THURSDAY, OCTOBER 19, 1876

MAUDSLEY'S "PHYSIOLOGY OF MIND"

The Physiology of Mind. Being the First Part of a Third Edition, Revised, Enlarged, and in great part Re-written, of "The Physiology and Pathology of Mind." By Henry Maudsley, M.D. (London: Macmillan and Co., 1876.)

MAN very long ago, probably about the time he became man, reflected that he felt and thought; since then no one has ever had the least doubt, as to whether a given object of thought was a fact of mind or of body; and every attempt to resolve the one into the other has been but the vain enterprise of a misguided intelligence. The physical and mental stand over against each other—the fundamental duality of being which no effort of thought has been able to transcend. How, after reflecting that they felt, our far-off ancestors came to refer their feelings and reflections to a soul or spiritual entity, which they supposed to inhabit and animate the body, to cause and direct its movements, can never be more than a subject of speculation. But that such was the universal belief of mankind, that such is still the creed of all save a few, and that all language has been evolved under this conception, scarcely requires to be stated. For ages the curious speculated around the fascinating mystery of the union of soul and body—and yet the mystery remained. A slow change of view, however, was taking place. From the belief that the life and movement, the health and disease of the body, were in some way directly dependent on a conscious, thinking soul, we have passed gradually, very gradually, to the view held by that body of thinkers who claim to be the scientific psychologists of the present day, which is, that mind, feeling, and thought, in a word, consciousness, is dependent on bodily organisation. Dr. Maudsley presents the volume before us as a treatise or "disquisition, by the light of existing knowledge, concerning the nervous structures and functions which are the probable physical foundations, or the objective aspects of, those natural phenomena which appear in consciousness as feelings and thoughts, and are known only in that way."

Ten years ago, when Dr. Maudsley published the first edition of his work, "The Physiology and Pathology of Mind," of which the volume before us is the first part much enlarged, there was a need, which no longer exists, for iterating and reiterating the evidence of the invariable and uniform connection of mental phenomena with nervous organisation. This useful work Dr. Maudsley did well, and as he himself says, "with all the vehemence of youthful enthusiasm." The dependence of consciousness on nervous organisation may be claimed as fairly established, and the great question that now presses on the attention of the scientific psychologist is, What mode of connection can we figure to ourselves as the relation subsisting between consciousness and the material organism? We are among those who maintain that to this problem no acceptable solution has yet been proposed. Dr. Maudsley thinks otherwise, and much of the present volume is taken up with attempts to unveil this deepest of nature's mysteries. We have come to believe, taking it as established, that for a given fact of nervous action the corresponding fact of consciousness will ever be the same. The question now is, in what relation does this fact of consciousness stand to the constantly related, but totally unlike, phenomena which we describe as nervous action? The phenomena, it is answered, are not so totally unlike. "Above all things it is now necessary," says Dr. Maudsley, "that the absolute and unholy barrier set up between psychical and physical nature be broken down, and that a just conception (of mind) be formed, founded on a faithful recognition of all those phenomena of nature which lead, by imperceptible gradations, up to this its highest evolution." (We continue the quotation simply as illustrating Dr. Maudsley's style, which we venture to think is still at times rather above the sober unimpassioned language of science.) "Happily, the beneficial change is being gradually effected, and ignorant prejudice or offended self-love in vain opposes a progress in knowledge which reflects the course of progress in nature; the stars in their courses fight for such truth, and its angry adversary might as well hope to blow out with his pernicious breath the all-inspiring light of the sun as to extinguish its ever-waxing splendour." Well, the unholy barrier between psychical and physical nature has to be broken down, and to this end we are called on to form a conception of mind which will enable us to conceive it as a product of physical evolution. Complete failure has, we think, been the reward of every one who has ventured on this most hopeless undertaking (see our article "Cosmic Philosophy," NATURE, August 5, 1875). Let us see, however, how Dr. Maudsley would have us proceed. In the first place, we are invited to perceive distinctly "that consciousness is not co-extensive with mind, that it is not mind but an incidental accompaniment of mind." This is certainly a large demand to begin with, and we would rather be with those who would maintain "that it is improper and indeed absurd to speak of mind except when speaking of states of consciousness." The word "mind" has been used by all mankind to denote states of consciousness, and not a material organism, nor the changes in an organism. The philosophers also are against Dr. Maudsley, but of these he makes little account. We may notice in passing, however, that, in preparing for criticism Prof. Bain's statement that mind is "the sum total of subject experiences, that which has not extension," he finds it convenient to "alter the wording of it" so as to make it "run thus—mind is the sum total of the experiences of that which has not extension, *that which has not extension being a subject!*" We can only suppose that Dr. Maudsley has wholly misunderstood Prof. Bain's words. Prof. Bain, as we understand him, means to give a double description of mind—it is the sum total of subject experiences, or again, it *is* that which has not extension. The difference between this and the statement—mind is the sum total of the experiences of that which has not extension—is obvious.

But to proceed. By insisting that "emotions good or bad are physical phenomena," "that ideas are insensible motions of nerve-molecules, of the nature of vibrations," Dr. Maudsley can without much further violence to language bring the relation of mind to the physical organism under the familiar conception of organ and function.

Cabanis spoke of the brain secreting thought as the liver secretes bile. Dr. Maudsley recognises, however, that this is a "fallacious comparison." "Here," he says, "as elsewhere, confusion is bred by the common use of the word secretion to express, not only the functional process but the secreted product, both the insensible vital changes and the tangible results of them." It seems, then, that by the word "mind" Dr. Maudsley really means to denote certain vital changes of the brain and nerves; when he speaks of an emotion or an idea, he means to refer to certain peculiar agitations of the white and gray matter of the nervous system. There is no difficulty now in seizing Dr. Maudsley's meaning when he speaks of "the performance of an idea," nor in conceiving "mind," that is, the nervous operations, as the functions of the nervous system. And we have no other criticism to make than is implied in the remark that Dr. Maudsley by using language in this way would not, we fear, commend himself to the old woman who was all her life so thankful that Adam had had the good sense to call all the animals by their right names.

Accepting Dr. Maudsley's nomenclature we have not the slightest difficulty in conceiving "that all the operations which are considered mental and to belong to psychology, may be performed as pure functions of the nervous system, without consciousness giving evidence of them, or having any part in them;" on the contrary we are, as will presently appear, disposed to be more thorough than Dr. Maudsley himself in regarding the physical machine as sufficient for all its own operations. But before proceeding to this the second branch of our criticism, let us recall the original, in fact, the only difficulty. Are we, now that we can talk of the "physiological mechanism" working out the "cognition of a logical necessity without the aid of consciousness"—are we in a better position to figure to ourselves the mode of connection between consciousness, this mere "satellite of mind," and those physiological operations? The darkness remains thick and impenetrable as ever. Here are some of the empty, worse than empty, phrases, with which Dr. Maudsley would have us conceal the limits of our intelligence, and our blank ignorance of what lies beyond these limits. "Consciousness," he says, "is a quality or attribute of the concrete mental act." It is, however, we may be permitted to think, a most singular quality of physiological operations—we must not forget that these are our mental facts—for it has, like Lucifer of old, rebelled against the supreme power, having, as Dr. Maudsley complains, "miraculously got rid of its substance, and then with a wonderful assurance assumed the office of commenting and passing judgment from a higher region of being, upon the nature of that whereof it is actually a function." Consciousness, then, is a function, an attribute, a satellite, a quality, the usual but "not the indispensable accompaniment of mental function." To ask *why* "cerebral organisation functions as conscious energy?" is, says Dr. Maudsley, "an unwarrantable demand." It would probably be so; what is asked of men of science is not *why?* but *how?* How are we to manipulate our conceptions of matter and motion, so as to get out of them our conception of consciousness? The thing is not to be done.

We come now to the second branch of our criticism. Strange to say, after preaching the gospel of Matter and "the wonderful works which it is continually doing before our eyes," Dr. Maudsley finds himself as helpless as the most bewildered of the metaphysicians, whom he holds in such supreme contempt, to work the animal machine without the assistance of something that is not physical. In arguing against the presence of consciousness in the performances of the decapitated frog, Dr. Maudsley contends that consciousness is not required for the performance of these movements, that they "may be explained satisfactorily without the assumption that its spinal chord possesses feeling and will," "that the frog acts necessarily and blindly." In thus contending for the "mechanical," "the entirely physical nature of the movements," Dr. Maudsley would distinguish them from another class of actions which he recognises as not entirely physical in their nature, as somehow "dependent on consciousness," not blind and necessary, but "instigated by will and guided by intelligence." Surely it is to little purpose that Dr. Maudsley has made thought and emotion physical phenomena, if he must after all call in the aid of consciousness of something not physical, to work the mechanism of a frog. This is not, as might be supposed, a mere slip in a subordinate argument. The thraldom of spiritualism, from which Dr. Maudsley has not escaped, betrays itself in all parts of his book, even when he is deliberately straining after his favourite conception of a thinking machine. "It may seem," he says, "an extravagant thing to say, but to me it seems conceivable that a man might be as good a reasoning machine without as he is with consciousness, if we assumed his nervous system to be equally susceptible to the influences which now affect him consciously, and if we had the means, by microscope or galvanoscope or some other more delicate instrument hereafter to be invented, of reading off the results of his cerebral operations from without." Why does Dr. Maudsley need to call in the aid of microscope, or galvanoscope, or some other more delicate instrument? Why should his unconscious man, equally susceptible to the influences which now affect him consciously, not be able when asked to tell us the results of his deliberations, or to write them down for us? Is it that after the sound waves of our question have agitated the tympanum and set the appropriate nervous mechanism in motion, these physical phenomena have to be translated into, or taken note of, by consciousness—by something that is not mechanism, or that is at least *more* than the working of mechanism—in order that this nervous stimulation from without should give rise to the movements implied in speaking or writing? Dr. Maudsley's unconscious reasoner who cannot tell us the results of his reasoning is a defective construction. In spite of himself Dr. Maudsley gives to consciousness, to "the witness," "the sense by which the (reasoning) operations are observed within," exactly the mysterious place and inconceivable functions which less advanced people attribute to the thinking soul, which they believe to inhabit the body to cause and direct its movements.

Our space requires that we should bring our criticism to an end; nor is there much occasion to carry it further.

We shall therefore conclude with a remark on Dr. Maudsley's attempt to make something of the will—that apparent point of contact of the physical and the mental, which has been the veritable will-o'-the-wisp of our psychologists. Towards the end of his book (p. 442) Dr. Maudsley, in examining a "simplest case of volition," tells us that it "sprang from that fundamental property of organic element by which what is agreeable is sought, what is painful is shunned." This is not advancing knowledge, but rather the reverse. How can any substance, whether we call it organic element or by any other name, seek the agreeable and shun the painful? By movements; we know of no other way of seeking and shunning. But how are these mental things, pain and pleasure, related to movements of any kind? Here we find ourselves, after much laborious groping, face to face with the very problem we set out to solve. Truly what we have to learn in psychology, before and above all things, is our ignorance.

DOUGLAS A. SPALDING

OUR BOOK SHELF

Annual Record of Science and Industry for 1875. Edited by Spencer F. Baird, with the assistance of eminent men of science. (London: Trübner and Co., 1876.)

FOR this admirable record we have again to thank our American friends. The volume now extends to some 900 pages, and each year brings improvement as well as enlargement of its contents. Unfortunately so many subjects are now embraced within the scope of this annual record, that the space devoted to each subject is necessarily curtailed. This, we think, is a misfortune. If feasible, we would venture to suggest a division of the record into two parts, inasmuch as pure, applied and very homely science are here in close and curious juxtaposition. Thus we find "tables of elliptic integrals," the "computation of the areas of irregular figures," or the "dissipation of energy," followed by "beautiful ornament for rooms," "renewing wrinkled silk," and "improved modes of closing barrel hoops." It is true the editor has most carefully and laboriously classified the whole, so that we are gradually let down from elliptic integrals at the beginning to barrel hoops at the end. Doubtless the editor considered that by these things men would learn "beer and skittles" was an integral part of the scientific as well as the popular need. The first half of the work, which gives a general summary of scientific and industrial progress for the past year, is more carefully edited than the brief notices of papers which form the second half. For example, turning to general physics, we find paragraphs on Mr. Crookes' experiments scattered about in several places; the same is true of Dr. Guthrie's researches on cryohydrates and of several others we might name. Again, in the index, which is extremely minute, the same name is put under different headings; thus, Mr., Mr. Frederick, and Prof. Guthrie are the same person, though separately referred to. But in spite of these criticisms, the book is a useful one and contains a vast mass of information. Sketchy as it is, nevertheless it is undoubtedly the best annual record of science—in fact the only one in the English language; and hence we are glad to observe that a suggestion we made in noticing the preceding volume, namely, having a London as well as a New York publisher, has now been carried out. The scientific bibliography of the year and the references to periodicals, giving the fullest reviews of the books themselves, is an excellent and valuable feature of this Record.

Causeries Scientifiques. J. Rothschild, Editeur. (Paris.)

THIS little book gives a brief and popular account of some of the principal discoveries and inventions of the past year. It is written in a lively, simple style, and doubtless has done something in France to extend an interest in science and to spread a knowledge, though but a superficial one, of the more striking results of experimental research. The absence of technical terms brings the volume before us within the comprehension of those who have had no scientific education. How is it we are so behind our neighbours in books of this kind? It would, however, have been well if the editor of this volume had paid a little more attention to the spelling of English names, and exercised closer supervision throughout, as we notice several misprints in its pages.

LETTERS TO THE EDITOR

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

The Self-fertilisation of Plants

SOME years ago my suspicions were strongly aroused by certain observations, against the importance of intercrossing; and since then other conclusions, such as the following, have been steadily forcing themselves upon me, and which will probably agree with Mr. T. Meehan's: (1) that self-fertilisation was the primordial condition of plants; (2) that conspicuous flowers of all kinds are a secondary result, due to insect agency, by increasing the size &c., of the perianth; (3) that this has, in its turn, caused a correlative disturbance in the sexual arrangements, viz., that it has caused to be sacrificed the (originally) normal state of self-fertilisation, and has set up cross-fertilisation instead; (4) that this latter being relatively less certain of being effected, is *compensated* for by a superabundance of pollen, alteration in its form and influence, dimorphism, the separation of sexes, and perhaps other details of sexual differentiation; (5) that the existing self-fertilising flowers are in no case primordial but degraded forms; (6) that in consequence of such degradation, of the perianth especially, the sexual organs have recovered their original energies, and so resume their long lost self-fertilising powers.

The *rationale* of the whole process I take to be "compensation." In the first instance, the enlargement of the corolla is accompanied either by a more or less degree of destruction of one or both of the sexual organs; as in the ray-florets of *Centaurea*, which are neuter; in "double" flowered compositæ, where the new "ligulate" ones pass from the usually hermaphrodite condition of the tubular disk florets, or male, as in *Calendula*, to female only, or even to the neuter state, as in Dahlia; or else by the above-mentioned differentiations of the sexual organs in their forms and functions. The primary cause of such increase in the perianth may, perhaps, be due to the mere mechanical influence of the insects themselves, which by constantly renewed pressure, may determine a flow of nutriment to those parts; this—which I only assume as probable—diverts it temporarily from one or both whorls of essential organs. The result is protandry or protogyny, &c. To compensate again for the loss of self-fertilisation, there come into play, as stated above, all the adaptations to *secure* inter-crossing. Hence the idea of plants abhorring inter-breeding appears to have arisen from observing conspicuous flowers only, and as these are in the majority nowadays, it was a reasonable conclusion: but the self-fertilising ones are extremely numerous altogether; and it is this small but highly significant minority, not to add the special contrivances which occur in order to secure self-fertilisation, which leads one to the opposite conclusion.

Self-fertilisation I believe not to be always an absolute but a purely relative condition; that although many species are now altogether self-fertilising, yet whenever conspicuous flowers become dwarfed, I suspect there is a tendency to their becoming self-fertilising. I have found it to be so in some cases, and should feel extremely glad if any readers of NATURE would kindly observe, at this season of the year especially, whether any dwarfed wild or other flowers they can find are self-fertilising or not, or else be good enough to forward the same to me. For example, I have, this September, found dwarfed blossoms of *Linaria vulgaris* often spurless and without honey, having the stigma situated between the two pairs of anthers, and the pollen-tubes pouring into it both from above and below. Similarly in small flowered specimens of *Potentilla reptans*, less than half an inch in diameter, and even in unexpanded buds, were the pollen-tubes penetrating the stigmas.

I call attention to pollen-tubes, because, unless they be observed, one cannot feel absolutely certain that the flowers are really self-fertilised; and even then, that fact must be associated with the relative positions of anthers and stigmas, and the resulting *abundance of fruit*.

