THURSDAY, MAY 21, 1874

ON THE ACTION OF THE HORSE

NO dynamical problem, whether physical or biological, can be considered to be based on a substantial foundation until some method has been applied to it, by which an accurate statical record can be obtained of the exact relations of all the forces which, at any given moment, operate in its production. The great preparations which are just completed for the observation of the approaching transit of Venus show how difficult it sometimes is to obtain the desired results; and the value attached to the production of photographic records of the phenomenon proves the importance of permanent registration.

The movement of the legs of quadrupeds during progression is a difficult problem, as is shown by the fact that there are still many contradictory opinions maintained by high authorities on the subject. The difficulty in this case depends on there being the four different limbs to be considered at the same time, which it is impossible to do without a considerable amount of practice. Till lately, those who have studied the point, as far as the horse is concerned, have relied on their sight or hearing, and have checked their results by the impression left on the ground by the animal's hoofs. The observational power of each individual author has therefore always been an element in the problem, and it is very difficult to estimate the magnitude of that part of it, in any given case, correctly. Within the last few years, however, a much improved method has been introduced, which, judging from the discussion that has been carried on in the *Times* with reference to the attitude of the horse in Miss Thompson's picture of the "Roll Call," is but little known by some who have very decided opinions on the movement of the legs.

In a work, published last year, entitled "La machine Animale," by the eminent French physiologist, M. E. J. Marey, of Paris, a full account will be found of an apparatus constructed by the author, by means of which the movements of each of the legs of the horse during progression are synchronously registered on a uniformly moving strip of paper, in such a way that the tracings obtained from all the four can be superposed and compared at the leisure of the experimenter, and the simultaneous positions of each leg accurately estimated. What is more, M. Marey has also introduced a beautiful writing language, as it may be termed, by means of which it is as easy as in music to transcribe the results obtained with his instrument and read them off in their proper sequence. A knowledge of this language makes it possible to refer any given position, such as that of the horse in the "Roll Call," to it; from which it may be compared with the results obtained by direct experiment. Such being the case, it is not difficult to transfer the vagueness of "opinion" into the certainty of fact, and settle a question once for all.

M. Marey's method is the following:—The record of the movement of each limb is obtained by the employment of small caoutchouc bags filled with air, similar in most respects to those with which he has obtained such valuable information on the movements of the heart. Two of these bags are connected together by an india-rubber tube; one is placed in contact with the foot, and the other with a small lever which writes on the recording paper. Each leg is provided with its pair of bags. Movements in either foot compress the bag connected with it, and this, by distending that at the other end of the tube, raises the lever. The levers write, one above the other, on a revolving drum held in the hand of the equestrian. We must refer our readers to the work itself if they desire to see the tracings obtained, mentioning that at the moment each foot touches the ground a sudden rise of the lever is the result, which is followed by an equally abrupt fall immediately it quits it.

Results even more satisfactory than those obtained by the use of the above-described air-bags might be obtained by adapting a simple electrical contact-maker and wirer to the shoes of the horses, which by acting on small electro-magnets would produce movements on levers which recorded similarly to those employed by M. Marey.

It will be necessary to give a short description of the mechanism of walking generally in order to explain that of the horse. Man in walking on level ground gives sufficient impulse to the body at each step to enable him to lift the one foot at the instant that the other touches the earth. Representing the time of contact of the right foot by a continuous line, that of the left foot by a superposed dotted line, and the exact period of the interval between the raising and lowering of either foot by the gap between the succeeding lines, the human walk on level ground would be drawn thus:—

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Whilst going uphill, however, there is a period during which both feet are on the ground together, which may be indicated thus:—

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Whilst, again, in running, there are periods, as we all know, during which both feet are off the ground together thus:—

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Turning to the case of the horse, and using the same method of illustration, we may employ the excellent comparison suggested by Dugès, in which he shows that any of its different steps may be imitated by two men, one behind the other. Now suppose these men, the hinder one with his hands on the shoulders of the one in front, to walk "in step," that is, with the right and left feet moving simultaneously; then, if their movements be recorded as above, with the steps of the hind man placed below those of him in front, the following would represent them:—

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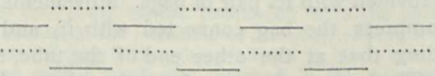
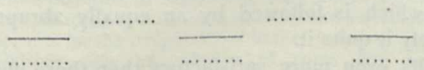
both would have their similar feet off and on the ground at the same time; and reverting to the horse, this formula, as it may be termed, which represents the legs of the same side off the ground together, is that of the "amble," a method of progression natural to the giraffe, but only acquired by special training in the horse.

Again, suppose that two men instead of walking "in step," do exactly the opposite, that is, place the *opposite* feet forward simultaneously; we then have the following formula :—



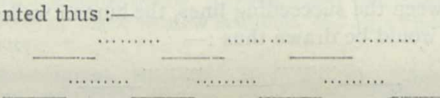
All will recognise this as the "trot" in the horse; although, as M. Marey has proved, there is always, in the true "trot," an interval between each of its two elements, during which all the feet are off the ground at once, thus :—



the upper of the last two formulæ, however, represents the walk of the elephant exactly.

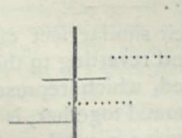
In the amble and the trot, therefore, each complete series of steps is formed of two parts which never overlap; it follows that the sounds produced by them are double also.

The walk of the horse is a phenomenon a little more difficult to realise at first sight. Again referring to the two men, suppose that they walk quite out of step, as it may be termed, in such a way that the front one *has raised* his right leg at the same moment that the hind one is *just raising* his, although they keep to the same number of steps. Such being the case the sequence of the steps would be *right front*, *left hind*, *left front*, and *right hind*, which is the order of succession in the horse, and may be represented thus :—



In this formula it is seen that at no time are there more than two feet on the ground at the same moment, and M. Marey states that in his numerous experiments such is always the case, except when a load is being taken down an incline in a wheeled vehicle, on which occasion three feet may be on the ground simultaneously. In the walk of the horse there are therefore four sounds produced in each complete series of steps, and these four are at equal or nearly equal intervals of time.

We are now in a position to judge of the accuracy of Miss Thompson's delineation of the "Roll Call horse," which is represented walking, with the left fore-foot fully raised from the ground, whilst the others are on it. The right fore-leg is nearly perpendicular and not bent; that is, about half-way between the commencement and the end of its step. The left hind-foot is somewhat in front of the perpendicular axis of the leg; that is, has just commenced its step; and the right hind foot, though on the ground, is on the point of leaving it. As the animal is walking, the lengths of the steps and of the intervals must be represented, as shown above, as of equal duration, and the following is its expression, the thick vertical line representing the moment at which the painting figures it :—



By comparing this with the formula of the walking horse, given above, it is evident that the representation is correct, except in a very slight point, which is that the right hind leg is on the ground, though just on the point of leaving it, whereas it ought to be just off it, because in walking there are never more than two legs on the ground at the same time. The general direction of the legs is quite correct. If the animal had been "ambling," the left hind-foot would have been off the ground, as well as the left fore. It is quite impossible to mistake the "walk" for the "trot," if their formulæ are compared and the positions at any given time worked out from them.

A. H. GARROD

CARPENTER'S "MENTAL PHYSIOLOGY"

Principles of Mental Physiology. By W. B. Carpenter, M.D., F.R.S. (Henry S. King & Co.)

THE title of the volume before us shows that its author is one of those philosophers—happily, an increasing number—who refuse to treat the phenomena of mind as though they were in no way connected with the body through which they find their expression. Mental Physiology is a comparatively new science, and does not date further backward than the days of Hartley. Before his time, and to some extent since, Physiology has been treated from what—to employ a word too often pressed into the service of a somewhat hazy idea—may be called the metaphysical point of view. The phenomena of mind have been abstracted from all their surroundings, and have been analysed by themselves, and the result has naturally been that we have been left but little wiser than before. Dr. Carpenter rejects this method, and bases his Psychology on the construction and working of the nervous system. But while shunning the metaphysical treatment of the subject, he does not adopt the other extreme, the doctrine, we mean, of the thorough materialist, who regards all mental phenomena without exception as the outcome of previous physical causes, which necessarily produce certain results. He steers a middle course, inasmuch as, while he advances the theory "of the dependence of the Automatic activity of the mind upon conditions which bring it within the nexus of Physical Causation," yet he believes in "an independent power, controlling and directing that activity, which we call Will."

This doctrine of the independence of the Will is the distinguishing characteristic of Dr. Carpenter's philosophy in the book before us; it runs through the entire work as the one grand exception among a series of physical sequences, interdependent, and standing to each other in the relation of cause and effect, of antecedent and consequent. Yet, even to a mind which is not "trammelled by system," this splendid anomaly may seem strange and surprising, though the prevalence of the belief in a Free Will, even among scientific thinkers, need cause no wonder, so long as the ethical bias is not rigidly excluded from psychological speculation. It is the meritorious timidity of the moral side of human nature which says, "whatever else may be under laws of necessity, the Will at least is free and independent, for the alternative doctrine deprives all actions of their moral value, and reduces man to the level of a mere machine."

It is clear that Dr. Carpenter is not satisfied with the doctrine of the so-called necessarian school indeed, he quotes Mr. Mill's Autobiography in his preface to show that the great necessarian himself wavered in his belief. He clearly thinks the explanation of human conduct offered by those who reject the theory of the independence, or rather the self-dependence, of the Will, inadequate. They would say that the unconscious operation of causes proceeds independently of the conscious conviction of the individual; that however much we may think that of two lines of conduct before us, either is equally possible for the human Will, yet, as a fact, we invariably follow the one to the exclusion of the other; the result, as it were, proves the cause, the apparent ultimate choice is the real physical consequent of antecedents engrafted in our nature, and acting in an invariable sequence, though it is true, as shown by Mr. Mill in the sixth book of his Logic, that Science is not sufficiently advanced to enable us to predict successfully the course of human action in any case, owing to the much greater complexity of the influences which operate in determining sociological phenomena when compared with other forms of activity. The necessarian philosopher would say that the operation of the Will is really nothing more than the force of the stronger motive asserting itself. Dr. Carpenter, and with him is the majority of mankind, says that the Will itself determines from within us which motive shall be the preponderating one.

But the chief merit of Dr. Carpenter's book lies, as we have said, in the explanation of the nexus which binds together the physical and the psychical elements in human nature. The well-known authority of what he says on such a subject constitutes the main value of his work. It is not too often that a great physiologist has turned his attention to mental phenomena, and we therefore welcome all the more gratefully any addition to the number of those who base their psychology on an exhaustive analysis of the functions and modes of action of the nervous system. In the first and second chapters of his book—the backbone, if we may so call it, of the work—Dr. Carpenter unravels carefully and exhaustively, step by step, all the interdependences of the nervous system and the psychical states. Without entering on all the mysteries of nervous ganglia and afferent and motor fibres, or the physiological comparison of Articulata and Vertebrata, we would say generally that Dr. Carpenter divides bodily movements in man into three classes:—(1) The primarily automatic; (2) the secondarily automatic; and (3) the volitional. Of these the first two "are performed in respondence to an internal prompting of which we may or may not be conscious, and are not dependent on any preformed intention, being executed 'mechanically'; while the last are called forth by a distinct effort of Will, and are directed to the execution of a definite purpose." But though thus clearly laying down the doctrine of the self-determining power of the Will, the author somewhat qualifies it afterwards, when he says that "even in the most purely Volitional movements the Will does not *directly* produce the result, but plays, as it were, upon the Automatic apparatus by which the requisite nervo-muscular combination is brought into action."

The conclusion at which our author arrives as to the general relations of mind and body is, in his own neatly-expressed words, "that the actions of our minds, in so far as they are carried on without any interference from our Will, may be considered as 'Functions of the Brain.'" These Functions of the Brain and of the Nervous System which supplies the brain with the materials which it works up into sensations and ideas, are lucidly and exhaustively expounded in the second and longest chapter of the work, in which the element of pure physiology preponderates, and into which we do not intend to enter, as no short summary of it can fairly represent its contents. Suffice it to say that in this part of the book Dr. Carpenter shows that the amount of intelligence (not instinct) shown by an animal is in a direct ratio to the relative size of the cerebrum and the sensorium, which latter organ in man is nearly eclipsed by the superimposed cerebral hemispheres, "the instrument of our psychical or inner life;" that the cerebrum is not concerned in the ordinary performance of our automatic movements, though in many cases it exercises control over them; its power, however, does not extend so far as to enable it to interfere with "the nervous system of organic life," or sympathetic system. The ruling monarch here at last meets with constitutional checks. It can exert no modifying influence on the "nutritive operations;" they, together with the rest of the sympathetic system, would rather seem to obey another power when they obey at all, the power, namely, of the emotions, which so often rebel against the Will, being, so to speak, the insurrectionary element which breaks in upon the dignified controlling influence of that thinking, purposeful, though sometimes eccentric monarch.

It is impossible in the short limits of a review to enter into the discussion of the part played by Attention, Sensation, Perception, and other physiological conditions in the production of mental results. These are all minutely treated of by our author, who carries us on in an easy progress from one to another with enviable clearness.

In treating of the succession of ideas Dr. Carpenter follows the doctrines of Prof. Bain in relation to the Laws of Association, and acknowledges the debt he owes to that most conscientious philosopher. All students of Prof. Bain's works on Mental Science are already familiar with the Laws of Contiguity and Similarity as explaining the principles of association of ideas, and we need not dwell further on them. The section which deals with Ideo-motor action is very interesting as leading us into the region of the marvellous. Ideo-motor action may be defined to be "the direct manifestations of ideational states, excited to a certain measure of intensity, or, in physiological language, reflex actions of the cerebrum." It is in this definition that we find the true key to the phenomena of table-turning and spirit-rapping, when practised by those who bring no dishonest arts to bear in their experiments. From this definition we should deductively infer that the revelations which reward those who take part in such experiments must be, as is in fact the case, in spite of assertions to the contrary, revelations of some matter known to at least one of the party engaged in the *séance*, whose mental activity and the play of whose ideas, apart from any exercise of Will, may influence the muscular movements *directly* and the more easily, inasmuch as the strained state of the hands on such occasions, after being stretched out for several minutes, renders them the easy and unresisting instruments of the ideational state, intensified, as it is, by the circumstances which surround it. So independent of volition is the influence of the ideational state in these cases, that it often operates in opposition to the dictates of the Will, and the writer has himself seen, more than once, answers extorted, as it were, from a member of a *séance*, unwillingly on his part, simply in consequence of his own highly-strained ideational condition conveying a knowledge through his muscles to those who sat with him at the innocent and obedient piece of furniture. Dr. Carpenter also shows that under this head of Ideo-motor action may be ranged "all those actions performed by us in our ordinary course of life," such as the use of language to express our thoughts, which requires no separate volitional effort, at all events when once we have entered on a train of speech.

But though giving up so large a field of human life to the non-volitional activity, Dr. Carpenter still keeps the Will in view, as a sort of abstract entity, as a "suppositítius," or reserve champion sitting in wait, ready to step in if occasion should call. "The dominant Idea *determines* these movements, the Will simply *permitting* them."

