the whole of the arrangements adopted. This spectrum was not continuous. I recognised at once the hydrogen lines and the green ray (1474).*

This is one point of the highest importance; I removed the slit, remaining always in the high regions of the corona; the spectra always presented the same constitution.

Starting from one of these positions, I descended little by little towards the chromosphere, examining very carefully the changes which might be produced. In proportion as I approached the moon, the spectra became more distinct and appeared richer, but they remained similar to the above in general constitution. In the middle heights of the corona, from 3′ to 6′ of arc, the dark line D was seen, as well as some obscure lines in the green; but these are at the limit of visibility. This observation proves the presence in the corona of reflected solar light, but it is seen that this light is drowned in an abundant extraneous (*étrangère*) luminous emission.

I then set myself to a very important observation, which I expected would give me the spectral relations between the corona and the protuberances. The slit was adjusted so as to take in a portion of the moon, a protuberance, and all the height of the corona. The spectrum of the moon was excessively pale; it appears due principally to atmospheric illumination, and gives a valuable idea of the feeble part which our atmosphere can play in the phenomenon of the corona.

The protuberance gave a very rich spectrum and one of great intensity; I had not time to make a detailed examination. The main point here is the establishment of the fact of the prolongation of the principal rays of the protuberance through all the height of the corona, which clearly demonstrates the existence of hydrogen in the latter.

The green line (1474), so vivid in the spectrum of the corona, appeared interrupted in the spectrum of the protuberance—a very remarkable result. I then gave a few moments to establish satisfactorily the exact correspondence of the lines of the corona with the principal lines of hydrogen in the protuberances.

There remained to me then only a few seconds for polariscopic observation.† The corona presented the characteristics of radial polarisation, and, it ought to be remarked, the maximum of effect is not observed at the lunar limb, but at some minutes from the edge.‡

I had scarcely finished this rapid investigation when the sun reappeared. JANSSEN

(To be continued.)

* My spectroscope was fitted with a very exact scale; but it will be seen how I afterwards made use of the lines of a protuberance as a scale.

† To study polarisation, I have an excellent telescope excellently constructed of biquartz, by M. Prazmowski. This polariscope, placed upon and adjusted to the telescope, can be consulted in an instant.

‡ M. Prazmowski has noted this fact in his excellent polariscopic observations of the eclipse of July 17, 1860.

NOTES

THE subject of the Transit of Venus in 1874 was for the first time officially brought before the notice of the Board of Visitors at the recent Visitation of the Royal Observatory. After a careful exposition of the matter by the Astronomer Royal, and a consideration thereof by the Visitors, it was proposed and seconded by the Astronomical Professors of Cambridge and Oxford, that the Government be requested to provide the means of organising some parties of observers in the Southern Ocean, in the hope that they may find some additional localities for observing the whole duration of the Transit of Venus. In other language, they recommend strongly a sort of *roving* expedition. The meteorological and climatic difficulties both North and South are extremely great: the practical difficulties in the South are very peculiarly so; in despite of the latter, the Board of Visitors were unanimous in their advice to try what best can be done in the sub-antarctic regions. The Astronomer Royal expressed his perfect acquiescence in the proposal of the Visitors; the final decision will rest with the Admiralty and the Government. In coming to this decision, it is proper to add that the Board was in no degree either influenced or assisted by certain discussions which have taken place upon the subject out of doors; their decision would have been just the same whether these discussions had or had not taken place; and the Board came to their conclusion under a full knowledge of the very peculiar climatic and navigational difficulties which seem to attend on the roving expedition which they recommend. It is, in fact, only a realisation of an old proposal by the Astronomer Royal himself, which seems to have been set aside on account of the many serious practical difficulties attending it. The Astronomer Royal also proposed to organise some additional stations dependent on Honolulu.

MESSRS. SAMPSON LOW AND MARSTON are about to publish a volume on the subject of Arctic Exploration, by Mr. Clements Markham, entitled the "Threshold of the Unknown Region." It is intended to give a full account of all that is known of the line which, at present, separates the known from the unknown; to explain the best route by which the unexplored region may be examined; and to enumerate the important scientific results to be derived from Arctic exploration.

NATURALISTS will be glad to hear that the long-talked-of new buildings for the National Museum of Natural History, at South Kensington, have been actually commenced, and that the contractors, Messrs. Baker, have arranged to complete them within three years.

MR. F. T. WARNER, of Winchester, who for some time has been collecting materials for a Flora of Hampshire, has kindly offered the use of his collections and materials to Mr. Frederick Townsend with the proposal that he should complete the Flora. Mr. Townsend has accepted the offer, and as much work remains to be done, he invites the assistance of other botanists in furnishing him with lists of plants or in forming these during the present season. The value of lists will be greatly increased if accompanied by specimens, and in all cases exact localities and dates should be given. It is proposed to divide the country into river-basin districts. Letters should be addressed to Shedfield Lodge, Fareham, but parcels to Botley Station, London and South Western Railway. Mr. Townsend will gladly pay postage or carriage of parcels.

PROFESSOR ROLLESTON, of Oxford, is appointed to deliver the Harveian Oration at the Royal College of Physicians on June 25, at five o'clock.

IT is rumoured that Prof. Tyndall is to receive the honorary degree of D.C.L. from the University of Oxford during the ensuing Commemoration.

THERE will be an election at Magdalen College, Oxford, in October next, to not less than six Demyships and one Exhibition. Of the Demyships, one at least will be mathematical, one at least in Natural Science, and the rest classical. The Exhibition will be in Natural Science. The stipend of the Demyships is 95*l.* per annum, and of the Exhibition 75*l.*, inclusive of all allowances; and they are tenable for five years, provided that the holder does not accept any appointment which in the judgment of the electors will interfere with the completion of his University studies. The examination for the Mathematical and Natural Science Demyships will be held in common with Merton College, at the same time and with the same papers. Each candidate will be considered as standing in the first place at the College at which he has put down his name, and, unless he shall give notice to the contrary, will be regarded as standing at the other College also. In conducting the Examination for Magdalen College Demyships in Natural Science, questions will be put relating to General Physics, to Chemistry, and to Biology, including Human and Comparative Anatomy and Physiology, with the principles of the classification and distribution of Plants and Animals; but a clear and exact knowledge of the principles of any one of the above-mentioned Sciences will be preferred to a more general and less accurate acquaintance with more than one. The Examination in Biology and Chemistry will be partly practical, if necessary. Candidates for Demyships in Natural Science and Mathematics have also to satisfy the Electors of their ability to pass the ordinary Classical Examinations required by the University. Very superior excellence, however, in Natural Science or Mathematics will be allowed to compensate for any deficiency which Candidates may show in the Classical part of the Examination, provided that the Candidate, if elected, undertake to make up this deficiency at a subsequent period. The next Examination will commence on Tuesday, October 7, at 9 A.M. Particulars relating to the examinations in the various subjects may be obtained by applying to the senior tutor.

