ceed Professor A. G. Smith, whose death occurred in the fall of 1916.

APPOINTMENTS at Cornell University have been made as follows: F. K. Richtmyer, professor of physics; John B. Bentley, jr., professor of forestry; Charles L. Gibson, professor of surgery, to succeed the late Professor Stimson; John A. Hartwell, associate professor of surgery and William C. Thro, professor of clinical pathology, Medical College, New York.

WILLIAM S. TAYLOR, acting professor of rural education at Cornell University, has been appointed professor of agricultural education at Pennsylvania State College.

DR. A. R. CUSHNY, F.R.S., professor of materia medica and pharmacology in the University of London (University College) since 1905 has been appointed to the chair of materia medica in the University of Edinburgh. Dr. Cushny was professor of pharmacology in the University of Michigan from 1893 to 1905.

DISCUSSION AND CORRESPONDENCE

PYRHELIOMETRY AND SOLAR RADIATION

TO THE EDITOR OF SCIENCE: I hope it may interest your readers, and more fully explain the discrepancy between Professor Bigelow's work and mine, if you can find space for the accompanying letter.

MAY 14, 1918

My dear Professor Bigelow:

1. I received yesterday from your publishers a complimentary copy of your new book entitled "Treatise on the Sun's Radiation." You are kind to have it sent to me.

2. I received to-day your communication on a 26.68-day solar synodic period.

3. Some time ago I received some other data from you relating to observations and computations of radiation.

4. While I appreciate your kindness in remembering me personally, I am obliged to tell you that I can not at all accept your views and I do not think you either fully understand or fairly weigh our work. My reasons are partly given below.

5. Among the words you use most is "Pyrheliometer." We carefully made and standardized Silver Disk Pyrheliometer S. I. III., at your request, and sent with it an accurate description of the method by which it must be read and reduced in order to give results to correspond with its constant of calibration. In your book "Atmospheric Circulation and Radiation," pages 263 to 267, I am surprised to see that you describe and *prescribe* another method of using it whereby it *can not give results agreeing with its constant of calibration.*

6. You use this word "Pyrheliometer" and its modifications often very objectionably when you mention our work. You make it appear as if we attach weight to empirical processes of extrapolation of total radiation of all wave-lengths combined. If an observer could operate on the moon, a pyrheliometer would be a very much more valuable instrument than it is here, and I believe you and others could not then avoid the true conclusions as to the value of the solar constant. Unfortunately, owing to the unequal transparency of the earth's atmosphere for rays of different wave-lengths, it is absolutely necessary to use spectrum-energy analysis to measure the solar constant of radiation, as Langley showed. We use a linear bolometer to measure the intensity and changes of intensity of all parts of the spectrum. We have employed it at Washington, Bassour, Hump Mountain, Mount Wilson, and Mount Whitney. In our experiments the solar beam traversed paths of air ranging from that where the sun was nearly vertically overhead at Mount Whitney, to that with the sun on the horizon at Mount Wilson. Anybody interested can learn exactly how we worked by studying our published papers, particularly Volumes II. and III. of our *Annals* and our paper "New Evidences on the Intensity of Solar Radiation Outside the Atmosphere," Smithsonian Miscellaneous Collections, Vol. 65, No. 4.

In all this work we treat the pyrheliometer as a subsidiary instrument. Its sole use and purpose in our investigations is to enable us to express the readings of the bolometer in calories.

As a result of spectro-bolometric investigations over fifteen years of time, we have shown that the solar constant is 1.93 calories, and the sun an irregular variable star. Others, Clayton and Bauer particularly, have shown how the solar variations we have discovered affect terrestrial things. If our results were wrong these correlations would not be found.

7. Not everybody has a spectro-bolometer. You haven't any, for one. From a wealth of experience that nobody else in the world ever had in the measurement of solar radiation, we have put out some tabular data and empirical formulæ connecting pyrheliometry and psychrometry with the solar constant. We did this, not because we had any occasion for them ourselves, but so that observers

who had pyrheliometers, but couldn't afford the expense of money, time, and experience necessary to really observe solar radiation satisfactorily by spectrum-energy work, might get approximate results of at least moderate value. It is to be distinctly understood that these empirical methods of solar constant work by pyrheliometry, though based on our work, are likely to yield results several per cent. from the truth, owing to differences in the atmospheric transparency due to various causes, and especially to the variable influence of water vapor. Pyrheliometric methods are mere economical make-shifts when unaccompanied by spectrobolometry.

8. You are, I am certain, misled in your attack on our use of Bouguer's formula of extrapolation when applied as we applied it to homogeneous rays. See for instance our paper "New Evidences on the Intensity of Solar Radiation Outside the Atmosphere." Logically conceived the mathematical treatment consists in diminishing the path of the sunrays in every layer of the atmosphere proportionally until none remains. The fact that this can not *conveniently* be carried through *experimentally* beyond the point corresponding to the atmospheric thickness found in a vertical solar beam does not prove that a continuation such as can be logically conceived up to the point where each thickness becomes zero is mathematically unsound. Imagine, for instance, a tube to be erected from the observer to the outside of the atmosphere, and by side tubes appropriately dimensioned let the atmosphere within the tube be exhausted until none remains. This fits the logical process applied with Bouguer's formula. No mathematician but you can see in it anything objectionable, so far as I know.

9. In order to verify, as far as could be done, the sound theoretical and experimental conclusion that if the standard pyrheliometer could be read on the moon at mean solar distance it would read there on the average 1.93 calories per square centimeter per minute, we sent up a registering pyrheliometer by balloon to 22,000 meters in 1914 and found there 1.84 calories, which is a very reasonable check.

10. You have exterpolated your thermodynamical discussion of meteorological measurements into the realms of the thin air above 22,000 meters, and into the realms of the sun, which is out of the range of laboratory conditions altogether. Your results widely disagree from those I have just quoted. It seems to me not to matter who makes the curves, whether yourself or another; by the time they get outside the well-observed range of atmospheric data, say 20,000 meters, even though they are sound at the bottom (and this I am not quite sure of), they rank rather as interesting speculations than as having quantitative value.

By authority of the Secretary:

Yours truly,

C. G. Abbot,
Director, Astrophysical Observatory

Professor Frank H. Bigelow,
Solar and Magnetic Observatory,
Pilar, Argentina.

Reply to Professor Wilder

Being much interested in a short article by Professor Wilder, appearing in Science of April 19, on the subject of "*Desmognathus fuscus* (sic)," it occurred to me that a few remarks might not be inappropriate. The object of the nomenclatorial code in zoology, as I assume Professor Wilder recognizes as fully as any other zoologist, is primarily to afford a means of naming the various species of animals. In view of this I think it will be admitted that philological conditions should play a secondary rôle to consistency and permanence. Most zoologists are in favor of ridding nomenclature of the idiosyncrasies continually occurring in language, in order to bring about absolute uniformity so far as may be possible. This tendency can be traced easily. In former times it was the custom, for instance, to begin all specific words founded upon proper names with the capital letter; then, the desirability of uniformity becoming increasingly evident, only specific designations founded upon the names of persons were so written; at the present time, in all parts of the world excepting continental Europe, the custom prevails of beginning all specific names, including the personal, with a small letter. It is now *Omus edwardsi,* for example, and not *Omus Edwardsii,* as originally published, the adoption of the single *i* in all cases to form the genitive ending, being another recently adopted rule formulated in the sole interest of uniformity. All this should horrify the philologist quite as much as the disregarding of irregular Greek genders.

Now in regard to genders, it is considered desirable by many systematists—and their