

anonymous, if we please, but which was referred by every one who read it to the Astronomer-Royal for Scotland, who showed not the slightest wish to conceal his identity. Doubtless on hearsay evidence (in which, however, he placed, I am sure, as much reliance as I placed in his own statement), Prof. Smyth asserted that Newcomb had anticipated Stone's labours. I took it for granted that it was so, since I saw no room or reason for doubt. There was my error. But, says Prof. Newcomb, whence comes the value 8'.87 "which it will be noted is Mr. Petrie's pyramid value?" and on what does Mr. Proctor found his comments "about my treatment of contacts? I am as much in the dark as ever." I will tell him. The value 8'.87 has nothing on earth to do (so far as I am concerned) with Mr. Petrie's pyramid value. It is simply the value insisted upon by Prof. Newcomb in a paper which appeared in the *Monthly Notices* of the Royal Astronomical Society for November 1868; respecting which Mr. Stone remarked (see the same number of the *Notices*) that "the point Mr. Newcomb has raised is a question of only 0".04, viz. between my value and 8'.87—a question, therefore, of comparative insignificance." Most just remark! With my belief as to Prof. Newcomb's prior work, was it wonderful that I concluded that 8".87 was his own pet figure for the parallax? Then it chanced that the Royal Astronomical Society, venturing to ignore Prof. Newcomb's objections, bestowed on Mr. Stone, in 1869, the Gold Medal of the Society for his researches into the Venus transit; and in the remarks which accompanied the presentation, it was stated that all preceding researches were imperfect in this respect, that (to use my own words) "no fixed rule had been adopted for interpreting the observations of internal contact." Prof. Newcomb cannot fail to see how this statement accounts for the estimate (not *my* estimate) of his supposed researches.

As a matter of fact, however—apart from the inference to which Prof. Newcomb is so anxious to give point—I am somewhat hardly treated in this matter. When I came to the part of my book where Prof. Newcomb's supposed researches should be dealt with, I thought thus in my mind: "Assuredly Newcomb has done this thing, for Prof. Smyth says so. Shall I leave his researches unnoticed because I can find no trace of them? That would be scarcely fair. Moreover, he is an American, and to omit all notice of his work will be so much the more objectionable. Verily I will repeat the statement of my esteemed friend at Edinburgh, and I will combine with it the weighty judgment of my friends at the council-board of the Astronomical Society. Thus will the researches of Newcomb be recorded, and due credit be assigned to him for his industry and skill, while yet no undue weight will be given to the numerical result of his labours."

That I thus fell into error I have already admitted. But the error is venial in its nature, and utterly insignificant in its effects. As I am conscious that it arose chiefly from my desire (shown in other ways and places) to do justice to our American fellow-workers in science, I am in no way ashamed of it; and I conceive that Prof. Newcomb should have been the last to comment in the manner he has done on the subject.

I shall not follow him in his discussion respecting irradiation, leaving Mr. Stone to deal, in his own good time, with the arguments by which two Continental astronomers (and one American mathematician) have sought to deprive him of his justly-earned credit.

I would submit, in conclusion, that February 1869 (the date of the presentation of the Astronomical Society's medal to Mr. Stone) can scarcely be described as "five years" ago even now, and my treatise on the sun was published in February 1871, Chapter I. being in type in November 1870. Nor has the council of the Astronomical Society (or any member of it) expressed any doubt, as yet, regarding the justice of the decision arrived at in 1869. Yet not a few members of the council have paid marked attention to Prof. Newcomb's attacks upon Mr. Stone. *Verbum sat.*

RICH. A. PROCTOR

Brighton, Nov. 24

The Density and Depth of the Solar Atmosphere

THE demonstration relating to the density and depth of the solar atmosphere, published in *NATURE* October 5, 1871, page 449, has been entirely misconceived by Mr. Ball. The volume of the terrestrial atmosphere is an element which obviously has nothing to do with the question. Atmospheric air, if raised to a temperature of 3,272,000° Fah., will expand 6,643 times; hence a vertical column forty-two miles high will reach a height of