THERE will be an election at Merton College, Oxford, in October next, to three Postmasterships, value 80*l.* per annum, tenable for five years, or so long as the holder does not accept any appointment incompatible with the full pursuance of his University studies. One of these Postmasterships will be awarded for proficiency in Mathematics, two for proficiency in Physical Science. In the examination for the Mathematical Postmastership, papers will be set in Algebra, Pure Geometry, Trigonometry, Theory of Equations, and Analytical Geometry of two dimensions. Candidates for this Postmastership must not have exceeded four terms of University standing. There is no limit of age. In the examination for the Physical Science Postmasterships, papers will be set in Chemistry, Physics, and Biology; and an opportunity will be given of showing a knowledge of practical work in Chemistry and Biology. The Postmasterships will be given either for special excellence in one subject, or for excellence in two of the three subjects; but no candidate will be examined in more than two subjects. A paper will be set in Elementary Algebra and Geometry, which, *cæteris paribus*, will be of weight in the election to the Postmasterships. Candidates for these Postmasterships must not have exceeded six Terms of University standing. There is no limit of age. The examination will commence on Tuesday, October 7, at 9 A.M., in Merton College Hall. Candidates are required to call on the Warden on the same day between 4 and 5 P.M. The examination will be held in common with Magdalen College at the same time, and with the same papers. Each candidate will be considered as standing, in the first instance, at the College at which he has put down his name, and, unless he has given notice to the contrary, will be regarded as standing at the other College also.

FROM the report on the progress and condition of the Royal Gardens at Kew during the year 1872, just published by Dr. Hooker, it appears that the number of visitors to the gardens shows an increase of 6,000 over that in 1871, very nearly half the number being Sunday visitors. Considerable additions and improvements have been made during the year in various parts of the gardens; the Pinetum now numbers about 1,200 species of coniferous plants, including almost every species that can be grown out of doors in this climate. Seeds and living plants have been received from various parts of the world, and a large number of parcels sent off to our colonies and elsewhere. The acquisitions to the Museums have been considerable, and those to the Herbarium quite exceptional in magnitude and importance, including an extremely valuable presentation by the Rev. C. New of plants collected on the Alpine zone of Kilima-njaro, the only hitherto visited snow-clad mountain in Equatorial Africa; 2,000 Brazilian plants from M. Glaziou, Director of the Botanic Gardens at Rio de Janeiro; and a beautiful collection of Appalachian mosses from Prof. Asa Gray of Cambridge, U.S. Among the publications issued during the last year either officially or by private botanists working at Kew, are the commencement of the second volume of Bentham and Hooker's "Genera Plantarum," the sixth volume of the "Flora Australiensis," by Mr. Bentham; the first part of the "Flora of British India," by Dr. Hooker; several parts of Martius's "Flora Brasiliensis;" Col. Grant's account of the plants collected by Capt. Speke and himself in Central Africa, &c.

SPECIAL certificates of proficiency have been taken at the recent examination for women of the University of London in the following scientific branches:—in Mathematics, by Miss Black and Miss Orme; in Chemistry and Natural Philosophy, by Miss Eaton and Miss Wood; in Human Physiology, by Miss Kilgour of the Ladies' College, Cheltenham, the first time this branch has been taken by a lady: and in Political Economy by Miss Lord and Miss Orme.

MR. GWYN JEFFREYS is about to join the *Challenger* at Madeira for a cruise to the Canaries, Cape de Verde Islands, and Bahia.

M. P. J. VAN BENEDEN describes, in the Bulletin of the Belgian Academy of Sciences, a fossil bird found in the Rupelian clay of Waes, in all respects similar to the existing *Anas Marila*.

LAST Saturday appeared the first number of a new French scientific periodical named *La Nature*. The articles are all popular, and the illustrations are plentiful and well executed.

DR. LEONE LEVI, the Consul-General for Paraguay, is arranging a scientific commission to inquire into the resources of Paraguay. The commission is to consist of botanical, agricultural, geological, mineralogical, and geographical surveyors. It is understood that the Consul-General has in view to appoint a French botanist, of great reputation, and a Scotch agriculturist, but has made no arrangement for the geologist and geographer. Dr. Levi would be glad to give information to anybody who might be willing to offer his co-operation in such a scientific expedition.

LETTERS from Sydney announce the arrival there of the Italian frigate, *Vettore Pisani*, with the naturalist D'Albertis on board, he having been forced to leave New Guinea by repeated attacks of fever. His companion, Odoardo Beccari, well known for the valuable collections he made between 1865 and 1868 in Borneo, and subsequently in N.E. Africa, and which are now in the civic museum of Genoa, has remained in New Guinea. Signor D'Albertis is coming overland to London, and will bring with him a large collection of Zoological specimens.

The second of the two parts of Prof. C. J. Sundevall's new Synopsis of the Classification of Birds has just reached us from Stockholm. This important contribution to ornithological literature, the work of so justly celebrated and painstaking an ornithologist, will be found replete with suggestions, as its author bases his methods of arrangement on details worked out mostly by himself, and with a truly scientific spirit. Some of the arrangements suggested are particularly striking, and though they will probably not all bear the test of future inquiry, yet are undoubtedly based on characters, the importance of which has been too little attended to. Among these peculiarities may be mentioned the placing of the Hoopoo with the Larks, quite away from *Irrisor;* and the adoption of Strickland's eccentric idea that the Pratincole is only a modified Nightjar; to say the least, would it not be more reasonable to call the Nightjar a modified Plover?

The correspondent of the *New York Herald* at Khartoum writes to that journal as follows, under date of April 30:—Three boats engaged in the ivory trade arrived from Gondokoro, April 7, with direct news that Sir Samuel Baker and family were well at Fatuka in the month of February. The reinforcement of 200 men which went forward from Gondokoro reached Baker, at Fatuka, February 5. It was said that with these troops Baker would renew his march towards the Albert Nyanza and the territory of Kaberego (formerly Kamrasi). We are hourly expecting the arrival of a fleet of nineteen Government vessels with mails, which will doubtless bring full particulars of Baker's recent movements.

In No. 145 of the *Gazzetta Ufficiale del Regno d'Italia*, Prof. Lorenzo Respighi, director of the observatory at Campidoglio, gives an account of his observations of the eclipse of May 26. He states that though the maximum phase was so small as to be of little importance, he considered it a good opportunity for making spectroscopic time observations. The method is very simple, and is well known to spectroscopists; it need only be said that it consists in observing accurately the moment at which the dark body of the moon cuts out one of the chromospheric bright lines. Prof. Respighi observed the C-line and was able to perceive the moon's approach across the chromosphere about one minute before first contact, which took place at 46°·30′ from north towards the west point of the sun at 8h. 42m. 35·9s. Roman mean time. The greatest phase occurred at 9h. 7m. when 0·05 of the sun's diameter was covered. The last contact was observed at 10° from the north towards west at 9h. 31m. 3·4s. Roman mean time. The dark moon was seen passing over the chromosphere for about a minute after last contact. The Sicilian expedition had before noticed the power which the spectroscope gave of observing the first and last moments of contact before the times given in the Nautical Almanac, and there can be no doubt that this method is of very great value for time observations of eclipses and transits. Unfortunately in the latter cases it is almost or quite impossible to keep the slit at the exact point at which the body is expected to enter the solar disc on account of the difficulty in obtaining perfect adjustments of the driving clock, &c. It might however be possible to follow the body in transit across the sun and note the exact time of last contact.