Another point I would mention of importance is the necessity of observing *the order of emergence of the whorls*. The subsequent rates of growth may prove a source of deception, so that it is necessary to go back to the very earliest condition when the parts are little more than papillæ, and if possible even before one or more of the whorls have put in an appearance at all. Now I find that in conspicuous flowers, with certain exceptions, the corolla is very often the *last* to emerge, though ultimately it attains by far the largest size when adult; that the stamens usually come directly after the calyx, which, if present, is *always* first, acting as a protecting and nourishing organ; and that the pistil comes next. Such an order results usually in protandry; but while conspicuous species, as *Stellaria Holostea*, and *Cardamine pratensis*, have the order, calyx, stamens, pistil, corolla, inconspicuous self-fertilising species are often as follows:—*e.g.*, *Cerastium glomeratum*—calyx, pistil, stamens, corolla, and *Nasturtium officinale*, calyx, stamens and pistil (together), corolla. These examples, out of many collected, appear to point to an important connection between the order of emergence and development on the one hand, and cross and self-fertilisation on the other. The connection between these two orders of facts I take to be, as already stated, due to the fact that the energy of conspicuous flowers is diverted into the corolla, which thereby delays the development of the pistil; but when the corolla is arrested, the pistil recovers itself, and its growth is equal to or precedes that of the stamens, the result issuing in a synchronous maturity, and consequently self-pollination. GEORGE HENSLOW

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Wallace's "Geographical Distribution of Animals"

ALLOW me to point out in NATURE a few errors which occur in Mr. Wallace's "Distribution of Animals," regarding the extinct mammalian fauna of India.

In the first place, there is a mistake regarding the locality of the Perim Island (vol. i., p. 362, vol. ii., pp. 157 and 221), from which Tertiary fossils have been obtained; in Mr. Wallace's book the Perim Island, at the entrance to the Red Sea, is the one referred to, whereas the true spot is Perim Island, in the Gulf of Cambay.

There is, therefore at present no known spot to the eastward of India which shows the former extension of its Tertiary mammalia into Africa and Europe, although such extension doubtless existed.

The extinct genus Enhydriodon, from the Siwaliks (*e.g.* vol. ii., p. 200), is always referred to as Enhydrion.

In vol. i. (p. 122) the genus Tapirus is mentioned as occurring in the Miocene of the Punjab; this determination is on the authority of Dr. Falconer, who hastily examined a single tooth (now in the Indian Museum); this tooth, and others subsequently found, turns out to belong to the European Miocene genus Listriodon; the only other mentioned occurrence of a fossil Tapir in India, is by Mr. Clift, who figured the symphysis of a mandible (*Geol. Trans.*, sec. ser., vol. ii.) from Burma; this may, however, also belong to Listriodon.

In vol. ii. (p. 202) the genus Ursus is mentioned as having been described from the Siwaliks and the Nerbudda Valley; it has only been described from the latter locality, Hyænactos being the Siwalik genus. A new species of tame Ursus has, however, been obtained this year from the Siwaliks, and will be subsequently described.

In vol. ii. (p. 212) Hipparion should also be mentioned as having been found in India as well as in Europe.

At p. 228 of the same volume, it is stated that Elephas has "perhaps one species Pliocene in Central India;" in reality there are two species undoubtedly from the Newer Pliocene of the Nerbudda Valley, viz., *E. nomadicus* and *E.* (Stegodon) *insignis*.

Vol. ii., p. 240, the genus Hystrix has been fossil in the Siwaliks of India as well as in Europe and America.

I may add that, as announced in the August number of the "Records of the Geological Survey of India," for the present year, I have determined the existence of a species of Manis (the first fossil species of the genus) and of a Cetacean, with other new forms, from the Siwaliks. RICHARD LYDEKKER,

Calcutta, August 27 Geological Survey of India

The Resistance of the Electric Arc

FOR the purpose of determining theoretically the best arrangement of cells for the production of the electric light, it was necessary to know the resistance of the electric arc. Not being acquainted with any source from which this information could be derived, we determined this resistance experimentally in two distinct ways.

1. The current from sixty new Grove's cells joined in series (and of which the immersed part of each platinum plate was about 13 square inches in area and of each zinc plate about 25 square inches) was used to produce an electric light with a Duboscq's lamp, when a small known resistance consisting of many metres of thick bare copper wire hanging in the air was also introduced in circuit. This wire was sufficiently thick for its resistance not to be sensibly altered by the passage of the current. The difference of potentials between the carbons was measured with a Thomson's quadrant electrometer, using the induction plate and compared with the difference of potentials between the two ends of the wire of known resistance. These two measurements were made rapidly one after the other and repeated very many times. Then since at any moment the same current is flowing through the electric arc and the wire, the two differences of potentials measured rapidly one after the other are proportional to the resistances.

The above method showed that the resistance of the electric arc varied considerably even when the light appeared quite steady, that the resistance was never more than 20 ohms, and had an average value of about 12 ohms.

2. On another occasion the current from eighty similar Grove's cells joined in series, which had been joined up for three hours, and used at intervals during this time for the production of the light, was sent through the coils of a differential galvanometer. In one circuit was a very high resistance and in the other the electric arc; each coil of the differential galvanometer was shunted with a wire of small resistance. Nearly the whole current, therefore, went through the arc. The shunts being properly adjusted to obtain balance, the resistance of the arc, as in the previous case, was found to vary much but never to exceed 29 ohms and to equal about 20 ohms when the light was best.

That the resistance would be larger than in the previous case was to be expected since the battery contained more cells, and a brighter light would, therefore, be obtained with the carbon points further apart.

At a convenient opportunity we hope to take time readings of the resistance together with photographs of the light on a revolving band of sensitive paper in order to determine the exact resistance corresponding with the brightest light for any particular number of cells.

The results, however, given above show that with cells such as we used, and which are the common Grove's cells employed in England, no attempt should be made to join any of the cells in parallel circuit until at least 200 have been joined in series, for since the resistance of each cell is about 0·2 ohms, 200 of them would have a resistance of 40 ohms, a resistance certainly less than *double* the electric arc for that battery corresponding with brightest light, and we have shown (*Telegraphic Journal*, March 15, 1873) that the cells of a battery should be joined in series until the battery of resistance is *double* the external resistance, at which point the battery should be joined in two rows each containing half the whole number of cells in series, and the two rows connected in parallel circuit. W. E. AYRTON

The Imperial College of Engineering, JOHN PERRY

Tokio, Japan

Habits of Animals Transmitted to Offspring

BREEDING many horses yearly on my station, I notice, as a matter of course, some of their peculiar habits. In a semi-wild state on a run horses graze together in large or small companies, which "station hands" call "mobs;" these mobs wander at will over a large area of country, finding abundance of good natural pasture and water. Some years since a mare became solitary in her habits, always seeking one particular creek; whenever released from work she made off to her favourite feeding ground by herself; if "rounded up with a mob" she would take the earliest chance that presented itself of reaching her usual haunt. One of her progeny some years after showed a similar liking for solitude; he was placed among several other horses (many of them he had known for years) on a small run intersected with bushy gullies, more or less rocky. He was soon missing, and search was made for him for some time without success; he was supposed to have come to grief in the bush; at length he was found, most unexpectedly, on a small patch of pasture between two rocky gullies thickly bushed; this spot was so difficult of access that a slight track had to be cut to get the horse back. Having been brought from a large station where he was bred and reared, he no longer enjoyed a great range by which he could place any long distance between his companions and himself; he displayed much tact and judgment in the way he secured the indulgence of hereditary habit, by discovering and reaching with difficulty an almost inaccessible solitude. One of the best and fleetest stock mares for the fast and hard work of "cutting out"[1] was a beautiful creature notorious as an incorrigible kicker; she has most faithfully transmitted this vice to her offspring.

Peculiarity in the formation of the hoof has been handed down to descent after descent by a grand old mare who had this blemish as a slight counterpoise perhaps to her many virtues.

A particular strain of Dorking fowls, which I have had for thirty years or so, always shows a restless desire for rambling, and that too under the difficulty of meeting with much persecution when straying beyond their ample range. This special family always exhibits what may be termed the gift of locality.

Ohinitahi, N.Z. THOMAS H. POTTS

Moon-Stroke

THERE is a popular belief that it is dangerous to sleep in full moonshine, as it is supposed to produce some injurious effect called moon-stroke. I have little doubt that the popular belief is well founded as far as the injury to some of those who have slept out at night is concerned, especially in full moonshine; nevertheless the injury is not, I think, due to the moon, but to another cause, which I shall here attempt to explain. It has often been observed that when the moon is full, or near its full time, there are rarely any clouds about, and if there be clouds before the full moon rises they are soon dissipated, and therefore a perfectly clear sky, with a bright full moon, is frequently observed.

A clear sky admits of rapid radiation of heat from the surface of the earth, and any person exposed to such radiation is sure to be chilled by rapid loss of heat. There is reason to believe that, under the circumstances, paralysis of one side of the face is sometimes likely to occur from chill, as one side of the face is more likely to be exposed to rapid radiation, and consequent loss of its heat. This chill is more likely to occur when the sky is perfectly clear.

I have often slept in the open in India on a clear summer night, when there was no moon, and although the first part of the night may have been hot, yet, towards 2 or 3 o'clock in the morning, the chill has been so great that I have often been awakened by an ache in my forehead, which I as often have counteracted by wrapping a handkerchief round my head and drawing the blanket over my face. As the chill is likely to be greatest on a very clear night, and the clearest nights are likely to be those on which there is a bright moonshine, it is very possible that neuralgia, paralysis, or other similar injury, caused by sleeping in the open, has been attributed to the moon, when the proximate cause may really have been the *chill*, and the moon only a remote cause acting by dissipating the clouds and haze (if it do so), and leaving a perfectly clear sky for the play of radiation into space. E. BONAVIA

Lucknow, August 26

The Memoirs of the Geological Survey

I DESIRE through the medium of your columns to call attention to the fact that most of the admirable memoirs of the Geological Survey appear to be out of print. A week or two ago I ordered a number of these publications and was informed that at least half of them are out of print. Prof. Ramsay's "Geology of North Wales" is in this category and the fact is stated in the printed list, but in a letter recently received from the professor he informs me that the work is being reprinted, and is expected to be published about the middle of next year. Without, in the absence of information, desiring to attach blame to any one, I shall be glad to know the reason why works admittedly of the highest value should have been permitted to fall into such apparent neglect. WM. HORSFALL

Manchester, October 9

[1] "Cutting out" is drafting a beast out of a mob, following it through all its wild rushes, twistings, and turnings, through perhaps many hundreds of cattle, never leaving it till it is fairly drafted out. This work often taxes the skill and energy of stockman and his horse pretty severely.

OUR ASTRONOMICAL COLUMN

CHACORNAC'S VARIABLE NEBULA NEAR ζ TAURI.—On October 19, 1855, Chacornac remarked that a star of the eleventh magnitude, north-preceding ζ Tauri, was enveloped in nebulosity, which was sufficiently bright up to the end of January following to occasion surprise that it had not been previously detected. The star had been repeatedly observed in 1854.

Chacornac gives the position of the star upon which the nebula was projected for 1852·0 in R.A. 5h. 28m., 35·6 N.P.D. 68° 52′ 42″. The form of the nebula was nearly rectangular, the longest side subtending an arc of 3½′ and the shorter, one of 2½′. The star occurs in the zone observed at Markree, January 16, 1850, without mention of surrounding nebulosity.

On September 12, 1863, and January 25, 1865, D'Arrest observed the star with the Copenhagen refractor, on the last occasion "cœlo valde eximio," without being able to detect any trace of the nebula. He estimated the star 11·12 m., and noticed another 13 m., about 40″ preceding nearly on the parallel.

From Chacornac's position for 1852, it appears the star precedes ζ Tauri 12·5s., and is N. 4′ 28″. It may be recommended for examination during the approaching winter, particularly with telescopes of moderate dimensions, which in the case of another suspected variable nebula (Schönfeld, 1858) have been shown to possess decided advantage over the larger instruments.

OLBER'S SUPPOSED VARIABLE IN VIRGO.—Mr. Tebbutt of Windsor, N.S.W., communicates the results of some observations of this object and neighbouring stars, made in July and August of the present year. For 1876·0 he found:—

Star.	Magnitude.	R.A. h.	m.	s.	N.P.D. °	′	″
1	7	13	3	7·7	105	51	12
2	9½	13	5	17·6	105	51	15
3	8½	13	7	32·4	105	53	50
4	9	13	9	12·9	105	37	1

No. 3 is the supposed variable. See this column, 1876, April 13.

RELATIVE BRIGHTNESS OF URANUS AND JUPITER'S SATELLITES.—On the evening of June 5, 1872, M. Prosper Henry, at the Observatory of Paris, took advantage of the very close approach of Uranus to Jupiter (difference of declination only 1″·2 at conjunction) to compare the light of the satellites of Jupiter with the former planet. He found the brightness of Uranus was equal to that of the third satellite, which was nearest to Uranus at the moment. If there existed any difference of light between the two others, it was to the advantage of Uranus, but in any case it was very small. The observations were made with the large Foucault telescope. So favourable an opportunity of making these comparisons may not occur again for a very long period.

BLANPAIN'S COMET, 1819.—A new reduction of the observations of this remarkable comet, taken at Paris, of which we have the particulars in detail, and recalculation of the elements thereupon, appears to lead to a somewhat longer period than was inferred by Encke, from the same observations as at first reduced. This somewhat longer period—a little over five years—would occasion a near approach of the comet to the planet Jupiter at the previous aphelion passage, and it is easy to see that the observations would allow of so close a proximity at this point of the orbit that a very material change of elements may have been then occasioned, perhaps sufficiently great to account for the difference of the elements from those of the first comet of 1743, which Clausen conjectured to be identical with Blanpain's.

PROF. HUXLEY ON UNIVERSITY EDUCATION[1]

THE actual work of the University founded in this city by the well-considered munificence of Johns Hopkins commences to-morrow, and among the many marks of confidence and good-will which have been bestowed upon me in the United States, there is none which I value more highly than that conferred by the authorities of the University when they invited me to deliver an address on such an occasion.

For the event which has brought us together is, in many respects, unique. A vast property is handed over to an administrative body, hampered by no conditions save these :—That the principal shall not be employed in building ; that the funds shall be appropriated in equal proportions to the promotion of natural knowledge, and to the alleviation of the bodily sufferings of mankind ; and, finally, that neither political nor ecclesiastical sectarianism shall be permitted to disturb the impartial distribution of the testator's benefactions.

In my experience of life a truth which sounds very much like a paradox has often asserted itself, viz., that a man's worst difficulties begin when he is able to do as he likes. So long as a man is struggling with obstacles he has an excuse for failure or shortcoming ; but when fortune removes them all and gives him the power of doing as he thinks best, then comes the time of trial. There is but one right, and the possibilities of wrong are infinite. I doubt not that the trustees of the Johns Hopkins University felt the full force of this truth when they entered on the administration of their trust a year and a half ago ; and I can but admire the activity and resolution which have enabled them, aided by the able president whom they have selected, to lay down the great outlines of their plan, and carry it thus far into execution. It is impossible to study that plan without perceiving that great care, forethought, and sagacity, have been bestowed upon it, and that it demands the most respectful consideration. I have been endeavouring to ascertain how far the principles which underlie it are in accordance with those which have been established in my own mind by much and long-continued thought upon educational questions. Permit me to place before you the result of my reflections.

Under one aspect, a university is a particular kind of educational institution, and the views which we may take of the proper nature of a university are corollaries from those which we hold respecting education in general. I think it must be admitted that the school should prepare for the university, and that the university should crown the edifice, the foundations of which are laid in the school. University education should not be something distinct from elementary education, but should be the natural outgrowth and development of the latter. Now I have a very clear conviction as to what elementary education ought to be ; what it really may be when properly organised, and what I think it will be before many years have passed over our heads in England and in America. Such education should enable an average boy of fifteen or sixteen to read and write his own language with ease and accuracy, and with a sense of literary excellence derived from the study of our classic writers ; to have a general acquaintance with the history of his own country and with the great laws of social existence ; to have acquired the rudiments of the physical and psychological sciences, and a fair knowledge of elementary arithmetic and geometry. He should have obtained an acquaintance with logic rather by example than by precept, while the acquirement of the elements of music and drawing should have been pleasure rather than work.

It may sound strange to many ears if I venture to maintain the proposition that a young person, educated thus far, has had a liberal, though perhaps not a full education. But it seems to me that such training as that to which I have referred may be termed liberal in both the senses in which that word is employed with perfect accuracy. In the first place, it is liberal in breadth. It extends over the whole ground of things to be known and of faculties to be trained, and it gives equal importance to the two great sides of human activity—art and science. In the second place, it is liberal in the sense of being an education fitted for free men ; for men to whom every career is open, and from whom their country may demand that they should be fitted to perform the duties of any career. I cannot too strongly impress upon you the fact that with such a primary education as this, and with no more than is to be obtained by building strictly upon its lines, a man of ability may become a great writer or speaker, a statesman, a lawyer, a man of science, painter, sculptor, architect, or musician. That even development of all a man's faculties, which is what properly constitutes culture, may be effected by such an education, while it opens the way for the indefinite strengthening of any special capabilities with which he may be gifted.

