phenomena, there must be some corresponding difference in the circumstances of observation, and that when among five observers three see the phenomena exactly as we know they are, while two see them as we know they are not, and even then do not agree between themselves, there is a strong presumption that the latter do not see them rightly. I am aware of but a single attempt to determine experimentally the causes why one-third of the observers of the late transit, and many observers of former transits, saw the planet distorted, namely, that of Wolf and André, to which Mr. Stone alludes. They found, in observations of artificial transits under various circumstances, that when they used a telescope of at least twenty centimetres aperture, with a good object glass, well adjusted to focus, they saw only the geometrical phenomena of contact, while, if the object glass was small, or not well corrected for aberration, or not well adjusted to focus, they saw the phenomena of distortion.*

In the absence of farther investigation, which is much to be desired, these results seem to me, at least, to "indicate" that the phenomena in question are due to insufficient optical power, or bad definition either in the object glass or the atmosphere. At the same time I by no means insist on this proposition as established, and it is a great defect in the experiments in question that they do not extend to the effects of using shades of different degrees of darkness in observing the sun; but of this anon.

In his letter Mr. Stone quotes the observations of Chappé, Wales, Dymond, &c., in 1769; but I cannot admit that they bear strongly on either of the points in question, till we have some better evidence than now exists that their object glasses were such as Clarke or Foucault would call good.

Again, the argument from irradiation, if it proves anything, proves too much. I do not see why, upon the theory of Mr. Stone, the distortion should not always be seen. To be satisfactory, any theory of the matter must explain why it is that A, B, C, &c., see the phenomena, while X, Y, Z, &c., do not, and that of Wolf and André is the only one which does this.

Mr. Stone objects to the experiments of the Paris astronomers, that their disc was not sufficiently illuminated to exhibit any optical enlargement. I do not know his authority for this assumption, but, whether well founded or not, it seems to me that if the sun were viewed through a dark glass, it would present the same optical phenomena of irradiation with a disc so illuminated as to appear of the same brilliancy with the darkened sun. Thus, in the absence of evidence to the contrary, the Paris experiments may be taken as showing how the phenomena would present themselves in the case of the sun viewed through a shade of a certain (unknown) degree of darkness.

Before we can make any application of the theory of irradiation to the phenomena of contact, we require to know whether the irradiation of an extremely minute thread of light, darkened so as to be barely visible, is the same with that of a large disc. I am decidedly of opinion that it is not, and, if not, the fact that the sun's disc is optically enlarged by the telescope or the eye of the observer, cannot be directly applied to the phenomena of a transit.

To sum up my views;—neither Mr. Stowe nor any one else will claim that the ligament he saw before the time of internal contact was a celestial reality—he considers it a result of irradiation, but whether of telescopic or purely ocular irradiation I do not understand. If the former, this is simply a species of bad definition, and there is little difference between us. I also admit, on my part, that if the telescope and the eye are such that from any cause whatever an exceedingly thin thread of light presents itself to the sense as a band several times thicker than it really is, then, as the real thread becomes invisible, the seeming band will appear to be broken through by what some may consider a ligament and others a black drop, and the really sharp cusps will seem to be rounded off at their points. If I rightly understand Mr. Stone, he holds that these results of the thickening of the thread of light by what he considers irradiation are unavoidable. But this view is conclusively negated by the fact that they actually were avoided by a large majority of the observers of the late transit. Admitting, then, that these spurious phenomena are not unavoidable, it matters little whether we call their cause irradiation or bad definition, though it is important that we should know its exact nature. The only attempt I know of to determine this is that of Wolf and André, and their results seem to me so nearly in accordance with what we should expect, as to be quite worthy of acceptance, at least in the total absence of rebutting evidence.

SIMON NEWCOMB

* Comptes Rendus, 1868, i. p. 921.

Solar Parallax

I HAVE waited somewhat anxiously for Prof. Newcomb's statement of the errors in a chapter on the Sun's distance ("The Sun," Chapter I.). His review was certainly so worded as to imply very gross inaccuracy, and his explanatory letter, in which he remarked that more than a column of NATURE would be needed for the mere record of my errors, did not improve matters. This morning I have received his notes. The errors enumerated amount but to seven in all; I will leave your readers to judge of their importance.

1. At p. 50, I assign to Hansen's letter of 1854 the announcement of the value $8''.9159$ for the solar parallax; whereas this value was not announced by Hansen until 1863. *Tanquam referat.* Hansen's priority remains unaffected by the change.

2. At p. 53, I mention that Prof. Newcomb deduced a value of $8''.84$ (probably a misprint for $8''.81$) for the solar parallax by a certain method. His real result was $8''.809$. Again my comment is *tanquam referat.*

3. At p. 53, Foucault's "parallax is given as $8''.942$, whereas the result actually deduced was $8''.86$." The matter again is utterly insignificant; but it chances that I have not given Foucault's estimate of the parallax as $8''.942$. I remark only that if Foucault's estimate of the velocity of light is correct, the parallax would be $8''.942$. I deduced this result by a calculation made on my thumb-nail as I wrote. It is correct, however, and Foucault's was not.