We have received the fifteenth report of the East Kent Natural History Society, containing reports of the scientific meetings for the year 1872, and various statistical reports. The society has probably never been in a more prosperous condition as to funds and members, the number of the latter being reported as 109, and the reports of the meetings show that the society is in good working trim. Prefixed is a brief but pointed address by the President, Dr. Mitchinson, in which he points out the utility and some of the dangers of Provincial Natural History Societies. He refers to one evil which is apt to result from the labours of such societies, an evil which has with justice been animadverted on from various quarters recently, viz. a morbid mania for indiscriminate collecting, which is apt to lead to the extinction of the rarer fauna and flora of a district. No doubt, as Dr. Mitchinson says, collecting is inseparable from the thorough study of botany and zoology; but, as he forcibly remarks, no surer sign exists of a spurious pursuit of either or both of these sciences than when rare plants are torn up, and rare animals made still rarer by that selfish acquisitiveness which passes with so many for a love of science. It is the duty of every Natural History Society to discourage such a practice.

The discovery of another planet, No. 131, is telegraphed from America.

It has been resolved by the United States' Government to hold an investigation into the circumstances connected with the loss of the Arctic exploring ship *Polaris* and the death of her commander, Captain Hall.

The publication of the West Kent Natural History, Microscopical, and Photographic Society, is mainly occupied by two valuable and extremely interesting addresses by the president, Mr. J. Jenner Weir, F.L.S. The first was delivered at the annual meeting in February last, and consists chiefly of some careful observations and facts illustrating the doctrine of evolution in the animal kingdom. His other address was delivered at a *soirée* held at the Crystal Palace, its subject being "The Aquarium and its Contents," Mr. Weir noticing some of the most remarkable facts connected with the organisation and habits of the different classes of animals in the aquarium. We are glad to see from the Council's report that the Society continues prosperous and efficient.

Additions to the Brighton Aquarium during the past week:—One Sturgeon (*Accipenser sturio*), from Rye Bay; Smooth Hounds, or Skate-toothed Sharks (*Mustelus vulgaris*); Toper, or White Hound (*Galeus canis*); Gurnards (*Trigla lyra et lineata*); Lesser Weevers (*Trachinus vipera*); Scald Fish (*Arnoglossus laterna*); Sea Trout (*Salmo trutta*); Surmullet (*Mullus surmuletus*); Conger Eels (*Conger vulgaris*); Octopus (*Octopus vulgaris*); Lobsters (*Homarus vulgaris*); Sea Crayfish (*Palinurus vulgaris*); Sea Cucumbers (*Cucumaria pentactes*); Zoophytes (*Alcyonium digitatum, Tubularia indivisa, Pleurobranchia pileus*).

The additions to the Zoological Society's Gardens during the past week include a Grey Ichneumon (*Herpestes griseus*) from India, presented by Mrs. W. Simpson; an Eyed Lizard (*Lacerta ocellata*) from S. Europe, presented by Mr. T. Blackmore; a Loggerhead Turtle (*Thalassochelys caouana*) from the Atlantic Ocean, presented by Lieut. N. Clark; a Rough-legged Buzzard (*Archibuteo lagopus*) from Europe, presented by Mr. W. Stokes; a Blotched Genet (*Genetta tigrina*) from W. Africa, presented by Mr. A. B. Worthington; two Emus (*Dromæus novæ-hollandiæ*) from Australia, presented by Hon. Sir A. Gordon; a Persian Gazelle (*Gazella subgutturosa*), presented by Captain Phillips; seventeen Turtle Doves (*Turtur auritus*) and a Barbary Turtle Dove (*Turtur risorius*), presented by Mr. Gassiot, Jun.; two Lions (*Felis leo*) from Persia; a Wapati Deer (*Cervus canadensis*) from N. America, purchased; four Trumpeter Swans (*Cygnus buccinator*) and a Purple Kaleege (*Euplocamus horsefieldii*) hatched in the Gardens; four Aldrovandi's Lizards (*Plestiodon auritus*) and two Ocellated Skinks (*Seps ocellatus*) from N.W. Africa, deposited.

*ON MUSCULAR IRRITABILITY AFTER SYSTEMIC DEATH**

THE object of the lecture was to put forward certain facts the author had learned on the phenomenon of muscular irritability after systemic death. He included in the same study certain examples in which muscular irritability has for a time ceased, but has become re developed under new conditions. He thus included the study of those states which favour the continuance of irritability or which destroy it, and those conditions which suspend it but do not destroy it. By this method of research the author thinks we may proceed backwards towards living irritability, and may determine upon what that depends with more facility than by experimenting on the phenomena of irritability in the living animal. He imagines that if he knew nothing of the construction of a watch, or why for a certain time a watch maintains its motion, and if he had nobody to teach him these things, he might be better able to arrive at the fact he wanted by trying to set the motionless watch into motion than by interfering with it while it is in motion.

The record of experimental endeavour carried out with the design above explained, included a review of the work of twenty-five years. The subjects brought under consideration were arranged as follows:—

(1) The effect of cold on muscular irritability after systemic death.

(2) The effect of motor forces, mechanical, calorific, electrical.

(3) The effect of abstracting and supplying blood.

(4) The effect of certain chemical agents, organic and inorganic.

Effects of Cold

Previous to the time of John Hunter it was supposed that cold was the most effective agent for destroying muscular irritability. The effects of cold employed in various ways in the author's experimental researches were now detailed systematically. The effect of cold in suspending the muscular irritability of fish, reptiles, and frogs was first described. On all these animals it was shown that cold could be made to suspend without destroying the muscular irritability, for a long period of time, and that in fish, carp (on which the author had made the greatest number of experiments) the restoration of irritability could be perfected to the extent of the restoration of the living function.

Passing to warm-blooded animals, the author showed that in the process of cooling in every animal that has been suddenly deprived of life without mechanical injury, there is a period in the process when general muscular irritability may be made manifest. He demonstrates this fact by the simple experiment of throwing a current of water heated to 120° Fahr. over the arterial system of the recently dead animal. If the surrounding temperature be high at the time of this experiment, the operation should be performed within a few minutes after death; but if the temperature be below freezing-point, it may be delayed for a long period. In one experiment the author reproduced active muscular contraction in an animal that had lain dead and exposed to cold, 6° below freezing-point, for a period of three hours. In this case the muscles generally remained irritable for seven minutes after the injection of the heated water, while in the muscles of the limbs, by repeating the injection at intervals, the irritability was maintained for two hours.

The author drew a comparison between these experimental results and the phenomena of muscular irritability that have been observed in the human subject after death by cholera. The movements were not conscious, nor were they promoted by electrical excitation; but the flexors and extensors belonging to each part in which there is movement are alternately contracted and relaxed as if from some internal influence.