In a country like this, where most men have to carve out their own fortunes and devote themselves early to the practical affairs of life, comparatively few can hope to pursue their studies up to or beyond the age of manhood. But it is of vital importance to the welfare of the community that those who are relieved from the need of making a livelihood, and still more, those who are stirred by the divine impulses of intellectual thirst or artistic genius, should be enabled to devote themselves to the higher service of their kind as centres of intelligence, interpreters of nature, or creators of new forms of beauty. And it is the function of a university to furnish such men with the means of becoming that which it is their privilege and duty to be. To this end the university need cover no ground foreign to that occupied by the elementary school. Indeed, it cannot ; for the elementary instruction which I have referred to embraces all the kinds of real knowledge and mental activity possible to man. The university can add no new departments of knowledge, can offer no new fields of mental activity ; but what it can do is to intensify and specialise the instruction in each department. Thus literature and philology, represented in the elementary school by English alone, in the university will extend over the ancient and modern languages. History, which like charity best begins at home, but, like charity, should not end there, will ramify into archæology, political history and geography, with the history of the growth of the human mind and its products in the shape of philosophy, science, and art. And the university will present to the student libraries, museums of antiquities, collections of coins, and the like which will efficiently subserve these studies. Instruction in the elements of social economy, a most essential, but hitherto sadly-neglected part of elementary education, will develop in the university into political economy, sociology, and law. Physical science will have its great divisions of physical geography, with geology and astronomy ; physics, chemistry and biology, represented not merely by professors and their lectures, but by laboratories, in which the students, under guidance of demonstrators, will work out facts for themselves and come into that direct contact with reality which constitutes the fundamental distinction of scientific education. Mathematics will soar into its

[1] Address (revised by the Author) delivered at the formal opening of the Johns Hopkins University at Baltimore, U.S., September 12. The total amount bequeathed by Johns Hopkins is more than 7,000,000 dollars. The sum of 3,500,000 dollars is appropriated to a university, a like sum to a hospital, and the rest to local institutions of education and charity.

highest regions; while the high peaks of philosophy may be scaled by those whose aptitude for abstract thought has been awakened by elementary logic. Finally, schools of pictorial and plastic art, of architecture, and of music should offer a thorough discipline in the principles and practice of art to those in whom lies nascent the rare faculty of æsthetic representation, or the still rarer powers of creative genius.

The primary school and the university are the alpha and omega of education. Whether institutions intermediate between these (so-called secondary schools) should exist, appears to me to be a question of practical convenience. If such schools exist, the important thing is that they should be true intermediaries between the primary school and the university, keeping on the wide track of general culture, and not sacrificing one branch of knowledge for another.

Such appear to me to be the broad outlines of the relations which the university, regarded as a place of education, ought to bear to the school, but a number of points of detail require some consideration, however briefly and imperfectly I can deal with them. In the first place there is the important question of the limitations which should be fixed to the entrance into the university; what qualifications should be required of those who propose to take advantage of the higher training offered by the university. On the one hand, it is obviously desirable that the time and opportunities of the university should not be wasted in conferring such elementary instruction as can be obtained elsewhere; while, on the other hand, it is no less desirable that the higher instruction of the university should be made accessible to everyone who can take advantage of it, although he may not have been able to go through any very extended course of education. My own feeling is distinctly against any absolute and defined preliminary examination, the passing of which shall be an essential condition of admission to the university. I would admit any one to the university who could be reasonably expected to profit by the instruction offered to him, and I should be inclined, on the whole, to test the fitness of the student, not by examination before he enters the university, but at the end of his first term of study. If, on examination in the branches of knowledge to which he has devoted himself, he show himself deficient in industry or in capacity, it will be best for the university and best for himself, to prevent him from pursuing a vocation for which he is obviously not fit. And I hardly know of any other method than this by which his fitness or unfitness can be safely ascertained, though no doubt a good deal may be done, not by formal cut and dried examination, but by judicious questioning at the outset of his career.

Another very important and difficult practical question is whether a definite course of study shall be laid down for those who enter the university; whether a curriculum shall be prescribed; or whether the student shall be allowed to range at will among the subjects which are open to him. And this question is inseparably connected with another, namely, the conferring of degrees. It is obviously impossible that any student should pass through the whole of the series of courses of instruction offered by a university. If a degree is to be conferred as a mark of proficiency in knowledge, it must be given on the ground that the candidate is proficient in a certain fraction of those studies; and then will arise the necessity of insuring an equivalency of degrees, so that the course by which a degree is obtained shall mark approximately an equal amount of labour and of acquirements, in all cases. But this equivalency can hardly be secured in any other way than by prescribing a series of definite lines of study. This is a matter which will require grave consideration. The important points to bear in mind, I think, are that there should not be too many subjects in the curriculum, and that the aim should be the attainment of thorough and sound knowledge of each.

One half of the Johns Hopkins bequest is devoted to the establishment of a hospital, and it was the desire of the testator that the university and the hospital should co-operate in the promotion of medical education. The trustees will unquestionably take the best advice that is to be had as to the construction and administration of the hospital. In respect to the former point, they will doubtless remember that a hospital may be so arranged as to kill more than it cures; and, in regard to the latter, that a hospital may spread the spirit of pauperism among the well to do, as well as relieve the sufferings of the destitute. It is not for me to speak on these topics—rather let me confine myself to the one matter on which my experience as a student of medicine, and an examiner of long standing, who has taken a great interest in the subject of medical education, may entitle me to a hearing. I mean the nature of medical education itself, and the co-operation of the university in its promotion.

What is the object of medical education? It is to enable the practitioner, on the one hand, to prevent disease by his knowledge of hygiene; on the other hand, to divine its nature, and to alleviate or cure it, by his know-knowledge of pathology, therapeutics, and practical medicine. That is his business in life, and if he has not a thorough and practical knowledge of the conditions of health, of the causes which tend to the establishment of disease, of the meaning of symptoms, and of the uses of medicines and operative appliances, he is incompetent, even if he were the best anatomist, or physiologist, or chemist that ever took a gold medal or won a prize certificate. This is one great truth respecting medical education. Another is, that all practice in medicine is based upon theory of some sort or other; and therefore, that it is desirable to have such theory in the closest possible accordance with fact. The veriest empiric who gives a drug in one case because he has seen it do good in another of apparently the same sort, acts upon the theory that similarity of superficial symptoms means similarity of lesions; which, by the way, is perhaps as wild an hypothesis as could be invented. To understand the nature of disease we must understand health, and the understanding of the healthy body means the having a knowledge of its structure and of the way in which its manifold actions are performed, which is what is technically termed human anatomy and human physiology. The physiologist again must needs possess an acquaintance with physics and chemistry, inasmuch as physiology is, to a great extent, applied physics and chemistry. For ordinary purposes a limited amount of such knowledge is all that is needful; but for the pursuit of the higher branches of physiology no knowledge of these branches of science can be too extensive, or too profound. What we call therapeutics again, which has to do with the action of drugs and medicines on the living organism is, strictly speaking, a branch of experimental physiology, and is daily receiving a greater and greater experimental development.

The third great fact which is to be taken into consideration in dealing with medical education, is that the practical necessities of life do not, as a rule, allow aspirants to medical practice to give more than three, or it may be four years to their studies. Let us put it at four years, and then reflect that in the course of this time a young man fresh from school has to acquaint himself with medicine, surgery, obstetrics, therapeutics, pathology, hygiene, as well as with the anatomy and the physiology of the human body; and that his knowledge should be of such a character that it can be relied upon in any emergency, and always ready for practical application. Consider, in addition, that the medical practitioner may be called upon, at any moment, to give evidence in a court of justice in a criminal case, and that it is therefore well that he should know something of the laws of evidence, and of what we call medical jurisprudence. On a medical certificate a man may be taken from his home and from his business and confined in a lunatic asylum; surely, therefore, it is desirable that the medical practitioner should have some rational and clear conceptions as to the nature and symptoms of mental disease. Bearing in mind all these requirements of medical education, you will admit that the burden on the young aspirant for the medical profession is somewhat of the heaviest, and that it needs some care to prevent his intellectual back from being broken.

Those who are acquainted with the existing systems of medical education will observe that, long as is the catalogue of studies which I have enumerated, I have omitted to mention several that enter into the usual medical curriculum of the present day. I have said not a word about zoology, comparative anatomy, botany, or materia medica. Assuredly this is from no light estimate of the value or importance of such studies in themselves. It may be taken for granted that I should be the last person in the world to object to the teaching of zoology or comparative anatomy in themselves; but I have the strongest feeling that, considering the number and the gravity of those studies through which a medical man must pass, if he is to be competent to discharge the serious duties which devolve upon him, subjects which lie so remote as these do from his practical pursuits should be rigorously excluded. The young man, who has enough to do in order to acquire such familiarity with the structure of the human body as to enable him to perform the operations of surgery, ought not, in my judgment, to be occupied with investigations into the anatomy of crabs and starfishes. Undoubtedly the doctor should know the common poisonous plants of his own country when he sees them, but that knowledge may be obtained by a few hours devoted to the examination of specimens of such plants, and the desirableness of such knowledge is no justification, to my mind, for spending three months over the study of systematic botany. Again, materia medica, so far as it is a knowledge of drugs, is the business of the druggist. In all other callings the necessity of the division of labour is fully recognised, and it is absurd to require of the medical man that he should not avail himself of the special knowledge of those whose business it is to deal in the drugs which he uses. It is all very well that the physician should know that castor oil comes from a plant, and castoreum from an animal, and how they are to be prepared, but for all practical purposes of his profession that knowledge is not of one whit more value, has no more relevancy, than the knowledge of how the steel of his scalpel is made.

All knowledge is good. It is impossible to say that any fragment of knowledge, however insignificant or remote from one's ordinary pursuits, may not some day be turned to account. But in medical education, above all things, it is to be recollected that in order to know a little well one must be content to be ignorant of a great deal.

Let it not be supposed that I am proposing to narrow medical education, or, as the cry is, to lower the standard of the profession. Depend upon it there is only one way of really ennobling any calling, and that is to make those who pursue it real masters of their craft, men who can truly do that which they profess to be able to do, and which they are credited with being able to do by the public; and there is no position so ignoble as that of the so-called "liberally-educated practitioner," who, as Talleyrand said of his physician, "Knows everything, even a little physic;" who may be able to read Galen in the original, who knows all the plants, from the cedar of Lebanon to the hyssop upon the wall, but who finds himself, with the issues of life and death in his hands, ignorant, blundering, and bewildered, because of his ignorance of the essential and fundamental truths upon which practice must be based. Moreover, I venture to say, that any man who has seriously studied all the essential branches of medical knowledge; who has the needful acquaintance with the elements of physical science, who has been brought by medical jurisprudence into contact with law; whose study of insanity has taken him into the fields of psychology; has *ipso facto* received a liberal education.

Having lightened the medical curriculum by culling out of it everything which is unessential, we may next consider whether something may not be done to aid the medical student toward the acquirement of real knowledge by modifying the system of examination. In England, within my recollection, it was the practice to require of the medical student attendance on lectures upon the most diverse topics during three years; so that it often happened that he would have to listen to four or five lectures in the day upon totally different subjects in addition to the hours given to dissection and to hospital practice: and he was required to keep all the knowledge he could pick up in this distracting fashion at examination point, until at the end of three years he was set down to a table and questioned pell-mell upon all the different matters with which he had been striving to make acquaintance. A worse system and one more calculated to obstruct the acquisition of sound knowledge and to give full play to the "crammer" and the "grinder" could hardly have been devised by human ingenuity. Of late years great reforms have taken place. Examinations have been divided so as to diminish the number of subjects among which the attention has to be divided. Practical examination has been largely introduced, but there still remains, even under the present system, too much of the old evil inseparable from the contemporaneous pursuit of a multiplicity of diverse studies.

Proposals have recently been made to get rid of general examinations altogether, to allow the student to be examined in each subject at the end of his attendance on the class; and then, in case of the result being satisfactory, to allow him to have done with it; and I may say that this method has been pursued for many years in the Royal School of Mines in London, and has been found to work very well. It allows the student to concentrate his mind upon what he is about for the time being, and then to dismiss it. Those who are occupied in intellectual work, will, I think, agree with me that it is important not so much to know a thing as to have known it, and known it thoroughly. If you have once known a thing in this way it is easy to renew your knowledge when you have forgotten it; and when you begin to take the subject up again, it slides back upon the familiar grooves with great facility.

Lastly comes the question as to how the university may co-operate in advancing medical education. A medical school is strictly a technical school—a school in which a practical profession is taught—while a university ought to be a place in which knowledge is obtained without direct reference to professional purposes. It is clear, therefore, that a university and its antecedent, the school, may best co-operate with the medical school by making due provision for the study of those branches of knowledge which lie at the foundation of medicine.

At present, young men come to the medical schools without a conception of even the elements of physical science; they learn, for the first time, that there are such sciences as physics, chemistry, and physiology, and are introduced to anatomy as a new thing. It may be safely said that with a large proportion of medical students much of the first session is wasted in learning how to learn—in familiarising themselves with utterly strange conceptions, and in awakening their dormant and wholly untrained powers of observation and of manipulation. It is difficult to over-estimate the magnitude of the obstacles which are thrown in the way of scientific training by the existing system of school education. Not only are men trained in mere book-work, ignorant of what observation means, but the habit of learning from books alone begets a disgust of observation. The book-learned student will rather trust to what he sees in a book than to the witness of his own eyes.

There is not the slightest reason why this should be so, and, in fact, when elementary education becomes that which I have assumed it ought to be, this state of things will no longer exist. There is not the slightest difficulty in giving sound elementary instruction in physics, in chemistry, and in the elements of human physiology in ordinary schools. In other words, there is no reason why the student should not come to the medical school provided with as much knowledge of these several sciences as he ordinarily picks up in the course of his first year of attendance at the medical school.

I am not saying this without full practical justification for the statement. For the last eighteen years we have had in England a system of elementary science teaching carried out under the auspices of the Science and Art Department, by which elementary scientific instruction is made readily accessible to the scholars of all the elementary schools in the country. Commencing with small beginnings, carefully developed and improved, that system now brings up for examination as many as seven thousand scholars in the subject of human physiology alone; and I can say that out of that number a large proportion have acquired a fair amount of substantial knowledge, and that no inconsiderable percentage show as good an acquaintance with human physiology as used to be exhibited by the average candidates for medical degrees in the University of London when I was first an examiner there twenty years ago, and quite as much knowledge as is possessed by the ordinary student of medicine at the present day. I am justified, therefore, in looking forward to the time when the student who proposes to devote himself to medicine will come, not absolutely raw and inexperienced as he is at present, but in a certain state of preparation for further study; and I look to the university to help him still further forward in that stage of preparation, through the organisation of its biological department. Here the student will find means of acquainting himself with the phenomena of life in their broadest acceptation. He will study not botany and zoology, which, as I have said, would take him too far away from his ultimate goal; but, by duly arranged instruction, combined with work in the laboratory upon the leading types of animal and vegetable life, he will lay a broad and at the same time solid foundation of biological knowledge; he will come to his medical studies with a comprehension of the great truths of morphology and of physiology, with his hands trained to dissect and his eyes taught to see. I have no hesitation in saying that such preparation is worth a full year added on to the medical curriculum. In other words, it will set free that much time for attention to those studies which bear directly upon the student's most grave and serious duties as a medical practitioner.

Up to this point I have considered only the teaching aspect of your great foundation, that function of the university in virtue of which it plays the part of a reservoir of ascertained truth, so far as our symbols can ever interpret nature. All can learn; all can drink of this lake. It is given to few to add to the store of knowledge, to strike new springs of thought, or to shape new forms of beauty. But so sure as it is that men live not by bread, but by ideas, so sure is it that the future of the world lies in the hands of those who are able to carry the interpretation of nature a step further than their predecessors, so certain is it that the highest function of a university is to seek out those men, cherish them, and give their ability to serve their kind full play.

I rejoice to observe that the encouragement of research occupies so prominent a place in your official documents, and in the wise and liberal inaugural address of your president. This subject of the encouragement, or, as it is sometimes called, the endowment of research, has of late years greatly exercised the minds of men in England. It was one of the main topics of discussion by the members of the Royal Commission of whom I was one, and who not long since issued their report, after five years' labour. Many seem to think that this question is mainly one of money; that you can go into the market and buy research, and that supply will follow demand, as in the ordinary course of commerce. This view does not commend itself to my mind. I know of no more difficult practical problem than the discovery of a method of encouraging and supporting the original investigator without opening the door to nepotism and jobbery. My own conviction is admirably summed up in the passage of your president's address, "that the best investigators are usually those who have also the responsibilities of instruction, gaining thus the incitement of colleagues, the encouragement of pupils, and the observation of the public."

At the commencement of this address I ventured to assume that I might, if I thought fit, criticise the arrangements which have been made by the board of trustees, but I confess that I have little to do but to applaud them. Most wise and sagacious seems to me the determination not to build for the present. It has been my fate to see great educational funds fossilise into mere bricks and mortar, in the petrifying springs of architecture, with nothing left to work the institution they were intended to support. A great warrior is said to have made a desert and called it peace. Administrators of educational funds have sometimes made a palace and called it a university. If I may venture to give advice in a matter which lies out of my proper competency, I would say that whenever you do build, get an honest bricklayer, and make him build you just such rooms as you really want, leaving ample space for expansion. And a century hence, when the Baltimore and Ohio shares are at one thousand premium, and you have endowed all the professors you need, and built all the laboratories that are wanted, and have the best museum and the finest library that can be imagined; then if you have a few hundred thousand dollars you don't know what to do with, send for an architect and tell him to put up a façade. If American is similar to English experience, any other course will probably lead you into having some stately structure, good for your architect's fame, but not in the least what you want.

It appears to me that what I have ventured to lay down as the principles which should govern the relations of a university to education in general, is entirely in accordance with the measures you have adopted. You have set no restrictions upon access to the instruction you propose to give; you have provided that such instruction, either as given by the university or by associated institutions, should cover the field of human intellectual activity. You have recognised the importance of encouraging research. You propose to provide means by which young men, who may be full of zeal for a literary or for a scientific career, but who also may have mistaken aspiration for inspiration, may bring their capacities to a test and give their powers a fair trial. If such an one fail, his endowment terminates and there is no harm done. If he succeed, you may give power of flight to the genius of a Davy or a Faraday, a Carlyle or a Locke, whose influence on the future of his fellow men shall be absolutely incalculable.

