the rabbit, or the guinea-pig. The frog is a very aberrant member of the Batrachia, and it would be very instructive for the student to examine a more typical representative of the class. Such a one is the American Proteus (*Necturus maculatus*, Ref.), used at Cornell University, Ithaca, N.Y. During the last few years I have received many specimens of Vertebrates from two fishermen—Mr. Russell Dee and Mr. F. C. Audibert, from Marietta, Ohio. Lately, Mr. Audibert has written to me that he could procure any quantity of material, if wanted, and he would charge only 25 cents. (a little over one shilling) for each specimen. From the list of specimens sent to me by Mr. Audibert I select the following, which appear the most important for laboratory use:—

Accipenser maculosus, Les.
Polyodon folium, Lat.
Lepidosteus osseus, L.
Necturus maculosus, Ref.
Menopoma alleghaniensis, Daud.
Trionyx muticus, Les.

The instructive value of these specimens is certainly very great, and the low price could enable any biological laboratory to secure this material. G. BAUR.

New Haven, Conn., September 30.

"Darwinism."

It has now become to me a matter of amusement to note how those naturalists who of late years have drifted most widely from the doctrines of evolution as these were held by Darwin, habitually accuse me of Darwinian heresy because I have not seen any adequate reason to depart from those doctrines in their entirety. Perceiving that there has been some change of relative position, while failing to perceive that the movement has been altogether on their own side, these naturalists represent that I have been falling away from Darwinism, when the fact is that they have been advancing beyond anything that was ever countenanced by the judgment of Darwin—and even expressly accepting the view which he so vehemently rejected, viz. that of regarding natural selection as the sole cause of organic evolution. Thus, for example, when in NATURE of October 10 (p. 569) Prof. Ray Lankester gravely designates my paper on physiological selection a "laborious attack upon Darwin's theory of the origin of species," it becomes evident how fast and far he has travelled from his Darwinism of two or three years ago. For, to put it briefly, unless it can be shown that Darwin considered natural selection the only possible cause of organic evolution, and did not consider sterility between allied species as probably due to some other principle of change, it is obvious that there *can* be nothing in my "additional suggestion on the origin of species" which may in any sense be designated an attack upon the distinctively Darwinian theory. Yet it is with regard to these very points that the opinion of Darwin was steadily opposed to that of Wallace; *i.e.* to the present opinion of Lankester. Therefore, quite apart from any question touching the truth of this "additional suggestion" or "supplementary hypothesis" (which, however, I may here parenthetically remark, will soon be shown to be in no way seriously affected by Mr. Wallace's sole remaining criticism), it is sufficiently evident that, when the object of publishing the hypothesis was expressly and repeatedly stated to have been that of meeting the main difficulties which had been advanced against the theory of natural selection, the present designation of this hypothesis as an elaborate attack upon that theory is simply absurd.

But my object in now writing is to state, *apropos* of Prof. Lankester's remarks on the inadequacy of Mr. Wallace's criticism of Mr. Gulick's paper, that I have just received a communication from the latter gentleman (who writes from Japan), requesting me to exercise my discretion as to publishing in these columns a reply to that criticism. Unfortunately this reply is too long for insertion, and as I do not see how it can be curtailed without serious detriment, I have refused to incur the responsibility of publishing it in an abbreviated form. At the same time it seems but just to let the readers of NATURE know that a full reply to Mr. Wallace's criticisms (in these columns and elsewhere) has been prepared; since otherwise the silence of its author might be misinterpreted.

To me it appears that Mr. Gulick's work is much the most profound that has ever been published on the important matters of which it treats (viz. isolation in all its forms, with its consequences in "segregate breeding" and "divergent evolution"); and therefore I am glad to take this opportunity of recognizing his priority, by some fifteen years, in thinking out, and largely verifying by his researches on land shells, the theory of physiological selection. GEORGE J. ROMANES.

Geanies, Ross-shire, October 12.

Sunset Glows.