We can give in a few words a summary of Dr. Carpenter's theory of the relation of the Emotions to the Will. He begins by saying that "the Will has no *direct* power over the emotional sensibility," it can only operate to withdraw the attention from the emotional state and fix it determinately upon some other object. Again, the Will "can exert itself in preventing the *expression* of the exerted feelings in action" by suppressing the muscular exhibition of our emotional states; and again, "where the Emotion is not a mere *passion*, but is a state of *feeling* connected with some definite *idea*, the power of the Will is most effectually exerted in withdrawing the mind from the influence of that idea, by *fixing the attention upon some other*"—the power of self-control extends itself from our *impulses* to the habitual *succession of the thoughts.*

We had already learnt our author's views on the relation of the Will to mental and bodily action, but in the middle of his treatise we come upon a full and careful amplification of his opinions on this head, developing his theory of the influence of the Will on the formation of beliefs and on the conduct. We cannot do better than give in his own words Dr. Carpenter's doctrine on the latter head:—

"To carry into *action* the volitional determination, to give to the 'I will' its practical effect, something more is usually needed than the mere preponderance of motives. The idea of *the thing to be done* (which we have seen to be the necessary antecedent of all volitional action) may indeed be so decided and forcible, when once fully adopted, as of itself to produce a degree of nervous tension that serves to call forth respondent muscular movements, as in the purely ideo-motor form of action. But in general a distinct exertion of the Will is needed to give to the ideational state the energy requisite to call forth the action that expresses it, and this is especially the case where either some powerfully opposing motive diminishes the force of the preponderance, or a state of fatigue causes the bodily mechanism to be less easily called into action."

Hitherto we have been dealing with what the author calls "General Physiology;" we come now to the other division of the work, on "Special Physiology," and the transition is marked by a change of matter and style. We feel that, in reading this latter portion of the book we are being rewarded for the care which is necessary to the mastery of the deeper and more valuable philosophy of the earlier chapters. We have got—we do not speak disrespectfully—out of school into the playground, and we revel in the contemplation of the "morbid conditions" of the mind, illustrated as they are by numerous relevant anecdotes. Mesmerism, somnambulism, and dreaming are all subjects which attract and entertain, especially when treated of by a scientific pen. But we feel that this portion of the work does not call for special criticism so much as what we have already gone through. "Morbid conditions" are very valuable as throwing light on the operation of normal and healthy conditions, but happily the epithet "morbid" is interchangeable with the epithet "exceptional," and therefore we think that the morbid does not require such close treatment as the normal.

Dr. Carpenter winds up his work with a chapter on Mind and Will in Nature, and in it brings to a poetical conclusion what he has so carefully and exhaustively unravelled in the preceding pages.

ANDRÉ AND RAYET'S "PRACTICAL ASTRONOMY"

L'Astronomie pratique et les Observatoires en Europe et en Amerique. Par C. André et G. Rayet. I[e] partie, Angleterre. (Paris: Gauthier-Villars. 1874.)

THIS little unpretending volume is of considerable importance. Not only is it the commencement of a series which is intended to include the history of practical astronomy throughout the civilised world, but independently of this, it has claims to notice which are not to be measured by its limited dimensions.

The wide outspreading in the present day of a taste for astronomical observation would lead us to regard with favour anything tending to increase our knowledge of what has been and is being done, especially when it is set before us in so pleasing a form; and we cannot but admit that our neighbours have in this respect got the start of us. Notwithstanding all our efforts to render Science generally intelligible and acceptable, we have not yet succeeded in bringing out such attractive little manuals as proceed from the presses of MM. Gauthier-Villars and Hachette. Our larger and more elaborate treatises may well bear a comparison with anything of a similar calibre produced elsewhere; but in familiar, inexpensive, tasteful manuals, the light artillery, so to speak, of the scientific campaign, we must own ourselves fairly beaten by our nearest neighbours, who have set us a worthy example. We cannot, happily, and if we could we would not, say in this instance, *fas est et ab hoste doceri.* There was a time when such a remark would have been thought appropriate, but "nous avons changé tout celà;" and if such a thoroughly ill-natured and reprehensible observation were to be attempted now, it would meet its ample refutation in this work, which adds to its other merits the charm of courteous and kindly feeling. Next to the cordial abandonment of individual hostility, or the loving, tender reconciliation of alienated friendship, what can be more pleasing than the abatement of national antipathies and the softening down of those asperities which have but too deeply marked the intercourse of different branches of the human race? That nations should think or feel exactly in unison is no more to be expected than that individuals of the same family should possess identical tastes and habits; but as in the smaller, so in the larger groups, these distinctive characters may and ought to exist apart from every unkind jealousy or envious bitterness. There had been far too much of this in past days, and we hail with pleasure the appearance of this friendly book, which has evidently been drawn up in a truly kind and genial spirit.

If it puts us somewhat to shame, that the Assistant-Astronomers of the Paris Observatory should be telling us what goes on at our own doors, we have only ourselves to thank for the omission, and them for the way in which they have supplied it. The plan they have adopted is an excellent one; and as to its execution there is very much to praise. A history of English observatories and their work could not be otherwise than somewhat unequal in its execution; it would probably be so to some extent even in native hands; to a foreigner, who must, generally speaking, depend upon communicated information, the difficulty would be insuperable; and to this cause we may evidently refer the omission of some finely appointed private observatories, such as those of the Rev. H. C. Key, with its 18-inch silvered speculum, of Mr. Bird, the Rev. E. L. Berthon, Capt. Noble, Mr. Neison, Mr. Barnes, and many others. It is, in fact, in these private "telescope-houses" that England is so rich, as was formerly remarked in substance to the present writer by M. Léon Foucault, and it is through their work that much of the physical astronomy of the day has been advanced to its present position. But this is exactly what would escape the notice of any but ourselves, and even the generality of ourselves; and in this respect there is, of course, a good deal of deficiency in the work which cannot well be blamed. But great pains have evidently been taken to insure correctness, and to impart knowledge which to many among ourselves will have all the interest of novelty; and this has been done, for the most part, as far as we can judge, in a very satisfactory way.

The French language is now so generally understood among us that a translation is perhaps not required; but should it be undertaken, or should the authors, as we hope, be encouraged to send forth another impression, we would request permission to offer a few suggestions. The Bedford Catalogue, which has had so marked an influence on English astronomy, would well come in for a share of notice: the names of the opticians, whose work is described—which are seldom given, and perhaps not always correctly—might be supplied with advantage; several orthographical slips, and one considerable error in the little map, might be rectified. With these improvements, and some difference in the arrangement and appropriation of the very pretty illustrations, this charming little volume, even now greatly to be commended, would meet our expectations in every way. T. W. W.

LETTERS TO THE EDITOR

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

Quantitative Relations of Cause and Effect

AFTER Mr. Spencer has implied that he will not himself continue the controversy further, Mr. Hayward, in his last letter, has confused the issues by misstating Mr. Spencer's position. In the circumstances, perhaps, it will not be improper for me, as one familiar with Mr. Spencer's psychological doctrines, to rectify Mr. Hayward's error and explain that which he misapprehends.

The cue may be taken from an experience described in Mr. Spencer's "Principles of Psychology" (§ 468, note), where it is shown that when with one hand we pull the other, we have in the feeling of tension produced in the limb pulled a measure of the reaction that is equivalent to the action of the other limb. Both terms of the relation of cause and effect are in this case present to consciousness as muscular tensions, which are our symbols of forces in general. While no motion is produced they are felt to be equal, so far as the sensations can serve to measure equality; and when excess of tension is fel tin the one arm, motion is experienced in the other. Here, as in the examples about to be given, the relation between cause and effect, though numerically indefinite, is definite in the respect that every additional increment of cause produces an additional increment of effect; and it is out of this and similar experiences that the idea of the relation of proportionality grows and becomes organic

A child, when biting its food, discovers that the harder he bites the deeper is the indentation; in other words, that the more force applied, the greater the effect. If he tears an object with his teeth, he finds that the more he pulls the more the thing yields. Let him press against something soft, as his own person, or his clothes, or a lump of clay, and he sees that the part or object pressed yields little or much, according to the amount of the muscular strain. He can bend a stick, the more completely the more force he applies. Any elastic object, as a piece of india-rubber, or a catapult, can be stretched the farther the harder he pulls. If he tries to push a small body, there is little resistance and it is easy to move; but he finds that a big body presents greater resistance and is harder to move. The experience is precisely similar if he attempts to lift a big body and a little one; or if he raises a limb, with or without any object attached to it. He throws a stone: if it is light, little exertion propels it a considerable distance; if very heavy, great exertion only a short distance. So, also, if he jumps, a slight effort raises him to a short height, a greater effort to a greater height. By blowing with his mouth he sees that he can move small objects, or the surface of his morning's milk, gently or violently according as the blast is weak or strong. And it is the same with sounds: with a slight strain on the vocal organs he produces a murmur; with great strain he can raise a shout.

The experiences these propositions record all implicate the same consciousness—the notion of proportionality between force applied and result produced; and it is out of this latent consciousness that the axiom of the perfect quantitative equivalence of the relations between cause and effect is evolved. To show how rigorous, how irreversible, this consciousness becomes, take a boy and suggest to him the following statements:—Can he not break a string he has, by pulling? tell him to double it, and then he will break it. He cannot bend or break a particular stick: let him make less effort and he will succeed. He is unable to raise a heavy weight: tell him he errs by using too much force. He can't push over a small chest: he will find it easier to upset a larger one. By blowing hard he cannot move a given object: if he blows lightly he will move it. By great exertion he cannot make himself audible at a distance: but he will make himself heard with less exertion at a gr ater distance. Tell him to do all or any of these, and of course he fails. The propositions are unthinkable, and their unthinkableness shows that the consciousness which yields them is irreversible. These, then, are preconceptions, properly so called, which have grown unconsciously out of the earliest experiences, beginning with those of the sucking infant, are perpetually confirmed by fresh experiences, and have at last become organised in the mental structure.

It is not, however, any such experiences which Mr. Hayward adduces to exemplify organic preconceptions. He asserts that his "principal 'exemplification of unconsciously-formed preconceptions' was of Mr. Spencer's own choosing, namely, Newton's 'Second Law of Motion.'" This is an error: Mr. Spencer gave *no* examples of unconsciously-formed preconceptions. If Mr. Hayward will refer to Mr. Spencer's letter in NATURE, vol. ix. p. 462, he will find that Mr. Spencer has described the unconsciously-formed notion of the relation between cause and effect in general terms, and without example or illustration. In his last letter he simply named the relation between muscular tensions and their effects. Probably he expected Mr. Hayward to seize his meaning without any specific example.

The examples given by Mr. Spencer were examples of *consciously*-formed conceptions based on this *unconsciously*-formed preconception acquired during childhood and boyhood. Mr. Spencer gave three instances into which this preconception tacitly enters: one chemical, another relating to the melting of ice, and a third to the process of weighing. The last is the only one into which the relation between force and motion can be supposed to enter. But the consciously-formed conception that double weights will balance double masses, and so on, is not one into which there really enters any relation between force and motion. The notion of weighing is that of the equal forces of equal masses at the ends of equal levers. So long as there is motion, there cannot be equilibrium. The idea of motion is excluded when weighing is complete.

When Mr. Hayward says that Mr. Spencer has taken Newton's "Second Law of Motion" as an example of unconsciously-formed preconceptions, he utterly misapprehends Mr. Spencer's meaning. The "Second Law of Motion" is one of those developed *conceptions* derived from the organic *preconceptions* above described.

Mr. Spencer's argument appears to be briefly this:—1. There are numberless experiences unconsciously acquired and unconsciously accumulated during the early life of the individual (in harmony with the acquisitions of all ancestral individuals) which yield the preconception, long anteceding anything like conscious physical experiments, that physical causes and effects vary together quantitatively. This is gained from all orders of physical experiences, and forms a universal preconception respecting them, which the physicist or other man of Science brings with him to his experiments.

2. Mr. Spencer showed in three cases—chemical, physical, and mechanical—that this preconception, so brought, was tacitly involved in the conception which the experimenter drew from the results of his experiments.

3. Having indicated this universal preconception, and illustrated its presence in these special conceptions, Mr. Spencer goes on to say that it is involved also in the special conception of the relation between force and motion, as formulated in the "Second Law of Motion." He asserts that this is simply one case out of the numberless cases in which all these conscious-reasoned conclusions rest upon the unconsciously-formed conclusions that precede reasoning. Mr. Spencer alleges that as it has become impossible for a boy to think that by a smaller effort he can jump higher, and for a shopman to think that smaller weights will outbalance greater quantities, and for the physicist to think that he will get increased effects from diminished causes, so it is impossible to think that "alteration of motion" is not "proportional to the motive force impressed." And he maintains that this is, in fact, a latent implicatiion of unconsciously organised experiences just as much as those which the experimenter necessarily postulates.

I may add that if mathematics included in its range the connection between objective phenomena and the answering subjective states, this question would be one for mathematicians; but at present it is, as it seems to me, a question pertaining to the psychological basis of inductive logic. JAMES COLLIER

Bayswater, May 18

The Glacial Period

I THINK there are but few points in Mr. Belt's letter ("The Glacial Period," NATURE, vol. x. p. 25) to which Geologists who have devoted much attention to the ice action will not take exception. May I be allowed to call attention to one or two?

1. I do not believe that there is evidence, which anyone accustomed to glacier "spoor" would admit, of an extension of the ice-cap so far south as the Thames valley.

2. It is in the highest degree improbable that the shells on Moel Tryfaen should have been scooped out of the bed of the North Sea by moving ice and transported to their present position. Apart from the difficulties of a glacier thus walking so far up-hill, and of shells having escaped utter smashing in this uncomfortable mode of transport, Mr. Belt has forgotten that Wales was a centre from which radiated glaciers, and at one time an ice-sheet, which surely would have warded off from its own hills the northern intruder. What evidence is there that the ice-sheet ever followed its path? All that I know points to local glaciation.

3. Mr. Belt forgets that the various sea-marks are often at very different heights above the present water-level—as is so well shown in Scandinavia—and that no lowering of the water will explain this. The height of even 600 ft. which he claims is one that rests on many assumptions and but little confidence can be placed on the numerical results.

It would be easy to discuss many other questions which he raises, but this would occupy far too much space. My present purpose is not so much to do this, as to utter a protest against such a portentous development of a theory which has for some time past been assuming nightmare proportions.

St. John's College, Cambridge, May 19 T. G. BONNEY

Lakes with two Outfalls

IT is quite possible that I am wrong in my memory of the Nystuen watershed; and as Prof. Stanley Jevons examined the place critically, I can have no doubt that I am so. I passed merely as a traveller, and described what I had seen, from a memory, not specially sharpened by a knowledge of the importance of the point, at the time the observation was made. I know well what tricks one's memory plays under such circumstances, particularly when one has been rambling over many similar localities; and my letter indicated that I was in doubt as to the particular lake which gave the double outfall. I passed, too, just after much heavy rain, and it is possible that the boggy bottom which Mr. Jevons describes was temporarily converted into the lake, which deceived me. I may add, that both the guide who brought me over the mountains from Aardal, and the Skydsgudt who took me to Skogstad, confirmed the double outfall.

My object, however, in writing, was chiefly to draw attention to Norway, as offering an admirable field for the settlement of the controversy, without going to the wilds of America. If there be such phenomena, and I believe there are, they may assuredly be looked for in that land of hard granite rock, mountain plateaux, and innumerable watersheds of all sizes and varieties, and if the hundreds of educated Englishmen who go there every year be only impressed with the importance of accurate observations, the point may soon be settled.