You have enunciated the principle that the "Glory of the university should rest upon the character of the teachers and scholars, and not upon their numbers or buildings constructed for their use." And I look upon it as an essential and most important feature of your plan that the income of the professors and teachers shall be independent of the number of students whom they can attract. In this way you provide against the danger, patent elsewhere, of finding attempts at improvement obstructed by vested interests; and in the department of medical education especially, you are free of the temptation to set loose upon the world men utterly incompetent to perform the serious and responsible duties of their profession.

It is a delicate matter for a stranger to the practical working of your institutions, like myself, to pretend to give an opinion as to the organisation of your governing power. I can conceive nothing better than that it should remain as it is, if you can secure a succession of wise, liberal, honest, and conscientious men to fill the vacancies that occur among you. I do not greatly believe in the efficacy of any kind of machinery for securing such a result, but I would venture to suggest that the exclusive adoption of the method of co-optation for filling the vacancies which must occur in your body appears to me to be somewhat like a tempting of Providence. Doubtless there are grave practical objections to the appointment of persons outside of your body and not directly interested in the welfare of the university; but might it not be well if there were an understanding that your academic staff should be officially represented on the board, perhaps even the heads of one or two independent learned bodies, so that academic opinion and the views of the outside world might have a certain influence in that most important matter, the appointment of your professors? I throw out these suggestions, as I have said, in ignorance of the practical difficulties that may be in the way of carrying them into effect, on the general ground that personal and local influences are very subtle, and often unconscious, while the future greatness and efficiency of the noble institution which now commences its work must largely depend upon its freedom from them.

I constantly hear Americans speak of the charm which our old mother country has for them, of the delight with which they wander through the streets of ancient towns, or climb the battlements of mediæval strongholds, the names of which are indissolubly associated with the great epochs of that noble literature which is our common inheritance; or with the blood-stained steps of that secular progress, by which the descendants of the savage Britons and of the wild pirates of the North Sea have become converted into warriors of order and champions of peaceful freedom, exhausting what still remains of the old Berserk spirit in subduing nature, and turning the wilderness into a garden. But anticipation has no less charm than retrospect, and to an Englishmen landing upon your shores for the first time, travelling for hundreds of miles through strings of great and well-ordered cities, seeing your enormous actual, and almost infinite potential, wealth in all commodities, and in the energy and ability which turn wealth to account, there is something sublime in the vista of the future. Do not suppose that I am pandering to what is commonly understood by national pride. I cannot say that I am in the slightest degree impressed by your bigness, or your material resources, as such. Size is not grandeur, and territory does not make a nation. The great issue, about which hangs a true sublimity, and the terror of overhanging fate, is what are you going to do with all these things? What is to be the end to which these are to be the means? You are making a novel experiment in politics on the greatest scale which the world has yet seen. Forty millions at your first centenary, it is reasonably to be expected that, at the second, these states will be occupied by two hundred millions of English-speaking people, spread over an area as large as that of Europe, and with climates and interests as diverse as those of Spain and Scandinavia, England and Russia. You and your descendants have to ascertain whether this great mass will hold together under the forms of a republic, and the despotic reality of universal suffrage; whether state rights will hold out against centralisation without separation; whether centralisation will get the better without actual or disguised monarchy; whether shifting corruption is better than a permanent bureaucracy; and as population thickens in your great cities, and the pressure of want is felt, the gaunt spectre of pauperism will stalk among you, and communism and socialism will claim to be heard. Truly America has a great future before her; great in toil, in care, and in responsibility; great in true glory if she be guided in wisdom and righteousness; great in shame if she fail. I cannot understand why other nations should envy you, or be blind to the fact that it is for the highest interest of mankind that you should succeed; but the one condition of success, your sole safeguard, is the moral worth and intellectual clearness of the individual citizen. Education cannot give these, but it can cherish them and bring them to the front in whatever station of society they are to be found; and the universities ought to be and may be the fortresses of the higher life of the nation.

May the university which commences its practical activity to-morrow abundantly fulfil its high purpose; may its renown as a seat of true learning, a centre of free inquiry, a focus of intellectual light, increase year by year, until men wander hither from all parts of the earth, as of old they sought Bologna, or Paris, or Oxford.

And it is pleasant to me to fancy that among the English students who are drawn to you at that time there may linger a dim tradition that a countryman of theirs was permitted to address you as he has done to-day, and to feel as if your hopes were his hopes and your success his joy.

REV. MARK PATTISON ON UNIVERSITY REFORM

ONE of the most valuable addresses at the Social Science Congress at Liverpool was that by the Rev. Mark Pattison, last Friday, on the subject of Education. He confined his remarks mainly to Lord Sandon's Bill and the Oxford and Cambridge Bills. In passing, however, he spoke in the strongest terms of the miserable state of the middle-class schools, "the wretched destitution of all intellectual nourishment in which the middle classes of England grow up." With regard to the Education Bill, Mr. Pattison showed that elementary education was in anything but a satisfactory condition, that as yet we have only the beginning of a school system. He then spoke at considerable length on the Oxford and Cambridge Bills, which our readers will remember were withdrawn last session on the distinct understanding that they should be introduced next session. Mr. Pattison referred to the scheme for endowing the University at the expense of the Colleges, and to Lord Salisbury's declaration that one purpose of the measure was "to promote science and learning." Mr. Pattison went on to say:—"When the Oxford Bill got down into the Commons the member of the Cabinet who had the charge of it there hastened to disavow any such intentions on the part of his Government. Lord Salisbury's declaration had been made in the House of Lords, and in the Upper House it did not seem altogether absurd to speak of science and learning in connection with a University. But such flimsy and unpractical notions are not for the atmosphere of the Lower House. Members of the Government in the Lower House vied with each other in eagerly repudiating any intention of making the University a seat of learning and science. This had been an unauthorised escapade of their impulsive colleague in the Lords. This disavowal was well received in the House. Antagonism was half disarmed. The member of the learned University of Oxford received the congratulations of the member of the learned University of London in having done with all that nonsense. The Bill that has been dropped was a Bill empowering certain commissioners to take funds now devoted to College purposes and devote them to university purposes. What these university purposes are is not stated—is not known—not known even to the promoters of the Bill. All that is known is that among those purposes is not the promotion of science and learning. This purpose, which was announced by Lord Salisbury, has been anxiously disavowed by Lord Salisbury's colleagues. In these circumstances it cannot be any great matter for regret that the Universities Bill should have been laid aside."

Mr. Pattison then spoke of the University itself. He briefly showed how our two great universities, from being national, became State Church institutions, and that notwithstanding the abolition of the Test Act, the ecclesiastical spirit is still practically supreme.

Something might be done to counteract this sinister influence by opening the headships of colleges to laymen, and by attaching to the University a number of eminent men of science. The universities, moreover, he went on to show, are anything but popular; with a population of twenty-one millions, and realised property of 6,000 millions, the total number of university students does not exceed 6,000 out of 114,000 males between eighteen and twenty-one that ought to be receiving a high-class education. This state of things, Mr. Pattison justly says, can be described as nothing less than a state of national destitution—an intellectual blight. It is not the mere cost, though this is large enough as contrasted with the cost of university education in Scotland and Germany, that deters the middle classes from sending their sons to a university, it is the prevalent belief that, unless to a professional man, a university education is worse than useless. Mr. Pattison then went on to show what he thinks a university ought to be.

"Universities are not to fit men for some special mode of gaining a livelihood; their object is not to teach law or divinity, banking, or engineering, but to cultivate the mind and form the intelligence. A university should be in possession of all science and all knowledge, but it is as science and knowledge, not as a money-bringing pursuit, that it possesses it. There is an old saying—so old that it is quite forgotten even in the universities—'A university is founded on arts'—founded, that is, its fabric of the special sciences is raised upon the liberal studies. Men are men, whether they are lawyers or physicians, merchants or manufacturers—they possess an intellect and a conscience; and it is with these as men, and not as lawyers or physicians, merchants or manufacturers, that liberal education has to do. What professional men should carry away with them from the university is not professional knowledge, but that which directs the use of their professional knowledge, and brings the light of general culture to illuminate the technicalities of a special pursuit. To go to Cambridge, like the youth in the old Latin grammar, "*ad capiendum ingenii cultum*," seems to the practical Englishman like telling him to feed on moonshine. The idea of education is a lost idea among the middle classes. When his school-time is over—and a very unprofitable time it has mostly been to him—he can't conceive that there is anything beyond, except qualifying for a bread-winning profession. The reason why the son of a wealthy middle-class family is not at the university is exactly the same as the reason why the son of a day-labourer is not at the village school. He does not see the good of it."

Mr. Pattison then referred to a statement made by Mr. Smith, of Halifax, at the Brighton meeting, that if parents saw their way to getting 5 per cent. on the sum laid out on a girl's education, then they would be as ready to spend 2,000*l*. on that as they are on a boy's.

"Mr. Smith, of Halifax, was very likely worth thousands; but his view is precisely the view of John Nokes, the day-labourer in our village, who doesn't want his boy 'to have no school-larning; he never saw no good come of it; the boy don't get more wages by it.' John Nokes earns twenty shillings a week; Mr. Smith, of Halifax, has 5 per cent. upon many thousands of pounds; but their ideas of education are the same—no sense of the value of life, of the intrinsic worth of the human soul, and of its capacities for being trained. Man or woman is a machine for earning an income. The charm and beauty of life, as it can be lived and adorned, is wholly unknown. The work of the British workman, we say, is deteriorated because he cares nothing for the work itself, but only for the wages it is to bring him in. At this we are all indignant. We have little right to be so, when we ourselves care as little for life for life's sake as he does for art for art's sake. It may be confidently asserted, then, that the universities in any country cannot rise above public instruction generally. They may fall below it."

Mr. Pattison then showed that the great reforms in the Oxford University curriculum during the last sixty or seventy years have been forced upon her from without.

"It is no longer now a question of breaking up the old monopoly of Latin and Greek, and of the introduction of a few popular branches of instruction by the side of the old. A far wider conception of a university has now been opened up, and of the function it is expected to fulfil for the nation at large. This conception is a consequence of the position which science has come to occupy in the world in the last quarter of a century. When scientific men had to speak to the wider public fifty years ago they used to dwell on the various applications of science to the arts of life. The industrial value of scientific knowledge had then to be inculcated. It was from this point of view that science first got recognition. This has been successfully done. Facts stronger than arguments have sufficiently proved the utility of scientific knowledge. On this point no more needs to be said. The public are alive to the truth. But a new consideration now emerges out of this proved utility. Science has been incessantly growing since the close of the great European war of 1815. It has been extending its boundaries, enlarging its mass, increasing its complexity, disclosing inner harmonies, and bringing the world of thought, of work, of life within its grasp. All this growth and movement has taken place outside the universities. Our most considerable names in science have often not been university men; when they have been so their scientific activity has been quite apart from their university employment. This scientific atmosphere, this consciousness of a common aim and a common inspiration among a multitude of labourers—this active pursuit of truth, which forms a bond as strong as the bond of charity—this is not the atmosphere of our universities. There exists, then, in the world outside a vast body of knowledge, of the importance of which intelligent people are well aware. And there exist inside the universities, colleges with considerable endowments. What is more natural than the wish to bring these two separate existences together? How are we to provide for the maintenance and transmission of all this rich treasure of knowledge which has been painfully accumulating in the past? Can a more proper place for the purpose be found than in our universities? A university, says Prof. Huxley, is a corporation which has charge of the interests of knowledge as such, the business of which is to represent knowledge by the acquirements of its members and to increase it by their studies. The change demanded consists in a change of the atmosphere of the university, in the diffusion of a disinterested love of knowledge. It may be that legislation can do little to promote it. But there is one change which legislation only can make, and which is a necessary condition of the establishment of a system of scientific study and instruction. This is the removal of the fellowship system. The history of this peculiar institution has been often given of late, and the time does not now allow of my repeating it. Suffice it to say that the present operation of these valuable prizes is directly antagonistic to their supposed objects. Instead of promoting science and learning they serve only to make the university an arena in which young men contend for money prizes, and those who should be teachers are engrossed in training, handicapping, and settling the conditions of the race. The operation of emulation, honours, and prizes as a stimulus in school education is somewhat doubtful. But in the highest stage of liberal education it is necessary, if science and letters are to work with their cultivating effect on the mind, that they should be disengaged from all mercenary attractions. But when prizes of such magnitude as Fellowships are employed to attract students they become themselves the all-engrossing objects of pursuit. In Oxford and Cambridge, taken together, an amount of not less than 150,000*l.* a-year is spent on prizes. The sum is in itself an insignificant fraction of the national income, but it far exceeds the whole outlay which the country makes on science and learning. The bestowal of these lavish prizes corrupts instruction at its sources. No reform, having for its object to make the universities the home of science and learning, can be effectual which does not begin by suppressing this wholesale pensioning of youthful sinecurists. I have reminded you of one old academical saying; there is another which recurs now to my recollection, 'A Fellowship is the grave of learning.' I have spoken only of our old Universities, or rather of Oxford, because I know it best. But I must not forget that there are younger institutions which are struggling upwards towards the ideal of a university, as I have described it in Prof. Huxley's words, 'a corporation which has charge of the interests of knowledge as such.' At the head of these I must place Owens College, not only because it is in Lancashire, but because in its staff of Professors it possesses a body of men who are truly representative of knowledge in a variety of its most important departments. In a single generation we have seen this College rise from humble beginnings to a position in which it can put forward a claim to be incorporated as a university, with the privilege of giving degrees. Its capitalised sources are, indeed, small. In addition to the original 100,000*l.* of Owens' bequest, about 220,000*l.* has been contributed by voluntary subscribers, an insignificant sum when compared with the wealth of the great manufacturing metropolis. These funds, too, have been raised almost exclusively in a very small circle and by a very few public-spirited individuals; they have not been drawn from the general mass of manufacturing wealth in Manchester or the neighbouring district. With material means so inadequate, the scientific eminence attained by this young institution is a remarkable example of intellectual vigour, which must dispose us to regard favourably its claims to incorporation. But there is, besides, an immediate practical requirement which compels Owens College to seek without delay the right of conferring degrees. It is this: that as long as its students are under the necessity of graduating through the University of London, they must pass through the examinations required for the London degree. Consequently the professors of Owens College can never take the free and independent position of teachers of science. It is inevitable that they must prepare their pupils for examination, and every true teacher knows too well that this process is incompatible with genuine instruction in letters and science. The efficiency of a local university is not to be measured by the amount of its annual income, nor its success by the number of its pupils. Does it profess to teach and represent human knowledge in all its main branches and in its most complete forms? Is each great department occupied by men who are in possession of the long tradition of the past and zealous in searching out what still remains unexplored? Is liberal culture recognised as its basis, and progressive science as its aim? Where these conditions are fulfilled it would be hard to say why such an institution should not be entrusted by the State with the privilege of marking its students with the public stamp of certified acquirement. If it were merely a question of comparative qualification it would be difficult to maintain that Durham possesses, and that Owens College does not possess, the capacities, extensive and intensive, which I have supposed to be required. But if in the next twenty years the growth of Owens College is in proportion to its advance in the last twenty, the question will by that time have settled itself."

No words of ours could add to the force of this address, coming as it does from one in the position of its author. When we contrast the actual state of things in our English Universities with the ideal which appears in the above address and in that of Prof. Huxley at Baltimore—an ideal which has almost become a reality in America—any well-wisher of his country and of learning cannot but feel regret at the opportunities that have been lost, and the almost hopelessness of any rapid improvement.

THE FIFTH MEETING OF RUSSIAN NATURALISTS

THE fifth meeting of Russian Naturalists was opened September 12 at Warsaw. The Russian Naturalists are not yet organised into a permanent association, although it is their wish, repeatedly expressed, to found an association on the same principles as the British. A special imperial permission must still be obtained before each meeting, the rules of the meeting being settled by imperial decree, and a sum of money allowed for expenses and publications. The sittings of the sections are open only to members and persons introduced by them, membership being allowed only to those who have made direct contributions to science, as ordained by the rules. The meetings of the united sections for the transaction of general business and for lectures of general interest, are held in public, usually in presence of a numerous audience. The meeting (for it can hardly be called an association) publishes a daily bulletin of transactions, and issues, in the course of the year, one or two large volumes of memoirs (*Troody*) containing lectures, and longer papers *in extenso*, together with such contributions as separate societies of naturalists have found too expensive to publish in their journals.

The Warsaw meeting was largely attended by naturalists from all parts of Russia, but especially from St. Petersburg, Moscow having but few representatives. The number of members was about three hundred, the sections of Scientific Medicine and Chemistry being especially full. There were very few foreign naturalists, the organising committee not being allowed by the rules to send invitations abroad. Prof. Brodofsky, president of the Committee, was elected president of the meeting, and the St. Petersburg professors, Mendeléeff and Butleroff, vice-presidents. The ten sections of the meeting transacted a great deal of business during the nine days the Naturalists were assembled, and we may give afterwards some account of the papers read, referring now only to lectures delivered at public meetings.