It is a curious fact that a revival of sunset-glows, similar to those described by Sereno E. Bishop in a letter published in NATURE for August 29 (p. 415), was observed in Western New York at almost precisely the same time that he saw them at Honolulu. I inclose a clipping from the *Rochester* (N.Y.), *Democrat and Chronicle*, which was published on July 21:—

"The skies at evening show signs of the gradual return of the red light. It will be of interest to ascertain if the phenomenon reappears as the solar disturbances continue to increase in energy to the maximum. It is quite apparent now that the minimum has been passed, and the tendency is toward an increase in the number and in the violence of solar disturbances. There are certainly three and probably four well-defined disturbances at present." M. A. VEEDER.

Lyons, New York, September 13.

"The Teaching of Science."

I beg that the following alterations may be made in the "Suggestions for a Course of Elementary Instruction in Physical Science," printed in NATURE of October 17.

HENRY E. ARMSTRONG.

P. 602, Problem II., line 11 from above, read "by means of iron" instead of "by means of phosphorus."

P. 603, Problem VII., line 20 from below, instead of "when metals are heated with acids," read "when metals are dissolved in acids."

P. 604, Problem IX., line 31 from above in right-hand column, read "dried hydrogen," instead of "dried oxygen."

P. 605, Problem XII., line 17 from above in right-hand column, read "zinc oxide," instead of "lime oxide."

TELESCOPES FOR STELLAR PHOTOGRAPHY.[1]

II.

In considering the essentials of a good system of control for equatorial clocks, it is necessary to keep in view the exact conditions required. It is not sufficient that the controlling apparatus (of whatever form it may be) should simply bring the *rate* of the clock, which has been interfered with by some adventitious disturbance, correct once more; it must do more, it must correct this error. For, suppose a star be set on the slit of a spectroscope, and the clock started, and say, as in Dr. Huggins's case, a photographic plate inserted for a two hours' exposure. Now suppose that five minutes after the commencement of the exposure, an error of one-tenth or two-tenths of a second occurs from some disturbing cause (a fragment of dirt on the tooth of a wheel, or other cause); if the controlling apparatus be of such a nature as simply to bring the clock-rate correct again, the position of the telescope will be the above quantity, one-tenth or two-tenths of a second, in error for the remainder of the exposure, although the rate may be absolutely correct for the whole times. In other words, the star will have moved off the slit, by a quantity equivalent to what the instrument would move in one-tenth or two-tenths of a second, and will continue off the slit for the remainder of the two hours. So it will be seen that no controlling apparatus is of any use whatever, unless, as well as keeping the rate uniform, it corrects the errors that have crept in. In consequence of not keeping this point in view, many most ingenious but useless arrangements have been from time to time proposed. A little consideration will show that this arrangement meets all requirements.

The above arrangement is somewhat similar to Dr. Gill's.

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[1] A Paper read by Sir Howard Grubb, F.R.S., before the Society of Arts, on April 18, 1888. Continued from p. 444.

It is simpler to attach to any existing clock, but not so delicate as his, and is open to the same objections. It is, however, capable of very good work, as may be judged from the chronograph sheet of the Dunsink Observatory chronograph.

The third is the form of control which I devised for Mr. Isaac Roberts, and which has been so successful with him, and with Prof. Pritchard (who has had it recently attached to the Oxford equatorial), that photographs have been exposed with the telescope to which it has been attached for fifteen minutes, and yielded perfect images of stars without any hand and eye guiding.

The arrangement consists, firstly, of a *remontoire* train, driving a good mercurial or other compensated pendulum—the driving of this train being of course entirely independent of the equatorial clock giving motion to the telescope; secondly, of a detector apparatus, which detects any difference between the rate of this standard pendulum and the equatorial clock; and thirdly, of a correcting apparatus, which corrects automatically any error discovered by the detector. This corrector itself consists of two parts—an "accelerator" and a "retarder"—and these we will first proceed to describe.

In S S′ S″ is one of the shafts, between the driving train of the equatorial clock and the worm which drives the right ascension sector, this shaft being cut into three parts, denoted by the letters just named. At one end the portion S of the shaft carries a wheel, 1, immediately adjoining which is the wheel 2, mounted on the portion S′ of the shaft. At the other end of this last-named section of the shaft is fixed a third wheel, 3, which is almost in contact with the wheel 4, fixed on the end of the shaft S″. The shafts S and S′ also have mounted freely on them the brass disks, *d d′*, which adjoin the two pair of wheels referred to above. Each of these brass disks is furnished with a stud on which a small pinion is mounted, the pinion *p*, belonging to the disk *d*, gearing across the pair of wheels, 1-2; while the pinion *p*, belonging to the disk *d′*, gears across the pair of wheels, 3-4.