Certainly I agree that Colonel Greenwood, who has kindly favoured me with a most interesting letter of advice, has done excellent service by his quite justifiable incredulity, and I shall myself be content to have made a mistake, if by it I shall be the cause of greater accuracy in others. W. B. THELWALL

27, Burghley Road

Glass Cells with Parallel Sides

I SEND you a brief description of a method I have recently employed for rapidly fitting up glass cells with parallel sides, believing that it may be of interest to your readers.

A piece of indiarubber tubing (or of solid rubber) bent into a semicircular form is placed between two equal-sized rectangular plates of glass, the ends of the tube terminating at the upper edges of the glass plates; the plates are then held together by passing two strong indiarubber rings over their ends. If the rings are of such a size as to exert the requisite compression a semicircular water-tight cell is thus obtained, which can be taken to pieces and cleansed with the greatest ease.

A trough so made served well to exhibit with an ordinary magic-lantern the experiments described on pp. 173 and 174 in Tyndall's "Heat a Mode of Motion," and smaller cells suitably fitted with platinum wires, and held in the wooden frame of an ordinary lantern-slide, enabled the galvanic decomposition of acidulated water and of saline solutions to be thrown upon a screen and thus rendered visible to a large audience.

Queenwood College, Hants. FRANK CLOWES

Brilliant Meteor

WHEN nearing Holyhead at 0.50 A.M. on the 19th inst. the most brilliant meteor I have ever seen passed slowly across the heavens. It formed near Antares, remained stationary for two or three seconds, and then slowly moved to the northward, disappearing in the Great Bear. Throughout, the soft green light showed every portion of the hull and rigging with as much distinctness as a number of pyrotechnic fires could have done. The shape was that of an elongated ellipse, slightly contracted at one end, with the major axis of the apparent diameter of the sun. A short time before it disappeared six sparks as large as Jupiter were discharged from the southern end, and I thought a crackling sound followed.

Celtic, May 20 WM. W. KIDDLE

THE U.S. ACADEMY OF SCIENCE

SESSION AT WASHINGTON

THE U.S. National Academy of Science held its meetings this year at the Smithsonian Institution, the venerable Prof. Henry, secretary of the Institution, presiding over the deliberations of the Academy. The session commenced on April 21 and lasted four days. By favour of the scientific editor of the *New York Tribune* we have obtained advanced reports. Our space permits us to give only the titles of the more important papers; but as Dr. Brown-Séquard's paper on the functions of the brain is of very great interest in reference to recent researches on the subject, we shall give a longish abstract of it.

Among the papers of importance were the following:—Dr. J. L. Le Conte read a paper On a classification of the *Rhynchophorous coleoptera*. Prof. Fairman Rogers described an automaton to play tit-tat-too, which he had constructed.

Prof. A. M. Mayer read three papers, one entitled "Suggestions as to the functions of the spiral scalæ of the Cochlea, leading to an hypothesis of the mechanism of audition." The second paper was headed "Abstract of a research in the determination of the law connecting the pitch of a sound with the duration of its residual sensation, and on the determination of the number of beats—throughout the range of musical sounds—which produce the most dissonant sensations; with applications of these laws to the fundamental facts of musical harmony, and to various phenomena in the physiology of audition." Prof. Mayer gave the particulars of a series of experiments by which it was ascertained what must be the frequency of successive sounds to have them blend indistinguishably together. The third described a series of experiments on the reflection of sound from flames and heated gases.

Prof. Simon Newcomb, the astronomer in charge of the Washington Observatory, gave a description of the preparations in America for the observation of the coming transit of Venus. These are most thorough and complete.

Prof. Wolcott Gibbs, of Harvard University, read a paper On metamerism in organic chemistry. Prof. Gibbs has discovered six metameric bodies, a seventh having been discovered by Prof. Erdmann.

Comparative velocity of light in air and in vacuo, by Prof. Stephen Alexander of Princeton College. This brief paper merely contained a few interesting suggestions on a small correction of the velocity of light as deduced from experiment.

In accordance with the undulatory theory the velocity of light must be less in atmospheric air than in vacuo, in the inverse ratio of the index of refraction of atmospheric air to 1; that is, as 1 to 1·000294. The velocity then as ascertained by experiment under the air should be increased by just about 0·000294 of itself to be equal to that in vacuo; *i.e.* to the extent, almost exactly, of 55 miles per second; a very small quantity indeed in comparison with the whole velocity of 185,000 miles per second; and yet, small as it is—and so small as to be below the limits of error of the experiments in question—it is yet very closely equal to three times the velocity of the earth in its orbit.

It is an outstanding excess, and no more, with which we often have to do, as, for example, in the measurement of temperature; but the scale on which those differences sometimes present themselves makes them, small as they may be in their original comparison, grand in comparison with ordinary standards. Prof. Alexander was not aware that anything has yet been put forward elsewhere on this subject.

Prof. Hayden gave a general account of the scientific explorations and survey in the West in which he has been engaged. With the results of these our readers are already pretty familiar.

In a paper On the laws of cyclones, by Prof. William Ferrel of the Coast Survey, the author gave a *résumé* of our knowledge on the subject and of some of the theories which have been advanced.

Dr. E. Bessels read a paper entitled "The History of Smith's Sound from a Geographical and Geological Point of View, and some other General Results of the *Polaris* Expedition." Dr. Bessels thinks that Smith's Sound must be regarded as the best of the three gateways to the pole. The land found between 81° and 82° seems to Dr. Bessels to be of great importance in demonstrating that Greenland has been separated from the continent in a south-north direction. Dr. Bessels stated several important facts bearing on the rising and sinking of the land on the Greenland coast.

Prof. Simon Newcomb gave a description of the great telescope at Washington; and a paper by Prof. S. Alexander of Princeton, N.J., On three of Jupiter's satellites, was read.

Prof. J. S. Newberry of Columbia College, New York, read a paper On Lower Silurian fossils. This was a memoir on the so-called land plants of the Lower Silurian in Ohio. Taking all the characters of these interesting fossils into consideration, Prof. Newberry is disposed to regard them as casts of the stems of fucoids.

The following papers were read by title only:—A memoir on the zodiacal light, by Prof. S. Alexander; On some points in Mallet's theory of vulcanicity, by Prof. E. W. Hilgard; The polarisation of the zodiacal light, by Prof. A. W. Wright. An exceedingly interesting and valuable paper on the mode of formation of the earth, its condition as to interior fluidity, and the probable limits within which it was reduced from a fluid state to its present condition, under the title of "A Criticism on the Contractional Hypothesis of the Earth's Surface Changes," was read by Capt. Clarence Dutton of the Ordnance Corps, U.S.A.

Dr. Brown-Séquard began his paper On the pretended localisation of the mental and the sensorial functions of the brain, by saying that the subject has been rendered more difficult by assumptions of physiologists upon insufficient data. Among the views which have been recently brought forward upon the localisation of nervous power in certain parts of the brain, there are two of importance: one relates to the seat of power actuating muscles, and the other is as to the seat of sensation for different nerves. In the latter particular, after noticing several exploded theories, some still pertinaciously adhered to by physicians, Dr. Brown-Séquard reviewed especially the assumption in respect to the seat of power for speech:—

"Let us consider the question of the locality of the intelligence of the brain. Most physiologists are agreed that this is the grey matter of the upper parts of the brain. But the method of communication is still open to research." (Here the lecturer went to the blackboard and drew a figure somewhat like a sheaf of wheat without a band around it; the stalks representing the nerves, the heads of wheat representing the cells.) "Now you may subtract from this, by disease or otherwise, say the upper third, and still you have the nerves and the nerve cells, and the processes can be carried on; but in the progress of such destruction downward there would eventually be reached a point where the functions of the brain could no longer exist. This view would explain the facts as we find them. But there is no case on record where the grey matter on both sides of the brain has been destroyed without the loss of intelligence, and we must regard the grey matter as the seat of the intelligence. But vast portions may be removed before the loss of intelligence becomes apparent. This I have myself tested and proved by vivisection of the lower animals.

"Now, in respect to the locality of the power of speech. It has been said that the loss of brain power to express ideas in speech was located in a certain part of the brain. This affection is called aphonia or aphasia. There are three modes of expressing ideas—by speech, by gesture, and by writing. It is with the first only that we are concerned. Some very bold theorists have tried to locate all these powers in a particular part of the brain. Let us confine ourselves to facts. Dr. Broca of Paris has advanced the view that a certain small portion of some of the convolutions of the brain holds the power of speech. I admit that facts seemed to favour this view. But we find that there is no relation between the degree of aphasia and the extent of the disease of that part, and there are cases where the destruction of those convolutions is very great, and the injury to speech very little. Secondly, we find that disease may have overtaken the anterior, the posterior, and the middle lobes of the brain, the particular convolution supposed to involve speech not being affected, and yet there is marked aphasia. Now, is some one of these lobes the locality of the power of speech? Such would be the reasoning of my opponents. We should be obliged to concede that in some persons the faculty of speech existed in one part of the brain, in some in another, in others another, and so on *ad infinitum*. This is a *reductio ad absurdum*.

"There is the case of the paralysis of the insane, where the grey matter may be diseased on both sides of the brain. In these cases the power of speech does not seem to be involved. There are cases of aphasia where the diseased person has had the power of speech restored during delirium. The speech is coherent though the sense may not be. It is evident, then, that the faculty of speech is not actually lost in such cases; and yet we find that the third frontal convolution is actually diseased in those aphasiacs who talk in their delirium. But the most decisive argument is found in the cases that I have seen, where the third frontal convolution, the alleged organ of speech, has been destroyed, and yet the patients have not lost the power of speech. Therefore the theory is itself destroyed. There are fifty cases on record to show that the question of right-handedness or left-handedness does not apply in the considerations." The lecturer here cited cases of Jacmet of Montpelier and Mr. Prescott-Hewitt of London. In the latter case the patient had suffered a destruction of that part of the brain for twenty years, and yet for twenty years had spoken.

"We shall now take up the question of the localisation of motion in certain parts of the brain. I am surprised at the avidity with which a certain series of facts has been accepted as proof of this theory in England. A very eminent man, of whom I should not like to say anything severe, my friend Prof. Carpenter, has accepted those views. I may say that all England has accepted them. Prof. Huxley, indeed, has written me, that he only accepted this view in part, but I cannot see how he can accept a part without accepting the whole, where even the part is incorrect. The famous experiments of Dr. Ferrier, of Guy's Hospital, must here be considered. As you will see, they are not, however, conclusive. By the application of galvanism to certain parts of the brain of animals, he produced certain movements. When we do not stop to think, this would seem to prove that there are in the brain certain centres of movement governing certain parts. But it is only a semblance. A part of the facts are taken for the whole. We should know all the series before we adopt the conclusions. Let us examine the other facts.

"It is perfectly well known that the cutting away of a large portion of the brain does not produce the least alteration of voluntary movement any where. Suppose that part of the brain, say the anterior lobe, being excited by galvanism, produces a movement in the anterior limb; now suppose that part of the brain is cut away, then the anterior limb should be paralysed, for its voluntary movement is gone. Admitting that the other half of the brain should supply the place of the missing part, let us take that away also; then certainly there should be a paralysis of the anterior limbs. But there is not. This should be sufficient to invalidate the conclusions of Dr. Ferrier. But there are abundant pathological facts of this nature proving the fact beyond question. And then there are the cases of recovery from paralysis. There is no such localisation of power as Dr. Ferrier has assumed. If galvanism be applied to the severed leg of the frog the leg will jump although there is no brain power in the question.

"What should have been done was to have cut the connection of parts, so that a general effect should not have been propagated throughout the brain by the application of galvanism to a part. This would be the *experimentum crucis*. My friend Dr. Dupré of Paris has made this experiment. I made it also, before he did, but he published his before mine. But there are many other facts almost equally impressive in their character which may be cited. We find many cases where the lesion of part of the brain produces paralysis on the same side of the body, and not on the opposite side, as in the majority of cases is the rule. There is a case recorded where a ball passed directly through the brain, and it produced paralysis on the right side, instead of the corresponding side." Here Dr. Brown-Séquard objected to having a certain class of brain affections named after him, stating that diseases should be named from their distinctive features, and not after physicians.

Dr. Brown-Séquard then applied a similar course of reasoning to the localisation of sensation in specific parts of the brain, concluding by stating that it is evident we cannot locate the centres of either sensation or motion in specific parts of the nervous system.

THE LONG PERUVIAN SKULL

I WISH to place before comparative anatomists and anthropologists a question which has been encumbered by some misleading inaccuracies, in a recent communication by Dr. J. Barnard Davis to the Anthropological Institute, ("On Ancient Peruvian Skulls" Journ. Anthropol. Inst., vol. iii., p. 94). So early as 1857, in communications to the British Association, and to the American Association for the Advancement of Science, I showed, in opposition to the views of Dr. Morton, and of all American ethnologists up to that date, that a dolichocephalic type of head is characteristic of certain widely diffused American races. At a later date I set forth, in "Prehistoric Man," my reasons for believing that this, which is now universally acknowledged as true in general, may be specifically asserted of the ancient Peruvians. This latter proposition Dr. Davis undertakes to refute; it is not a mere matter of personal controversy, but a question of some ethnical significance. As a Canadian, I lie outside of the charmed circles of home science and criticism, and only receive tardy news even of such communications as this, in which I have a personal interest.

Dr. Davis has not himself had an opportunity of examining the evidence on which my opinion was formed; and, in the communication above referred to, shows that he fails to appreciate its nature or true bearing. He says, Dr. Wilson's view, "which is that the dolichocephalic Peruvian skulls are of natural form, was combated in the 'Thesaurus Craniorum.' Since that book was printed, I have received ample and satisfactory evidence as to the truth of the proposition that the long skulls owe their quality to artificial means. By the politeness of Dr. J. Aitken Meigs, of Philadelphia, I have obtained two Peruvian skulls which at one period belonged to Dr. Morton's collection, as a specimen of each kind. One of these is brachycephalic, the other is dolichocephalic, but they both present distinct traces of artficial distortion. *This fact is conclusive.*" So says Dr. Davis. But conclusive of what? So far as I can see, it is simply conclusive as to the fact that both skulls have been artificially distorted. He then quotes Professor Wymann, of Boston, who, after an examination of the specimens referred to by me, settles the question thus summarily: "The upshot of the whole is, the crania do not confirm Dr. Wilson's statement. One of Dr. Wilson's points—in fact it is his chief point—is, that *skulls are natural because they are symmetrical;* and that it is next to impossible that a distorted skull should be other than unsymmetrical."

The thing I find most conclusive in all this is, that Dr. Davis and his correspondent both accredit me with inferences or opinions of their own, utterly inconsistent with my published views. So far am I from affirming "skulls are natural because they are symmetrical," that when my two critics have leisure to extend their reading to pp. 500-512 of the volume they refer to ("Prehistoric Man"), they will find many natural causes specified as tending to modify and distort the human skull. They will also find in the notes reference to papers in the *Canadian Journal*, and elsewhere, in which various aspects of this question have been repeatedly discussed. Dr. Davis has, I believe, received copies of all of those from myself; but, at any rate, there is one which can scarcely have escaped his attention—"On the Physical Characteristics of the Ancient and Modern Celt." It was published in the *Canadian Journal* in 1864, reprinted in the *Anthropological Journal* soon after, and became the subject of a good deal of reference in the famous copyright action of "Pike *v.* Nicholas." In this the explicit statement is repeated: "The normal human head may be assumed to present a perfect correspondence in its two hemispheres; but very slight investigation will suffice to convince the observer that *few living examples satisfy the requirements of such a theoretical standard.* Not only is inequality in the two sides of frequent occurrence, but *a perfectly symmetrical head is the exception rather than the rule.*" There is no possibility of mistaking the opinion thus expressed. It was published by me so long ago as 1862 (*Can. Journ.* vii. 414), and is repeated in substance in the very work from which Drs. Davis and Wymann profess to derive their absolutely contradictory dictum as "one of Dr. Wilson's points—in fact his chief point!"

