rather than Lord Kelvin's, which has been familiar to me for many years.

1. The error which has arisen in judging my paper proceeds from the habit of dealing with common solids in the laboratory, and the supposition that I am using the same method in dealing with the effective rigidity of the matter within the earth. The question as to how the stresses are applied to a cubical element does not need to be considered, for we are not experimentally shearing or otherwise deforming the elemental cubes of the earth to get the resulting mean rigidity. Inside the limit of pressure which gives the matter the property of an elastic solid, the simple fact is that there is an effective rigidity in spite of the high temperature. Pressure operating through the agency of molecular forces, therefore, is the sole cause of the effective rigidity and I have taken the effective rigidity everywhere proportional to the pressure, which is a perfectly legitimate hypothesis. If others wish to adopt a different hypothesis, they are at liberty to do so. The present hypothesis is satisfactory on theoretical grounds, and apparently confirmed by the numerical calculations given in *Astronomische Nachrichten*, No. 4,104.

2. It may be well to observe that it is a matter of the utmost indifference to me how the elemental cube may be distorted, or whether it be distorted at all. I am not determining coefficients of rigidity for the different elements within the earth. *For my purpose of calculating the earth's mean rigidity, it is sufficient to have something which these rigidity moduluses would be proportional to if they could be determined, and that is the pressure, as calculated from the theory of gravity and Laplace's law of density.*

3. The rigidity of ordinary solids may be expressed in atmospheres; and in dealing with bodies made rigid by pressure, it is convenient to employ the same measure, since this enables us to compare the rigidity of a cold solid to that of a hot body made rigid by confining pressure.

4. There is an old saying that 'facts are stubborn things.' Such, it seems to me, are the numerical results obtained in my paper, by processes of entire mathematical rigor. I calculate that the rigidity of the earth will lie between 750,000 and 1,000,000 atmospheres. In finding this lower limit, the effect of the earth's crust is neglected, and there is, moreover, some slight defect in the gravitational method near the surface even in the case of encrusted bodies. In the case of gaseous bodies, the outermost layers can hardly be regarded as having the properties of an elastic solid, and hence the integration for the mean pressure should stop before we reach the surface. But as we do not know at what depth to stop, I took the mean pressure of the entire planet as giving its most characteristic property.

From these considerations I believe that those who study the paper in *Astronomische Nachrichten*, No. 4,104, will agree that the points raised relate to the experimental determination of moduluses of rigidity, and not to the rigidity of the earth and other planets, which are found by theoretical methods fully explained in the paper itself.

T. J. J. SEE.

U. S. NAVAL OBSERVATORY,
MARE ISLAND, CALIF.,
October 3, 1906.

ANATOMIC NOMENCLATURE: AN OPEN LETTER TO PROFESSOR LLEWELLYS F. BARKER.

Dear Dr. Barker: Through absence from home I have but just received from the publishers your "A Description of the Basle Anatomical Nomenclature [B N A], advance sheets from Dr. Llewellys F. Barker's forthcoming book, 'Anatomical Terminology.'" I rejoice that the subject is to be so fully and ably presented to English-speaking teachers and students of anatomy. Although many of the terms of the [B N A] are not preferred by me, yet—pending the expected eventual general acceptance of my own—I should hail their provisional adoption to the exclusion of their numerous even less worthy synonyms, as enabling me to replace a 'shot-gun policy' by rifle-practise.

I take for granted that the paragraph on page 5 was intended to represent justly my own share in terminologic discussion. Later I may comment upon certain points, *e. g.*, the alleged 'obscurities of the system' (which—in view of my long preaching and practise of clearness as the first essential of all scientific composition—you must pardon me for regarding as subjective), and the nature and extent of my philologic transgressions (in which connection I may refer to a paper read, by invitation, before the American Philological Association last winter). Now, in view of the fact that all my publications upon the subject either have been sent you or are otherwise accessible, I must express surprise and regret that the foot-note (translated from His) should cite only three of my less extended contributions (two of them privately printed), without mentioning the earlier, the later and the more comprehensive, *e. g.*, the article 'Anatomical Terminology' by S. H. Gage and myself, in the first edition of the 'Reference Handbook of the Medical Sciences,' 1889, our 'Anatomical Technology,' 1882 and 1897, my 'Neural Terms, International and National' (*Journal of Comparative Neurology*, 1896), and 'Some Misapprehensions as to the Simplified Nomenclature of Anatomy' (1898), SCIENCE, April 21, 1899. The several reports of the committees of the Association of American Anatomists, the American Neurological Association and the American Association for the Advancement of Science should have been specified, and it would have been simple justice to name Mrs. Gage, Gerrish, Gould, Huntington, Leidy, the Spitzkas, father and son, and others. Finally, American students should be aware that the subject was definitely brought before the American Association for the Advancement of Science as long ago as 1880, and that a committee of that body was appointed in 1884, three years prior to the date when, as stated by you, 'Germany took the lead.'

In my 'Neural Terms' and 'Some Misapprehensions' I tried to give due credit to earlier simplifiers, Barclay, Owen, Henle, etc. When you and some other anatomists in this country take equal pains to inform yourselves fully as to the facts and principles involved, I believe you will concede that the good and enduring features of the neurologic portion of the [B N A] had been previously adopted or proposed by me, and you will realize that the unprejudiced consideration of the terms preferred by me would have been more advantageous to anatomy and more creditable to yourselves than their premature condemnation.

A copy of this letter will be sent to SCIENCE and *American Medicine*.

Very truly yours,
BURT G. WILDER.

October 11, 1906.

LEFT-HANDEDNESS.

TO THE EDITOR OF SCIENCE: The question of right-handedness has been brought to my notice, and I should like to inquire whether any of your readers has actually counted the number of left-handed men and women in a tribe. Very few implements of savagery are reliable witnesses. The throwing sticks of Eskimo men and the short-handed skin dressers of the women are infallible, since they fit only one hand. In the National Museum, among a great number of throwing sticks—from east Greenland to Sitka, only two are left-handed and both are from the same locality. There is not a left-handed woman's implement in the museum.
O. T. MASON.

October 20, 1906.

SPECIAL ARTICLES.

THE RELATIVE MERITS OF THE 'ELIMINATION' AND 'FIRST SPECIES' METHOD IN FIXING THE TYPES OF GENERA—WITH SPECIAL REFERENCE TO ORNITHOLOGY.

IN attempting to fix the types of any group of genera we shall find that a large number are monotypic, another lot have had their types designated by their authors, a few are fixed by the rule of tautonomy[1] and a certain number are left without any indication of a type—usually complex heterogeneous genera of the older authors. It is these that are always giving us trouble and these alone with which the problem of fixing types is concerned.

It seems to me that it is the duty of those engaged in nomenclatural work to-day to establish our names on as firm a basis as pos-

[1] See SCIENCE, V., No. 16, pp. 114–115, July 18, 1902.