The influence of cold in suspending without destroying muscular irritability was further evidenced by the experiment of subjecting some young animals to death by the process of drowning them in ice-cold water. It was shown that in the kitten the muscular irritability may be restored to the complete re-establishment of life after a period of two hours of apparent systemic death, and although the muscles when the animal is first removed from the water give no response to the galvanic current. This same continuance of irritability after apparent systemic death by drowning in ice-cold water has been observed in the human subject, not in so determinate, but in an approximated degree. An instance was adduced in which a youth who had been deeply immersed for twelve minutes in ice-cold water retained muscular irritability so perfectly that he recovered, regained consciousness, and lived for a period of seven hours.

Commenting on the method of restoration of irritability, the author showed that a certain period of time is required before the irritability is raised from a mere passive condition, in which it responds only to external stimuli, into the condition necessary for independent active contractility. The change of condition from the passive to the active, when it does occur, is so sudden as to seem instantaneous at first, then it is slowly repeated. This rule holds good in respect to voluntary muscles and involuntary. It is specially true in regard to the heart, which organ, the author states, may perform its office under two distinct degrees of tension or pressure—a low tension, in which the organ itself is reduced in size, and moves almost insensibly; and a full tension, in which it is of larger size, and moves with a sufficient power to impel the blood so as to overcome the arterial elasticity and the capillary resistance.

Another fact bearing on this subject is that in rapid decline of muscular irritability the muscles most concerned in the support of the organic functions, namely, the heart and the muscles of respiration, are the last to yield up their spontaneous power; but when they have lost their power, they are the last to regain it. To this rule there is one exception, viz., in the muscular fibre of the right auricle of the heart.

The author then explained that the degree of cold which suspended irritability is fixed within certain measures of degree, from 38° to 28° F. being the most favourable degrees of exposure.

Effect of Motor Forces

Cold, by the inertia it induces, suspends, under certain conditions, but does not destroy muscular irritability. The motor forces, on the contrary, quicken the irritability for a brief period, and then completely destroy it. The mode in which all the motor forces act in arresting irritability is by the induction of a contractile state, which, once established, remains permanent. The author here related his experiments on the effect of the different forces upon the right auricle of the heart, and reported as the result of his observations that, while all the forces act ultimately alike in producing permanent contraction, the mechanical excitation is much slower than the calorific; while electrical excitation appears to hold an intermediate place, as if it were a combination of mere mechanical motion with an increased temperature. Electrical tension may nevertheless be increased so as to rival heat in its immediate effect on contraction.

The author here traced out the results of a series of short sharp irritations of muscle with a needle-point, and compared them with the effect of a blow, showing that in each case rigidity follows, but is much slower in development when it is excited by the needle.

The influence of heat in destroying irritability by its power in producing permanent contraction was described from experiments bearing on the relation of temperature to the muscular contraction of different animals—frogs, pigeons, and rabbits. It was shown that a relative rise in temperature in each class, a rise averaging 12° in Fahr. scale, from the natural temperature of the animal was the efficient for producing permanent rigidity, the cause of the ultimate rigidity being coagulation of the myosine.

The effect of electrical excitation is in the same direction, but is varied according to the mode in which the excitation is performed. Discharge from the Leyden jar produces contraction, which is permanent or intermittent in accordance with the mass of the muscle and the intensity of the discharge. This fact was elucidated by reference to a series of experiments with a Leyden battery, placed in cascade, and the effect produced by the discharge from 96 feet of surface upon animals of different sizes and weights, from sheep down to pigeons, as well as on sections of the bodies of the same animals immediately after death. The experimental facts demonstrated that with an efficient discharge the whole muscular system of a small animal could be fixed instantly in the rigidity of death, and that the precise position of the animal at the period immediately preceding death was retained with such perfection, so sudden was the change, that nothing but physical examination by the hand could bring to the mind the fact that the animal had passed from life into death.

But the same shock passed through a sheep weighing 54 pounds produced only a temporary contraction of muscle,

* The Croonian Lecture, by Benjamin W. Richardson, M.A., M.D., F.R.S.

and required repetition before the rigidity was rendered permanent.

By employing discharges of less tension it was found that muscles, or special tracts of muscles, in the same animal immediately after its death, could be made rigid quickly or slowly by variation of the intensity of the discharge.

The effect of the intermittent electro-magnetic current was next brought forward, and was shown to resemble closely that of the simple electrical discharge from the Leyden phial.

Intensified it induces permanent contraction; and if it be repeated even with low tension, so as to call forth contraction, it destroys the irritability, *cæteris paribus*, more quickly than if the muscle had been left to itself.

Parenthetically, the lecturer dwelt here on the common practice, after sudden death, of endeavouring to excite the action of the enfeebled heart by passing through it an electrical current. Some practitioners, said the author, have gone so far as to introduce a needle into the heart itself, and to make the needle act as one of the conductors from a battery. Such experimentalists, before they undertake this operation on the human subject, should at least observe the effect of the agency they are employing on the exposed heart of an inferior animal recently and suddenly killed by drowning or by a narcotic vapour. They would learn then with what infinite facility the muscular irritability of the heart, in all its parts, is excited for a moment only to be permanently destroyed. They would learn that if blood be not passing through the muscular structure concurrently with their exciting current, they could not more effectually arrest function than by the very method they have adopted to sustain it.

The influence of the continuous current on muscular irritability was introduced by the author, together with a special reference to the first experiments of Aldini on the bodies of malefactors who had been recently executed; and it was shown from Aldini's most noted experiment how largely the phenomena of motion he induced in a dead man, and the recital of which caused so much sensation in the year 1803, was due, not to the galvanism, but to the circumstance that the dead body had been exposed for the hour after death and before the experiments commenced, to the action of cold two degrees below freezing-point. On the whole the continuous current acts on muscular fibre after the manner of heat. If the muscle, recently dead, be exposed to cold, the current, when sufficient, restores for a limited period the irritability, and finally destroys it by inducing persistent contraction. If the muscle, recently dead, be left at its natural temperature, the current simply shortens the period of irritability by quickening contraction.

Abstraction and Supply of Blood

Under this head the author first considered the effect of abstraction of blood from the living muscular fibre. He showed that when the flow of blood was very rapid, there was invariably a given period of muscular excitation. In sheep killed in the slaughterhouse he found that this muscular excitement occurred at the time when the proportion of blood removed from the animal was equivalent to about the 320th part of the weight of the animal. The increased irritability passes rapidly into general convulsion without consciousness, and, as a rule, ceases for a time with a temporary cessation of further loss of blood. After this the irritability remains, if the bleeding be arrested altogether, and can be called into action by any external stimulus, although it is rarely spontaneously manifested when the vessels are left divided and open. After an interval of one or two minutes there is a recurrence of loss of blood, followed by a muscular excitement which marks the moment of systemic death.