But over and above all this, in the previous paper results derived from a careful study of eleven hundred and four English and French head-forms are set forth with this conclusion: "It thus appears that the tendency to unsymmetrical deformity is nearly as three to one; and that in the abnormal head the tendency towards excess of development towards the left is upwards of two to one." This tendency, it is further added, is more decidedly manifest in the brachycephalic than in the dolichocephalic head (*vid. Anthropol. Journ.* vol. iii. p. 82). The views thus repeatedly set forth, and supported by such proofs, are certainly not open to any charge of ambiguity. It is somewhat amusing, therefore, to find two such high authorities as Dr. Davis and his Boston correspondent summarising the whole, in this off-hand fashion, in a communication to a scientific body: "The upshot of the whole is," that, according to Dr. Wilson, "the skulls are natural because they are symmetrical, and that it is next to impossible that a distorted skull should be other than unsymmetrical."

By what process such opinions have been arrived at, and then accredited to me, I need not attempt to guess; but one thing unaccountably overlooked is the distinction on which I insist, between undesigned natural deformation, traceable to such simple causes as the one-sided pressure of the mother's breast, of the cradle-board, &c., and purposed modifications of the head, such as those practised at the present day among the Flatheads on the Columbia river. Three points on which I have insisted, not without evidence in their support, are: That the shape of the human head may not only be designedly altered by artificial means; but that it is much more frequently modified undesignedly, and rendered strikingly unsymmetrical, in infancy; while a third source, that of posthumous distortion, has also to be kept in view.

So far as to the general question. The specific one sought to be determined is the universality of a brachycephalic Peruvian type of head; or, as I have asserted, the occurrence of well-defined dolichocephalic heads in ancient Peruvian cemeteries. Dr. Davis informs the Anthropological Institute that my view was combated by him in his "Thesaurus Craniorum" (1867), and indeed it is with a view to the substantiation of "the criticisms of Dr. Wilson's statements in the 'Thesaurus,'" p. 246, that Dr. Wymann's "upshot of the whole" is produced. As one of the subscribers to Dr. Davis's valuable Catalogue, as well as a contributor to his collection of crania, I am familiar with the work, and with the pages specially set apart for my correction. I have had it, indeed, for years in my possession, without thinking that it needed refutation. I recommend any readers interested in the question to turn to the aforesaid p. 246, and read the curious narrative of Dr. Davis's conversion, in consequence of the receipt of a "skull next to unique in Europe," which belongs to "the long-headed race" of Peruvians, but yet is decidedly not long, or only long-headed "in a conventional sense," whatever that may mean.

I still believe it to be a fact, confirmed by my examination of examples referred to, that there is a well-defined dolichocephalic type of Peruvian cranium, although a brachycephalic type is the prevalent one. I have on three different occasions visited Philadelphia with the express object of studying the Morton collection there. One result has been to lead me to form a clear idea as to the source of Dr. Morton's later views. He had asserted the predominance of one uniform cranial type throughout the New World. "The long-headed Peruvians" were a disturbing element in this otherwise universal law. When therefore he turned to the examples in his own collection, and detected evidence of malformation by art in skulls which he had previously recognised as exceptions to his comprehensive theory, he welcomed the conclusion it suggested to his mind "that all these variously formed heads were originally of the same rounded shape." Dr. Davis informs us that he has obtained two Peruvian skulls formerly in Dr. Morton's collection, "a specimen of each kind," *i.e.* I presume, an occipitally flattened, and an elongated skull, both of the prevalent brachycephalic type. He has also the Titicaca skull already referred to, long, and yet not long, except "in a conventional sense." Possibly both Dr. Morton's and Dr. Davis's views are correct deductions from such premisses.

If a skull of the brachycephalic type, common to many American tribes (such as the Peruvian skull figured by Prof. Busk, vol. iii. pl. 7, "Journ. Anthropol. Inst."), is subjected to extreme depression of the frontal bone, with corresponding affection of the parieto-occipital region by the action of the cradle-board, such a form results as is shown in Fig. 78, p. 245, of Dr. Davis's "Thesaurus Craniorum." Examples of this are not rare. Here, if the length is measured from the projecting base of the frontal bone, immediately above the nasal suture, to the extreme posterior point, that will fall, not on the occipital bone, but nearly mid-way between the lamboidal and coronal sutures. Such a measurement is the actual extreme length of the modified skull; but if it is accepted as the true longitudinal diameter, without reference to the displacement of the points of measurement in the normal head, it is manifestly deceptive. It is, in fact, nearly equivalent to the substitution of the diagonal of a square for a diameter drawn parallel to its two sides. Such a skull, notwithstanding its actual length by measurement, is properly classed as brachycephalic. But take such a form as that which I have designated a "Peruvian dolichocephalic skull" ("Prehist. Man," 2nd ed. Fig. 50, p. 449). It is reproduced here; Fig. 1. Compare it with the above-cited example, in Dr. Davis's collection; or again compare the Peruvian child's dolichocephalic skull ("Prehist. Man," Fig 60, p. 451), also reproduced here, Fig. 3, with another juvenile skull, from the Peruvian cemetery of Santa, but of the brachycephalic type, as shown here, Fig. 2, reduced from Morton's "Crania Americana," pl. vii. The question is

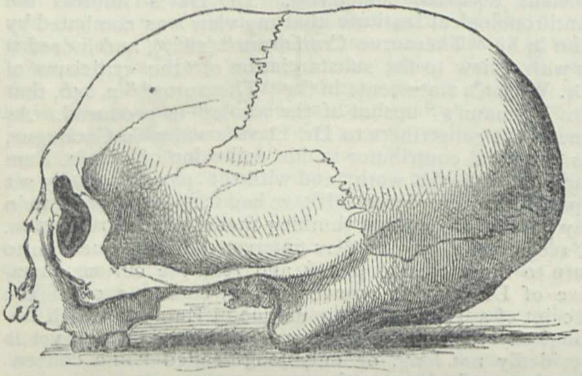


FIG. 1.—Peruvian Dolichocephalic Skull.

not, as Dr. Davis and Dr. Wymann would have it, whether the one is in its natural state, and the other artificially elongated? but whether it would be possible, by any elongation of the one, or abbreviation of the other, to reduce them to the same form? Compare the juvenile skull, Fig. 3, which is little, and probably not at all designedly, affected by art, with another of the same type, but purposely deformed by artificial means, Fig. 4. The same form is traceable in both, notwithstanding the modification of art. Both I conceive to be of the true dolichocephalic type; in contrast to the Santa skull, Fig. 2, which, whether or not affected by the parieto-occipital flattening so commonly resulting from the cradle-

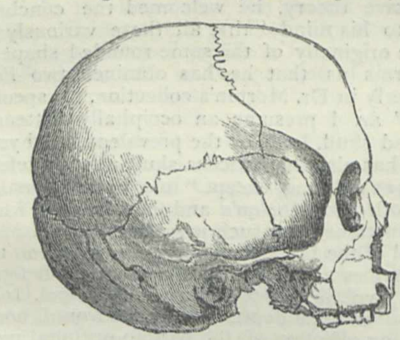


FIG. 2.—Peruvian Child's Skull, Santa.

board, is no less obviously of the brachycephalic type; and could not be transformed into the other.

The primary form of the skull, as determined, for example, by the relative proportion of the parietal bones, remains a factor to the last, however extreme may be the modifications superinduced by art. Only in the case of premature ossification of the sutures, consequent on the pressure applied in one direction, can this fail; though, no doubt in two approximate head-forms, the one only slightly dolichocephalic, and the other equally slightly brachycephalic, the original distinctive characteristics may escape observation in the modified skulls.

The question, then, turns mainly on this point—strangely ignored by Dr. Davis and his correspondent,—that a dolichocephalic and a brachycephalic skull are equally susceptible of distortion; but the same compression applied to the two types will beget different results;—will not, in any strongly marked example of either type, wholly efface the original character;—could not transform such a dolichocephalic skull as Fig. 1, into anything analogous to the elongated brachycephalic skull, Fig. 78, of Dr. Davis's "Thesaurus."

I have necessarily left untouched various collateral points, for want of space; but enough has been said to show that what strikes Dr. Wymann as so "curious," and manifestly in his estimation so "conclusive" against me,

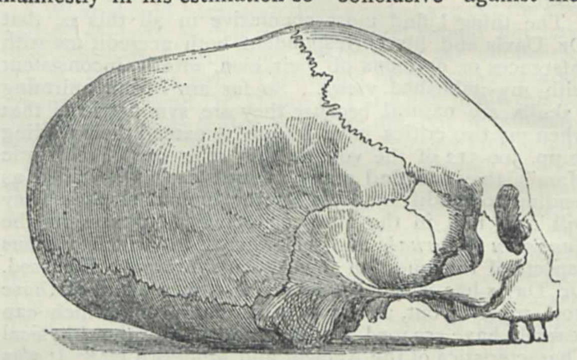


FIG. 3.—Peruvian Child's Skull, Normal.

in the projection of the occiput farther on the left than on the right side, is a feature I am very familiar with, in skulls which I should still call "natural," as distinguished from those designedly modified by art.

I shall refer only to two marked examples of this irregularity, in proof of such unsymmetrical forms existing among races in no way given to artificial cranial distortion. The first—a brachycephalic one—is "the skull of a young Greek," No. 1,354 of the Morton collection; a cast presented by Retzius. Dr. J. A. Meigs describes it minutely in his catalogue, p. 29, but takes no notice of its symmetry; although when viewed vertically it resembles some of the distorted Flathead skulls. The

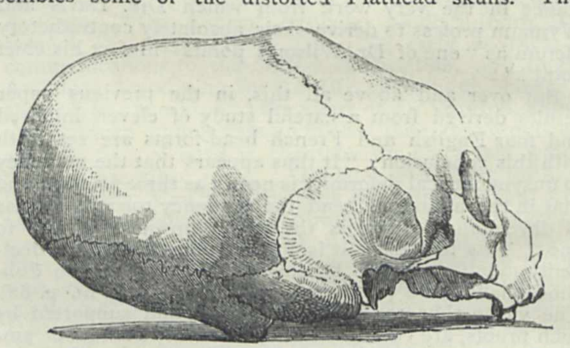


FIG. 4.—Peruvian Child's Skull, Abnormal.

second—a dolichocephalic skull—Dr. Wymann will find alongside of the Peruvian skulls, No. 15 in the Warren Collection at Boston. It is that of a "Chinese," or was at any rate brought from China by Capt. Edes. It approximates in malformation to the "Hochbelaga skull," Fig. 67, "Prehist. Man," p. 501, as an example of posthumous distortion. But in this skull from China the sutures are close, with no trace of dislocation or other indications of posthumous modification of forms. Those are extreme examples; but I repeat what I have long ago asserted; that a perfectly symmetrical head is the exception, rather than the rule.

DANIEL WILSON

THE COMING TRANSIT OF VENUS *

V.

It is probable that the observations of contact will be very materially supported by additional observations made with the double-image micrometer. This instrument was devised many years ago by Sir George Airy.† It is the most convenient eye-piece micrometer which can be used for measuring the distance between a pair of stars, or, as in the present case, between the limbs of the sun and Venus. The peculiarity of Airy's double-image micrometer consists in this, that one of the lenses forming an ordinary terrestrial eye-piece is divided in two, like the object-glass of a heliometer. The one half can be slid past the other, and the amount of displacement accurately measured by a divided circle, concentric with the screw which gives this motion. When the halves of this lens are relatively displaced, two images of the object are seen, as in the heliometer. If the distance between a pair of stars be the subject of measurement, the line of separation of the half-lenses is made to coincide with the line ioining the two stars. The screw is now turned in one direction, until the image of one star given by one half of the lens coincides with the image of the other star given by the other half of the lens. The amount of displacement is now read off. The halves of the lens are again brought to coincidence. The screw is now turned in the opposite direction, and a similar observation made. Knowing the value of the divisions on the divided circle, these two observations give us a means not only of determining the distance between the two stars, but also of fixing accurately the reading of the instrument when the half-lenses are in coincidence.

It is easy to see that after the internal contact at ingress, and before the internal contact at egress, measurements may thus be made of the distance of Venus from the sun's limb, from which the true time of contact may be deduced, just as in the Janssen photographic method.

But, besides, this double-image micrometer gives a means of estimating the true time of contact in a manner which may possibly be one of very great accuracy indeed. Consider the case of ingress two minutes before the time of true contact. From this time up to the actual contact the distance between the cusps, where the limbs of Venus

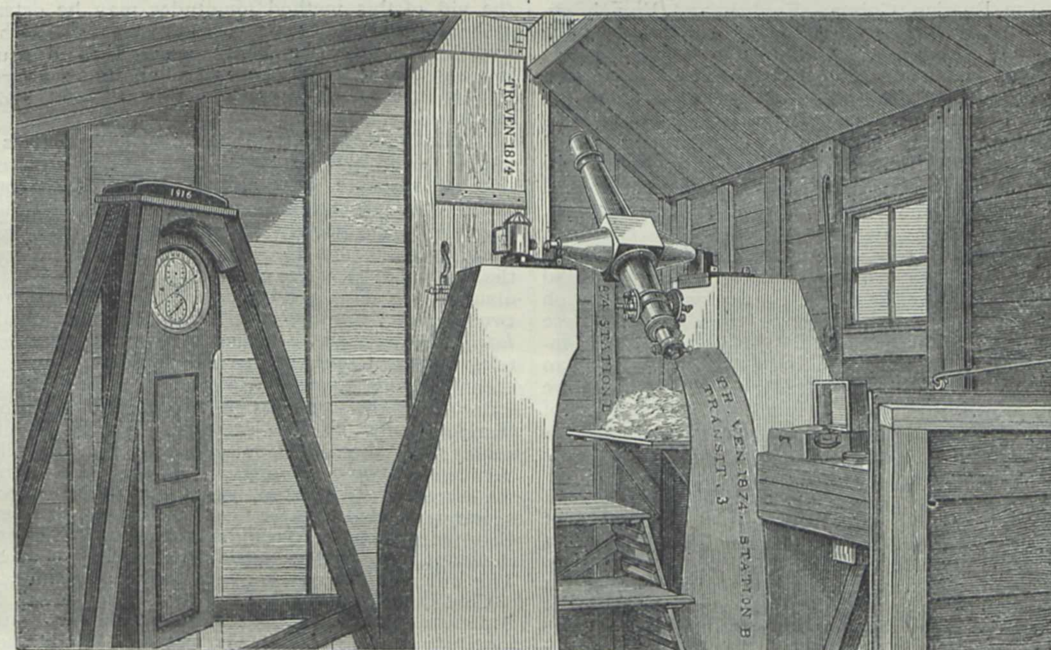


FIG. 16.—The Transit-instrument of the British Expedition.

and the sun meet, will diminish with very great rapidity. By turning the micrometer so that the line of junction of the half-lenses is in a line with the points of these two cusps, the distance between them may be very accurately measured. The observation may be repeated a number of times. The great rapidity with which these cusps approach, with a very slight motion of the planet, makes it probable that each of these observations will give the means of determining very closely the true time of contact.