Under normal conditions, if no error exists in the equatorial clock rate, the arrangement of wheels and pinions just described revolves as one piece, the three sections, S S′ S″, of the shaft rotating at the same speed; but it is possible by an arrangement which we shall explain presently, to stop the rotation of either of the disks, *d d′*, and as soon as this occurs the pinion of the stopped disk has to act as a transmitter of motion from one of the wheels into which it gears to the other. If the two wheels of each pair had the same number of teeth, the speed of both wheels would still remain the same, but in reality the number of teeth in the two wheels of each pair is different, and hence the stopping of one of the disks, *d* or *d′*, causes a variation in the rate of rotation of the two adjoining wheels relatively to each other. For instance, in the case of the first pair of wheels, let wheel 1 have 30 and wheel 2 have 29 teeth, and suppose that the shaft S is rotating once every 60 seconds. Thus, if the disk *d* be stopped, the wheel 2 will be made to revolve in $\frac{29}{30}$ of the time occupied by the wheel 1, or in other words the rate of the section S′ of the shaft will be accelerated to one revolution in 58 seconds. In the same way by reversing the positions of the wheels in the other pair 3-4, the stoppage of the disk *d′* can be made to effect a retardation of the portion S″ of the shaft relatively to S′. The edges of the disks *d* and *d′* are cut into very fine teeth, and the stoppage of the disks when desired is effected by causing a comb attached to the armature of an electro-magnet to engage with these teeth.

The whole apparatus just described constitutes a very convenient arrangement for accelerating or retarding the driving motion imparted to the telescope by the equatorial clock, and that it is capable of very good work is shown by the photographs which have been taken by Prof. Pritchard and Mr. Roberts, in which the star disks are perfectly round, though exposed for 15 to 60 minutes, and no hand guiding used.

I have now to describe how this apparatus is, when necessary, automatically brought into action by the "detector."

In Figs. 5, 6, and 7,[1] W is a scape-wheel mounted on the sixty-second spindle of the controlling clock, and driven from that spindle through a spiral spring, X X, so that no error in the equatorial clock can affect its rate or that of the standard pendulum. On the same spindle there is also mounted behind the scape-wheel an ebonite disk, E E, Fig. 5; this disk, which is driven by the equatorial clock, carrying two insulated rings, *b b′*, which are respectively connected metallically with two platinum plates, B B′, inserted in the face of the disk. Between the scape-wheel and the ebonite disk there is also mounted loose on the spindle a lever, A A, which carries at one of its ends a platinum bridge, B, which is of such a length as to fit between the platinum plates, B B, and which in its mid-position bears against a piece of rock crystal let into the ebonite disk between the two plates just named. At the other end the lever, A A, is formed into a fork, between the arms of which projects a pin carried by the scape-wheel; the arms of the fork are provided with set screws, by means of which the amount of play allowed to this pin in the fork can be adjusted.

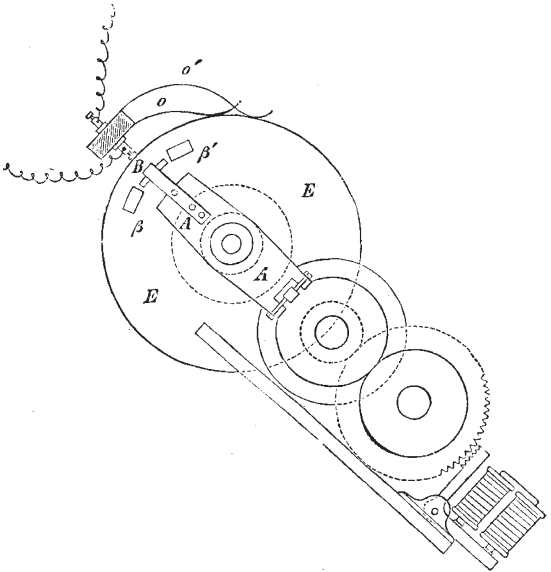

FIG. 5.