The fact of the two stages of exalted muscular irritability during abstraction of blood is important, as indicating the two different tensions of muscle to which reference has already been made. The first convulsive action, convulsion of syncope, marks a definite period, when the tension of the heart and therewith the whole vascular system is reduced to a degree of action well defined and attended with definite phenomena. The second excitement, convulsion of death, indicates the period when the passive or lower tension of the muscular power ceases.

A distinction was here drawn by the author between the muscular conditions present during syncope and during death. Syncope, it was urged, means the continued action of the heart at a low tension, from which it can be suddenly raised into full tension with restoration of the powers of life; death means the cessation of the lowest tension at which the heart can effectively work.

It was shown that in all the cases of restored animation after apparent death, the condition of the heart was that of a muscle acting under the lower degree of tension.

The experiments of the author for re-establishing artificial respiration together with artificial circulation, and of these combined with electrical excitation of the nervous centres, were next referred to; but as they had already formed the subject of a paper read before the Society, they were but briefly dwelt upon.

Effect of some Chemical Agents

In this portion of his lecture the author adduced a series of experimental researches with various chemical substances, organic, inorganic, and intermediate, which tend to prolong the period of muscular irritability by diffusion through the tissues of animals recently dead. These substances, which suspend irritability, act in two ways. Some, like chloride of sodium and other soluble saline substances, act merely by holding the coagulable fluid of the muscular tissue in a continued state of fluidity; others seem to have a different action, and to hold the nervous function also in suspense. The nitrite of amyl and other members of the nitrite series belong to this last-named class of agents, and some of the cyanogen bodies exert a similar influence. In experiments with nitrite of amyl on cold-blooded animals (frogs), the author had suspended muscular irritability for a period of nine days, and had then seen it restored to the extent even of restoration of life. In one instance this restoration took place after the commencement of decomposition in the web of the foot of the animal. In warm-blooded animals a series of suspensions had been effected by nitrites and also by cyanogens, not for so long a period, but for periods of hours, in one instance extending to ten hours.

In the whole series of his inquiries no fact had impressed the author more forcibly than this: that the muscular irritability, in so far as it belongs to the muscle, may be sustained for hours after the nervous excitation which calls it into spontaneous action has ceased. Hereupon he infers that after death the nervous matter undergoes a change of condition which, *in result*, is identical with that change in muscle which we call rigor. There is evidence, moreover, from some rare cases, that the final inertia of nervous matter may be suspended and revived, so that all the muscles may be reanimated. This point was elucidated by reference to the phenomena that had recently been observed by Mr. Wadsdale Watson, of Newport, Monmouthshire, on a double monster, drawings of which were placed before the society. In this instance two children were born so attached that the separation of them was impossible. Both lived equally for three hours after birth, and then one died and remained dead for three hours, while the other lived. At the end of the time named the dead child recommenced to breathe, and showed other signs of restored muscular power; then it sank into a seemed death, but at intervals of about four hours moved again; at length, twenty-three hours after its first apparent death, during a fit of crying of the living child, it recovered sufficient power to breathe and even to cry, and manifested evidence of life in all its muscles, except the heart, for twenty minutes, when it had a severe convulsion, which closed all further motion.

In this instance the author believed that the retention of spontaneous muscular irritability depended upon the retention in the nervous organism of the conditions necessary for independent action. He then concluded by giving a description of his researches as to the possibility of suspending nervous changes incident to death, so as to retain the conditions requisite for the communication of nervous impulse to muscular fibre.

SCIENTIFIC SERIALS

Annalen der Chemie und Pharmacie, Neue Reihe, Band xci., Heft 1, May 6, 1873.—The number opens with a long paper by Oscar Jacobsen on the gases of sea-water. Notices of former researches on this subject are given. In a table the results of 93 analyses by the author are given with the localities of collection. These are in the North Sea and the Baltic.—On the oxidation of allantoin by means of potassic ferricyanide, by F. C. E. van Embden. The two bodies were mixed, one molecule of each, in solution, and the mixture acidulated with acetic acid. A crystalline precipitate was produced, having the formula $C_4N_3H_2KO_4$. This the author regards as the potassium salt of the new acid $C_4N_3H_3O_4$, which he proposes to call allantoxanic acid. Various other salts are described. The acid is found to be bibasic.—On the action of sodium-amalgam on dinitrohephtylic acid, by H. A. Kullhem. The result of the action appears to be the formation of a monobasic acid having the formula $C_6H_{10}(NO_2)O_2$.—On the products of the decomposition of the chlorhydrin of glyceric acid, by Messrs. Werigo and Okulitsh.—On a new acid from aloes, by P. Weselksy. The body in question was obtained from Socotra aloes; its formula is, $C_9H_{12}O_4$ when dried in the air, and its anhydride has the formula $C_{18}H_{18}O_5$. The acid is apparently dibasic.—Dr. H. Sprengel communicates a paper on the water air-pump.—On liquid carbonic anhydride, by L. Callitel, is a translation from the author's late paper in the *Comptes Rendus*—On the addition of cyanamide, by Dr. E. Baumann, is an account of the compounds formed when this body is added to various others.—On the combination of bromine and ether, by P. Schutzenberger, has already appeared in the *Comptes Rendus*.—An examination of a new alkaloid, by Prof. Hlasiwetz. The body in question is a product of the oxidation of cinchonin.—On the isomers of dinitrophenol, by H. Hübner and W. Schneider.—On the nature of sulpho and sulphonitrobibrombenzolic acid, by H. Hübner and R. Douglas Williams.—On the synthesis of carbazol and on phenathren, by C. Graebe.—Contributions to the history of the orcins, by J. Stenhouse, has already appeared in the Proceedings of the Royal Society, the present communication, No. III. of the series, deals with the amido-derivatives of those bodies.—On a new method of preparing carbonic tetrabromide from bromoform, by J. Habermann. The author acted on bromoform in the presence of potash with bromine. The mixture exposed to direct sunlight for 5–6 days gives a good product of tetrabromide. In the dark, after an exposure of three months, only a trace was formed. The reaction occurs as follows:—

$$CHBr_3 + Br_2 + KHO = CBr_4 + KBr + H_2O.$$

SOCIETIES AND ACADEMIES

LONDON

Royal Society, May 1.—"On the Condensation of a Mixture of Air and Steam upon Cold Surfaces." By Prof. Osborne Reynolds.

The object of this investigation is to ascertain how far the pressure of a small quantity of air affects the power of a cold surface to condense steam.

The conclusions which the author draws from the experiments are as follows:—

1. That a small quantity of air in steam does very much retard its condensation upon a cold surface; that, in fact, there is no limit to the rate at which pure steam will condense but the power of the surface to carry off the heat.

2. That the rate of condensation diminishes rapidly, and nearly uniformly as the pressure of air increases from two to ten per cent. that of the steam, and then less and less rapidly until thirty per cent. is reached, after which the rate of condensation remains nearly constant.

4. That in consequence of this effect of air the necessary size of a surface-condenser for a steam-engine increases very rapidly with the quantity of air allowed to be present within it.

5. That by mixing air with the steam before it is used, the condensation at the surface of a cylinder may be greatly diminished, and consequently the efficiency of the engine increased.