At the first meeting Prof. Dobrzycki read an interesting medical paper, "On the Principles of Research into the Causes of Diseases." Several propositions as to the permanent organisation of future meetings, the opening of a Society of Naturalists at the Warsaw University on the principles adopted for the societies already existing in connection with all universities in Russia, the holding of an international meeting of naturalists, and the publication of an international daily scientific paper, were read and referred for discussion to the sections.

The second public meeting was especially crowded with the public. Two papers were read by Prof. Goyer and Prof. Halubinsky. The former, "On the Importance of Practical Scientific Institutions" (laboratories, physical cabinets, zoological stations, &c.), insisted on the necessity of such institutions for the successful teaching of natural science, and pointed out how little time is generally allowed in universities for the practical study of science, the greater part of the students' time being occupied by the lectures of the professors. M. Goyer forcibly illustrated the influence exercised by practical studies on the student, not only by affording him the only possible means of acquiring a profound knowledge of science, but especially by developing the independence of his judgment, the critical powers of his mind, and his inventive faculties.

The lecture of Prof. Halubinsky, "On the Genetic Method in the Teaching of Natural Science," treated a closely allied subject. The professor pointed out the deplorable state to which the teaching of natural science was lately reduced in Russian colleges, and insisted that only a thorough study of the natural sciences can adequately develop the analytical faculties of the mind, and that such development cannot be sufficiently attained by the study of languages and mathematics. He insisted further on the urgent necessity of fundamental changes in the arrangement of most of our handbooks of natural sciences, these handbooks beginning mostly with generalisations, instead of simply helping the scholar to arrive at them himself by means of comparison and of the analysis of the properties of objects and phenomena. The lecture provoked a lively discussion, some opposition being manifested by college teachers.

Prof. Famintzin presented Collections (*Sborniki*) made from separate copies of all papers published since the last meeting in the *Memoirs* of the six Societies of Naturalists annexed of the universities. The societies having agreed to print their journals in one uniform size, 100 separate copies of each paper published are sent to the St. Petersburg Society which makes up from them *Recueils* arranged under the heads of Geology, Botany, and Zoology. Thus those who are interested in only one of these branches can dispense with purchasing whole periodicals, the *Recueils* being sold at the St. Petersburg Society at a very low price. Here is a fine example for imitation by our various English provincial societies.

The proposal to request from the Minister of Public Instruction permission to found a Society of Naturalists at Warsaw, was met most favourably, as well as the proposal of Prof. Wagner to establish on the Solovetzky Islands a Zoological Station on the same principles as that at Sebastopol; as also was the proposal of M. Grimm to request the help of the Naval Department for dredgings in the Black Sea. MM. Grimm and Bogdanoff informed the meeting that they had undertaken two publications, a popular periodical, "Herald of Natural Science," for which they begged the co-operation of the naturalists, and a periodical in French or German, which would give to foreign readers brief notices of scientific work in Russia. This last idea was warmly supported by Prof. Mendeléeff, who proposed to request the government for pecuniary help for the publication; but this proposal having met with some opposition, it was returned for discussion in the sections.

A few excursions were made by the members, and a visit was paid, among others, to the Warsaw Institute for Deaf-Mutes and the Blind. The director of the Institute, Prof. Poplavsky, delivered on this occasion an interesting lecture on the causes of deaf-muteness, tracing them not only to the bad constitution of parents, but also to marriages between near relations. He energetically combated the opinion of Mr. George Darwin, who has endeavoured to prove by statistical evidence the fallacy of the generally accepted opinion as to the importance of the latter cause, and said that Mr. Darwin would probably change his opinions, had he the opportunity of examining the registers kept at the Warsaw Institute and elsewhere, as to the parentage of the deaf-and-dumb. The visitors had also an opportunity of witnessing the remarkable educational results arrived at by the Warsaw School. Mimic language being almost totally prohibited, the pupils are taught to understand the motion of the lips and to speak more or less distinctly; and after a four years' residence in the Institute they generally attain in both a high degree of perfection. The best result of the school is, that pupils who finish their education (technical) in the Institute immediately find employment in trades, the situations offered to them generally exceeding the number of candidates.

The usual dinner of naturalists was most animated, a very rare occasion now-a-days, as the correspondent of the *Golos* says, when Poles and Russians meet together in Warsaw. The want of friendship which was observable during the first days of the meeting, gradually disappeared, and all united most heartily in support of the toasts for the international influence of science, for the prosperity of natural science in schools, &c. Of course, a public meeting being now impossible in Russia without manifestations in favour of the struggle for independence of the southern Slaves, the usual collections were made, and a telegram was sent to General Tchernaieff with wishes for victory.

At the closing public meeting Dr. Rothe read a paper "On the Insane, and on Asylums for them." Treating the subject at great length, he concluded by animadverting on the insufficient number of asylums existing now in Russia, and proved by figures that the insane, when submitted to early medical treatment, recover in far larger numbers than is generally supposed; 70 per cent. if the treatment begins during the first months after the appearance of the disease, while those who enter the asylums with the disease about two years old, have hardly any chance of recovery. After the delivery of the lecture, various conclusions and propositions of the sections were discussed. St. Petersburg and Odessa being recommended as the place for the next meeting, a ballot decided in favour of the capital, the time of meeting to be announced during the coming winter. Resolutions were carried to request the Societies of Naturalists annexed to universities (which were organised by the initiative of the first meeting), to present in 1877 reports of their ten years activity; to change the name of the gathering into "Meeting of Naturalists and Physicians;" to raise a fund for a permanent student's scholarship in honour of Prof. Kessler, to whose initiative and many years' labours the first meeting was due. The proposal of Prof. Dobrzycki as to an inquiry into the causes of diseases, was negatived as involving too many practical difficulties, as were also the proposals of M. Vakoolofsky in reference to an international congress, daily scientific paper, &c. A committee, consisting of representatives of all sections, appointed to discuss the subject of a French-German periodical, warmly advocated the proposal, and the meeting coming finally to the conclusion that pecuniary help from the Government would be desirable, intrusted the societies of the St. Petersburg's University (Naturalist, Physical, and Chemical), to draw out rules for the conduct of the periodical. Discussions on subjects relative to the teaching of natural sciences in Governmental schools being totally prohibited in the meetings (in order to avoid opposition to the anti-Natural Science tendencies of the ministry), a pedagogical committee, appointed to discuss the proposals of Prof. Halubinsky, decided that permission should be requested from the ministry to allow the meetings a pedagogical section to discuss at least some of the more special questions relative to the subject. The conclusions of the committee were accepted, as well as those of the Zoological Section, to request from the Naval Department the use of ships for scientific explorations in Russian seas. Finally, the small sum produced by the members' fees at the meeting (993 roubles from 331 members) was allowed for the publication of memoirs. The discussion of these various subjects having taken up much time, the members dispersed, and very few attended the lecture of M. Kostareff "On the Inductive and Deductive Methods of Reasoning and of Inquiry." The meeting was closed by a short address by the president, Prof. Brodofsky.

A. L.

PRINCIPLES OF TIME-MEASURING APPARATUS[1]

II.

The Pendulum.

In that early apparatus I recently described, you will remember that the balance, after being set swinging in one direction, had its motion completely destroyed, and was then set swinging in the other, all by the direct agency of the clock-train. If it had possessed no other property than that merely of vibrating against the earth's attraction, the pendulum would have been an immense improvement upon this state of things, because every impulse delivered to it is, so to speak, stored up there, and is gradually expended therefrom as occasion requires in overcoming the friction due to its connections and the resistance of the atmosphere.

The discovery of the pendulum is generally attributed to Galileo, whose attention was attracted to the subject by watching the oscillations of a chandelier suspended

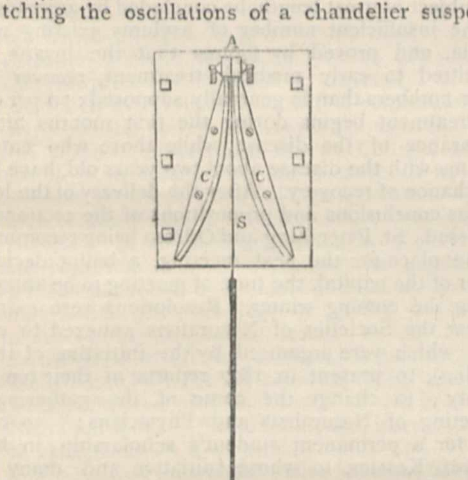


Fig. 9.

by a very long line at a church in Pisa. The story is very likely to be a true one; anybody observing the shorter oscillations of a very long pendulum (fifty or sixty feet in length say) could scarcely fail to be impressed by them.

The celebrated Dutch philosopher, Huygens, first worked out its theory. He discovered that if a pendulum, instead of swinging in a circular arc (which it obviously does) could be made to move in a cycloidal, it would perform all its oscillations, whether large or small, in precisely equal times.

He succeeded in obtaining this motion for his pendulums by the following contrivance (see Fig. 9):—Two curves or cheeks, C C, starting from the axis of motion are placed one upon each side of the pendulum, which is suspended by a flexible line or spring S. As the pendulum swings, this line wraps around either curve and deflects the pendulum from its circular path, K U R, into the cycloidal, D U L. As you could almost infer from inspection the time of a pendulum swinging a cycloidal, is rather faster than when it swings a circular arc, the cycloidal being the more rapid curve. Also the time of the swing of a pendulum in a circular arc gets longer as the swing increases, that is to say, as it travels further up the curve; for instance, if the arc of a pendulum which was swinging 2° was increased to 2½°, the loss of time due to the increased length of its swing would be four seconds a day.

The invention of these cycloidal cheeks or curves must have been looked upon as the *ne plus ultra* of perfection

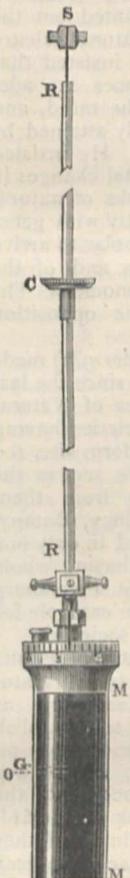


Fig. 10.

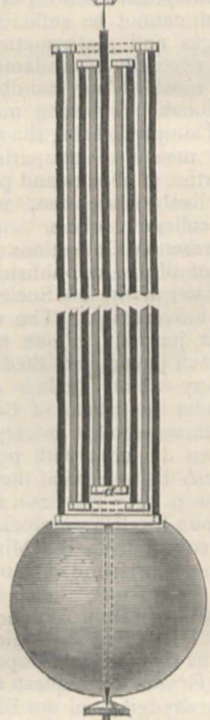


Fig. 11.

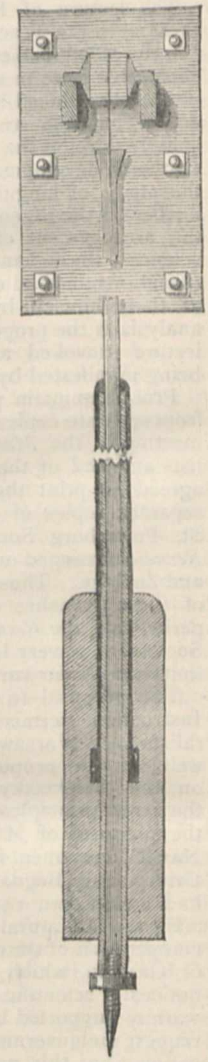


Fig. 12.

at the time; but in the first place they did not deflect the pendulum without a good deal of friction; and in the second it is rather advantageous than otherwise that a pendulum should gain in its shorter vibrations, because it never gets into them without retardation (which implies loss of time), and one error tends to correct the other.

Huygens also discovered that the time of one swing of a pendulum varies as the square root of its length. The length of a pendulum swinging in one second is nearly 39·2 inches, and if you wish to find the time in which a pendulum of any other length will perform one swing, you divide the square root of that length by the

[1] Lectures by Mr. H. Dent Gardner, at the Loan Collection, South Kensington. Continued from p. 531.

square root of 39·2 inches; thus the time of the swing of a pendulum 61 inches long

$$= \frac{\sqrt{61}}{\sqrt{39 \cdot 2}} = \sqrt{\frac{61}{39 \cdot 2}} = 1 \cdot 25 = 1\tfrac{1}{4} \text{ seconds.}$$

On the other hand, if you wish to find the length of a pendulum to swing in a given time, all you need do is to multiply 39·2 by the square of the time; thus the length of a pendulum to swing in $\frac{1}{2}$ second $= 39 \cdot 2 \times (\frac{1}{2})^2 = 9 \cdot 8 = 9\frac{4}{5}$ inches.

But with reference to an ordinary clock pendulum, such as is shown in Fig. 10, you may ask me what is its length? do we measure its length from the point of suspension to the end or centre of the bob, or to the point at its extremity? We measure it to none of these places. Its true length is determined by multiplying every particle into the square of its distance from the point of suspension, adding all these together and dividing by the sum of every particle multiplied into its distance from the point of suspension simply. Of course an operation of this kind is not very easily performed, but the upshot of the calculation is, in general, to give a distance to a certain point, O, called the centre of oscillation, just below the centre of gravity, G, of the pendulum. This is the true length of our pendulum so far as its time of vibration is concerned, and if we could take a perfectly simple pendulum (that is one with a rod without weight, and all the matter of its bob accumulated in one point at its extremity) of the same length, we should find that the times of their swings would exactly correspond.

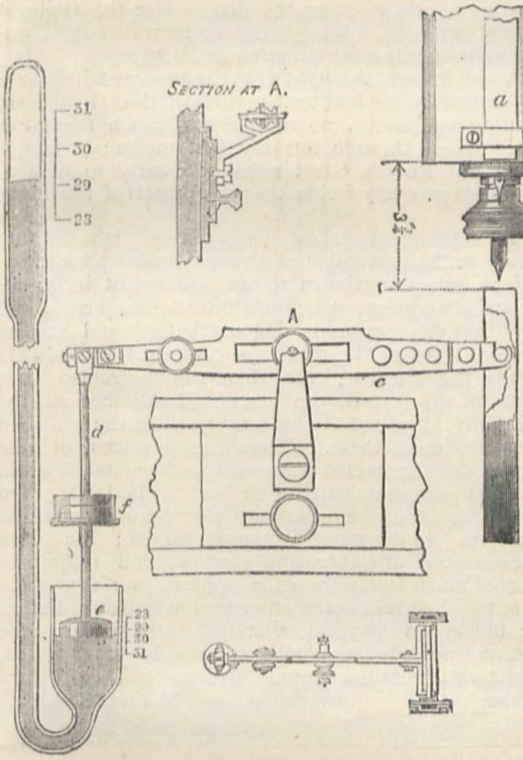


FIG. 13.

What will happen if at any point above the centre of oscillation we add a little weight to our pendulum, say at point C? Evidently the effect is just the same as if we tied another short pendulum of length S C to our main one—it will urge it on and make it swing faster. At a point just half-way up the pendulum, the effect of any given weight will be greatest. From which follows the curious fact that a weight moved upwards or downwards away from this point will, in *either* case, increase the time of the swing of the pendulum, that is to say, make the clock lose.

The finer regulation of pendulums is performed upon the principle of adding or withdrawing weight at a point above the centre of oscillation. The collar C upon the pendulum, is placed there to carry subsidiary weights for the purpose.

The Pendulum Compensation.

Pendulums, like other things, lengthen as they get warmer, and shorten as they get colder, and the time of their swing is varied in consequence. For instance, a plain iron rod pendulum for every 10 degrees rise in the thermometer, will expand sufficiently to make the clock controlled by it lose nearly 3 seconds a day.

The earliest and one of the best methods of correcting or compensating this error is the mercurial pendulum designed by Graham (see Fig. 10). The bob of the pendulum is formed of a glass or iron vessel containing mercury, M M. When there is any increase of temperature, the rod R R expands and lets down the bob, but the mercury in the bob also expands, and from the manner it is confined expands upwards. The expansion of the mercury therefore tends to raise the centre of oscillation, and its amount is so calculated as exactly to neutralise and destroy whatever error would otherwise result from the lengthening of the rod. The action of this compensation may very readily be increased by adding or withdrawing a little of the mercury. Of course after each addition or withdrawal of mercury the clock will have to be regulated to time again by altering the nut upon its pendulum for the purpose.

A slight tendency to vary its rate after first being put up may sometimes be noticed in a clock fitted with one of these pendulums. This arises from air bubbles in the mercury, which gradually approach the surface; as they do so the mercury upon the other hand of course falls.

Another method of compensation is the gridiron pendulum of Harrison. Different metals expand at different rates, for instance—

Steel	expands	·000064	of its length.
Brass	,,	·0001	,,
Zinc	,,	·00017	,,

for every 10 degrees rise in temperature.

Suppose we take a central steel rod (see Fig. 11) about 3 feet long, and fasten to its extremity a cross piece *a*, upon which we erect two (for the sake of symmetry) brass rods, one upon each side of it; and to the summit of these we attach two other rods of steel, and at the extremity of these again two other rods of brass, and then let fall two more rods of steel, joined at their extremities by a cross piece, and to the cross piece attach the pendulum bob by another short length of steel so as to make up 39·2 inches of length between the centre of oscillation and the point of suspension. Supposing that the four supplementary lengths of brass and steel upon each side of the original steel rod average 2 feet 11 inches long, we have, in between the point of suspension and the centre of oscillation 109·2 inches of steel and 70 inches of brass, and further, that the expansion of this amount of brass is exactly equivalent to the expansion of the steel. But we have so arranged that all the brass expands *upwards* and all the steel *downwards*; therefore one destroys the other, and the position of the centre of oscillation does not change, whatever be the alteration of temperature. The worst of this method of compensation is, owing to the great weight of the rods, the centre of oscillation generally ceases approximately to correspond with the centre of gravity of the bob, and the true amount of compensation has to be determined by experiment, which is seldom done.

