

all calculations of dimensions depend, and therefore it will be valuable as an introduction to works concerned with special practical applications of the rules described and exemplified. Moreover, it should play a useful part in schools, by illustrating the concrete applications of abstract geometrical principles. The large number of original examples will be found of great assistance by teachers, and the questions, selected from papers set by the principal examining bodies, will prove of service as tests of the students' capabilities in working out mensuration problems.

Of a less detailed character is the Rev. Dawson Clarke's primer, intended "for the use of schools, and Woolwich, Sandhurst, and Home Civil Service candidates." The book is a collection of rules and formulæ, with examples to explain their use, and numerous exercises selected from various examination papers. It particularly appeals to students who learn the rules of mensuration in order to utilise their knowledge in the examination-rooms of the Civil Service Commissioners; but it is, also, a concise text-book which other students will find serviceable.

Physical Measurements. By Frank C. Weedon. Pp. 232. (London: G. Gill and Sons, 1895.)

THIS volume is another help towards the establishment of rational methods of instruction in elementary science. It is a laboratory manual of practical physics for organised science schools under the Department of Science and Art, and other secondary schools. Of the educational value of the course contained in the book, there can be no doubt; for the experiments (which are of a character suited to beginners) follow a natural order, and are such as will develop the faculties of observation, investigation, and common sense; in fact, they will lead the student to think as well as learn. The book is divided into three sections, dealing respectively with measuring and weighing, relative densities, and experimental mechanics. Experiments on these matters elucidate the fundamental principles which form the basis of a scientific education. The knowledge cannot be labelled "Sound, Light and Heat," or "Magnetism and Electricity," and therefore superficial critics, and syllabus-bound teachers, think it is not Physics. We are of the opinion, however, that experimental work in measuring and weighing, constitutes the foundations of physics. The student who is able to weigh and measure carefully, and to observe and think accurately, knows more of the realities of physical investigation than if he had spent a dozen years in learning scraps of information about other people's contributions to knowledge.

LETTERS TO THE EDITOR.

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The New Actinic Rays.

A BRIEF account of some experiments which I have been making in my laboratory at Blythwood, in connection with the new photographic rays, may, I hope, be of interest to the readers of NATURE.

Three or four years ago I constructed a very powerful Wimshurst electrical machine. It has 128 plates, three feet in diameter, and is driven by an electric motor of about $1\frac{1}{2}$ horsepower. With this machine, which was specially built for quantity, I can obtain a torrent of sparks a foot and a half or two feet long: and it occurred to me to try to obtain photographs, after the manner of Röntgen, but without the intervention of a vacuum tube.

A thick sheet of lead was placed upright between the poles of the electric machine, as a screen, and was connected to the

ground, the two poles being insulated. A sensitive dry plate was put into the camera dark slide, with a metallic object to be photographed (a steel washer with holes in it), and this was connected, by a wire which passed out of the dark slide, to the ground. The whole was wrapped up in four folds of a black velvet focussing cloth, and was put, in some cases between the negative pole and the lead screen, and in other cases between the positive pole and the lead screen, the plane of the slide being perpendicular to the line of discharge. In all cases good strong negatives were obtained with exposures of about twenty minutes. The machine was arranged to give a silent brush discharge during the experiments.

I next tried similar experiments with the dark slide containing the sensitive plate quite out of the line of discharge, and with the plane of the plate parallel to the line of discharge, and obtained equally good results. It seems, therefore, that the vacuum tube is not essential to the production of the Röntgen rays. With reference to this, however, I am not so sure, as I think I may have been deceived by using isochromatic plates—at all events I am engaged in further experiments either to confirm or the contrary.

BLYTHWOOD.

Blythwood, Renfrew, February 10.

WITH reference to Mr. Porter's letter regarding the amount of electric energy and exposure required for obtaining photographs by the Röntgen method, I may mention that against his photograph taken with a 3-inch coil and four minutes' exposure, I can instance a successful human foot that shows the bones very distinctly almost up to the ankle-joint, in taking which I used a 10-inch coil working at about half power without Leyden jars, and for which fifty-five seconds' exposure proved ample.

For living physiological subjects, it is very important to shorten the exposure as much as possible, and to attain a minimum in this respect, very high vacua and considerable E.M.F. are requisite.