The insulating rings, *b b′*, are electrically connected with the accelerator and retarder already described by means of fine platinum wires, O O′, wiping against them, and the action of the whole arrangement is as follows. The scape-wheel, W, being driven by the control clock, has an intermittent movement corresponding to the beats of the pendulum, while the ebonite disk, E E, being driven by the equatorial clock, has a constant movement, so that even if the scape-wheel and disk make a whole revolution in the same time, the pin carried by the scape-wheel will be constantly oscillating between the pins of the fork at one end of the lever, A, this lever being driven by friction from the ebonite disk. The pins just named are adjusted so as to allow of this oscillation taking place without interference, so long as the rates of the equatorial and control clocks remain uniform, but if the equatorial clock either loses or gains with respect to the standard, the pin on the scape-wheel comes into contact with one of the fork pins of the lever, A, and shifts that lever on the spindle, bringing the bridge, B, into contact with one of the platinum plates,

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[1] These blocks have been kindly lent by the editor of *Engineering*.

B or B′, and transmitting a current which brings into action the accelerator or retarder as may be required. The period during which the accelerator or retarder remains in action will depend upon the amount of the error to be corrected, and the proportions of the pairs of wheels, 1, 2, and 3, 4. With the proportions described above, the correction introduced is one-thirtieth of the rate, so that, to correct an error of one-fifth of a second, the accelerator or retarder, as the case may be, would have to remain in operation $\frac{30}{5} = 6$ seconds. As soon as the correction has been made, the lever, A, will resume its normal position, and the bridge, B, coming then between the two platinum plates, B B′, a current will cease to be transmitted, and the accelerator or retarder thrown out of action.

It is to be noted that the apparatus above described not only corrects any temporary disturbance of the equatorial clock rate, but cancels errors which have already occurred.

It will be seen that the third form of control is free from the objections of the first and second. The detector part of the apparatus is close to the screw spindle, only removed from it by one pair of wheels, and the correction is not applied in the same manner by checking the speed of the clock, but by introducing a differential gear, which acts until the error be cured, and then drops out of gear automatically.

The fourth and last form of control, however, is that to which I would invite your special attention, for I believe it to be capable of results beyond all the others.

I have endeavoured in it to select all the good points of the other forms, and to combat the weak points. I may not have as yet produced it in as perfect a form as is possible, but I am satisfied it is capable of development into a very perfect control, and even at present it is the most perfect I have constructed.

As long as the control applied its correction by altering the speed of the governor, it was necessary to keep down the *vis inertia* of the governors, but now, as the correction is not applied in this way, I have made the governor very heavy, and running at a very high speed.

The *vis inertia* of the governor is represented by some 10,000 foot-pounds per minute; consequently it is little affected by any small or short differences in friction or driving powers.

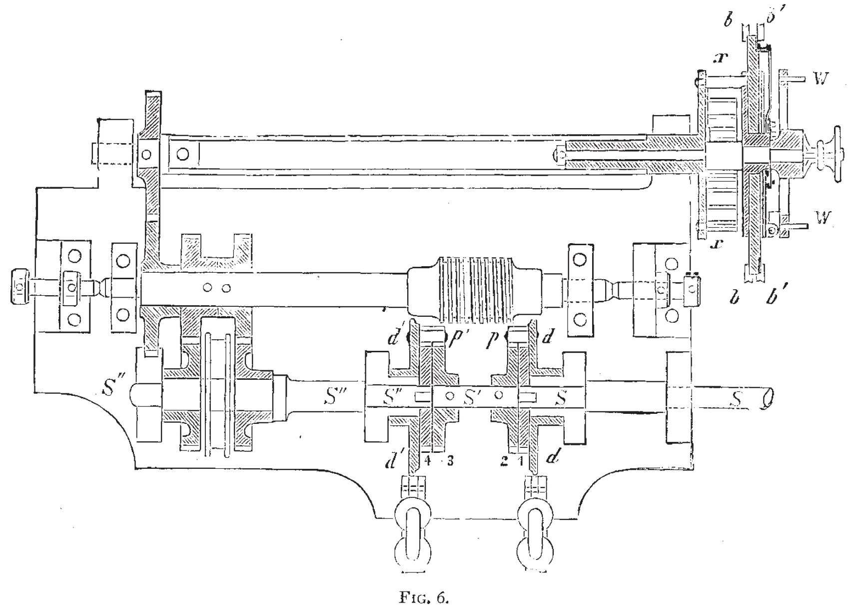

Fig. 6.