6. That the maximum effect, or nearly so, will be obtained when the pressure of the air is one-tenth that of the steam, or when about two cubic feet of air at the pressure of the atmosphere and the temperature 60° F. are mixed with each pound of steam.

As this investigation was nearly completed, the author's attention was called to a statement by Sir W. Armstrong, to the effect that Mr. Siemens had suggested as an explanation of the otherwise anomalous advantage of forcing air into the boiler of a steam-engine, that the air may prevent, in a great measure, the condensation at the surface of the cylinder. It would thus seem that Mr. Siemens has already suggested the probability of the fact which is proved in this investigation. The author is not aware, however, that any previous experiments have been made on the subject, and therefore he offers these results as independent testimony of the correctness of Mr. Siemens's views as well as of his own.

"On the effect of Pressure on the Character of the Spectra o Gases." By C. H. Stearn and G. H. Lee.

May 8.—"Contributions to the Study of the Errant Annelides of the Older Palæozoic Rocks." By Prof. H. Alleyne Nicholson, M.D., F.R.S.E.

In this communication the author endeavoured to elucidate the abundant and obscure organic remains which are found so commonly in the Palæozoic Rocks, and especially in the Silurian strata of Britain, and which are generally known by the vague and convenient names of "Fucoids," "Annelide burrows," and "Tracks." After expressing his opinion that the first step towards the study of these obscure fossils lay in the provisional grouping and naming of the more marked forms which are already known to exist, the author proceeded to divide the remains under consideration into two great groups. In the first of these groups are those fossils which are truly the *burrows* of marine worms, as distinguished from mere trails and surface-tracks. Some of these burrows (*Scolithus*) are more or less nearly vertical in direction as regards the strata in which they are found; and they are to be looked upon as being true burrows of habitation. In this section are placed the genera *Scolithus*, *Arenicolites*, and *Histioderma*.

The second great group of Annelide remains comprises genuine surface-trails or "tracks," which of necessity never pass below the surface of the bed on which they occur.

"The Action of Light on the Electrical Resistance of Selenium." By Lieut. Sale, R.E. Communicated by J. N. Lockyer, F.R.S.

The following were the general results of the experiments:—

1. That the resistance of selenium is largely affected by exposure to light.

2. That this effect is not produced by the actinic rays, but is at a maximum at, or just outside the red rays, at a place nearly coincident with the locus of the maximum of the heat-rays.

3. That the effect of varying resistances is certainly not due to any change of temperature in the bar of selenium.

4. That the effect produced on exposure to light is sensibly instantaneous, but that on cutting off the light the return to the normal resistance is not so rapid.

It would seem that there exists a power in rays, nearly coincident with the heat-rays of high intensity, of altering instantaneously and without change of temperature the molecular condition of this particular element.

May 15.—"On Jeypoorite, a Sulph-antimonial Arsenide of Cobalt." By Major W. A. Ross, R.A. Communicated by Prof. H. Miller, Foreign Sec. R.S.

"Determination of the Number of Electrostatic Units in the Electromagnetic Unit made in the Physical Laboratory of Glasgow University." By Dugald M'Kichan, M.A.

The object of this paper is to describe experiments made at intervals from 1870 to 1872 in the Physical Laboratory of Glasgow University to determine the relation between the fundamental units in the two systems of absolute electrical measurement, the electromagnetic and the electrostatic. A summary is also given of the results of similar observations made by W. F. King in 1867 and 1868.

The two systems of electrical measurement, or the units which they employ, are founded on the fundamental units of time, mass, and space applied to the observed effects of electricity at rest and electricity in motion. The dimensions of quantity in the two systems are such that the ratio of the electromagnetic and the electrostatic unit of quantity is expressible as a velocity.

This velocity, usually known as v, is not only of great importance in all combinations of electromagnetic and electrostatic action, but it is also of great scientific importance in the theory of the propagation of electromagnetic disturbances through a dielectric medium. It occupies a very important place in the development of the electromagnetic theory of light by Professor Clerk Maxwell, according to whose theory this velocity v is the same as the velocity of light.

The first experimental determination of v was made by Weber from a common electrostatic and electromagnetic measure of capacity. The result of Weber's experiments was that v was $310{\cdot}74 \times 10^8$ centims. per second.

Another determination was made by Prof. Clerk Maxwell in 1868, by means of a direct comparison of electrostatic attraction with electromagnetic repulsion. His experiments gave $v = 288{\cdot}0 \times 10^8$ centims. per second.

The value of v given by the experiments here described is 293×10^8 centims. per second. The method employed was that of obtaining an absolute electrostatic and an absolute electromagnetic measurement of the same electromotive force. v is defined as the ratio of the units of quantity in the two systems; but it follows from the definition of electro-motive force, that v is also the ratio of the units of electromotive force in the two systems.

The electromotive force, or the difference of potentials between the two poles of a constant Daniell's battery, was measured electrostatically by means of Sir William Thomson's absolute electrometer. The absolute electromagnetic value of this electromotive force was given by the effect of the current which it maintained in the circuit of an electrodynamometer. The determination of this value depended on the resistance of the electrodynamometer-circuit, which was reckoned in terms of the absolute value of the British-Association standard unit of resistance. Any correction which may hereafter be found to be applicable to the absolute value of this standard coil, as measured at King's College by Professors Clerk Maxwell, Balfour Stewart, and Fleeming Jenkin, must be applied to the value of v give above.

The comparisons made in 1867 and 1868 by Mr. King gave as the mean value of v, $284{\cdot}6 \times 10^8$ centims. per second. The experiments made in 1870 with the new absolute electrometer gave as the mean result $v = 294{\cdot}5 \times 10^8$ centims. per second. The result of the later observations made under much more favourable circumstances was $v = 292{\cdot}4 \times 10^8$ centims. per second. The latest observations (1872) furnish the most probable value of v, 293×10^8 centims. per second.

Zoological Society, June 3.—Viscount Walden, F.R.S., president, in the chair. The secretary read a report on the additions that had been made to the Society's collection during the month of May. The following, among other objects, was exhibited:—The figure of a supposed new species of *Chelodina* from the Burnett River, Queensland.—A letter was read from Dr. George Bennett, F.Z.S., referring to the supposed existence of a species of Tree Kangaroo (*Dendrolagus*) in Northern Queensland, some such animal being apparently well known to the blacks of Cardwell.—A memoir was read by the Viscount Walden on the birds of the Philippine Archipelago, founded mainly on the recent collections of Dr. A. B. Meyer, but containing a complete account of all the known species of Philippine birds, and remarks on their geographical range. The total number of known Philippine species was estimated at 215, but a large number of the islands remained unexplored.—A paper was read by Sir Victor Brooke, Bart., F.Z.S., on the antelopes of the genus *Gazella*, of which 20 species or "persistent modifications," as the author preferred to call them, were recognisable. Sir Victor Brooke entered at full length into the questions connected with the present geographical distribution of the group, and its supposed descent from pliocene and miocene forms.—Mr. A. H. Garrod read a paper on the pterylosis and on some points in the anatomy of the Guácharo (*Steatornis caripensis*) and showed that this singular bird must be constituted a family *per se*, related in some respects to the Caprimulgidæ and their allied forms, and in other respects to the Owls (*Striges*).