There are great difficulties connected with observations of the sun at such low altitudes as are required for the application of De l'Isle's and other methods. These will materially affect the definition of the cusps, and it is not certain that the micrometer method will give results so valuable as might have been anticipated.

But even in the eye-observation of contact the low altitude of the sun will be a serious drawback. This difficulty has been fully recognised by the Astronomer Royal, and, with the assistance of Mr. Simms, he has devised an ingenious eye-piece, which is likely largely to reduce the inconvenience.* The chief difficulty is, that at such low altitudes not only are the rays of light enormously refracted by the earth's atmosphere, but the colours are actually dispersed, as with a prism. Hence the definition cannot be perfect. The principle of the new eye-piece consists in employing a hemispherical lens for the one next the eye. The surface of this lens next to the eye is plane ; and the lens can be moved, by means of a screw and slight spring, in a socket which is a portion of a sphere the same radius as the lens. By turning the screw, various inclinations can be given to the plane surface next the eye. But the curvature of the other surface remains the same, though a different portion of it is used. The practical result, then, of such an inclination of the lens in its socket is simply the introduction of a prism whose angle can be so varied as to correct totally the atmospheric dispersion.

* Continued from p. 30. † Greenwich Observations, 1840.

* Monthly Notices of the R.A.S. vol. xxx. p. 58.

But in the case of photography the low altitude of the sun introduces a much more serious difficulty. The light has in this case to pass through a great length of the earth's atmosphere, in its lowest and densest regions. Much of the light is absorbed by the atmosphere, as is shown by the fact that the rising or setting sun may be gazed at with impunity. But further, it is found that of all the colours composing the sun's light, those which affect most powerfully a photographic plate are the most greedily absorbed. Hence it has been found at St. Petersburg that at mid-winter a photographic plate must be exposed to the sun 360 times as long as at the equinoxes, when the altitude of the sun is about 6° or 7°. This is a difficulty which cannot be surmounted except by exposing the plate a longer time than is desirable.

It has been already stated that considerable discrepancies in determining the times of contact might arise from observers noting different phenomena. The employment of the Model Transit of Venus ensures concordance among the observers of each nation; but all European observers will be much indebted to M. Struve, who has actually compared his own observations with those of the Russian, German, English, and French observers, so that comparisons will be possible between the observations of these different nations.

Everything being now prepared for observing as successfully as possible the actual phenomenon of contact, it remains to describe the means by which the time can be determined accurately. All clocks and watches are set and regulated by observations of the stars, or by comparison with other clocks so regulated. An astronomical clock counts the hours up to 24h. The clock is set to oh. at the instant when a particular star passes the meridian. If then we have a means of determining the time when this happens, we can set our clock accurately to local time. But a star does not pass the meridian of Greenwich at the same time as it passes the meridian of a place having any other longitude. By the aid of a transit instrument the local time can be determined; but to determine actual Greenwich time at another place we must, as before stated, know accurately the longitude of that station. *These two things, the absolute time and the longitude, are so connected, that if we know the one, the other can be immediately deduced.*

The longitude may be determined in a variety of different ways. If the two places whose difference of longitude is to be determined be not very distant, a simple method may be employed. A rocket is sent up from some point between the two stations. An observer at each station notes the local time at which the rocket is seen to burst. The difference between these times gives the difference of longitude. A flash from a lamp or reflected sunlight may be similarly employed.

The absolute time (and consequently the longitude) can also be found by transporting chronometers from one station where it is known to another where it is not known. First-rate chronometers must be used, and a large number to check one another's errors. The main error of a chronometer is due to the influence of temperature on the momentum of the balance wheel and the strength of its spring. The Russians have of late years introduced with great success a method of secondary correction for this error. Along with the compensated chronometer at least one is sent without any compensation. The difference between this chronometer and others is a measure of the sum total of the temperatures to which they have been exposed; and by the aid of a table carefully drawn up from a number of observations, the amount of secondary correction necessary can be fairly estimated. It is said that the employment of this device is of the very greatest service. Ten well-tried chronometers, accompanied by a single uncompensated one, if carried between stations ten days apart (*e.g.* St. Petersburg and Cazan) will, in one journey, give the longitude of an intermediate station (such as Moscow) correctly within $\frac{1}{10}$ of a second of time. By the aid of this contrivance chronometers may be employed, even for very long journeys, to determine the longitude. This method is quite new, and has not been tested by any nations except the Russians. The results obtained by them are, however, perfectly satisfactory. Theoretically the idea is almost perfect; the outstanding temperature error being the main fault of chronometers, and the employment of an additional chronometer uncompensated giving us a means of determining the amount of this error, the time deduced by this means ought to give very satisfactory results. There is but one objection to the method, which is only a partial one. After a series of alternately very hot days and very cold nights, the difference between the compensated and uncompensated chronometers might be the same as after the same period, with a tolerably uniform temperature; but the correction necessary in these two cases might be very different indeed. It is easy, however, to keep chronometers at a temperature which does not vary rapidly, and the experiments made by the Russians warrant us in saying that by the aid of this method longitudes may be determined, with very great accuracy indeed, in voyages of such length that the ordinary chronometric method would be unavailing, and that in every case where longitudes are required by the use of chronometers this method should be employed.

A third way of determining the absolute time is by the use of telegraphic signals. An operator at Greenwich may arrange to telegraph a signal to another at Alexandria at a certain definite time of day. If the transmission of the current from Greenwich to Alexandria were instantaneous the person at Alexandria would at that instant receive the exact time. But a current through a submarine cable is retarded. Suppose it to be retarded two seconds; the time received at Alexandria will be *too late* by two seconds. If now an operator at Alexandria telegraphs to Greenwich he will dispatch the signal two seconds *before* it reaches Greenwich. The longitudes determined by the two currents in opposite directions will therefore differ by four seconds. The mean of these values gives the true longitude, and half the difference between the two determinations is the time of transit of the currents. It is found, however, both from theory and experiment, that if there be a leak in the cable nearer to Greenwich than to Alexandria the current will pass more slowly in going to Alexandria than in the reverse direction. This difference, however, can never be very great.

Considerable differences have been found by the Americans to exist between comparative observations of longitude by the telegraphic method and by the lunar method, which will presently be described. The Americans rushed to the conclusion that the error existed in the lunar method. This is not necessarily so. The American system of telegraphing over long distances consists in using a *relay*. A relay is an arrangement to overcome the difficulty of sending a current through a long line. It is placed at an intermediate station. It consists essentially of an electro-magnet which attracts a piece of iron when a current which has originally been sent through the primary station passes through its coils. This attraction of a piece of iron makes contact with a new electric circuit with a separate battery, and so the current is passed on to the final station, or to a second relay. The piece of iron must move through a sensible distance before the second circuit is completed. It has hitherto been supposed that the time lost in employing a number of relays could be eliminated by sending the current in alternate directions as above described. This is certainly not the case. The time elapsing before contact is made by a relay depends upon the strength of the current. The strength of the current depends upon the length of the wire through which it is passing, and also upon the strength of the battery. Consider now the case of a relay at the junction of a long and short wire. The current passing through the long wire is weaker than the other. Hence if the current first pass through the short wire, the loss of time introduced by the relay is less than when the current is first sent through the long wire. For this reason the time taken by the current to pass in one direction is less than in the other direction. It appears then that the employment of a number of relays is injurious in longitude determinations, and if extraordinary precautions be not taken the resulting longitude will be erroneous. The same takes place with a submarine cable, with a leak near one end of it.

It must be noticed that in all the methods here described for determining the longitude, the local time must be accurately known. This is done by aid of a transit instrument as before described. One of the transit instruments of the British Expedition, in its wooden hut, is shown in Fig. 16.

Another class of method for determining the longitude depends upon the motions of the moon. It has already been stated that what we want is to know at some instant the absolute Greenwich time. If then we could get something analogous to a huge clock in the heavens which an observer at any part of the world could see we should be able to determine our longitude. The moon may be taken to represent the hand of such a clock, and the stars the hours and minutes. The moon is chosen in preference to the planets because she moves more rapidly among the stars. She moves around the earth, that is through 360°, in 27½ days, or through 1° in two hours, or through one second of arc in two seconds of time. If then the tables in the *Nautical Almanac* predicting the place of the moon are absolutely correct, an observer by watching the instant at which she seems to come to the position of any star, and knowing from the tables the Greenwich time at which she reaches that position, receives an intimation of the absolute time from this gigantic celestial clock. Or, if there be no star, it will suffice to observe the time when the moon reaches any definite position among the stars. As a matter of fact the tables of the moon are by no means perfect; but this difficulty is overcome by the regular series of observations of the moon's place made at Greenwich on every possible occasion. Thus while the tables are sufficiently accurate to give the navigator a fair knowledge of his longitude, an observer in any country can, when convenient, compare his observations with those made at Greenwich, and so determine the longitude with great accuracy.

It is a fact of interest in connection with the present subject, that the transits of Venus will aid materially in perfecting the Lunar Tables. The motions of the moon are rendered irregular by the disturbing attraction of the sun. But we cannot determine with great accuracy either the amount or the direction of the sun's attraction upon the moon until we know accurately the sun's distance. Hence if we wish to be able to compute tables of the moon sufficiently correct for the exact determination of longitude, we must employ every means in our power to perfect our knowledge of the sun's distance.

Of the methods available for determining the moon's position, three will be employed in the coming transit. The first is by observing, with a powerful telescope, the exact time at which the moon extinguishes the light of a star in front of which it is passing. This is technically called an occultation of a star by the moon; and when the occultation is made by the non-illuminated portion of the moon the observation has great precision, and, the position of the star being known, is very valuable for determining longitude.

The second method is by observing, with a transit instrument, the exact time at which the moon passes the meridian, and by observing about the same time the transits of stars whose positions are well known.

The third method is by employing an instrument called an altitude-and-azimuth instrument, or shortly, an alt-azimuth. This instrument is shown in Fig. 17, and consists essentially of a telescope mounted upon two divided circles so arranged that the one shall give the altitude of an object towards which the telescope is pointing, while the other gives its azimuth or its angular distance from the meridian measured in a horizontal direction. An instrument of this class has long been employed at Greenwich with great success for determining the position of the moon when out of the meridian. It thus acts as a supplement to the transit-circle, of the utmost value in so cloudy a climate as our own. One disadvantage of this instrument is that the numerical reductions are extremely troublesome; but no trouble is too great in an observation of so much importance.

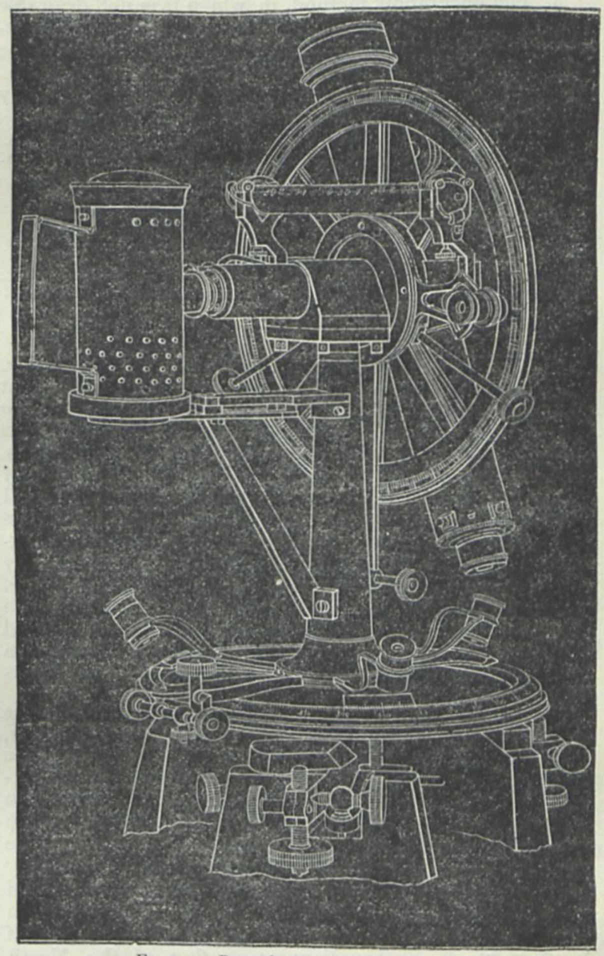


Fig. 17.—Portable Alt-azimuth Instrument.

It is not absolutely necessary that both altitude and azimuth should be observed. In equatorial regions the motion of the moon is chiefly in altitude, while in places of high latitude the motion is chiefly in azimuth. Hence among the English stations the vertical circles alone are provided for the stations within 30° of the equator, while at Rodriguez, Kerguelen's Island and New Zealand the azimuth circles are accurately divided. All these instruments have been well tested, and are found to be remarkably perfect. Not only the alt-azimuths but also most of the other instruments to be employed by the British have been constructed by Troughton and Simms; they have all been well tried, and the results have been so satisfactory that these makers deserve great credit for the help they have thus given to the success of the expeditions.

In all observations of the moon for determining the longitude there are of course numerous corrections which must be applied. Among these none is more important than the correction for the parallax of the moon.

RECAPITULATION.—In the case of every nation depending upon De l'Isle's method and in the case of every expedition when only one contact is observed, the longitude must be determined with very great accuracy. This can be done by any of the following methods :—

1. By rockets, or flashing signals.
2. By a trigonometrical survey.
3. By the aid of chronometers, in which it would be unwise to neglect the method lately introduced of adding to the chronometers one which is uncompensated.
4. The telegraphic method, in which it is not desirable to use relays, since very long lines with a Thomson's reflecting galvanometer will give good results, while the employment of relays is objectionable.
5. By observations of the moon's position which may be made by either of the three following methods :—
   (*a*) By occultations of the moon.
   (*β*) By transit observations of the moon and moon-culminating stars.
   (*γ*) By aid of an alt-azimuth.

GEORGE FORBES

(*To be continued.*)

OCEAN CURRENTS

I OBSERVE that in NATURE, vol. ix. p. 423, Dr. Carpenter re-states and maintains his opinion that polar cold rather than equatorial heat is the *primum mobile* of his general oceanic circulation. In my papers in the *Philosophical Magazine* for Oct. 1871 and Feb. 1874 I have proved, I trust, to the satisfaction of any physicist who will be at the trouble to examine what I have written on the subject, that this notion is based upon a confusion of ideas in regard to the way in which difference of specific gravity produces motion. It is not my object at present to enter into any further discussion of this elementary matter ; but I wish briefly to refer to a new and somewhat plausible-looking objection advanced in Dr. Carpenter's article against the views I advocate in reference to under-currents. The following is the paragraph to which I refer :—

"According to Mr. Croll's doctrine the whole of that vast mass of water in the North Atlantic, averaging, say, 1,500 fathoms in thickness and 3,600 miles in breadth, the temperature of which (from 40° downwards), as ascertained by the *Challenger* soundings, clearly shows it to be mainly derived from a polar source, is nothing else than *the reflux of the Gulf Stream.* Now, even if we suppose that the whole of this stream, as it passes Sandy Hook, were to go on into the closed Arctic basin, it would only force out an equivalent body of water. And as, on comparing the sectional areas of the two, I find that of the Gulf Stream to be about 1-900 that of the North Atlantic underflow ; and as it is admitted that a large part of the Gulf Stream returns into the Mid-Atlantic circulation, only a branch of it going on to the north-east ; the extreme improbability (may I not say impossibility ?) that so vast a mass of water can be put in motion by what is by comparison a mere rivulet, the north-east motion of which as a distinct current has not been traced eastward of 30° W. long. seems still more obvious."