Again, at the suggestion of Dr. Gill, I make the governor spindle gear directly into the counter spindle of the screw, in order to have as few wheels and pinions to deal with as possible. Errors of wheels behind the governors have nothing whatever to do with the accuracy of its rate.

As to the nature of the control, I use Dr. Gill's form of detector and my own form of corrector, viz. accelerator and retarder. I use Dr. Gill's detector because it seems to be capable of being made on a larger scale than mine, and consequently ought to be more delicate.

I now propose to say a very few words on the optical part of the instrument.

The first question that naturally occurs is whether a refractor or reflector should be used. I own that when I first considered the subject I inclined to the belief that reflectors would be found to be the most suitable; and in a paper of mine, which was read before the Royal Astronomical Society last spring, I urged that comparative trials should be made before a final decision was arrived at. I have found reason, however, to modify my views on this point.

My reason for thinking that the reflector might possibly prove the best was founded on the consideration that in reflecting instruments rays of all refrangibilities are brought to a focus at one and the same point, whereas in the refractor rays of various refrangibility have different foci, and the best we can do is to so arrange the curves that those rays most active in impressing the photographic plate may be brought as nearly as possible to the one focus.

If we draw a curve which represents the position of the focal point for various rays of the spectrum in an object-glass corrected for photo work, it will be something like this figure (Fig. 8). The same for a reflector will be represented by a straight line. Looking at these curves it is certainly a natural conclusion that the reflector ought to be best, and therefore it was that I urged that a fair comparative trial should be made between the reflector and refractor as to their suitability for this work.

The Congress, however, decided upon the use of refractors, from the simple fact (as Dr. Gill says) that the best work done (to that time) had been done by refractors, not taking into consideration the very much more favourable conditions under which the refractor photographs were taken. Since that time further experiments have been made with both forms of instruments, which tend to show that, as against the refractor of the ordinary construction, the reflector can well hold its own, but that while it is obviously impossible for the optician to improve the field of the reflector, it is by no means impossible to do so with the refractor, and time and patient experimenting have shown that, by a modification of the curves of an objective, equally good definition of the central pencil can be obtained, combined with a very much better and flatter field, so that, however well reflectors could compete with the ordinary form of refractor, they cannot do so with forms constructed with special reference to field.

It should be borne in mind that the question of field is one which the optician was never before asked to consider in telescope objectives. The field of such a sized telescope used for visual work would not be more than $\frac{1}{2}$°, even with the lowest power. It has been found possible to obtain good definition over a field of 2° with either reflector or the ordinary form of refractor, and with the modified form considerably more. This question of field is, as I said, a very important one, for on it depends the amount of time required to complete the survey. If one instrument gives equally good definitions over 3° square, *i.e.* nine square degrees, as another does over 2° square, *i.e.* four square degrees, it is evident that the first instrument, equally energetically worked, is capable of completing the survey in less than half the number of years it is possible to do with the second instrument.

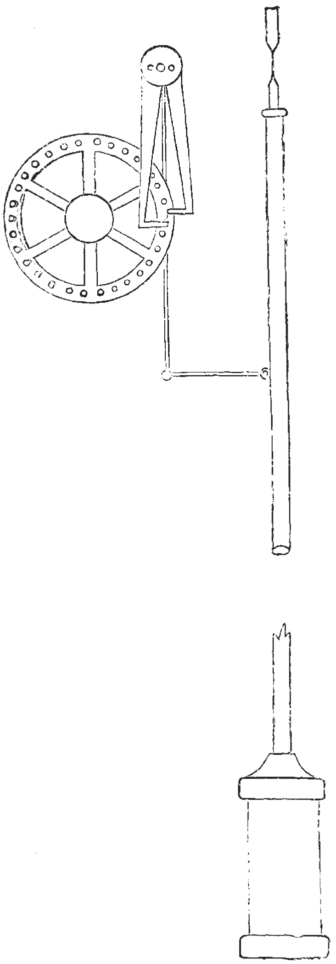

Fig. 7.

There is one point connected with this question of field which is of great importance.

Various forms of objectives give various characters of images of star disks at the edge of the field. Some give a bright nucleus with a tail like a comet, some assume a form approaching to a cross, and some give elliptical disks.