4. At p. 59, I say that Mr. Stone deduced the solar parallax from observations of Mars made at Greenwich alone, and then by combining these observations with others deduced the solar parallax at $8''.943$. Now, Prof. Newcomb says that he "finds no discussion of the observations at Greenwich alone, in the paper here referred to." But I refer to no paper whatever. A rough calculation of the parallax was certainly made from the Greenwich observations alone, though, as Mr. Dunkin remarks at p. 507 of his edition of "Lardner's Astronomy," "the observations by this method (single-station observations) were comparatively unsuccessful," "owing to unfavourable weather at Greenwich." Apart from the facts, which fully justify my statement—what could the correction be worth in any case? Only the final result was insisted upon.

In a note on this matter, Prof. Newcomb makes "in passing" the really important observation that the method of determining the sun's distance by observations on Mars from a single station was applied by the Bonds as far back as 1849. Mr. Carrington had already told me that he believed the Bonds had anticipated the Astronomer Royal. I wrote to Prof. Young asking for further information, and was waiting for his reply, I am obliged to Prof. Newcomb for aiding me in this matter. The priority of the Bonds in this matter should certainly be more widely known than it is.

5. At p. 61. This is a very curious correction. I speak of Prof. Newcomb as having successfully treated the problem which was afterwards discussed by Mr. Stone; and he remarks that he knows nothing of the matter, and has read my statement with great bewilderment. I am not responsible for it. There is a letter in the *Astronomical Register* for December 1868, signed only "P. S.," but with unmistakable internal evidence of coming from the Astronomer Royal for Scotland, in which the following passage occurs:—"I must not say a word about the pyramid sun-distance here, or my letter will never be allowed to see the light; something, however, on the score of modern justice to our contemporaries I must beg leave to put in. Admirable is the praise given to Mr. Stone, and worthy, in so far, the credit abundantly bestowed on him at every step of the undertaking; but why is there not one word about Prof. Simon Newcomb, of America, having already gone over that same problem similarly, and published the results a year sooner?" Of course, as Prof. Newcomb now writes that "he has no recollection of ever having made any independent investigation of the observations of the transit of Venus," Prof. Piazz Smyth was mistaken, and "the abundant discussions of Prof. Newcomb's paper in various northern scientific societies last winter" (so speaks Smyth) were founded on some misconception of its purport. But Prof. Smyth's statements were permitted to remain uncorrected;—*hinc illa lacrymæ.*

6. At pp. 61, 62. "The distortion of Venus at the time of internal contact is described as an ever-present phenomenon, and the apparent formation of the ligament as contemporaneous with true internal contact." If what I say in pp. 61, 62 admits of being so misinterpreted (which I question), the same cannot be said of

my remarks in p. 63. My belief is now, as it has been for years (long before Mr. Stone's paper was published), that under favourable conditions an exceedingly fine ligament must be visible at the moment of real internal contact, the planet's outline being otherwise undistorted. But in most instances a coarser ligament is formed *not* contemporaneously with the moment of real contact. I have shown that the true moment of contact can be *inferred* from the formation of a coarser ligament as exactly as when a fine ligament is observed. This I still maintain, and I further believe that Mr. Stone's opinion as to the cause of the phenomenon, an opinion independently enunciated by myself in November 1868 (see *Scientific Opinion*) is correct, and that the experimental tests which have been supposed to disprove it, have in reality no sufficient bearing on the question at issue.

7. At p. 63. It is unfortunate if my account of Stone's proceedings suggests that I maintain he was the first to consider whether real or apparent contacts had been observed; for I have but lately been maintaining the contrary view in a correspondence with an ex-president of the Royal Astronomical Society. I have invariably opposed the opinion here ascribed to me. Mr. Stone himself has never claimed what I am said to have claimed for him. He has made a definite claim, and that claim I have repeated and still hold to be just.

Prof. Newcomb concludes with some general statements. He considers I am mistaken in supposing that astronomers generally regard observations on Venus in transit as the most trustworthy method of obtaining the solar parallax; mistaken again in supposing that Mr. Stone has removed any "difficulties that had perplexed astronomers;" and so on. Such statements are so vague that I shall scarcely be expected to discuss them. Until proof, or at least some evidence to the contrary, is supplied, I can only say that now, as when I wrote "The Sun," my opinions on these points seem to me to be just. I am certainly not alone in holding them.