Chemical Society, June 5.—Dr. Odling, F.R.S., president, in the chair.—Six communications were read before the society, the first being "On the dioxides of calcium and strontium," by Sir John Conroy, Bart., in which the author gave the method of preparation and properties of these substances.—Mr. T. Wells then described a new form of ozone generator which gives abundance of ozone and has the advantage of being easily constructed and not liable to be broken.—The other papers, which contained but little of general interest, were entitled "On the behaviour of acetamide with sodium alcohol," by W. N. Hartley; "On iodine monochloride," by J. B. Hannay; "On triferrous phosphide," by Dr. R. Schenk; and "On sulphur bromide," by J. B. Hannay.

Anthropological Institute, June 3.—Prof. Busk, F.R.S., president, in the chair.—The president exhibited and described a new apparatus for measuring, with ease and accuracy, the cubic capacity of skulls. Prof. Rolleston, while approving generally the method of Prof. Busk, differed with him in the nature of the material to be employed; he thought that sand was objectionable as being subject to hygrometric variation from which rape-seed was entirely free.—Prof. Robinson exhibited a remarkable bronze sword found in the bed of the Charwell, Oxfordshire, a bronze spear from Speen, near Newbury, and other implements of bronze and stone.—The president exhibited a series of stone implements from the Island of St. Vincent, West Indies, and Mr. A. W. Franks exhibited a bow and poisoned arrows lately used by the Modoc Indians, and found in Captain Jack's stronghold in the lava beds of Siskiyon County, California—The Rev. Dunbar I. Heath contributed Notes on a Mural Inscription, in large Samaritan characters, from Gaza, and claimed for it a higher antiquity than the date of the Moabite Stone.—Mr. H. Howorth read a paper entitled, "Strictures on Darwinism, part II., the Extinction of Types." The substitution of species involved two factors; 1st, the extinction of certain types; 2nd, the introduction of certain others. The paper dealt with the former factor only. Pre-Darwinian naturalists, and some of those who now oppose Darwin, have agreed that species become extinct through the operation of causes, such as climatic change, &c., acting *ab extra* and operating upon whole classes at once from without. Mr. Darwin has argued, on the other hand, that this extinction has arisen from the mutual struggles of individuals by which a certain strong and vigorous type has been evolved, and a certain weak and decrepit type extinguished; the difference between the two theories being that one relies upon external, the other upon internal causes for the explanation of the extinction of certain types. In the present paper the author examined the problem and attempted to show that the old view was the correct one. The paper passed in review the various elements that have gone to destroy types of life, changes in physical geography, changes in climate, epidemics, &c., and showed how the evidence of all of these supported the old view that extinction of type is the result of external influences, and not, as Mr. Darwin contends, of an internal struggle for existence. Prof Rolleston, Mr. Boyd Dawkins, and the president, combated the criticisms of the author.

Royal Microscopical Society, June 4.—Chas. Brooke, F.R.S., president, in the chair. The secretary read a paper by Mr. F. Kitton, of Norwich, descriptive of a new species of *Navicula*, with remarks on *Aulacodiscus formosus*, *Omphalopelta versicolor*, &c., collected in Peru by Captain Perry, of Liverpool; the paper was illustrated by specimens exhibited in the room.—A paper was also read by Mr. J. Stephenson, on the appearances of the inner and outer layers of *Coscinodiscus* when examined in bisulphide of carbon and in air, in which the author pointed out the different effects obtained by mounting the diatoms in media of different refrangibility, and showed the value of such comparisons in determining the nature of the markings, as well as the general structure. The paper was illustrated by a number of very carefully executed drawings by Mr. Charles Stewart, and by specimens exhibited under the microscope. The meetings of the society were adjourned until October.

BERLIN

German Chemical Society, May 26.—A. W. Hofmann in the chair. Dr. Seligsohn investigating the origin of the oxalates deposited in the human body, has found that oxamide can be transformed into urea by ozone, and thinks therefore that oxamide is an intermediate product of digestion between the higher compounds of carbon and urea.—Dr. Rüdorff has found that saturated solutions of chloride of ammonium and nitrate of potassium are not influenced in their composition by adding either of these salts, while saturated solutions of nitrate of ammonium and chloride of potassium are changed in their composition by adding either one or the other to these solutions. In the same way behave most salts, so that the solution of one couple is influenced, while the other couple remains unchanged. But when K_2SO_4 and $NH_4\ NO_3$ are dissolved to saturation, this solution is influenced in the way described, and solutions of the opposite couple show likewise the alteration mentioned. These changes were proved by analyses and by determination of the changes of temperatures occurring. Self-evident conclusions offer with regard to the old question, if two salts in solution represent two or four different compounds.—C. Bulk spoke on the manufacture of arsenic acid from fuchsine-residues; by sublimation, as used in Elberfeld. The same chemist described a simple apparatus replacing spring-clamps in volumetric analysis. It consists of a piece of glass-rod inserted into an indiarubber tube. By pressing it cautiously drop after drop can be let out of the burette.—J. Grosshaus continued his speculation on the nature of chemical elements.—H. Vogel denied the existence of what Becquerel called "rayons continuateurs." He explains the fact that photographic negatives, exposed for a few seconds to chemical light, and then to the red and yellow part of the spectrum, are acted upon by these rays, by admitting that during the first exposure chloride of silver is reduced only to the state of sub-chloride, which in its turn is acted upon by yellow light, and thus reduced to the metallic state. This explanation appears the more probable, as iodide and bromide of silver do not exhibit the same property, iodine and bromine forming but one compound with silver.—Julius Thomsen reported on the amount of heat yielded by mixing nitric acid and water. The result of his experiments he sums up as follows:—A diluted nitric or sulphuric acid, when further diluted with the same quantity of water it already contains, will yield the smallest amount of heat, when the molecular heat of the acid is equal to that of the water which is contained in it.—Henry Armstrong sent a summary of his researches on isomeric derivatives of phenole, most of which are familiar to the English public.—Heinrich Baumhauer published some remarks on the natural system of chemical elements, and the relations between atomic and specific weights.—F. Birlstein and A. Kullberg have found that α-dinitro-naphthaline treated with a mixture of nitric and sulphuric acids yields a new γ-trinitro-naphthaline fusing at 147°, while fuming nitric acid produces only the ordinary α-trinitro naphthaline.—E. Mulder obtained a yellow solid combination by precipitating cyanamide with nitrate of silver. Its composition, CN_2Ag_2, leads the author to suppose cyanamide to be constituted according to the formula $C(NH)_2$ of carbodiimide.—R. Siemens submitted sulfo-acetic acid to the action of perchloride of phosphorus in order to investigate the chloride thus obtained as well as its reduction. To the former he gives the formula

$$\begin{array}{l} CHCl = SO_2Cl \\ | \\ COCl \end{array}$$

to the compound obtained from it by the action of tin and hydrochloric acid, the formula of thio-glycollic acid:

$$\begin{array}{l} CH_2.SH \\ | \\ CO.OH \end{array} \quad \text{forming the lead-salt} \quad \left.\begin{array}{l} CH_2.S \\ CO.O \end{array}\right\} Pb.$$

The chloride is decomposed by water into the body formerly described by Kolbe under the name of trichlor-methyl-sulforchloride, $CCl_3.SO\,Cl$.

