affirmative; consequently, it should be demonstrated that my reasons are no good. Nobody ever attempted this, and when arguments were given purporting to be opposed to my ideas, these invariably were not my views but only what the critic fancied to be my views.[13]

A. E. Ortmann
Carnegie Museum, Pittsburg, Pa.,
October 4, 1906

SPECIFIC NAME OF NECTURUS MACULOSUS

In the last number of the *American Naturalist* (Vol. XLI., January, 1907, pp. 23–30) there is an elaborate paper by Professor F. C. Waite under the above title, in which he shows that the name employed there has the priority over *N. maculatus,* the term most commonly adopted by anatomists. Towards the end of the paper (p. 27) he makes the following statement: "In the past ten years although many papers have been written on *Necturus,* two only have, as far as I know, used the correct nomenclature."

I wish to say that the 'correct' name was pointed out and the proper references given by the late Dr. G. Baur as early as 1897 (*Zool. Bull.,* I., p. 41). Since then it has been employed by various systematists. Thus the name *N. maculosus* is used in the eighth edition of D. S. Jordan's 'Manual of the Vertebrate Animals of the Northern United States,' 1899, p. 175, in which I tried to bring the nomenclature up to date. It has since been used, both in this journal (Science, N. S., XI., 1900, p. 555) by Fowler, and in the *American Naturalist* (XL., 1906, p. 159) by Stone.

Leonhard Stejneger
Smithsonian Institution,
January 14, 1907

THE DEFINITION OF SOLID AND FLUID

To the Editor of Science:—The point I have raised (October 26) as to the definition of solid and fluid seems quite timely in view of the discussion going on between Hoskins and See, and the letter of Mr. Willcox (November 9). Note the use of the term 'solid' in one, of 'substance' in the other, of the two definitions of rigidity cited by Hoskins. Their difference seems to be as to whether it is proper to speak of the rigidity of a fluid or a gas. The real question of fact, how much the interior of the earth yields to a certain variation of pressure, has not thus far entered the discussion.

Again, Mr. Willcox defines fluid and solid quite other than was suggested by me and the line between as the curve of the plastic yield point.

His definition is quite tenable, if we agree to it, may be made as exact, and fits quite as well the Latin derivation of the word fluid, but I am not sure that it agrees as well with usage or is as practical. We could then speak of no substance as solid or fluid without knowing under what pressure it is. Whether a body were solid or fluid would then depend not merely on the state of the body itself, including its temperature, but also on its surroundings—the pressure. We cannot, then, as he writes me, 'properly refer to any substance as a plastic solid.'

The earth's interior would be classed as a fluid, and not, as has been lately common, on account of its high rigidity, as solid.

The one point which is not quite clear, as he brings it in parenthetically, is whether the plastic yield point, and so his definition, depends on the time or rate of application of pressure. I judge not, according to the molecular theory which he adopts (dear to T. Sterry Hunt) that there are three states of molecular aggregation, solid, fluid and gas, and that the solid molecules are heavy and complex aggregates of the liquid molecules, as these are in their turn of the gas, and that sufficient temperature and pressure will break up the large solid molecules.

The definition which occurred to me, that a fluid is a body that can not rest under stress, *i. e.,* in a strained condition, is, however, just as definite and draws just as sharp line as that of Mr. Willcox. We may express it in his terms thus—*a fluid has a temperature such that its plastic yield point is reached even at zero pressure.* The relative content of the two concepts can be expressed graphically thus.

[13] See also my reply to Gager's criticism in Science, August 17, 1906, pp. 214–217.

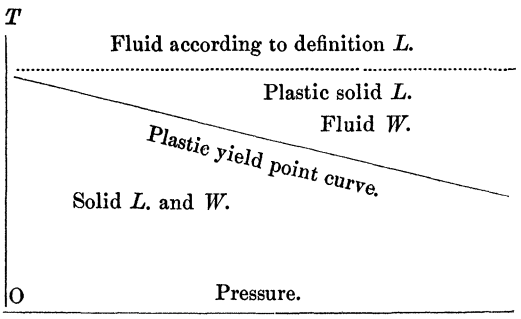

Figure illustrating varying conceptions, as to where the line between fluid and solid should be drawn.

Neither definition is absolutely rigorous, perhaps. It may be that every fluid can rest under a very minute amount of strain, and that every solid has plasticity.

No discussion of facts can settle usage, so I call for others, either here or in the scientific meetings, to express themselves as to what usage will best fit that of the past and present, and be most practical for the future.

ALFRED C. LANE

A CORRECTION

TO THE EDITOR OF SCIENCE: To a communication by the writer, which appeared in SCIENCE for January 18, 1907, the name of the U. S. Geological Survey was attached without authority of the director. The writer wishes to record his disavowal of any desire to commit the survey to an indorsement of the sentiments expressed in said note. For these he alone is responsible. In explanation he would add that the note was written before the writer became a member of the U. S. Geological Survey. It was not offered for publication, however, until about the time he was planning to enter on field work, when his new address was attached without due consideration.

Moreover, on deliberate reading, the writer is conscious that unintentionally there appears to be in the language employed a tone of discourtesy, which he regrets.

C. H. GORDON

January 22, 1907

SPECIAL ARTICLES

THE CASE OF ANASA TRISTIS

IN a paper read before the December meeting of the American Society of Zoologists in New York, illustrated by a very beautiful series of photomicrographs taken from smear-preparations, Miss Foot and Miss Strobell announced the following conclusions, which have since been published in a preliminary form in the January number of the *Biological Bulletin:* (1) There is no odd or 'accessory' chromosome in *Anasa tristis.* (2) The number of spermatogonial chromosomes is 22. (3) All the chromosomes divide in both maturation divisions. The so-called odd or accessory chromosome is only a 'lagging' chromosome, and it divides with the others in the second division. (4) The so-called 'chromosome-nucleolus' of the growth period is not a chromosome, but a nucleolus.

These results are at variance with my own, and since the differences in regard to the first three involve the important more general issue of the relation of the chromosomes to sex-production, I will make the following reply.

The fourth of the above conclusions, though materially different from my own, is not altogether irreconcilable with it. I have for some time had reason to suspect (in case of certain other genera) that a stage may have been overlooked in the prophases in which the odd chromosome temporarily loses its compact nucleolus-like form. For the study of this question smear-preparations offer decided advantages; and I am ready enough to admit that in regard to these stages Miss Foot and Miss Strobell may have made an important addition to our knowledge, though I still believe that the chromosome-nucleolus of the earlier stages is the odd chromosome. On the other and more vital points their results are irreconcilable with my own, and only these will further be considered here.

Since the announcement of these results I have carefully reexamined my old preparations (including those of Paulmier) and a series of new ones from material collected during the past summer. They include sections of material fixed in Flemming's, Her-