Of course perfection would mean absolutely circular disks all over the plate, but when this cannot be obtained, the last or elliptical disks are very much preferable to either of the others. It is quite possible to fairly estimate the most central point in an ellipse if the illumination over it be tolerably equal, but in the case of the comet or irregular form this is not possible.

The newer forms of objectives are peculiar in that the distortion of the lateral star images are of their least objectionable character.

It has been suggested that good results might be obtained by using the new "Jena" glass with rational spectra, but I have made inquiries respecting this, and it is not considered by the makers themselves that this glass in its present state would be suitable for the purpose of these photographic objectives. Until the permanency of that glass be thoroughly tested by exposure to various climates for some years, it does not appear safe to use it for such important work as this. There can be little doubt, however, that for ordinary visual work this glass, if capable of being made into large disks, will allow of the production of objectives superior to anything hitherto made.

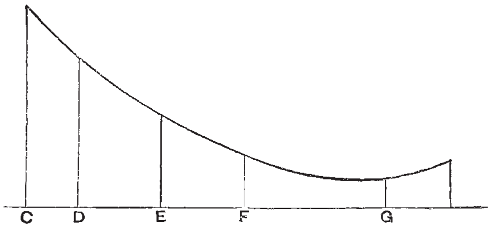

Fig. 8.

I ought perhaps to mention that it is possible to make an objective which can be adjusted to work either as a photographic or visual objective.

By separating the lenses of an ordinary objective, the chromatic correction can be so much reduced as to render it suitable for photographic work, but unfortunately the spherical aberration is reduced at the same time and definition destroyed.

Prof. Stokes, however, suggested to me that by constructing an objective in such a manner that the crown lens would be unequally convex by a certain amount, and that the spherical aberration would be correct when the flatter side was outermost, that then the chromatic aberration could be reduced as before by separating the lenses, and the reduced correction for spherical aberration raised by turning the crown lens with its more convex side outerwards.

This I tried, and found act perfectly.

I described this last spring at the Royal Astronomical Society, but it has since been re-invented in America.

It remains for me now only to describe how these new photographic objectives are tested.

The testing of objectives for visual work is a matter almost altogether of eye experience.

In these photographic objectives a different course must be adopted, as the image appears to be badly corrected to the eye when it is rightly corrected for photographic rays.

The Paris Congress decided that the rays G of the spectrum should have in their objectives a minimum focus; how is the correction to be verified? It is usual in the testing of the chromatic aberration of objectives, by those whose eye experiences cannot be a sufficiently accurate guide, to allow the image of a star, as found by the objective, to fall on the slit of a spectroscope, and to judge of the focus of each particular ray by the breadth of the spectrum at that ray. Wherever the light is brought to a focus the spectrum is of insensible breadth. When it is out of focus, a more or less sensible breadth, by moving the spectroscope in and out of the position of maximum and minimum foci, can be obtained.

I tried this plan, but found it very unsatisfactory, as it was very difficult to determine the exact place where the spectrum was narrowest, the curves being so very shallow.

After much thought I arrived at the following very simple and efficient plan, which I described in full, as it may be useful to some for other purposes. It should be remembered that the object is to get the focus of the objective for various parts of the spectrum.

If, therefore, we could obtain various objects when light was derived for such portions, and such portions only, of the spectrum as we required, our object would be accomplished.

I take a spectroscope with a fairly large dispersion equal to about 2 prisms of 60° and with a pencil of light of about 2° diameter. I remove the observing telescope, and substitute one of very long focus, so that the linear dimensions of the spectrum shall be as large as possible. I observe with this the solar spectrum, and note the position of such lines as I intend to work on. I then remove the eye-piece and insert in its place a tube carrying a small convex mirror. The apparatus is left till dark, and a small electric glass lamp attached outside the slit. The observing telescope is then placed at such a reading as I know will bring any certain line into the centre of the field, and on looking at the small mirror through a long slit which is purposely made on the top of the tube to allow it to be viewed from the front, you see a small bright star whose light is due to that particular line in the spectrum, and to no other part. The apparatus is placed at a sufficient distance in front of the photographic telescope, and these stars are the objects examined. In this way I can produce a small bright star of a colour corresponding to any of the lines in the spectrum, and the foci of these, as observed in the photographic telescope, can be measured with great exactitude.