In the construction of compensation pendulums care must be taken that they are formed so that each part shall simultaneously take up any change of temperature. This was brought prominently to light during the time that the normal sidereal clock for Greenwich was under trial. That clock had been fitted with a heavy mercurial pendulum, and it was found that the rod got warmer or colder some time in advance of the mercury; of course the compensation *failed* for such interval of time.

The following form of pendulum was afterwards substituted. The expansion of zinc is, as you see, nearly double that of brass, and consequently a good deal less of it is required to compensate a pendulum. To the extremity of an internal steel rod (see Fig. 12) a collar is fastened, and a zinc tube inclosing the steel rod rests upon it. To the summit of the zinc tube is attached a steel tube, which in turn incloses both it and the rod, and the pendulum bob is fastened midway of its length to the extremity of this tube. The outer steel tube is cut away at its sides, and holes bored in the zinc in order to let in changes of temperature rapidly.

The action of the combination is similar to that of the gridiron pendulum, the expansion of the zinc upwards exactly neutralising and destroying the expansion of the steel downwards. It is important (as suggested by Mr. Buckney) that the bob should be suspended at its centre, because otherwise it would also operate as an expansion length, and although its effect could be counterbalanced by shortening the zinc tube, yet owing to its bulk it would be sure to lag behind the rest of the compensation, and cause such an error as I have referred to.

Barometric Compensation.

When you aim at the very highest time-keeping, barometric compensation becomes necessary; that is to say, compensation against the disturbance to the pendulum due to changes of atmospheric pressure. For instance, when there is any rise in pressure, when the atmosphere becomes denser, our clock will lose, and will gain when the atmosphere becomes more attenuated, the variation in the Greenwich clock having been at about the rate of ·3 of a second a-day for a difference of one inch in the barometer.

The following compensation (see Fig. 13) is one designed by Sir George Airy:—

C is a lever moving around an axis at A. One arm of the lever carries a horse-shoe magnet, *b*, and the other a float, *c*, supported upon the mercury in a barometer cistern. Two bar magnets (the front one, *a*, only is shown, the other being behind the bob) are fastened upon the pendulum bob, the north pole of one pointing upwards, and of the other downwards (in order to render the combination astatic).

The poles of the horse-shoe magnet face the opposite poles of the two bar magnets, and attraction goes on between them. When the barometer rises the mercury in the cistern falls, and with it the float. The other arm of the lever, therefore, rises, bringing the poles of the horse-shoe magnet closer to the poles of the two bar-magnets, and increases the attraction between them, which is a force acting in the same direction as gravity. The pendulum consequently moves faster (for we increase the pull upon it), the tendency to go slow arising from the increased atmospheric pressure is by this means compensated. Dr. Robinson, at the Armagh Observatory, effected the same correction by attaching a barometer to the pendulum rod. He also noticed that changes in atmospheric pressure would disturb a mercurial pendulum to a very considerable extent if there were air-bubbles in the mercury.

(*To be continued.*)

CROOKES'S RADIOMETER

I HAVE recently made a few experiments with this instrument which may not be uninteresting to the readers of NATURE.

The radiometer used had discs of aluminium polished on one side and blackened on the other; it was more than usually sensitive, and would sometimes continue its rotation for twenty minutes after the sun had set in the sea.

The instrument being in a room in which the radiation was far too feeble to cause the arms to move, I grasped the bulb with both hands, so as still further to exclude it from light. The vanes immediately began to revolve briskly, the polished sides first. Removing my hands after two or three minutes, the movement soon stopped; and then, after a very brief interval of rest, began in the opposite direction, and so continued for several minutes.

I now placed the instrument in a room, near to a window through which the light of the full moon in a clear atmosphere was shining. The arms of the radiometer did not move. By means of a large lens the moonlight was then concentrated about 200 times, and allowed to fall full upon the blackened side of one of the circular discs, in such a way as to cause the intensely brilliant image of the moon to nearly cover the disc. Not the slightest movement occurred, although the concentrated light impinged upon the disc for a quarter of an hour.

As is well known, the light of the moon contains, for a given luminosity, far less heat rays than does light from any terrestrial source, no matter how much the latter may be strained through intranscalent media; in fact it require Lord Rosse's 6-feet reflector clearly to demonstrate the excessively feeble thermal power of the lunar rays.

These experiments show, firstly, that light is not necessary to the movement of the radiometer; secondly, that light only contributes to the movement in so far as, by its absorption, it is transformed into heat; and thirdly, that the motion is due to the unequal heating of the two sides of the discs, the cooler surfaces always preceding the warmer; for when the instrument was grasped by the hands, the blackened surfaces of the discs rapidly absorbed the heat rays, whilst the polished surfaces reflected them. Thus the surfaces of the blackened discs remained warmer than the metal beneath, but gradually communicated their heat to the latter. On removing the hands from the bulb, the thermal condition of the discs would soon become reversed; the black surface—a good absorber and also a good radiator—would cool much faster than the opposite surface, which being of polished metal was an exceedingly bad radiator.

The blackened surfaces, therefore, now became the coolest, and preceded the polished ones, in other words, the direction of rotation became reversed.

October 17 E. FRANKLAND

THE GEOLOGY OF ENGLAND AND WALES[1]

THE well-known volume of Conybeare and Phillips, entitled "Outlines of the Geology of England and Wales," which was published in 1822, and was based on an earlier and slighter work of the second-named author, has long held an honourable place among geological classics. It has served, indeed, to supply to some extent the want so universally felt of a descriptive memoir or handbook to William Smith's Geological Map, a work which "the father of English geology" could never be prevailed upon to write himself. The "Outlines," however, is but a fragment, the second part of the work, which was to have dealt with the oldest rocks and with questions of Economic Geology, never having been published; and more than half-a-century of research, carried on in connection with a science which appears to have as

[1] "The Geology of England and Wales: a Concise Account of the Lithological Characters, Leading Fossils, and Economic Products of the Rocks; with Notes on the Physical Features of the Country. By Horace B. Woodward, F.G.S., of the Geological Survey of England and Wales. (London: Longmans, Green, and Co., 1876.)

yet lost none of the vigour and elasticity of youth, have of course rendered much of the information contained in it obsolete. The only work of more recent date which occupies somewhat the same ground is D'Archaic's "Histoire des Progrès de la Géologie," which aimed at doing for all those portions of the globe which had been geologically explored, what Conybeare and Phillips had attempted for England alone. This work is one of the very highest order of merit; its author being equally distinguished for his industry in the compilation of materials, his skill in arranging them, and his boldness and originality in generalising from them. But such a design as that of the "Histoire des Progrès" was perhaps too ambitious to be within the compass of the efforts of any single individual; at all events, after the portions relating to the Tertiary and Secondary strata had appeared in a series of eight volumes, between the years 1847 and '60, the work, which had up to that time been published by the Geological Society of France under the auspices of the Minister of Public Instruction, was finally abandoned.

It will be seen, therefore, that Mr. Woodward's handy volume, the title of which is given above, appears very opportunely; and, supplying as it does a real need of the geological student at the present time, it is certain at once to take its place as the most useful general work of reference on English Geology which exists. After a careful perusal of it, we find scarcely anything calling for qualification of those terms of high commendation in which we are constrained to speak of its general accuracy and excellence of arrangement; of the happy way in which the mean has been hit between conciseness of description and fulness of detail; and of the manner in which the work has been made to include the latest results of geological research.

At the time when Conybeare and Phillips wrote, many portions even of those Secondary strata of England, the successful classification of which had been the chief among

FIG. 1.—The Cheddar Cliffs.

the triumphs of William Smith's genius, were as yet almost unknown to geologists; the labours of Sedgwick and Murchison, which were destined to replace the confusion that reigned among all the older deposits, by the clear succession of the Cambrian, Silurian, and Devonian systems, had not then commenced; and as yet the palæontological studies of Lyell and the stratigraphical researches of Prestwich had not dispelled the almost equal obscurity which prevailed concerning the order of the Tertiary formations. There are perhaps few ways in which the strides made during the last fifty years in our knowledge of the geology of this country can be more vividly realised than by a comparison of the sketch-maps prefixed to the volume of Conybeare and Phillips, and to that of Mr. Woodward respectively. Such a comparison will render strikingly apparent the great advances which have been made in developing the true structure of the country, both through the researches of private individuals and the labours of the National Survey; and it will equally serve to demonstrate the necessity of such a work as that which Mr. Woodward has now given to us.

The avoidance by the author of this work of all references to the equivalent formations on the continent of Europe, or even to those in other parts of the British Islands—although perhaps a necessity dictated by the limits he had set himself—creates some serious difficulties, which are more especially felt when questions of classification come to be treated of. It is altogether vain to hope that such problems can be decided by an appeal to the English representatives of the formations alone. To discuss, for example, the question of the classification of the Silurian, Devonian, and Permo-Triassic (Poikilitic) formations, without any reference to the typical developments of these strata in Bohemia, the Eifel, and Central Germany respectively, is surely a most unsatisfactory and inconclusive proceeding.

In adopting Sedgwick's classification of the Cambrian and Silurian strata instead of that of Murchison, the

FIG. 2.—Purbeck and Portland Beds at Tilly Whim, near Swanage; the former being the light-coloured strata above, the latter the darker strata below.

FIG. 3.—Section at Writtle, near Chelmsford.—2. Chalky Boulder Clay (Upper Glacial). 1. Sand and Gravel (Middle Glacial).

FIG. 4.—Penrhyn Slate Quarry.

author may possibly have been actuated by the conviction that unless the pendulum of opinion, which has so long been firmly held at one end of the arc by official influences, were allowed to rebound to the extreme limit in the opposite direction, there would be little chance of its finally attaining a position of stable equilibrium between them. Looked at from any other point of view, we must confess that we cannot regard the attempt here made totally to revolutionise the classification in question with much satisfaction. We had hoped that the day had long since gone by when the divisions between geological periods were to be regarded as governed by anything more than convention, or as serving any other purpose than that of convenience of reference. Breaks, whether stratigraphical or palæontological, in the series of formations, are *purely local phenomena;* and it is certain that if stratigraphical geology had taken its rise only so far away as in Eastern instead of in Western Europe, the divisions of the great systems, and even of those larger periods (which Mr. Woodward calls "cycles") would have been wholly different to that which has been actually adopted. But although the classification of the geological periods is a purely artificial one, yet it has its uses, and nothing but confusion can result from attempts to unsettle its landmarks without sufficient cause. Such being the case, we are surely entitled to ask what useful purpose can possibly be served by including, as our author does by his own showing, considerably more than one-third of the whole thickness of British sedimentary deposits under the name of Cambrian? Is not a Cambrian system, enlarged beyond all reasonable proportions, equally objectionable with an overgrown Silurian? This question has passed beyond the stage when it can be regarded simply as a battle-ground for the partisans of rival reputations. Now that Sedgwick and Murchison have both passed away, let us rather seek to be guided by the principles which determined the action of the greatest of their contemporaries in respect to this controversy; gladly availing ourselves of that which is good and true in the splendid work of both the observers, let us build it into our geological system, there to stand as the noblest monument of their genius; and for their mistakes, let these pass into the oblivion which awaits the memory of the injustice and animosity which were unworthy of either of them.

There are one or two other points which we would venture to suggest for the author's consideration in the event of his being called upon, as we hope he will be, to prepare a second edition of this work. As the different formations or groups of strata belonging to the same system which occur in different parts of the country are treated of consecutively, although in many cases they were doubtless formed contemporaneously, it would be well to keep the latter fact as prominently before the mind of the student as possible; and this, we think, might best be accomplished by prefixing to each chapter diagrammatic sections of the succession of strata, exhibiting their equivalences in different parts of the country. Again, although we recognise with the author the impossibility of quoting in such a work as the present the authority for every statement, yet we think that a well selected series of references to those original memoirs, in which fuller details concerning each formation may be found, would greatly add to the value of the book without materially increasing its bulk.

We cannot but commend the manner in which Mr. Woodward has resisted all attempts at fine writing, and has sought rather to produce a work characterised by accuracy and soundness than by showiness and superficiality; in this respect following the example of his father, the late Dr. Samuel Woodward, to whose memory the work is dedicated. We anticipate for the "Geology of England and Wales" a sphere of usefulness not less extended than, and a reputation as enduring as that which has been attained by, the "Manual of the Mollusca;" and higher praise it would scarcely be possible to award to it.

It only remains to add that the work is illustrated, not only with a very clear chromo-lithographed map prepared by Mr. Griesbach, but by woodcuts of such excellence (as will be manifest from the specimens we give of them) that we can only regret that they are so few in number.

J. W. J.

SUMNER'S "METHOD AT SEA"

IN reference to our review of Sir William Thomson's work on this subject (vol. xiv. p. 346), our attention has been called by Sir G. B. Airy to the following paper in the *Proceedings* of the Royal Society, vol. xix. p. 448:—

"Remarks on the Determination of a Ship's Place at Sea." In a Letter to Prof. Stokes. By G. B. Airy, LL.D., &c., Astronomer-Royal.

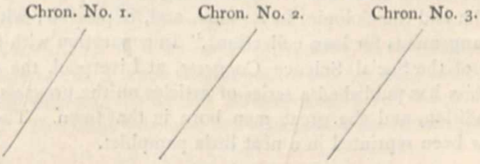
Royal Observatory, Greenwich, S.E., 1871, April 5.

MY DEAR SIR,—In the last published number of the *Proceedings* of the Royal Society (vol. xix. p. 259), there are remarks by Sir William Thomson on the proposed method for determining the *locus* of a ship's place at sea, by making one observation of the sun's (or other body's) altitude, and founding, on this, computations of longitude with two assumptions of latitude; and there are suggestions, with a specimen of tables, for solving the spherical triangles which occur in all similar nautical observations, on the principle of drawing a perpendicular arc of great circle from one angle of a spherical triangle upon the opposite side.

In regard to this principle and the tables which may be used with it, I may call attention to the employment of a similar method by Major-General Shortrede, in his "Latitude and Declination Tables," pp. 148 and 180. In p. 150, line 11 from the bottom, it will be seen that the "column" gives the trial-value of the perpendicular arc by which the two right-angled triangles are computed. This is not the same (among the various elements which may be chosen) as Sir William Thomson's; but it is so closely related that in some instances the tabular numbers are identically the same as Sir W. Thomson's, though in a different order. General Shortrede's object was "Great Circle Sailing," in which the trigonometrical problem is the same as in the nautical observation. I think, however, that Sir W. Thomson deserves thanks for calling attention to the application of this method to time-determinations.

In regard to the problem of the "locus," allow me to point out the geometrical circumstances of the case. If, upon a celestial globe, an arc of small circle be swept with the sun's (or other body's) place for centre, and the observed zenith-distance for radius, the ship's zenith will be somewhere in that curve; and if, with the pole for centre, arcs of parallels be swept with the two assumed colatitudes for radii, the intersection of these two curves with the first drawn curve will give the ship's zenith on the two assumptions; and if within the celestial globe there be placed a small terrestrial globe, and if these zenith-points be radially projected upon the terrestrial globe, the terrestrial places of the ship on the two assumptions will be marked. But the practical application of this requires that the position of the terrestrial globe, or of the earth, be known in respect of rotation—that is, it requires that the Greenwich sidereal time, or solar time, be known; in other words, it requires a perfect chronometer. Now the experience of Capt. Moriarty, cited by Sir W. Thomson, does not apply here. Capt. Moriarty received time-signals from the Royal Observatory through the cable every day, and he had therefore a perfect chronometer. But other ships have no such perfect chronometer; and though the *direction* of a *locus*, as determined above, may be sufficiently certain, yet its *place upon the earth* will be uncertain, by a quantity depending on the uncertainty of the chronometer. Thus three chronometers may give the following positions for the locus-curve:—

Chron. No. 1. Chron. No. 2. Chron. No. 3.



And the question now presents itself, which uncertainty is the greater—the uncertainty of latitude, which it is the real object of this problem to remedy? or the uncertainty of the chronometric longitude, which must be used in attempting to find the remedy? I do not doubt the instant reply of any practical navigator, that the chronometric longitude is far more uncertain than the latitude; and if it be so, the whole method falls to the ground.

I fear that a publication like that which has been given to this method may do very great injury among navigators who are not accustomed to investigate the geometrical bearings of such operations, and may lead them into serious danger.

I am, my dear Sir, yours very truly,

G. B. AIRY.

Prof. Stokes, Secretary of the Royal Society.

[From a general recollection of a conversation I had with Sir W. Thomson before the presentation of his paper, I do not imagine his object to have been exactly what the Astronomer-Royal here describes, but partly the saving of trouble in numerical calculation, partly the exhibition, for each separate observation of altitude at a noted chronometer time, of *precisely what that observation gives, neither more nor less*, which introduces at the same time certain facilities for the determination of a ship's place by a combination of two observations. Of course the place so determined is liable to an error east or west corresponding to the unknown error of the chronometer; and doubtless, under ordinary circumstances, this forms the principal error to which the determination of a ship's place is liable. This remains precisely as it did before; and it is hard to suppose that the mere substitution of a graphical for a purely numerical process could lead a navigator to forget that he is dependent upon his chronometer, though perhaps the general tone of Sir W. Thomson's paper might render an explicit warning desirable, such as that which Mr. Airy supplies.—G. G. STOKES.]