PARIS

Academy of Sciences, June 2.—M. de Quatrefages, president, in the chair.—The president announced the death of M. de Verneuil, *membre libre*, which occurred at Paris, May 29.—M. de Chevreul communicated the principal results of his researches on avic acid, which will shortly be published. The president presented the first part of the work on the crania of the human race upon which he and Dr. Hamy are engaged.—The following papers were read:—Note accompanying the presentation of a work on cellular anatomy and physiology, by M. Ch. Robin.—On the transit of Venus in 1882, by M. Puiseux.—Trial, during an eclipse of the sun, of the new spectroscopic method proposed for the observation of the next transit of Venus by Father Secchi. The method consists in placing a direct-vision system of prisms before the slit of the spectroscope, and then observing the interruption of the chromosphere by the dark body. The author compares observations by his method with those of Prof. Respighi, published in the *Gazetta Officiale*, No. 145. Respighi saw the approach of the moon 21·9 secs. before Secchi, but Secchi saw the last contact 12·3 secs. before Respighi. The Rev. Father therefore suggests the use of the ordinary method (that used by Respighi) for first contact, and of his own for last.—A study of the action of the principal derivatives of amylic alcohol on polarised light, by MM. Pierre and Puchot.—Development of the freshwater algæ of the genus *Batrachospermum*, alternate generation; second note, by M. Sirodot.—On the nature and treatment of ear tumors (*oreillons*), by M. Bouchut.—On *Hylodes martinicencis*, by M. A. Bavay.—Documents relating to the short-period Comet II, 1867, by Mr. Hind, M. Stephan, MM. Paul and Prosper Henry, M. André, and M. Baillaud. Communicated by M. Le Verrier.—Discovery of a new small planet by Mr. J. Henry, at Washington, U.S.A.—Displacement of a body subjected to four conditions, by M. Ribaucour.—On the action of the electric fluid on flames, liquids, and powders, second note, by M. Neyreneuf.—On the detection and estimation of plumbic sulphate in the lead chromates of commerce, by M. Duvillier. The author adds nitric acid and alcohol, the chromate is then reduced, the lead and cromic oxide are dissolved by the nitric acid, and the sulphate, if present, remains insoluble.—On the action of nitric acid on plumbic chromate, by the same author.—On a base isomeric with piperidin, and on the nitrated derivatives of the hydrocarbons of the formula C_nH_{2n}, by M. H. Gal.—On the molecular rotation of gases, by M. Hinrichs.—Experimental researches on the pathogeny of infarctus, &c., by M. V. Feltz.—Observations on a recent note, by M. Rabuteau, relative to the toxic properties of the iodides of tetramethylammonium and tetramylammonium, by Messrs. A. Brown and Th. Fraser.—General results of the analysis of the Geyser springs of the island of San Miguel, Azores, by M. Fouqué. During the meeting an election to the place in the Mechanical section left vacant by the decease of M. Ch. Dupin, was held with the following results:—M. Resal, 31 votes; M. Bresse, 17; M. Boussinesq, 3; M. Haton de la Goupillière and M. Maurice Lévy, 1 each. M. Resal was accordingly declared duly elected.

DIARY

THURSDAY, JUNE 12.

ROYAL SOCIETY, at 4.—Election of Fellows.

SOCIETY OF ANTIQUARIES, at 8.30.—What Parts of Lincoln Cathedral are really of the Time of St. Hugh of Grenoble, A.D. 1192–1200? J. H. Parker, C.B.

MATHEMATICAL SOCIETY, at 8.—Some general Theorems relating to Vibrations: Hon. J. W. Strutt.—Invariant conditions of three and four concurrence of three Conics: J. J. Walker.—Locus of the point of concourse of tangents to an epicycloid inclined to each other at a constant angle: Prof. Wolstenholme.

FRIDAY, JUNE 13.

ASTRONOMICAL SOCIETY, at 8.

QUEKETT CLUB, at 8.

HORTICULTURAL SOCIETY, at 3.—Lecture.

SOCIETY OF ARTS, at 12.—Purchase of Railways by the State: Wm. Galt.

SATURDAY, JUNE 14.

ROYAL BOTANIC SOCIETY, at 3.45.

MONDAY, JUNE 16.

ASIATIC SOCIETY, at 3.

TUESDAY, JUNE 17.

ANTHROPOLOGICAL INSTITUTE, at 8.—The Ainos: Lieut. S. C. Holland, R.N.—Account of an Interview with a Tribe of Bushmans in South Africa: G. W. Stow.—Specimens of Native Australian Languages: Andrew Mackenzie.

ZOOLOGICAL SOCIETY, at 8.30.—On the Osteology of the Maltese fossil Elephants: Dr. A. Leith Adams, F.R.S.—On the Geographical Distribution of Asiatic Birds: H. J. Elwes.

STATISTICAL SOCIETY, at 7.45.

WEDNESDAY, JUNE 18.

METEOROLOGICAL SOCIETY, at 7.—On some Results of Temperature Observations at Durham: John J. Plummer.—On the Meteorology of New Zealand, 1872: C. R. Marten.—On the Climate of Vancouver Island: Robert H. Scott, F.R.S.—Meteorological Observations at Zi-Ka-Wei, near Shanghai: Rev. A. M. Colombel and Rev. S. J. Perry.—Notes on the Connection between Colliery Explosions and Weather: R. H. Scott, F.R.S., and Wm. Galloway.—Annual General Meeting.

HORTICULTURAL SOCIETY.—Exhibition.

THURSDAY, JUNE 19.

ROYAL SOCIETY, at 8.30.

SOCIETY OF ANTIQUARIES, at 8.30.

LINNEAN SOCIETY, at 8.

CHEMICAL SOCIETY, at 8.—On the Influence of Pressure upon Fermentation. Part II.: Horace Brown.—Researches on the Action of the Copper-Zinc Couple on Organic Bodies, III., and on Normal and Iso-Propyl Iodides: Dr. J. H. Gladstone and A. Tribe.—On Cymenes from different sources optically considered: Dr. J. H. Gladstone.—On the Action of Bromine on Alizarine: W. H. Perkin.—On some Decompositions and Oxidation Products of Morphine and Codeine Derivatives: G. L. Mayer and Dr. C. R. A. Wright.—On the Decomposition of Tricalcic Phosphate by Water: R. Warrington.—On a new Tellurium Mineral, with Notes on a Systematic Mineralogical Nomenclature: J. B. Hannay.—Communications from the Laboratory of the London Institution, No. XII:—On New Derivatives of Cresol: Dr. H. E. Armstrong and C. L. Field.

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