RICHARD A. PROCTOR

Brighton, September 23

Elementary Geometry

THE question raised on this subject naturally consists of two parts. The first relates to the unsuitableness of Euclid as a text-book, and the need of a work which shall so commend itself to examiners and teachers, so to supplant it. The second question is—given the authoritative text-book, how is the geometry of which it treats to be taught to young students? The arguments on the first of these questions have been so ably and conclusively stated lately by several mathematicians, especially by Mr. Wilson, Dr. Joshua Jones, and Dr. Hirst, that there is no need to revive the discussion; but I entirely agree with your correspondent in his conclusion that the book which is to supplant Euclid is at present a desideratum, and that it will probably be the work of more heads than one. Several books have been written during the last four years, and have formed the basis of the discussions which have since taken place on the requirements of the new programme. By their means, the questions at issue between the opponents and supporters of Euclid have become more clearly defined, and a greater unanimity of action has resulted amongst those who are labouring to supply this desideratum of modern education. But I am sure that most of these authors will admit that the issue of works intended for permanent text-books was premature.

When the first question is settled, the second remains. Geometry is not essentially difficult, nor is it generally distasteful to young students. It becomes so, however, when they are required to commit the propositions to memory before they understand them. The educational purpose which geometry serves is not the discipline and exercise of the memory. A choice and pregnant passage from a good author may be learnt and retained in the memory without much difficulty, although its meaning may be very imperfectly understood, and it will richly repay the labour of its acquisition. It will be recalled again and again, and receive new light, and afford new pleasure with every fresh association. Not so with geometry; it is useless if not understood. A child should be made to comprehend even the definitions before he commits them to memory. Let us suppose, for instance, that the definition of a circle is to be learnt, the preliminary explanation should take some such form as the following.

The teacher at the black-board, with chalk and compasses, and the pupils at their desks, with paper and compasses—the teacher draws a circle and names the figure—he tells each boy also to make a circle, and then proceeds to question. What name is

given to such a surface as that on your drawing paper? What kind of a figure shall we call one which can be drawn on a plane surface? Compare a triangle and a circle, and say how many lines form the boundary of the triangle? How many lines contain the circle? Explain exactly what you do with the points of the compasses when you use them to make a circle? Why must the joint of the compasses be tight? Fix a drawing-pin in your drawing-board, and with a piece of thread construct a circle. What purpose does the thread serve in the construction? The defining properties of a circle are, therefore, these—(1) it is a plane figure; (2) it is bounded by one line, termed the circumference (3); every point of the circumference is at the same distance from a fixed point, termed the centre.

After the definition is worded in its permanent form, and repeated, and written several times on paper, it will be remembered.

Again, let us suppose the propositions on the equality of triangles to be the subject; the following introductory questions and exercises suggest themselves. Draw two straight lines, one 5 in. long, and the other 8 in.; then make with your protractor an angle of 43° . Construct a triangle having one of its angles equal to the angle drawn, and the sides of this angle equal to the given straight lines. Take the figures drawn by different boys, and compare them as regards size. Now consider the parts of each; how many sides has each? How many angles? How many sides are drawn from given dimensions? Letter them and then name them. How many angles? Name it. How many angles were not originally given? Name them. How many sides? Name it. Compare this third side, B C, in two of the figures. If the figures are all accurately drawn throughout the class, what must necessarily follow with regard to this third side B C in all the figures, &c.?

Finally, the proposition should be enunciated, and the proof learnt in the form in which it is to be remembered.

Then the teacher may give three angles which may form the angles of a triangle, and when the constructions are made compare two figures from distant parts of the class. Similarly he may treat all the allied propositions. When taught in this way, the subject becomes so easy and attractive that it may be commenced at an early age.

If, as some teachers maintain, Spartan severity be necessary to secure mental discipline, then this plan of teaching elementary geometry will not be an improvement on that of forcing into the memory Euclid, pure and simple, without note or comment; but when the test of success is applied, I am sure the plan of making the early school work as easy and as pleasant as possible will require no other argument to support it.

R. WORMELL

Deschanel's "Heat"

It is remarkable that Prof. Everett asserts h to represent the reduced height of the mercurial column, when the *unreduced* height is carefully indicated in Fig. 264 by the same symbol h . Moreover it is distinctly stated on page 362 that "the tension of the vapour is evidently equal to the external pressure minus the height of the mercury in the tube."

Prof. Everett writes, "In some instances I have endeavoured to simplify the reasonings by which propositions are established or formulæ deduced" (Preface, part I). This would lead most people to expect simplicity, which includes accuracy; and they may well be astounded when they find not only unexplained but inaccurate formulæ. Prof. Everett's promises, and not his complaint, were the grounds of expectations which have not been realised.

THE REVIEWER OF DESCHANEL'S "HEAT"

Sept. 22

Newspaper Science

MR. FORBES does not stand alone in his experience of newspaper science. The *Globe*, however, is not generally looked on as a scientific paper, and no one would be likely to go to it for information on matters other than political. What shall we say, however, to the following paragraph, copied *verbatim et literatim* from the columns of the *Mark Lane Express* for September 4?—

"CHARLOCK.—A correspondent inquires what he must do to abate the annoyance of it in his crops. We do not believe there is any mode of preventing its presence. Some seasons are distinguished by its appearance. We do not think they come from