NOTES

WE hear with sincere regret of the death of the eminent French meteorologist, M. Charles Sainte-Claire Deville. We hope next week to give some details of his life and work.

WE publish on another page an abstract of the Rev. Mark Pattison's forcible and outspoken address at the Social Science Congress, Liverpool, on the state of our universities. Many other valuable papers were read, but they were for the most part too special for notice in our columns. We should, however, mention the remarks of Mr. W. H. James, M.P., in connection with the discussion of the question of incorporating a professional and technical training with a sound system of general education. Mr. James traced the history of the City Guilds of London, showed how enormously wealthy they must be, how this wealth is totally misspent, and maintained that the country had a perfect right to ask an account of their stewardship, and appropriate the funds, if necessary, for educational purposes. He proposed that the funds should be devoted to the establishment of a science and practice institute for working men. All the speakers in the Education department of the Congress seem to be agreed that there is vast room and urgent need for improvement in the education of the country. When so many intelligent and influential men are agreed on this point, how is it so little is done to mend matters? After the reading of a paper on Tuesday by Mr. W. J. Watts on the proposed Imperial Museum for India and the Colonies, a proposal was unanimously adopted by the Section of Economy and Trade, "that the Section recommend the Council to consider the propriety of memorialising her Majesty's Government in favour of establishing an Imperial Museum for India and the Colonies in London, and, if possible, with special arrangements for loan collections." In connection with the meeting of the Social Science Congress, at Liverpool, the *Liverpool Albion* has published a series of articles on the progress, present condition, and the great men born in that town. These have now been reprinted in a neat little pamphlet.

SOME account of Mr. Giles's trans-Australian journey has reached this country; he arrived in South Australia in August. Mr. Giles, who started on April 10 from a spot 27° 7′ South latitude and 116° 45′ East longitude, says:—"I made a generally north-east by east course by way of Mount Gould, in latitude 26·46, till the 24th parallel was reached. I traced the Ashburton to its sources, and determined the old watershed by the western rivers, which is simply a mass of rangy country abutting upon the desert in longitude 120° 20′. From the depot on the Ashburton I went up to the 23rd parallel. No watercourses flowed eastward. From the end of the watershed in that longitude, the latitude being near the 24th parallel, to the Rawlinson Range of my last horse expedition, in longitude 127°, the country was all open spinifex sandhill desert. At starting into the desert most of the camels were continually poisoned, the plant which poisoned them not being allied in any way to the poison plants of the settled districts of Western Australia. I now know it well, and have brought specimens. The longest stretch without water was a ten days' march. One old cow camel died after reaching the water. We had some rain on May 8 before reaching the Ashburton, and some of it must have extended into the desert. It was the only chance water we obtained. We had some more rain north of the Alfred and Mary ranges. Portions of the Rawlinson and Petermann ranges had been visited by rains, but the further we went eastward the more desolated with drought the country became. We struck the telegraph line at the angle poles close to Mount Halloran, on the Neal's River, sixty miles from the Peake, and travelled thence down the line to the station. We were all attacked with ophthalmia before the rains fell in May. The winter was excessively cold, the thermometer in the morning for weeks being down to 18°. No natives were met with from Mount Gould to the Petermann Ranges, at which last-named place they were friendly. In Musgrove Range they stole a few things, but I was absent at the time. The camels have travelled splendidly."

A MUSHROOM Exhibition will be opened on the 23rd inst. at the rooms of the French Botanical Society, 84 rue de Grenelle, Paris, which is likely to be of interest both from a scientific and an economical point of view. It is proposed to bring together all species of mushrooms, either in a fresh or a dry state, eatable, poisonous, hurtful to agriculture, as well as books, drawings, and engravings bearing on the subject. The exhibition will last eight days, during which there will be suitable lectures, as well as excursions to the neighbourhood of Paris. The following questions are proposed by the Society:—1. On the development of the reproductive organs of mushrooms; what is the exact signification of the terms *spores*, *chlamydospores*, *stylospores*, *conidia*, *spermatia*, &c. 2. Fungoid protoplasm compared with that of the vegetable chlorophylls. 3. On the classification of the *Agarici*, and generally the relative value of characteristics among mushrooms. 4. Study of the substrata necessary to the development of various fungoid species and of the relation which exists between the substrata and these species; questions relative to parasitism. 5. On edible mushrooms in various regions. 6. The necessity of encouraging chemical investigation on mushrooms; a *résumé* of the facts ascertained in this department to the present time. 7. The best processes for preserving mushrooms for study. 8. Bibliographical researches on the mycologists of last century.

A TASHKEND telegram of October 6 announces that the scientific staff of General Skobeleff's Alai Expedition have accomplished their work most successfully. The Alai and Trans-Alai mountains and the northern part of the Pamir plateau were surveyed along the routes followed, and astronomical determinations of latitude and longitude made. The highest spot, where astronomical observations were made, was at a height of 14,500 feet, and is in the part of Pamir called Khorgota. The height of the Oos-Bel pass was 15,500 feet. Measurements of the magnetic declination were also made on the Pamir plateau, and valuable collections brought home. The map of the Alai, plotted by Dr. Petermann on the basis of the surveys and descriptions of the late M. Fedchenko, proved to be very satisfactory.

THE congress of the International Geodesical Association, established by several European governments, was held this year at Brussels, and will be held in 1877 at Stuttgart. For a number of years the French Government abstained from sending delegates, but they were represented this year by M. Faye, M. Yvon Villarceau, and Major Perrier, director of the French Survey. The president was General Ibanez, the Spanish delegate. Switzerland was represented by M. Hirsh, Prussia by General von Baeyer, Austria by Oppolzer, Belgium by Major Adan, Saxony by M. Bruhm, Russia by General de Forsh. Neither England nor the United States sent any delegates. A report was presented by Major Adan on the registering meteorological instruments established at Ostend by Prof. Rysselberghe, of the Ostend Navigation School. These instruments, which obtained an exceptional reward at the International Geographical Exhibition at Paris in 1874, were praised in very warm terms. It is said that they will be used at a number of maritime stations for registering the tides. On the proposition of General Ibanez a requisition is to be sent to the French Government asking them to take the necessary steps for joining the French and the Spanish triangulations.

WE are glad to be able to state, at the request of the Hon. W. B. D. Mantell, of the New Zealand Legislative Council that he has publicly repudiated the contemptuous words in reference to scientific men attributed to him in NATURE, vol. xiv. p. 90. Such a statement, he says, would be an act of "gross and insane ingratitude" towards many men whom he is proud to call his friends. He was speaking only of "the shams and Dousterswivels of science," for nobody could have a greater or more devoted esteem for scientific men than he had. He was perfectly serious in proposing that an inquiry should be made in reference to the discovery of the skeleton referred to.

DR. MCKENDRICK has been appointed to the Chair of Physiology in the University of Glasgow.

THE Fellows of the College of Physicians of Dublin have deliberately determined to admit Miss Edith Pechey to the examination for the L. K. Q. C. P. I., and have thus thrown open the portals of the medical profession to all comers, whether they be "persons" of the male or female sex. However pregnant of results this decision may be, says the *Medical Press and Circular*, it does not seem to us that any other conclusion was possible, and we expect to see a similar ingress allowed to the ladies by all other bodies. The Queen's University, it is anticipated, will be the next to follow suit, and these fortresses having surrendered at discretion, it is impossible that others can long sustain the siege.

A REPORT that Mr. Lucas, the African traveller, had given up exploration in consequence of illness is unfounded. Mr. Lucas had an attack of fever, but is now at Cairo waiting for stores which have been ordered from England, on the arrival of which he will proceed by steamer to Zanzibar, and again make for the interior. Mr. Lucas is in communication with the Royal Geographical Society.

MRS. NASSAU SENIOR writes to the *Times* on the curious behaviour of tempered glass. She furnished twelve gas burners with tempered glass globes purchased in London, and having the veritable label of M. de la Bastie affixed to each. On the night of the 6th inst. after the gas had been extinguished for exactly an hour, one of the globes burst with a report and fell in pieces on the floor, leaving the bottom ring still on the burner. These pieces, which were, of course, found to be perfectly cold, were some two or three inches long, and an inch or so wide. They continued for an hour or more splitting up and subdividing themselves into smaller and still smaller fragments, each split being accompanied by a slight report, until at length there was not a fragment larger than a hazel nut, and the greater part of the glass was in pieces of about the size of a pea, and of a crystalline form. In the morning it was found that the rim had fallen from the burner to the floor in atoms. The subject deserves careful investigation.

THE Science Loan Exhibition has been so successful that the time for closing it has been postponed, and the evening lectures are to be recommenced immediately.

WE have received *Études sur les Mouvements de l'Atmosphère*, Part 1, by Professors C. M. Guldberg and H. Mohn, of Christiania. In this first part of what promises to be an important contribution to the physics of the atmosphere, the authors confine the discussion to some simple elementary cases of the mechanics of the atmosphere relative to its equilibrium, temperature, humidity, and horizontal and vertical currents. We join the authors in hoping that the results will demonstrate the necessity of more extensive observations than have yet been made in tropical regions, and in the higher regions of the atmosphere on mountains or by captive balloons, and that the true path of progress for meteorology to follow is the development of the difficult question of atmospheric mechanics. We may add that in order to obtain the physical data required for its discussion, the only rational step to be first taken is to plant numerous meteorological stations over limited areas, the stations being so closely planted as to secure approximations to the barometric gradients between the observing stations and to the wind-velocities, sufficiently close to the true gradients and velocities as to meet the demands of the problem to be investigated.

THE teaching body of the French National School of Agriculture, established at the Conservatoire des Arts-et-Metiers, has now been organised. The director of studies is M. Boussingault, the founder of agricultural chemistry in France. The number of professorships is twenty, and a competition will take place for three of them. Amongst the seventeen others who have been appointed by decree, M. Lavergne, Professor of Agricultural Economy, M. Leon Bocquerelle, Professor of Physics and Meteorology, and M. Tany, Professor of Sylviculture, were formerly professors at the Versailles School of National Agriculture, which was suppressed in 1852. The former imperial farmhouse at Vincennes will be utilised for experimental agriculture. Amongst the professorships which have been created ought to be noticed one of Comparative Agriculture, or the systematic comparison of French and foreign agriculture.

M. WADDINGTON, the French Minister of Public Instruction, has published a circular warning the several municipal administrations of France, that he is to ask from Parliament next session a credit for increasing the salaries of professors who, having not taken any superior degree, are nevertheless useful and steady workers. But he desires the cities to enter into an agreement with the Government to secure to competent teachers in the several municipal secondary schools a rate of remuneration not below a sum named. It is only when that rate shall have been granted as a permanency by the local authorities that the Government will give any addition.

M. WADDINGTON is said to be preparing to present to both Houses of the French Parliament a Bill to alter the law for granting degrees, giving the power entirely to the State examiners. The same proposal was rejected by the Senate last spring.

A NEW municipal school, the École Monge, was opened at Paris on October 8. The peculiarity of the establishment is a covered yard situated in the centre of the building, and occupying a space of 18,000 square feet for winter recreations. When the weather is favourable, the pupils are turned into an open ground of 27,000 square feet. A portico for gymnastics has been erected in the winter grounds. To each studio is annexed a small museum, so that pupils may have constantly at their disposal the principal objects or models which are described in the course of the lectures given by the teachers. The school is intended for 800 pupils, but only 500 have been admitted, a part of the work being yet unfinished.

THE *Tarbes Observeur* states that a strong earthquake was felt at Bagnères de Bizarre (Hautes Pyrenees) on Friday, October 6, at five in the morning. The water of Salies, a thermal spring in the vicinity, which generally flows at 59° F., had its temperature suddenly altered to 72°, owing to the subterranean action. A few hours afterwards the same commotion was felt by General Nansouty, who has taken his post as observer on the Pic du Midi. The duration was three seconds, and direction south by north.

ON September 22, an earthquake motion was felt at Corleone, near Palermo, and from that time to September 27, seismic commotions were almost continuous. Great damage has been done to a large number of houses, and the inhabitants desert the city every night and encamp in the vicinity; cold is becoming intense during the now long nights. Some are said to have turned insane.

MESSRS. C. G. MAYNARD, of Newtonville, Massachusetts, and W. F. Parker, of West Meriden, Connecticut, are about to undertake an investigation of the natural history of the Bahama Islands, which promises to be of great interest to science in view of the fact that, with the exception of the examination made by Dr. Henry Bryant, of Boston, U.S., but little has been done in this respect since the time of Catesby, whose work was published nearly 150 years ago. These gentlemen propose to fit out a yacht in Boston, suitably equipped and provisioned, and send her to the Gulf of Mexico, there to embark some time in the present month, and to make a minute investigation of the natural history of each island, obtaining specimens of its land fauna and of the inhabitants of the waters along their shores. They will be accompanied by several assistants, and hope to make very large collections of all kinds. Dr. Lewis E. Sturtevant, of Boston, will accompany the expedition for the purpose especially of assisting Mr. Maynard in making drawings and dissections on the spot of the various animals.

A NAVAL testimonial will be presented to Commander V. L. Cameron, R.N., C.B., at the Royal United Service Institution, on Saturday, at 3 o'clock. Admiral Sir G. P. Sartorius will preside.

PROF. W. K. PARKER, F.R.S., and Mr. G. T. Bettany, B.A., of Caius College, Cambridge, are preparing a work on the Morphology of the Skull, in which for the first time will be brought together for comparison descriptions of the remarkable succession of modification through which the skull passes in development in the principal types of vertebrated animals; the forms illustrated will be the sharks and rays, the salmon, the axolotl, the frog, the snake, the fowl, and the pig. A special value will attach to the work inasmuch as it will record many corrections of facts and important modifications of view since the publication of Prof. Parker's elaborate papers in the *Transactions* of various societies, and will also include many observations yet unpublished. A simple description of each form at successive stages will be followed by a chapter dealing with theoretical questions, and summarising the results of study. The work will be illustrated by a large number of woodcuts, and will be published by Messrs. Macmillan.

THE scintillation of stars, and its close connection with changes of weather, has, as is known, much interested Humboldt, Arago, Kaemtz, Secchi, and many others; and recently it has also been the subject of valuable spectroscopic researches by M. Respighi. M. Montigny, who some time ago investigated scintillation in relation to the special characteristics of the light of different stars, publishes in the *Bulletin* of the Belgian Academy, 1876, No. 8, an elaborate report upon his researches into the connection existing between scintillation and various meteorological elements. The chief results arrived at after a discussion of 1,820 observations made on 230 days on 70 different stars, are as follow:—The intensity of scintillation (measured by a special apparatus, the scintillomètre) increases invariably with the occurrence or approach of rainy weather, and with the increase of tension of vapour in the air on one side, and the increase of pressure and decrease of temperature on the other; the influence of the two former factors being far more sensible than the combined influence of the two latter. The scintillation, which is on an average stronger during winter than during summer, increases with the arrival of moist weather at all seasons. It increases also not only on rainy days, but one or two days before, decreasing immediately after the rain has ceased. Moreover, the intensity of scintillation increases during strong winds, and with the approach of barometric depressions, or *bourrasques*, the increase being most pronounced when the depression passes near to the observer. It then largely exceeds the average increase corresponding to rainy days, and the influence of great movements in the atmosphere totally counteracts the contrary influence of a lowering of pressure. M. Montigny is thus correct in saying that a continued investigation of scintillation would be of great service, not only for the prevision of weather, but also for the general study of meteorology, affording a very useful means for the exploration of the higher regions of the atmosphere.

THE additions to the Zoological Society's Gardens during the past week include a Chacma Baboon (*Cynocephalus porcarius*) from South Africa, presented by Mr. Henry S. Wright; a Macaque Monkey (*Macacus cynomolgus*) from India, presented by Mr. H. Jones; a Little Grebe (*Podiceps minor*), European, presented by Mrs. Johnson; two Snowy Owls (*Nyctea nivea*), European, presented by Mr. L. W. Gardiner; nine Red-bellied Newts (*Triton alpestris*) from Tyrol, presented by Mr. P. L. Sclater, F.R.S.; a Tamandua Ant-eater (*Tamandua tetradactyla*) from South America, purchased: an Ocelot (*Felis pardalis*) from America, two Indian Cobras (*Naia tripudians*) from India, deposited; a Geoffroy's Dove (*Peristera geoffroii*) bred in the Gardens.

SCIENTIFIC SERIALS

Journal of the Chemical Society, July, 1876.—Mr. Thomas Carnelley, B.Sc., communicates the results of investigations recently made by him, on the action of water and of various saline solutions on copper. Mr. Carnelley has found that distilled water dissolves an appreciable amount of copper, on standing in contact with the metal even for the comparatively short space of an hour.—Mr. M. M. Pattison Muir, F.R.S.E., gives the second part of a paper on certain bismuth compounds. There are also two communications from Dr. Thudicum's Physiological Laboratory. The first is by Dr. Thudicum and C. T. Kingzett, on glycerophosphoric acid and its salts, as obtained from the phosphorised constituents of the brain. The second is by Dr. Thudicum, on some reactions of biliverdin. There are besides a note on the occurrence of benzene in rosin light oils, by Mr. Watson Smith, F.C.S., and a second paper by the same gentleman on a new method of preparing diphenyl and isodinaphthyl, and on the action at a high temperature, of metallic chlorides on certain hydrocarbons.