Again, for an extensive subject, a large tube placed at a considerable distance from the subject is required, and more electric energy is needed for this than for a small subject, for which a smaller tube in closer proximity will suffice.

66 Victoria Street, S.W.

A. A. C. SWINTON.

HAVING made some experiments on the lines laid down by Mr. Gifford, of Chard, I think the two enclosed photographs will prove of interest, as showing perhaps that Mr. Gifford's method of dispensing with a Crookes' tube introduces elements of another character. Both these negatives were taken without a tube, using the discharge from the terminal of a small Tesla transformer. In each case a metal plate was placed behind the film in communication with the other terminal of the coil. Under these conditions a stream of "discharge" passes from one terminal through the photographic film.

The interesting point is that not only does the outline of the coin come out, but also the impression. And that in the case of the florin the coin was placed *behind* the film. The same sparkling appearance as described by Mr. Gifford is evident.

From the fact that it is immaterial on which side of the photographic film the coin is placed, it is evident, I think, that we have here to do with a "contact" phenomenon, and not with Röntgen's rays at all.

SYDNEY D. ROWLAND.

38 Wimpole Street, W., February 2.

"The Astronomical Theory of the Glacial Period."

As it was my two letters which initiated the interesting and not unfruitful discussion now going on in your pages on the above subject, I think it right to say a few words in reply.

The object of my letters was to point out (perhaps I did it in somewhat too heated language) that Sir Robert Ball, whose personal and official distinction give his words exceptional weight, had in his work entitled "The Cause of an Ice Age" given fresh currency to a discredited theory, and further that when this had been pointed out, he had refused to take any notice of his critics, and continued to publish his book.

In his letter to you, Sir R. Ball (if I do not misunderstand him) entirely breaks away from the position maintained in his book, and gives up the case there argued, definitely and completely. While Prof. Darwin, who had given the book the advantage of his friendly recommendation and countenance, tells us he is now reluctantly compelled to take the other side.

So far as I know, there does not now remain a single mathematician or astronomer who favours a purely astronomical theory of an Ice Age; a theory which, as Arago, Humboldt and Croll, all urged long ago, is quite inadequate to explain the climatic effects required. Every one, as far as I know, now agrees with the American astronomer Meech, who subjected the astronomical theory to a most searching analysis, as far back as 1857, that "the causes of notable geological changes must be other than the relative position of the sun and earth under their present laws of motion." It is with this sentence that I close my own analysis of the problem in chapter ix. of my "Glacial Nightmare."

As I understand, Sir R. Ball in surrendering his old view, which was *that astronomical causes by themselves* are sufficient to produce an Ice Age, falls back upon a modification of Croll's meteorological argument. While, however, he no longer relies on the adequacy of astronomical causes alone as competent to produce an Ice Age, he does not admit the conclusiveness of Mr. Culverwell's argument, but bids us remember that the world cannot be cut up into a number of parallel zones shut off from each other by solid partitions, each one of which can be treated as a separate climatic region, but that the climate of every zone is very largely indeed the result of heat brought in or carried away by air and water from or to other zones. No one disputes this. It is in fact an elementary postulate of meteorology, and applies as much to Sir R. Ball's arbitrary zone termed a hemisphere as to any other.

What we want Sir R. Ball to do is not to rest content with this barren postulate, but to apply it as Croll applied his postulates, and to prove that, granting the greatest possible alteration of the relative length of the seasons due to eccentricity, &c., which, as Mr. Culverwell has shown, will by itself tend to shift the climate of each zone about five degrees, how is this going to affect the circulation of the air and of ocean currents sufficiently to constitute an Ice Age? This was the problem Croll virtually set himself to analyse by a minute and ingenious investigation.

Croll's arguments have been riddled through and through by several writers, and in this behalf I may perhaps venture to again refer to a minute dissection of them in a chapter, headed "Transcendental Meteorology," in the work already cited, namely, the "Glacial Nightmare," and which I have been told by some eminent physicists is unanswerable. I can, at all events, say it has not been answered.

If Dr. Ball can discover some method of curing the radical defects of Croll's arguments, he will have made us a valuable present. Meanwhile, if I do not entirely misunderstand his present position, it is more clear than ever that he owes it to us all to withdraw his "Cause of an Ice Age" from circulation, for it has not only been condemned by its distinguished and formerly friendly critic, but has been actually condemned by its own author.

HENRY H. HOWORTH.

Athenæum Club, January 30.