The objection seems to me to be based upon a series of misapprehensions : (1) that the mass of cold water 1,500 fathoms deep and 3,600 miles in breadth is in a state of motion towards the equator ; (2) that it cannot be the reflux of the Gulf Stream, because its sectional area is 900 times greater than that of the Gulf Stream ; (3) that the immense mass of water is, according to my views, set in motion by the Gulf Stream.

I shall consider these in their order : (1) That this immense mass of cold water came originally from the polar regions I of course admit, but that the whole is in a state of motion I certainly do not admit. There is no warrant whatever for any such assumption. According to Dr. Carpenter himself the heating power of the sun does not extend to any great depth below the surface ; consequently there is nothing whatever to heat this mass but the heat coming through the earth's crust. But the amount of heat derived from this source is so trifling that an under-current from the Arctic regions far less in volume than that of the Gulf Stream would be quite sufficient to keep the mass at an ice-cold temperature. Taking the area of the North Atlantic between the equator and the tropic of Cancer, including also the Carribean Sea and the Gulf of Mexico, to be 7,700,000 square miles, and the rate at which internal heat passes through the earth's surface to be that assigned by Sir William Thomson, we find that the total quantity of heat derived from the earth's crust by the above area is equal to about 88×10^{15} foot pounds per day. But this amount is equal to only 1-894th of that conveyed by the Gulf Stream, on the supposition that each pound of water carries 19,300 foot pounds of heat. Consequently an under-current from the polar regions of not more than $\frac{1}{35}$ the volume of the Gulf Stream would suffice to keep the entire mass of water of that area within 1° of what it would be were there no heat derived from the crust of the earth. That is to say, were the water conveyed by the under-current at 32°, internal heat would not maintain the mass of the ocean in the above area at more than 33°. The entire area of the North Atlantic from the equator to the Arctic circle is somewhere about 16,000,000 square miles. An under-current of less than $\frac{1}{17}$ that of the Gulf Stream coming from the Arctic regions would therefore suffice to keep the entire North Atlantic basin filled with ice-cold water. In short, whatever theory we adopt regarding oceanic circulation, it follows equally as a necessary consequence that the entire mass of the ocean below the stratum heated by the sun's rays must consist of cold water. For if cold water be continually coming from the polar regions either in the form of under-currents or in the form of a general underflow, as Dr. Carpenter supposes, the entire under portion of the ocean must ultimately become occupied by cold water, for there is no source from which this influx of cold water can derive heat save from the earth's crust. But the amount thus derived is so trifling as to produce no sensible effect. For example, a polar under-current one-half the size of the Gulf Stream would be sufficient to keep the entire water of the globe (below the stratum heated by the sun's rays) at an ice-cold temperature. Internal heat would not be sufficient, under such circumstances, to maintain the mass 1° F. above the temperature it possessed when it left the polar regions.

(2) But suppose that this immense mass of cold water occupying the great depths of the ocean were, as Dr. Carpenter assumes it to be, in a state of constant motion towards the equator, and that its sectional area were 900 times that of the Gulf Stream, it would not therefore follow that the quantity of water passing through this large sectional area must be greater than that flowing through a sectional area of the Gulf Stream, for the quantity of water flowing through this large sectional area depends entirely on the rate of motion.

(3) I am wholly unable to understand how it could be supposed that this underflow, according to my view, is set in motion by the Gulf Stream, seeing that I have shown that the return under-current is as much due to the impulse of the wind as the Gulf Stream itself.

I am also wholly unable to comprehend how Dr. Carpenter should imagine that because the bottom temperature of the South Atlantic should happen to be lower, and the polar water to lie nearer to the surface in this ocean than in the North Atlantic, that therefore this proves the truth of his theory. This condition of matters is just as consistent with my theory as with his. When we consider the immense quantity of warm surface water which, as has been proved,* is being constantly transferred from the South into the North Atlantic—a quantity which to a large extent is compensated by cold currents from the Antarctic regions—we readily understand how the polar water comes nearer to the surface in the former ocean than in the latter. In fact the whole phenomena is just as easily explained upon the principle of under-currents as upon Dr. Carpenter's theory.

Dr. Carpenter lays considerable stress on the important fact established by the *Challenger* expedition, viz. that the great depths of the sea in equatorial regions are occupied by ice-cold water, while the portion heated by the sun's rays is simply a thin stratum at the surface. It seems to me that it would be difficult to find a fact more hostile to his theory than this. Were it not for this upper stratum of heated water there would be no difference between the equatorial and polar columns, and consequently nothing to produce motion. But the thinner this stratum is the less is the difference and the less there is to produce motion. I have been favoured by the Hydrographer to the Admiralty with a series of temperature soundings taken along the equator, and from these I find that to so small a depth does the super-heating extend that the surface of the ocean at the equator requires to stand only four and a half feet above that at the poles in order to the ocean being in perfect equilibrium. In this case if we suppose, in order to constant circulation, that the polar column is kept in excess of the equatorial by the weight of say two feet of water, there would then remain only a slope of two and a half feet between the equator and poles.

There is another point to which, with some reluctance, I am compelled to refer. Dr. Carpenter is continually representing that eminent physicists have adopted his theories while none of them share in my objections. I can assure Dr. Carpenter that such is not the case. Only a few weeks ago one of the most eminent mathematical physicists of the present day stated to me that no one familiar with the elements of physics and mechanics, who would be at the trouble to make himself acquainted with Dr. Carpenter's theories, could ever adopt them.

JAMES CROLL

* Phil. Mag. for March 1874, p. 170.

BIOLOGY AT CAMBRIDGE

ON the evening of Monday, 11th inst., Cambridge biologists mustered at least a hundred strong at the meeting of the Philosophical Society to hear a communication from Prof. Huxley, one of the honorary members of the Society, on the morphological conclusions to be drawn from the distribution of the cranial nerves, with especial reference to those of the seventh pair. Prof. C. B. Babington, F.R.S., president of the Society, occupied the chair. Prof. Huxley took occasion to refer in terms of the highest commendation to the researches of Stannius more than twenty years ago, on the morphological teaching to be derived from studying the distribution of nerves, and also spoke of the deductions drawn from nerve-supply by Gegenbaur, especially in his work on the "Skulls of Plagiostomous Fishes." Prof. Huxley sketched in considerable detail the distribution of the portio dura or seventh cranial nerve in man, and compared it with the homologous nerve in the frog, showing how the arrangements of branches, especially the course of the chorda tympani, which seemed anomalous in man, were a necessary consequence of perfectly obvious and natural arrangements in the lower vertebrates. He also demonstrated how the morphology of the parts might be learnt from such homologies; how a circuitous and apparently useless path taken by a nerve was full of meaning and instruction, and when studied in connection with facts of development and function would lead to an explanation which might be very much trusted. The relation of the tympano-eustachian tube to the bifurcation of the seventh nerve was dwelt upon, as leading to the identification of the comparatively small and simple auditory passage of the frog with the complex one of the mammal, and further to the homological identity of these passages with the spiracle of the Plagiostomes. The distribution of the fifth and seventh pairs of cranial nerves was held to agree with the view, suggested by development, that the trabecular arch is a pre-oral visceral arch, and that the pterygo-palatine is but an outgrowth of the mandibular arch.

The paper, which was illustrated by black-board drawing, with the professor's well-known aptitude, and which was a model of lucidity and careful reasoning, was loudly applauded. In a discussion which followed, Prof. Humphry drew attention to labours of his own having the object of showing the value of the teaching of nerve distribution. He acknowledged the strong case which was now made out in favour of the trabecular arch taking its position in the series of visceral arches, and thought that Prof. Parker's paper on the development of the pig's skull made it almost equally clear that the pterygo-palatine arch was similar in homology. It was also remarked that the same conclusions seemed deducible from Prof. Parker's paper on the development of the salmon, where the pterygo-palatine arch was distinct from the first and in all respects like the other visceral arches.

The practical class for the study of elementary biology, conducted by Dr. Michael Foster and Dr. Martin, is very successful this term. When thirty students entered last year the number was thought very large, and it was made up of men of several years who had previously had no opportunity of attending such a course. It was expected that a much smaller number would attend this year; but the large number of nearly forty have availed themselves of the course, and work proceeds in a most satisfactory and instructive fashion. Adequate superintendence is provided at all hours of the working day by the co-operation of four advanced students in addition to the lecturers. These are Messrs. P. H. Carpenter, Trinity College, A. M. Marshall, B.Sc., and Langley, St. John's College, and S. H. Vines, B. Sc., Christ's College.

G. T. BETTANY

NOTES

ON Tuesday, Sir Samuel Baker delivered the Rede lecture in the Senate House, Cambridge, before a numerous assemblage, which included all the leading men of the University in residence, and many ladies. The subject of the address was "Slavery," and Sir Samuel's narrative of his personal experiences in Africa was listened to with much interest.

IT is said to be in contemplation to confer honorary degrees at the Cambridge commencement upon Sir Bartle Frere, Sir Garnet Wolesley, Sir James Paget, and Prof. Helmholtz.

IT is stated that if the authorities of Owens College, Manchester, can show that they really require it, Government are prepared to make a considerable grant ot money to the College.

THE Founder's Medal of the Royal Geographical Society has been granted to Dr. Schweinfurth, and the Victoria Medal to Col. P. E. Warburton, who recently succeeded in crossing the interior of Western Australia.

BY later advices from Australia we learn that Major Warburton accomplished exactly what he set out to do. He traversed the continent from the MacDonnell Ranges to the coast north of

Nickol Bay, passing over 800 or 900 miles of ground never before trodden by the foot of white man. The expedition has been useful only in a scientific point of view; the country for nearly the whole distance is utterly worthless. Barren, scrubby, and in the last degree wretched, the explorers had the utmost difficulty in forcing their way through. With poor food for the greater portion of their dreary journey, with water often scarce, and little game, the brave band were reduced to the utmost extremities. For three months they had nothing to live on but dried camels' flesh, and as much roots and bulbs as they were able to gather.

IT is said that the king of Sweden has conferred upon Mr. Leigh Smith, the Arctic explorer, the Order of the Polar Star. Mr. Smith succeeded last spring, at his own expense, and with much difficulty, in rescuing the Swedish expedition, which had been caught by the ice in the preceding winter.

THE Albert Gold Medal of the Society of Arts has been awarded for the present year to Dr. C. W. Siemens, F.R.S.

THE German Emperor a few days ago at Wiesbaden, received Herr Rohlfs, the German explorer, who has just returned from the Lybian desert. It was by the Emperor's special command that the well-known traveller repaired to the palace and gave his Majesty an interesting account of his latest travels. The Emperor, as a further distinction, desired Herr Rohlfs to dine that day at the imperial table.

WE are glad to learn that, in accordance with the wish expressed at the Meteorological Congress held at Vienna in 1873, a commission has been nominated by the Imperial Academy of Sciences at St. Petersburg and by the Imperial Ministry of Marine, to prepare a project for the establishment of a central office for Maritime Meteorology in Russia, including a system of meteorological telegraphy and storm warnings. Prof. H. Wild, Director of the Central Physical Observatory, and Capt. M. Rikatcheff, Assistant in the same establishment, are appointed members of the commission.

M. PRJEWALSKY, a staff-officer of the Russian army, is about to publish an account of a journey in which he has successively explored Dzangaria, Koukou Noor, and Moupin. Like Armand David (NATURE, vol. x. p. 32), he brings back with him extensive collections. Insects hold a large place in both; those of Père David, said to be exceedingly interesting, have been presented to the Musée National, at Paris, where they have remained unnoticed by the French entomologists, one of whom says, that now "they will probably always remain unknown."

M. J. LIAGRE has been elected Perpetual Secretary of the Belgian Academy of Science, in succession to the late M. A. Quetelet.

A GEOLOGICAL excursion on a somewhat extended scale has been proposed, to those localities in the Swiss Alps which have become household words amongst those who have studied the changes of the earth's surface, and the action of ice and water more especially. A gentleman whose local knowledge is undoubted has been requested to act as cicerone to the party, and to deliver discourses upon the more interesting spots. He has accepted the first task, but wishes to secure the kind offices of an indigenous geologist for the second. It is hoped to arrange for a large party of ladies and gentlemen to start early in August and be absent for a month. This is the first time the interests of Science will be added to the enjoyments of a summer's holiday, with the exception of the short excursions near home of the Geologists' Association.

THE President of the Institution of Civil Engineers and Mrs. Harrison held a reception on Tuesday evening in the western gallery of the International Exhibition, at which over two thousand guests were present. In addition to the picture galleries and rooms containing machinery in motion, the west quadrant was open, and in it were placed illustrations of recent scientific inventions specially lent for the evening. With the exception of Mr. Crook's experiments showing attraction and repulsion accompanying radiation, and Tisley and Spiller's compound pendulum apparatus, all were applications of scientific inventions to the wants of life, if wicked war may be included among our wants, for Sir W. Armstrong, and other firms, sent models of appliances for the hydraulic mounting of large guns, whereby they can be placed in position with ease. One of the most recent applications of electricity is to a self-recording "waybill" of omnibuses. An apparatus brought out by Messrs. Whitehouse and Clark counts up once every minute the number of passengers in the omnibus and prints this number and the exact time in plain figures. Each seat is separate, and the weight of the passenger on the seat brings the wire from that seat in communication with the recorder. The instrument also records the speed of the omnibus at every moment of the journey, and shows the exact time of arrival and departure from each station. The cost is said to be but a few shillings a week, but it does away with the need of a time-keeper. A sample was exhibited of bills made May 15, in Liverpool, showing that the invention is practicable as well as ingenious. Nearly all other models were of docks, lighthouses, or railway appliances.

AT a meeting of the Sedgwick Memorial Committee held at Cambridge on the 12th inst., the treasurers, Mr. Vansittart of Trinity, and Mr. Ewbank of Clare, announced that more than 10,000*l*. had been promised, of which 9,000*l*. had been received. The money is to be expended in the erection of a Geological Museum, to be called the Sedgwick Museum. After discussion it was agreed that the time had arrived when it is desirable that the University should take the subject into consideration. The chairman, Prof. Humphry, was desired to communicate the resolution to the Vice-Chancellor, with a request that he would bring the subject under the consideration of the Council of the Senate.

ON Thursday last in the House of Commons Lord E. Fitzmaurice gave notice that in the event of the Royal Commission reporting on a sufficiently early day before the close of the session, he would call attention to the subject of University reform, and move a resolution.