Gazzetta Chimica Italiana, Fasc. v. and vi.—The following papers comprise the contents of this number:—The inactive amylic alcohol of fermentation, by L. Balbiano.—An alkaloid which they found in spoiled Indian corn and in stale maize bread, by T. Brugnatelli and E. Zenoni. The authors consider this alkaloid to be the cause of "pellagra," a disease which commits great ravages in Lombardy.—Concerning a series of compounds derived from ammonaldehyde, by R. Schiff.—On gelatine, considered especially as regards its reducing agency, by G. Bizio.—On the emission of nascent hydrogen from vegetables, by G. Pollacci.—G. Scurati Manzoni contributes two papers; the first, on the action of certain reagents upon the principal organic colouring matters, is accompanied with extensive tables, which contain much valuable information; the second treats of the employment of sodic hydrosulphite as a reagent in the analysis of the colours fixed upon tissues.—On the natural poison of the human body, by A. Moriggia.—Concerning the methods of preparing the iodides of potassium and sodium, and of potassic bromide, by P. Chiappe and O. Malesci.—Observations on a process for obtaining iodic acid, by causing chlorine to act upon iodine suspended in water, by G. Sodini.—On the precipitate of sulphur, by M. Sansoni and G. Cappellini.—A method for detecting the adulteration of plumbic iodide, by L. Alessandri and C. Conti.—A new reagent for the investigation and estimation of glucose, by A. Soldaini.

Memoria della Società degli Spettrocopisti Italiani, May, 1876.—Prof. Tacchini gives the statistics of solar eruptions observed at Palermo in 1872. In 134 days of observation fifty-two eruptions were seen—twenty-four on the eastern limb and twenty-eight on the western, and none apparently occur within 40° of either pole. There also appears a detailed statement by Prof. Tacchini of the positions on which magnesium was seen on the limb during the months of August, September, and October, 1875.—Observations of the partial eclipse of the sun on September 29, 1875, made at Padua by Dr. Abetti.—Spots and faculæ on the sun's limb, observed at Palermo; the lines seen bright in the spectrum of the jets are b^1 b^2 b^3 b^4, 1474, 4923, 5017, and sodium lines. A sheet showing the chromosphere on each day in August, 1874, accompanies this number.

June, 1876.—Observations of spots and faculæ made at Palermo in May, 1876, with a table showing the numbers of positions at which the *b* and 1874 line were visible at the limb.—Observations of solar protuberances from June 29 to December 11, 1875, showing the number in each 10° of the sun's circumference, their heights, and area.—A note by Father Secchi on the change of position of the lines in the spectra of stars caused by their movement in space. In his experiments the author placed the vacuum tube for comparison in front of the object-glass, and he and his assistants found the stellar and tube lines could be made to change places by the motion of the telescope, and that the results by this method are not trustworthy. The author then gives a list of stars with their motions as given by Huggins, Greenwich, Secchi, and Vogel, showing a great discrepancy between the observers.—On the observation of the zodiacal light, made by Rev. Geo. Jones, from April, 1853 to April, 1855, by A. Serpieri. About thirty-nine observations with the lat. and long. of the place of the observer appear, together with other tables of the positions of the light, and a lengthy paper of remarks on the same. Drawings of the chromosphere for September, October, and November, 1874, accompany the number.

July, 1876, commences with a continuation of A. Serpieri's paper on the observation of the zodiacal light, by G. Jones.—Father Secchi contributes a second note on the change of position of the lines in stellar spectra due to the motion of the stars. The author in this, as in the last note, throws doubt on the reliability of the method in practice.—Observations of solar protuberances made during the first half of the present year at Rome. This consists of a table showing the number of prominences seen on each 10° of solar circumference, the height, size, and area of the prominences, and the extension of faculæ.—Spectroscopic and direct observations made at Palermo in the months of June and July. This paper includes a table showing the number of spots and faculæ on each day, with notes of the positions in which the *b* and 1474 lines were seen.

August, 1876, contains three papers by Prof. Ricco, the first of considerable length, on the absorption spectrum of water, with a plate showing the method of experiment and the spectrum of sea-water seen; the second on the spectral study of the green of plants; and the third on a new form of direct-vision spectroscope. In this new form the rays of light from the collimator pass through a prism of 60° in the ordinary way; they then fall on the side of a prism of 90°, having its base nearly in the same plane as that of the first; they are thus totally reflected internally from the base of the prism, and emerge from the other face parallel to their original position.

Reale Istituto Lombardo di Scienze e Lettere. Rendiconti. April—July.—A controversy which has been going on between M. Lombroso and a Committee of the Institute as to the poisonous properties of decayed maize and the disease of pellagra (in Northern Italy) is referred to here.—In the treatment of vines with sulphur for oidium, the destruction of the parasite has been shown to be due to formation of sulphydric acid. Whether the necessary hydrogen came from the oidium or from the grapes was uncertain, till it appeared that grapes that were quite free from the disease, gave sulphuretted hydrogen when sprinkled with sulphur. M. Selmi proved the development of nascent hydrogen from mould, and M. Polloni, having experimented on a number of plants, now sprinkled with sulphur, gave sulphydric acid. Most of it is produced in those parts in which the vegetation is most active (as the flowers and young buds). Plants with saccharine fruit (as the vine and mulberry) do not produce it in greater quantity than others. The author concludes from indirect experiments that all plants, in certain phases of their growth, and as the result of physiological acts, produce hydrogen *in the nascent state.*—A valuable paper of statistics and information regarding diphtheria in Milan, in the three years 1873, 1874, 1875, is contributed by Dr. Fellice Dell'Acqua. With reference to meteorological conditions, it is concluded that neither the maximum nor the minimum of air pressure, of temperature, of vapour tension and relative moisture, seemed to have the least influence in raising the number of cases of diphtheria. In winter and autumn the number of individuals taken ill was less, but the less number of deaths was in spring and summer.—M. Montegezza gives a careful analysis of the phenomena of expression of grief.—The course of storms is studied by M. Frisiani.—In biology we find notes on the nucleoli in the envelopes of some Protozoa, the mieline in Infusoria, the fresh-water Rhizopods of Lombardy.

Zeitschrift für Wissenschaftliche Zoologie, vol. xxvii., Part 2.—Prof. Selenka opens this number with a very interesting contribution to the embryology of the Holothurians, accompanied by beautiful figures. He describes the early stages of *Holothuria tubulosa* and *Cucumaria doliolum.* Among his conclusions may be mentioned the following:—The mesoderm arises entirely out of the entoderm; the mesoderm gives off motile cells from which the subcutaneous circular muscles, the primary alimentary canal, and parts of the internal skeleton are formed; the first-named species undergoes complete, the second incomplete metamorphosis; the transformation of Echinoderm larvæ can only be regarded as metamorphosis, not as alternation of generations.—Prof. Salensky, of Kasan, contributes a monograph of the development of *Salpa democratica*, from fecundation to the establishment of all the organs. At the conclusion of his paper he discusses the evidence which embryology affords as to the true position of the Salpæ. He shows that they lack the provisional organs as well as the mantle and foot, found in all mollusca. The cellulose test is in no way homologous with the molluscan mantle. The respiratory cavity is simply a differentiated part of the alimentary canal. The author considers the Vermes also to be nearer the Mollusca than the Salpæ, by reason of the provisional organs of many of their embryos. He emphasises the differences between the development of the Salpæ and the Ascidians, and, allowing that the viviparous reproduction of the Salpæ may account for much, he thinks that we are still considerably in the dark on the matter. He makes no allusion to the hypothesis that the Iunicata may be degenerate Vertebrates.—Ernst Zeller gives an account of the anatomy and life history of *Polystomum integerrimum*, a Nematode worm which inhabits the urinary bladder of frogs in its adult condition, and is found in the respiratory cavity of tadpoles during its larval state. Migration takes place through the alimentary canal of the host when the frog has undergone its metamorphosis; some individuals become sexual while in the respiratory cavity; these do not migrate, are short-lived, and do not appear to mature their eggs.

Gegenbaurs' Morphologisches Jahrbuch, vol. ii., Part 1.—Dr. von Ihering, of Göttingen, has an important article on Gasteropods, expounding the structure of the opisthobranchiate *Tethys leporina*, and making deductions equally unfavourable to the views of Prof. Huxley on morphology, and of Haeckel on phylogeny. He sees no ground for believing that the larval velum is the fore part of the epipodium, and expresses his astonishment that Prof. Huxley's paper on the morphology of the cephalous mollusca should be deemed authoritative. Haeckel's dogmatic system of phylogeny is stated to be not in accord with facts as regards the mollusca. The author believes that the prosobranchiate Gasteropods are derived from segmented worms, the opisthobranchiates from flat worms.—R. Hertwig endeavours to unify the differences in the structure, behaviour, and mode of formation of nuclei.—A brief contribution on the Cœlenterata, by G. v. Koch, is noticeable as describing a mesoderm in Halisarca.—Dr. W. Rolph has a long account of Amphioxus, increasing its abundant literature by nearly eighty pages, illustrated by three plates. He claims to have made it clear that its "body cavity," formed by the downgrowth of lateral lobes, is a respiratory cavity, homologous with the perivisceral chamber of ascidians, with the respiratory cavity of the tadpole, and the gill-cavity of symbranchii. He strongly objects to the identification of this chamber with the proper body-cavity of Vertebrata.

SOCIETIES AND ACADEMIES

London

Entomological Society, October 4.—Sir Sidney Smith Saunders, C.M.G., vice-president, in the chair.—M. Alfred Preudhomme de Borre, secretary of the Belgian Entomological Society, was elected a foreign member.—Mr. Bond exhibited varieties of *Hepialus humuli* and *Epunda lunulenta*, and also specimens of the new Tortrix (*Sericoris irriguana*), all taken near Loch Laggan by Mr. N. Cooke.—Mr. Forbes exhibited a weevil (evidently not indigenous to Britain) taken alive among some orchids at Highgate. Mr. Pascoe pronounced it to be a species of *Cholus*, a South American genus, for which he proposed the name of *C. Forbesii.*—Mr. W. Cole exhibited numerous bred specimens of *Eunomos angularia*, showing differences in coloration according as the larvæ had been fed on oak, hawthorn, lime, or lilac.—Mr. Enock exhibited microscopic slides containing some beautiful preparations of minute species of *Hymenoptera.*—Mr. Frederick Smith communicated "Descriptions of new species of Cryptoceridæ belonging to the genera *Cryptocerus*, *Meranoplus*, and *Cataulacus*," accompanied by figures of the several species. The author gave some interesting particulars relative to the habits of these insects, especially of *Meranoplus intrudens*, which constructs its formicarium in the thorns of a species of *Acacia.* These thorns were some 4 or 5 inches in length, and at a distance of about half an inch from the pointed end, a small round hole was made for ingress and egress to and from the nest. The thorns contained a kind of spongy pith in which the channels and chambers of the nest were constructed.—A catalogue of the British Hemiptera (Heteroptera and Homoptera) compiled by Messrs. J. W. Douglas and John Scott, published by the Society, was on the table.

Manchester

Literary and Philosophical Society, October 3.—Rev. William Gaskell in the chair.—On the action of water and saline solutions upon lead, Part 2, by M. M. Pattison Muir, F.R.S.E., Assistant Lecturer on Chemistry, Owens College. It appears to be shown by Mr. Muir's experiments that the solvent action of dilute saline solution upon lead tends to attain a maximum when large surfaces of liquid are exposed to the surrounding air, and when the volume of liquid is large in proportion to the surface of lead exposed. Further, that under these conditions, and in the presence of those salts which aid the action—especially nitrates and more especially ammonium nitrate—the quantity of lead dissolved increases in an increasing ratio with the time during which the action is allowed to proceed.

Paris

Academy of Sciences, October 9.—Vice-Admiral Paris in the chair. The following papers were read:—On the absorption of free nitrogen by the immediate principles of plants, under the influence of atmospheric electricity, by M. Berthelot. He used, this time, the weaker normal electric tension in the atmosphere. One closed tube of thin glass was inclosed in another. In the former was a roll of platinum joined to a conductor electrified by the atmosphere (at a height of 2 metres), while a thin sheet of tin round the outer tube was connected to earth. Into the annular space was (previously) introduced pure nitrogen or ordinary air, along with moist strips of blotting paper or a few drops of syrupy solution of dextrine. Twelve tube-systems, varying as described, were connected in position, from July 29 to October 5; the mean electric tension being that of 3½ Daniell elements, but oscillating from $+ 60 D$ to $- 180 D$. In all the tubes nitrogen was fixed by the organic matter—one to several millimetres per tube. In two cases green spots of microscopic algæ were found on the strips of moist paper in nitrogen tubes, showing a greater fixation of nitrogen in these. The experiments indicate an influence, little suspected hitherto, in vegetation.—Note on capillary affinity, by M. Chevreul. The name comprises all cases of union of a solid with a gas, a liquid, or a body held in solution by a liquid, where the solid retains its apparent form. The present note refers to action of massicot or calcined litharge on lime, strontium, or baryta water. The facts of capillary attraction are specially important for agriculture.—On the action which boric acid and the borates exert on plants, by M. Peligot. French beans watered once with solutions of borate of soda or potash, or boric acid, soon withered and died. He doubts if a substance so deleterious to plants can be quite innocuous to animals, where used to preserve meat.—On the reciprocal action of oxalic acid and monoatomic alcohols, by MM. Cahours and Demarçay. Where oxalic acid is caused to act on a mixture of propylic and isopropylic alcohols, propylic oxalate is produced almost exclusively. If the corresponding alcohols be extracted from this mixture of oxalates by saponification, a mixture rich in propylic alcohol is had, which, etherified anew by oxalic acid, furnishes oxalate of propyle almost pure. Hence we have a very simple mode of separation for two alcohols, which it would be almost impossible to separate by present processes.—On the stercoral anguillule, by M. Bavaz. This is the nematoid found in the stools of patients subject to diarrhœa of Cochin China. It is closely related to the *Rhabditis terricola*, Dujardin. It has been met with in the stomach, pancreatic duct, choledochus, hepatic ducts, and the walls of the gall-bladder, and in at least thirty patients.—On the flow of blood by tubes of small calibre (transpirability of Graham), by M. Haro. Heat accelerates the flow of defibrinated blood, and more so the richer the blood is in corpuscles; on serum heat acts much as on distilled water. Defibrinated blood which has had a current of CO_2 passed through it some time, and has then been filtered through fine linen, flows more slowly than the same blood made rutilant by decantation in free air. Sulphuric ether, containing no trace of alcohol, retards the flow of defibrinated blood, serum, and water. Chloroform retards the flow of water and serum, while it favours that of defibrinated blood. These facts have important physiological bearings.—Geological study of the prehistoric grottoes of Greaulx, in their relation to thermal waters, by M. Saubert. The latter are shown to be the cause of the former.—New observations on the Phylloxera of the oak, compared with that of the vine, by M. Balbiani. The new facts prove a great resemblance between the two in their habits and the phenomena of their reproduction.—Results obtained in treatment of phylloxerised vines with sulphide of carbon; measure in which this treatment should be applied, by M. Allies.—On the orders and classes of certain geometrical positions, by M. Halphen.—Determination of nitric acid in organic substances; chemical composition of certain gun-cottons, by MM. Champion and Pellet. Organic substances containing nitrogen are completely reduced, in certain conditions, by ferrous salts, and behave like nitrates. Hence, to determine the nitrogen, the authors adopt a modification of the process of Pelouze or Schlœsing. The composition assigned to gun-cotton corresponds to the pentanitrocellulose of Pelouze, $C_{24}H_{15}O_{15}5NO_5$, not trinitrocellulose (Abel).—On the limits between which fire-damp explodes, and on new properties of palladium, by M. Coquillion. The first limit, with *excess of fire-damp*, is 1 of fire-damp to 6 of air; the second, with *excess of air*, 1 of fire-damp to 16 of air. Palladium may with impunity be raised to a red heat in one of the most detonant mixtures known.—Note on the crystalline form of melinophane, by M. Bertrand.—The formula of seiches, by M. Forel.

Geneva

Society of Physics and Natural History, September 7.—M. Raoul Pictet described observations by him made on an intermittent fountain in the neighbourhood of Vichy, department of Allier. The case under consideration does not belong to the class which may be accounted for by the ordinary explanation of a siphon charging and discharging itself subterraneously. The fountain is here projected at intervals from an artificial vertical hole pierced in the ground to the depth of more than 100 metres. Other borings made in the locality tend to prove that there exists at that depth an underground collection of water, under pressure, which permanently maintains the level of the water in the tube at three or four feet below the level of the ground. At intervals occurring four or five times during the day, bubbles of gas begin to rise in the liquid; then, in the space of two seconds, the water rushes out in force, and for a certain time to a height of twelve metres. No siphon hypothesis can be applied to the locality; the phenomenon must be explained by a mechanical action of another kind. M. Pictet supposes the pressure of the subterranean gas to accumulate above the surface of the water underneath. In certain places the surface of the earth above may present hollows the upper part of which is at a higher level than the lower orifice of the tube. The pressure increasing, gas may then enter the tube, diminishing the pressure of the liquid, which causes equilibrium with the subterranean pressure, and effecting an emission of water which will last until that equilibrium is restored. M. Pictet has devised an apparatus to prove his theory and which completely illustrates it. (Vide *Archives des Sciences Physiques et Naturelles*, September, 1876.)

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