The Positions of Retinal Images.

PROF. KULPE, in his "Outlines of Psychology" (translation by Prof. Titchener), sets out with much effectiveness the argument in favour of believing that the visual perception of extended surface is an original datum of consciousness attached to the extended retinal surface (and no more to be explained than why the sensation red feels the way it does, and not otherwise); and he also shows conclusively that the peculiarity of nerve-excitation by which right- and left-ness and up- and down-ness are distinguished, is of peripheral (and not of central) origin; by adducing the facts of metamorphopsia, that is, the cases in which a portion of the retina has become detached by a wound, and has afterwards grown on again, and in which vision is correspondingly inverted—exactly as when a piece of the skin of the forehead has been grafted upon the nose, say, and upon touching it we seem, for a long time afterwards, to be touching the forehead. He thus attaches himself to the innate-space-sensation theory of James and Sumpf. But his effort to show that the out- and in-sensation is fundamentally dependent upon the different shape of the image cast upon the two retinas by an object, carries less conviction with it. This is, of course, an essential element of the sensation when the object looked at is so complex as to consist of two points at a given distance from each other. But when it consists of a single bright point only, we are still perfectly able to determine its position in depth (if it is looked at with two eyes), and the sensation-element which enables us to do this is plainly more *fundamental* than the other.

To say the least, it is something which ought not to be overlooked.

It is plain that in this case the only criterion which is left us (granting, what is the case, that the localisation can be effected with certainty with two eyes, but only vaguely and indefinitely with one) is *the distance apart of the double images*; it is that which we estimate, unconsciously of course, in spite of the fact that one image is in one eye and the other in the other, and it is that which we translate, without difficulty, into a feeling of depth. But there is always an ambiguity; for every point, O (Fig. 1), without the horopter-circle, which casts images upon the retina at the points r and l , there is a congruent point, O', within the horopter-circle, which casts images upon the corresponding retinal points, l' and r' , and which, therefore, gives images which are at the same distance apart. We have no difficulty in sensation in distinguishing between a bright point at O and one at O', but how can this be effected? There is still a difference in sensation between the two cases. The nasal half of each retina gives distinctly brighter images than the temporal half; in the case of the object O, which gives the two images, r and l , the remoter one is the brighter, while in the case of the object O', which gives the images l' and r' , it is the nearer one which is the brighter. A bright image of the object, which seems to us to be the thing itself, is attended by a somewhat fainter secondary self, whose presence we are absolutely unconscious of, in our non-scientific lives, as an image,¹ but which we evaluate with the utmost nicety as a sign of the distance away of the real object,

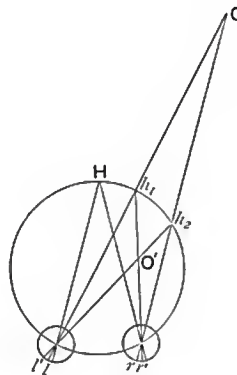


FIG. 1.

and which has, moreover, a different significance according as it stands nearer to, or further from, the fixation point than the image which we regard as the object itself. This explanation may seem at first to vary much in the air, but its correctness has been demonstrated by Schön in a very ingenious manner (*Arch. f. Ophthalm.*, xxii. and xxiv.). His experiment has been unaccountably overlooked by all the makers of text-books, as far as I have seen, but it is of critical importance. He arranges a series of screens with openings in them in such a way that *two different* bright objects are seen, one by the right eye only, in the line $h_2 O$ produced, and the other by the left eye only, the line $h_1 O$ produced. The *positions* of the double images now correspond equally well for an object at O or at O'; and the person experimented upon thinks he sees an object now at O and now at O', exactly in accordance with the way in which the relative brightness of the objects beyond the screens is made to vary. When the image which falls at r is brighter than that which falls at l , the object is seen at O; when the image which falls at l is brighter than that which falls at r (sufficiently brighter, of course, to counteract the relative efficiency of the different halves of the retina) the object is seen at O'. It is therefore demonstrated that it is the relative brightness of the images which is the determining factor in enabling us to localise objects in one or other of the two congruent worlds without and within the horopter-circle. I have myself repeated the experiment with perfect success.

C. LADD FRANKLIN.

Baltimore, January 13.

¹ There are many people who cannot bring the secondary image into consciousness, no matter how hard they try, when it falls at any distance from the fovea.