There are, of course, small matters of detail which I have been unable to touch upon in the present communication, many of which are very important for the effective working of these instruments, and which require special treatment. I have, however, confined myself to the principal and more important parts, but I trust that I have been able to show that we have at least made a substantial advance; and it remains for us to hope that when these instruments are placed in the hands of astronomers they may yield a rich harvest of work, and leave their mark on the history of astronomical science.

HOWARD GRUBB.

ON THE PRINCIPLE AND METHODS OF ASSIGNING MARKS FOR BODILY EFFICIENCY.[1]

THE question to be solved is of this kind. Suppose that one man can just distinguish a minute test object at the distance of 25 inches, another at that of 35, and again another at 45 inches, how should we mark them? We should be very rash if we marked them in the proportion of 25, 35, and 45, or even if, for some good reason, we had selected 25 as the lowest limit from which marks should begin to count, we should mark them as 0, 10, and 20.

Two separate considerations are concerned in the just determination of a scale of marks—namely, absolute performance and relative rank, which are apt to be confused in unknown and varying proportions.

Absolute performance is such as is expressed by the 25, 35, and 45 inches just spoken of. It is perfectly correct in some cases to mark, or let us say to pay, for this, and this alone, upon the principle of piece-work—namely, that the pay ought to be proportionate to the work accomplished, or to the expected output in after life.

Relative rank is, however, on the whole, a more important consideration than the absolute amount of performance by which that rank is obtained. It has an importance of its own, because the conditions of life are those of continual competition, in which the man who is relatively strong will always achieve success, while the relatively weak will fail. The absolute difference between their powers matters little. The strongest even by a trifle will win the prize as completely as if he had been strongest by a large excess. Undertakings where many have failed, are accomplished at last by one who usually is very little superior to his predecessors, but it is to just that small increment of absolute superiority that his success is due. Therefore it is clear that relative rank has at least as strong a claim for recognition as absolute performance, if not a much stronger one. They have each to be taken into separate consideration, and each to be separately marked. The precise meaning intended to be conveyed by the phrase "relative rank" will be better understood further on.

Recurring to the example of keenness of eyesight, let the test object be words printed in diamond type, and the persons tested be Englishmen of the middle classes, between the ages of 23 and 26, then the performance of reading diamond type at 25 inches happens to be strictly mediocre. Fifty per cent. of the many persons who were tested performed better than this, and 50 per cent. performed worse. The 35-inch performance was exceeded by only 2½ per cent. of the persons tested; and as to the 45-inch performance, it has not in my experience been reached at all. I have had altogether 12,000 persons tested in this way, of both sexes and of various ages, but not one of them has succeeded in reading diamond type at the distance of 45 inches. It is very rare to find one who can do so at 40 inches. Wherever superiority in eyesight is eminently desirable, it would be absurd to make the marks for the three supposed cases to run proportionately either to 25, 35, and 45, or to 0, 10, and 20. The achievement of 45 inches would deserve much higher recognition. Relative rank and absolute performance should not be confused together.

I use the term relative rank in a large sense, with reference to all persons who have been, or are likely to become, candidates, and not to the small number of them who may happen to be present at a particular examination. Statistical tables concerning the class of persons in question have to be compiled from past examinations, and the rank of the individual has to be determined amidst these. I have often described how this is to be done ("Natural Inheritance," p. 38, Macmillan and Co., 1889), but the diagram (Fig. 1) is, I think, the simplest of all forms for the use of an examiner. It tells at a glance the rank held by a man among his fellows in respect to any single and separate faculty. The class from which it is constructed might consist of any large number of persons subject only to the condition that the distance between the limits *within which* it extends shall be always divided into centesimal grades; that is to say, running from 0° to 100°. The grades are printed along both the top and the bottom of the diagram, and refer alike to every line. As a specimen of the way to read it, let us take the line of keenness of eyesight among the males. Here we see that the performance of reading diamond type at the distance of 25 inches is appropriate

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[1] Read at the British Association, by Francis Galton, F.R.S.; but slightly revised, in order to introduce the diagrams herewith printed. Followed by remarks on experiments made at Eton College, by A. A. Somerville.