279,006 miles, if brought to the stated temperature. The basis of computation adopted by Captain Ericsson being an area of one square inch, he shows that a medium similar to the terrestrial atmosphere containing an equal quantity of matter for corresponding area, transferred to the solar surface, will, owing to the superior attraction of the sun's mass, exert a pressure of $14.7 \times 27.9 = 410$ pounds. And that, if the said medium be heated to a mean temperature of 3,272,000° Fah., it will expand to a height of $\frac{279,006}{27.9} = 10,000$ miles above the solar surface. But, if a gas composed chiefly of hydrogen 1.4 times heavier than hydrogen the specific gravity of which is $\frac{1}{1.4}$ of that of air, be substituted, the height will be $\frac{14 \times 10,000}{1.4} = 100,000$ miles. Admitting

that the ascertained coefficient of expansion, 0.00203 for 1° Fah., holds good at the high temperature before referred to, the stated altitudes of the solar atmosphere cannot be disputed. Mr. Ball's announcement concerning the properties of spheres, it is scarcely necessary to observe, has no bearing on the foregoing calculations. With reference to the effect of intense heat, it will be well to bear in mind that the before-mentioned rate of expansion holds good for atmospheric air—within an insignificant fraction—under extreme rarefaction as well as under high temperatures. We have no valid reason, therefore, to suppose that any deviation from the ascertained law of expansion takes place in the solar atmosphere, sufficient to alter materially the before-mentioned computations of its depth.

Mr. Ball, in expressing the opinion that we shall not gain much correct knowledge of the solar atmosphere by the inquiry instituted by Captain Ericsson, forgets that the retardation which the radiant heat suffers in passing through our atmosphere has been ascertained, and that the properties of atmospheric air with reference to temperature and expansion are nearly identical with those of hydrogen, now admitted to be the chief constituent of the solar atmosphere. It is evident that Mr. Ball does not comprehend the object of adopting the terrestrial atmosphere as a means of determining the extent and depth of the solar atmosphere, since he does not perceive that the comparison instituted by Captain Ericsson has brought out the fact that either the depth of the sun's atmosphere exceeds 100,000 miles, or it contains less gaseous matter than the earth's atmosphere for equal area. The importance of this conclusion with regard to the determination of the retardation of the radiant heat in passing through the sun's atmosphere is self-evident to all who regard solar radiation as energy which cannot be absorbed unless an adequate amount of matter be present. Mr. Ball's suggestion that the retardation depends on the "chemical, *i.e.* molecular-constitution" of the solar atmosphere, calls to mind how flippantly some physicists talk of "arresting" one half, or more, of the solar energy. These reasoners apparently do not perceive that the means of arresting such stupendous energy is more difficult to conceive than the means of producing it.

Respecting the experiments which have been made with incandescent cast-iron spheres, and inclined discs, it is important to mention that previous experiments had established the fact that the radiant heat of flames transmits equal temperature, under similar conditions, as incandescent cast iron. The inference, therefore, which has been drawn by Captain Ericsson from the results of his experiments with incandescent cast-iron spheres regarding the feebleness of radiant heat emanating from the sun's border is not unwarrantable as supposed by Mr. Ball.

New York, Nov. 10

THULE

An Aberrant Foraminifer

I CHANCED upon an aberrant form of Peneroplis the other day, in which the free terminal series of chambers of this Foraminifer, ordinarily single, is constricted into two distinct tubes.

Though new to me, it may not be so to some of your readers; Dr. Carpenter, however, does not mention it in his monograph.

St. John's College, Cambridge

W. JOHNSON SOLLAS



"New Original Observation"

ERNST FRIEDINGER, of Vienna, begins a communication on the subject of "which cells in the gastric glands secrete the