COUNT WILCZEK, we learn from the *Geographical Magazine*, has announced his readiness to give a reward of 1,000 florins (100*l*.) to anyone who will bring home any news of the Austro-Hungarian Arctic Expedition. The *Tegethoff* steamer, with the members of the Expedition on board, was last heard of on Aug. 21, 1872, on the north-west coast of Novaya Zembya, in about 76° N. lat., when Count Wilczek himself parted company with them and sailed southward in his yacht *Isbjörn*.

A LETTER from the *Daily News* correspondent with H.M.S. *Challenger* gives some account of the work done by that ship between Simon's Bay and Melbourne. The usual sounding, dredging, and trawling operations were carried on with excellent results; many new specimens have been obtained by the dredge. The ship was at Kerguelen's Island on January 7, and stayed about the island during the month of January, making careful surveys and observations, and collecting specimens both from the sea and land. Previous to the ship's departure from Christmas Harbour, Kerguelen, a cairn was built, and papers of instruction, &c. for the Transit of Venus party left in it. On February 11 the first iceberg was seen when making for Heard Island, and from this time till the beginning of next month, ice bergs and drift-ice were met with in large quantities, the ship making one or two narrow escapes; on Feb. 24 the ship was quite close upon the so-called "Termination Island," but no sign of land was seen at all. On March 17 the *Challenger* anchored near Melbourne, all well.

A TRAIN arrived at Algiers from Oran on the 18th inst., six hours behind time, having been delayed by a thick layer of grasshoppers which covered the rails.

THE first meeting of the Board of Governors of the Yorkshire College of Science was held in the Philosophical Hall, Leeds, on April 30. Dr. Heaton was called upon to preside. The business of the meeting was the election of the president, treasurer, council, and auditor for the ensuing year, also the appointment of six endowed grammar schools and ten institutions, each of whose governing bodies should elect a Governor of the College. Lord F. C. Cavendish, M.P. and Mr. W. B. Denison were respectively elected president and treasurer of the College. The following grammar schools were placed in Schedule A :—Leeds, Bradford, Batley, Halifax, Wakefield, and Giggleswick. The institutions placed in Schedule B were the Philosophical Societies at York, Leeds, Bradford, Halifax, Sheffield, and Huddersfield, the Clothworkers' Company of the City of London, the West Riding Coalmasters' Association, the Cutlers' Company, Sheffield, and the Trustees of Ackroyd Charity. Each of these bodies is invited to nominate a member of the Board of Governors.

THE *Times of India* states that Dr. David Wilkie has been appointed by the Government of India to conduct a scientific investigation into the nature, pathology, and causation of the fever prevailing in the Burdwan and Hoogly districts. He is to work in communication with Dr. Lewis and Dr. Cunningham, and under the direction and general superintendence of the Sanitary Commissioner with the Government of Bengal.

UNDER the direction of Mr. Liversidge, Professor of Geology and Lecturer in Practical Chemistry, the Laboratory of the Sydney University is being improved in a way to make it similar to the Laboratory of the Royal School of Mines and the University of Cambridge, and to afford appliances for the proper conduct of the exercises in practical chemistry.

MR. WILLIAM H. DALL resumed his Alaskan explorations under the U. S. Coast Survey, about April 20, at which date he expected to sail for Sitka and more northern points. It is probable that his labours during the present season will be in the neighbourhood of Cook's Inlet and the peninsula of Alaska, and the coast of the mainland as far as the islands of Nunivah and St. Michael's. His duties are to complete a coast pilot of the territory, and to make careful magnetical and other observations. Should his regular work permit, he hopes to make large collections in natural history and ethnology, in continuation of those of previous seasons, and transmitted through the Coast Survey Office to the National Museum at Washington, and which have done him and the Survey so much credit.

HEFT V. of Petermann's *Mittheilungen*, contains Contributions to the climatology and meteorology of the East Polar Sea, by Prof. Mohn; an account of some of the results of Gerhard Rohlf's expedition into the Lybian desert, with a map; and a German translation of the journal kept by Jacob Wainwright, while marching with Livingstone's body from Central Africa to Zanzibar. A copy of this journal was obtained by the late Richard Brenner, the African traveller and Austrian Consul at Zanzibar.

THE additions to the Zoological Society's Gardens during the last week include a Crested Curassow (*Crax alector*) from Guiana, presented by Mr. G. Bruce; a Ring-necked Parakeet (*Palæornis torquata*) from India, presented by Mrs. A. de Normanville; a Coati (*Nasua nasica*) from South America, presented by Miss E. Waller; a Common Paradoxure (*Paradoxurus typus*) from India, presented by Mr. G. R. Colbeck; two Muscovey Ducks (*Cairina moschata*) from Monte Video, presented by Mr. S. J. Oliff; a Koodoo (*Strepsiceros kudu*) from Africa, deposited.

THE METEOROLOGICAL CONGRESS AT VIENNA *

II.

WITH reference to the organisation of a system of meteorological observations on the Chinese coasts, for advice regarding which the Congress was applied to, a report was adopted setting forth the general principles of organisation suited to the circumstances of China.

In addition to the above, General Myer, as commissioned by the War Department of the United States, proposed that with a view to their exchange at least one uniform observation of such character as to be suitable for the preparation of synoptic charts be taken and recorded daily and simultaneously at as many stations as practicable throughout the world. This proposal the conference adopted, and, as the readers of NATURE are aware, is now in operation.

On these various subjects much valuable information will be found in the discussions in the Reports of the Committees, and in the communications printed in the Appendices, particularly on the subjects of weather telegraphy, sheet lightning, atmospheric electricity, ozone, clouds, atmometers, rain-gauges, and the protection of thermometers.

In the review of the Leipsig Conference (NATURE, vol. viii. p. 342) a hope was expressed with reference to the protection of the thermometers, which is really the vital question of meteorology, that the Vienna Congress would face it, seriously discuss it, and either arrive at some decision, or at least suggest some steps to be taken that might ultimately lead to the uniformity which is so imperatively called for. Unfortunately this has not been done. We say unfortunately, for scarcely two of the head observatories in the British Isles and on the Continent, where continuous or hourly observations are recorded, could be named at which there is uniformity in the protection of the thermometers as respects the box in which they are placed, height above the ground, and position with reference to walls and other surrounding objects. Now till uniformity in the position and exposure of the thermometers be obtained, there can be no comparableness in the results, and consequently the observations are of little value as data for the determination of what must be regarded as the most important fundamental facts on which the science rests, viz. the diurnal and seasonal march of the temperature and humidity of the atmosphere. It is only from the range of the temperature and the humidity of the atmosphere of different regions as ascertained by observations made on a uniform method that we are furnished with physical data for the scientific treatment of such questions as the daily fluctuations of the barometer, and the changes and movements of the atmosphere generally.

Prof. Wild's paper on the exposure of thermometers (p. 77) we recommend to the careful consideration of meteorologists. His observations, instituted at the Pulkowa Observatory at heights of 6¼ ft., 52 ft., and 86 ft., are, as far as we are aware, the best that have yet been made for the purpose of disclosing the influence which mere height, as such, has on the temperature. The thermometers were *placed on a scaffolding constructed of timber lightly put together, and standing in an open field*, being in these essential points in striking contrast with those placed for a similar object on the Chinese pagoda in the Royal Gardens at Kew, it being evident that observations made with thermometers placed like those at Kew will give results which possess little, if any, value in an inquiry touching the vital question of the position and exposure of thermometers.

From the small differences among the mean temperatures he obtained at the different heights, Wild concludes that the height of thermometers above the ground need not necessarily be the same, but may vary between 6 ft. and 33 ft. The differences he obtained as regards mean temperature, though by no means insignificant, are doubtless small; but when we regard the maxima and minima and the observations at particular hours, which in their practical bearings are so important, the influence of height becomes well marked. Hence, if in any meteorological system uniformity as respects height be disregarded, the results so obtained fail to supply the data necessary for a satisfactory comparison of climates. This condition is all the more indispensable when the thermometers are placed at a height of 4 ft. above the ground, at which they should be placed as being the height which gives the best results as regards the application of meteorology to human mortality and other important questions affecting animal and vegetable life.

* Continued from p. 18.

We are probably yet a long way from any simple method, suited for general adoption, for observing the *true temperature of the air* at any place by means of the thermometer, so as to eliminate completely the disturbing influence of radiation as regards the thermometer and its protecting screen, or box. This is a problem which may well engage the serious attention of the chief observatories of this and other countries for some years to come. The inquiry may be conducted by ascertaining the true temperature of the air at different hours and seasons by Joule's method, described in a communication to the Philosophical Society of Manchester, November 26, 1867, and comparing the results with those simultaneously obtained by thermometers protected in boxes of different constructions and materials. On this point Wild's paper contains some very valuable observations —valuable, not because they are conclusive, but because they are suggestive, as indicating the line of inquiry which should be pursued. In the meantime all that can be secured is *uniformity*, which would be sooner attained if meteorologists recognised that the following positions of the thermometer are, on physical grounds, inadmissible in researches into the horary fluctuations of the temperature and humidity of the air, viz. the roofs of houses, close or near to walls, over bare soil, in the shadows of trees, walls, or other obstructions, or outside windows. Let it be recognised that observations made under these conditions are of less, and in most cases of no value, then the adoption of 4 ft. as the standard height would follow, and with it the question of uniformity would be almost, if not altogether, settled.

As regards *rain-gauges*, the Congress adopted as the best form for the receiver of the rain-gauge the circular one, with a diameter of 14 in., and at a height of 3 ft., or better 4½ ft., above the ground, a decision which was agreed to by all the delegates except Mr. Buchan, who lodged his protest against it. We have taken the trouble of looking over Mr. Symons' last published *British Rainfall*, and observe that there are not more than half a dozen gauges in the British Isles of this dimension. The readers of NATURE are no doubt aware of the extensive experiments and observations made on this subject in England for some years past, and published annually in the *British Rainfall*, from which it has been experimentally proved that gauges of all sizes from 3 in. to 24 in. inclusive collect amounts not differing more than 2 per cent. from each other. We have had a communication from Mr. Scott, by which we are glad to learn that the Meteorological Office has resolved to retain at its stations the 8 in. gauges hitherto in use. This decision as to the size of the gauge a future Congress will no doubt rescind. Equally in error is the decision as regards height of gauge above the ground, especially large gauges. It is certain from numerous observations made on the subject, that gauges placed at from 3 ft. to 4½ ft. above the ground will not indicate with sufficient correctness the amount of the rain which falls at the place of observation in cases where wind accompanies the rain, owing to the disturbance caused by the obstruction offered by the gauge itself, and by the eddies generated within the funnel. Now owing to the enormous dragging influence of the earth's surface of the wind, these disturbing effects are reduced several fold at the surface and at one foot above it as compared with 3 to 4½ ft. high. On these grounds we cannot recommend British Meteorologists to follow the decision of the Congress. Owing to the extreme variableness of the rainfall, particularly in such countries as Great Britain, where the surface is so uneven, the proper observation of the rainfall requires twenty times more observers than are required to observe any of the other meteorological elements. It is therefore well that a cheap gauge is also a good one, since it facilitates an adequate observation, through numerous observers, of the rainfall, which from its practical and scientific bearings it is so important to know.

In fixing the hours of observation it is essential that those hours be selected which give approximately the mean temperature of the day. The combination of hours which seems to have been most approved both at the Leipsig Conference and the Vienna Congress, and referred to by some very able meteorologists as unconditionally the best, is 6 A.M., 2 P.M. and 10 P.M. The merits of this combination consist in the equal interval of eight hours between the observations, in the close approximation to the daily mean temperature it affords, and in its suitableness for tri-daily charting of the weather. It is, however, a combination of hours which, since it all but absolutely excludes the hours of occurrence of the daily thermometric, barometric, and hygrometric extremes and means, cannot be recommended as generally suitable for meteorological observations of all countries. Indeed, its adoption in tropical and sub-tropical countries would be a blunder. As generally suitable for all latitudes, and for the observation of the principal daily atmospheric phases of temperature, pressure, &c., the best hours are 9 A.M. 3 P.M. 9 P.M., or 10 A.M. 4 P.M. 10 P.M., it being assumed that self-registering thermometers are also used.

We are glad to see that it has been proposed to convene another Meteorological Congress in three years, and hope that some of the questions that form the life-blood of the science will be seriously and adequately discussed by the members of that Congress. The more important of these questions are :—(1) The position and protection of the thermometer for the temperature of the air ; (2) A more satisfactory method for observing the humidity of the air, and of making the deductions therefrom ; (3) The observation of earth-temperatures, especially at and near the surface, and the depth at which fixed thermometers cease to be suitable ; (4) Solar and terrestrial radiation ; (5) The examination of the drying qualities of the air by atmometers, so as to secure comparable results ; (6) A statement of the conditions which anemometrical stations ought to fulfil, so that the instrument shall indicate the true movement of the air over the region where it is placed, or, if this be unattainable, a means of valuing the observations so as to approximate to it ; * (7) Anemometers (Wild's, &c.) for stations of the second order, with which trustworthy observations of wind-force may be made ; and anemometers of velocity which admit of their errors being readily ascertained from time to time ; (8) an adequate nomenclature of clouds ; and (9) the question of atmospheric electricity.

Though the Vienna Congress can properly be regarded as having only concerned itself with questions lying on the outskirts of meteorology, it has done commendable work in thus paving the way for future Congresses, entering on the really important practical questions which united action on the part of meteorologists can alone settle. Until tolerable uniformity be arrived at as regards (1), (2), (5), (6), (7), and (8), in the above paragraph, meteorologists can scarcely be said to have begun to collect data of such a nature as will satisfy our best physicists, and thus lead them to undertake the investigation of the more important of the intricate and difficult problems of the science.

* NATURE, vol. viii. p. 342.

M. COGGIA'S COMET

THE following is an ephemeris to the comet discovered by M. Coggia. It will be seen that the comet will be vastly increased in brilliancy by the month of August.

Berlin Mean time.	R.A.	D.	Brightness (brightness at time of discovery = 1).
	h. m.	° ′	
May 19·5	6 30·9	+ 68 49·4	
28·5	34·34	53·1	
27·5	39·49	59·8	
31·5	45·55	+ 69 8·4	
June 4·5	52·57	19·2	3·8
8·5	7 0·57	31·6	4·5
12·5	10·0	45·1	5·5
16·5	20·17	58·1	6·7
20·5	31·57	+ 70 9·3	8·4
24·5	45·8	16·3	10·6
28·5	59·59	15·2	13·6
July 2·5	8 16·36	0·7	17·8
10·5	56·47	+ 68 15·0	32·3
18·5	9 35·50	+ 62 45·2	64·8
26·5	10 8·57	+ 47 10·9	146·3
Aug. 3·5	21·30	+ 8 52·7	245·0
11·5	10·55	÷ 28 17·2	130·8

SOCIETIES AND ACADEMIES

LONDON

Royal Society, May 7.—Note on some Winter Thermometric Observations in the Alps, by E. Frankland, F.R.S.

During the past winter the author spent a fortnight at the village of Davos, Canton Granbünden, Switzerland, and had thus an opportunity of experiencing some of the remarkable peculiarities of the climate of the elevated valley (the Prättigän) in which Davos is situated. The village has of late acquired considerable repute as a climatic sanitarium for persons suffering from diseases of the chest.

The peculiar winter climate of Davos appears to depend upon the following conditions :—

1. *Elevation above the Sea*, which causes greater rarity of the air, and consequently less abstraction of heat from the body, and also secures greater transcalency in the atmosphere by a position above the chief region of aqueous precipitation, and comparatively out of the reach of the dust and fuliginous matters which pollute the lower stratum of the air.

2. *Thick and (during the winter months) permanent snow*, which reflects the solar heat and prevents the communication of warmth to the air, and consequently the production of atmospheric currents. In still, though cold, air the skin is less chilled than in much less cold air, which impinges with considerable velocity upon the surface of the body. The effect of motion through the air upon the sensation of warmth and cold at Davos is very striking. Sitting perfectly still in the sunshine, the heat in mid-winter is sometimes almost unbearable ; on rising and walking about briskly, a delicious feeling of coolness is experienced, but on driving in a sledge the cold soon becomes painful to the unprotected face and hands.

3. *A sheltered position favourable for receiving both the direct and reflected solar rays*.—In this respect Davos-Dörfli, situated opposite to the entrance of the Dischina valley, has the advantage over Davos-Platz two miles lower down the valley, in which latter village the sun rises on December 21 1h. 9m. later, and sets about ten minutes earlier than at Dörfli.

All these conditions contribute not only to a high sun-temperature during the winter months, but also to a comparatively uniform radiant heat from sunrise to sunset.

Addition to the paper, Volcanic Energy : an attempt to develop its true Origin and Cosmical Relations,* by Robert Mallet, C.E., F.R.S., &c.

Referring to his original paper (Phil. Trans. 1873), the author remarks that from the want of necessary data he had refrained from making any calculation as to what amount in volume of the solid shell of our earth must be crushed annually, in order to admit of the shell following down after the more rapidly contracting nucleus. This calculation he now makes upon the basis of certain allowable suppositions, where the want of data requires such to be made, and for assumed thicknesses of solid shell of

100
200
400 and
800 miles respectively.

He tabulates his results for these four assumed thicknesses of shell, and shows that the amount of crushed and extruded rock necessary for the supply of heat, for the support of existing volcanic action, is supplied by that extruded from the shell of between 600 and 800 miles thick, and that the volume of material, heated or molten, annually blown out from all existing volcanic cones, as estimated in his former paper, could be supplied by the extruded matter from a shell of between 200 and 400 miles in thickness.

On data, which seem tolerably reliable, the author has further been enabled to calculate, as he believes for the first time, the actual amount of annual contraction of our globe, and to show that if that be assumed constant for the last 5,000 years, it would amount to a little more than a reduction of about 3·5 in. on the earth's mean radius. This quantity, mighty as are the effects it produces as the efficient cause of volcanic action, is thus shown to be so small as to elude all direct astronomical observation, and, when viewed in reference to the increase of density due to refrigeration of the material of the shell, to be incapable of producing, during the last 2,000 years, any sensible effect upon the length of the day. The author draws various other conclusions, showing the support given by the principal results of this entirely independent investigation, to the verisimilitude of the views contained in his previous memoir.

Linnean Society, May 7.—G. Busk, vice-president, in the chair.—Prof. Thiselton Dyer exhibited a fruit of *Telfairia occidentalis* Hook. f., the seeds of which are used parched by the natives of Calabar, and the young leaves and shoots much prized as a green vegetable. The native name is Ubōng. With reference to the fruit of the *Aristolochia*, hitherto undescribed, Dr. Thomson writes as follows :—" I have seen it, but only so far back as 1859. . . . I cannot trust myself to say more than that the fruit was of a red-brown colour, 5 or 6 in. long, and six-celled, with six well-marked ridges."—Mr. J. R. Jackson exhibited a piece of copal from Zanzibar riddled by ants. After having been some time in the Kew Museum, the living creature was found in the copal and sent to Mr. Walker, who determined it to be a species of *Termes* or white ant, *Eutermes nemoralis* Walk.—The following papers were then read, viz. :—On the discovery of *Phylica arborea*, a tree of Tristan d'Acunha, in Amsterdam Island, in the South-Indian Ocean ; with an enumeration of the Phanerogams and vascular Cryptogams of that island and of St. Paul's, by Dr. J. D. Hooker, vice-president. Labillardière stated in 1791 that the islet of Amsterdam (generally confounded with that of St. Paul), lat. 37° 52′ S., long 77° 35′ E., in the Indian Ocean, was covered with trees, while that of St. Paul, only 50 miles south of it, is destitute of even a shrub. The nature of this arborescent vegetation was unknown until H.M.S. *Pearl* touched at the island in the summer of 1873, when Commodore Goodenough brought off a specimen of what he states to be the only tree growing in the island, together with a fern in an imperfect state. The former proves to be the *Phylica arborea*, of Tristan d'Acunha, and the fern a frond of a *Lomaria*. Amsterdam Island and Tristan d'Acunha are separated by about 5,000 miles of ocean, and are nearly in the same latitude ; and Dr. Hooker discusses the various hypotheses which suggest themselves to account for the extraordinary fact of the occurrence of the same species in such widely separated localities. Near the hot springs on St. Paul's Island *Lycopodium cernuum* is found, an interesting example of the occurrence of a tropical species under special conditions beyond its normal range, a phenomenon of which other instances also occur.—Additions to the lichen flora of New Zealand, by Dr. J. Stirton. Communicated by Dr. Hooker, vice-president. The lichens here described were collected by John Buchanan, of the Colonial Museum, Wellington, N.Z., and include a large number of species now described for the first time. —Enumeratio muscorum Cap. Bonæ Spei, by J. Shaw. The general results arrived at in this paper are summed up as follows : —(1) The great majority of the Cape mosses are of northern-hemisphere types, a few being cosmopolites. (2) Some Australian and New Zealand forms are represented ; a much larger proportion than is the case with flowering plants. (3) Many forms are strictly localised to particular soils and conditions of climate. (4) The moss flora of the Cape is characterised by an almost total absence of Alpine forms.—Contributions to the botany of the *Challenger* expedition :—No. XV. Notes on Plants collected in the islands of the Tristan d'Acunha group, by H. N. Moseley. Communicated by Dr. Hooker. No. XVI. List of algæ collected by Mr. H. N. Moseley at Tristan d'Acunha, by Dr. G. Dickie. Two new species are described.—On a new Australian Sphæromoid (*Cyclura venosa*) ; and notes On *Dynamene rubra* and *D. viridis*, by the Rev. T. R. R. Stebbing. Communicated by W. W. Saunders. This form belongs apparently to a new genus. It was found in Sydney Harbour, under stones at the lowest ebb-tides.—Descriptions of five new species of *Gonyleptes*, by A. G. Butler. These are additional to the monograph of the genus already published by the writer.—Observations on the fruit of *Nitophyllum versicolor*, by Mrs. Merrifield. Communicated by the secretary. The paper contains a description of the coccidia of this species hitherto unknown, although the plant was described in 1800.—On *Hieracium silhetense* DC., by C. B. Clarke. The writer disagrees with Mr. Bentham's identification of this species with *Ainsliæa angustifolia* Hook. f. et Thoms.—Notes on Indian Gentianaceæ, by C. B. Clarke.—On some Atlantic Crustacea from the *Challenger* expedition, by R. von Willemoes-Suhm. Communicated by Prof. Wyville Thomson, F.R.S. The paper is divided into seven parts as follows :—(1) On a blind deep-sea Tanaid ; (2) On *Cystosoma neptuni* (*Thaumops pellucida*) ; (3) On a *Nebalia* from Bermudas ; (4) On some genera of Schizopoda with a free dorsal shield ; (5) On the development of a land-crab ; (6) On a blind deep-sea *Astacus* ; (7) On *Willemoesia* (Grote), a deep-sea Decapod allied to *Cryon*.

Anthropological Institute, May 12.—Prof. Busk, F.R.S., president, in the chair.—Messrs. R. and S. Garrard and Co., of the Haymarket, exhibited a very interesting collection of gold objects recently brought from Ashanti. In the discussion Col. Harley, C.B., stated that the Ashantis, and indeed all the tribes of and near the coast, could originate nothing ; they were simply copyists, and from frequent repetition of European models, as well as of natural objects, they often attained great skill in the art. —Mr. Francis Galton gave some results of school statistics which he had obtained from Marlborough and Liverpool Colleges. If his applications for co-operation from other head-masters and assistant-masters were equally successful as from these two, he would soon have sufficient material

* Read June 20, 1873 : Phil. Trans. for 1873, p. 147.

to enable him to establish with certainty the law of growth of the English boys of the present date who are sons of professional men and clergymen and who are educated in the country and reared on the present system of diet and physical and mental work. The result so obtained would serve as a standard of comparison for future periods and for other countries and conditions of life.—A paper, also by Mr. Galton, was read, On the excess of female population in the West Indies.—A paper was read On the probability of the extinction of families, by Rev. H. W. Watson, with prefatory remarks by Mr. Francis Galton. The author remarked that it is not only the families of eminent men, or of the aristocracy, who tend to perish, but also those of municipal notabilities and others. The conclusion that was drawn was that an element of degradation must be inseparably connected with one of amelioration, and that our race is necessarily maintained chiefly through the "proletariat." The problem, which was one purely for the mathematician, was to ascertain what proportion of specified families will necessarily become extinct after a few generations. It would be easy then to measure the diminution of fertility by the frequency of extinction.—Major Godwin-Aussten contributed a paper On the rude stone monuments of the Nágás.

Geologists' Association, May 1.—Prof. Morris, vice-president, in the chair.—On some Carboniferous Polyzoa, by Robert Etheridge, jun. The author showed that, until recently, *Synocladia* was known in this country only from rocks of Permian age, being one of the characteristic corallines of the magnesian limestone. From the Carboniferous series of America, however, a species had been described under the name of *S. biserialis*, agreeing in general habit with the typical *S. virgulacea*, but in some essential characters differing widely. From the Scottish Carboniferous series the author had recently described a species of *Synocladia*, which he termed *Carbonaria*, but which he now believes to be only a well-marked variety of the American Permio-carboniferous *S. biserialis*. The author then proceeded to notice the occurrence of *Polypora* and *Thamniscus* in the Scottish Carboniferous rocks, and concluded by drawing attention to the increasing number of forms, which are gradually becoming recognised as common, in our own country, to the Carboniferous and Permian formations.—On some geological puzzles, by Ed. Charlesworth, F.G.S. Out of many hundreds of teeth of terrestrial mammals, as *Sus*, *Castor*, *Tapirus*, *Felis*, *Hipparion*, *Cervus*, *Bos*, &c., which have been discovered in the red crag of Suffolk and Essex, all, with three or four exceptions, are molars. No bones are found along with the teeth of these land animals. This we can understand, as teeth are so much the hardest parts of the animal frame. There is, however, one curious exception. The *Astragalus* of one or more species of deer is far from uncommon in the red crag. The teeth most abundant in the red crag are those of various kinds of sharks; some of these have a circular perforation, not unlike that made by South Sea islanders in the teeth of sharks at the present day. The occurrence in the red crag of certain stones of a cylindrical form, generally abruptly truncate at one extremity, and having a central cylindrical canal passing through the long axis. Though exhibiting transverse segmental division, if struck with a hammer, they do not separate at the segmental lines. That they did so once may be inferred, from the occurrence of detached segments throughout the crag. The phragmocone of the Belemnite is never found in chalk, or chalk flint, though the guard is extremely abundant. The nature of the cylindrical body, which is occasionally observed to pass in a spiral direction through the body of the Choanite. When a chalk Echinite is filled with flint, but not enveloped more or less in that substance, it is found that the calcite of the shell is partially replaced by silica. This does not occur in those parts of the shell which have flint on the outside.

GLASGOW

Geological Society, April 16.—Mr. E. A. Wüncsh, vice-president, in the chair.—Dr. Robert Brown, F.L.S., read a paper On the Noursoak Peninsula and Disco Island, North Greenland.—Mr. David Robertson, F.G.S., then read a paper On the Recent Ostracoda and Foraminifera of the Firth of Clyde, with some notes on the distribution of the Mollusca. The author said there appeared to be too much readiness to adduce climatal change as a cause of varieties in the fauna, which might only be the consequence of local circumstances. For example, *Terebratula caput-serpentis*, an arctic species, is well-grown and abundant in Loch Fyne, but dwarfed and rare at Cumbrae, in the same depth of water and on similar bottoms, which must be attributable to conditions of habitat and not of climate. With regard to the minuter organisms, Mr. Robertson mentioned a remarkable fact, that they are found in greater abundance in many places exposed to the tossings of the sea than in more sheltered bays and lochs. There can be no doubt that such circumstances as the depth of water, the force of currents, and the condition of the sea-bottom, whether it afforded a suitable habitat for certain species, supplying the food best fitted for their healthy development, as well as furnishing them with a degree of immunity from their enemies, such circumstances, often not easily cognisable, would affect the distribution of animal life in the seas of any given period, and account in a great measure for the absence or sparseness of certain species in one locality and their abundance in another.

PARIS

Academy of Sciences, May 11.—M. Bertrand in the chair.—M. J. A. Serret communicated some remarks on the note by M. l'Abbé Aoust, inserted in the *Compte rendus* of the last meeting.—M. Jamin presented a paper On the internal distribution of magnetism in a bundle composed of several laminæ.—On the carpellary theory according to the (order) Hippocastanæ, by M. A. Trécul.—General ideas on the mechanical interpretation of the physical and chemical properties of bodies, by M. A. Ledieu.—On the permanence of the intensity of the calorific radiation of the sun, by M. A. Duponchel, a defence of a previous memoir criticised by M. Faye.—Memoir on the determination of the true simple bodies by the actions of electric currents in the voltameter, by M. E. Martin. The author considers the two electricities as imponderable bodies endowed with powerful and opposite chemical affinities, and states views concerning the compound nature of the gases obtained from water by electrolysis, which differ but little in principle from the old theory of phlogiston.—On the mechanical employment of heat, by M. G. West. The author held out hopes of the possibility of utilising the waste heat of engines.—On albuminoid matters, by M. A. Commaille. The author restated the results of his researches on these bodies *àpropos* of M. Béchamp's recent note on the subject. The bodies in question are representable as amides of capronamic acid and of tyrosine, which is the amide of acetobenzoic acid ($C_{18}H_{11}O_6N$).—M. F. A. Abel presented the continuation of his third memoir on the properties of explosive bodies.—Researches on coniferine. Artificial formation of the aromatic principle of vanilla, by MM. F. Tiemann, and W. Haarmann. The formula assigned to coniferine is $C_{16}H_{22}O_8$ + 2 Aq. The substance is a glucoside decomposing in the following manner:—

$$C_{16}H_{22}O_8 + H_2O = C_6H_{12}O_6 + C_{10}H_{12}O_3.$$

This last product of fermentation ($C_{10}H_{12}O_3$) when oxidised by a mixture of sulphuric acid and potassic dichromate gives aldehyde and a crystalline substance identical with the aromatic principle of vanilla having the formula $C_8H_8O_3$. The reaction was thus represented:—

$$C_{10}H_{12}O_3 + O = C_2H_4O + C_8H_8O_3$$

—On the absolute magnetic declinations observed on the Adriatic coast, by M. Diamilla-Müller.—Observatious relating to the memoir by MM. Crocé-Spinelli and Sivel on their (balloon) ascent of March 22, by MM. Lartigue. The facts observed by the aëronauts mentioned, confirm the author's view of the origin of the wind known as the "mistral" which may be generally explained by the great difference of temperature existing between the torrid zone and the temperate and glacial zones.

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