

late Horatio Stone, Rockford College, Rockford, Ill., receives \$28,000. Donations to the University of Pennsylvania during the past month amount to \$69,370.23.

At the meeting of the Board of Trustees of Princeton College, held on February 13th, Mr. J. Bayard Henry, '77, of Philadelphia, was elected trustee in place of William Libbey, of New York City, deceased, and Mr. Howard Crosby Warren, '89, was appointed assistant professor in experimental psychology.

ON the birthday of Mr. Henry W. Sage, celebrated at Cornell University on January 30, the following list of his gifts to the University was noted:

Sage College for women, with endowment fund (1873)	\$266,000
Sage Chapel (1873)	30,000
Contribution towards extinguishment of a floating debt (1881)	30,000
House of Sage professor of philosophy (1886)	11,000
Susan Linn Sage chair of philosophy (1886)	50,000
Susan Linn school of philosophy (1886)...	200,000
University library building (1891).....	260,000
University library endowment (1891).. ...	300,000
Casts for archæological museum (1891)....	8,000
	<hr/>
	\$1,155,000

A MEMORIAL praying for the admission of women to degrees at Cambridge University has received the signatures of 2,200 university members.

DR. CESARE LOMBROSO has been transferred from the chair of legal medicine in the University of Turin, to the post of professor of psychiatry. He has also been made director of the University Clinic for Mental Diseases.

WILLIAM WARDE FOWLER, M.A., Fellow of Lincoln College, Oxford, has been appointed a Curator of the Botanic Garden, in place of Edward Chapman, M.A., Fellow of Magdalen College, resigned.

CORRESPONDENCE.

AMERICAN JUDGMENTS OF AMERICAN ASTRONOMY.

THE astronomical notes published in the last two numbers of SCIENCE afford instructive illus-

trations of a habit of judging American and foreign scientific work which is too prevalent among us. While in nearly every other country scientific investigators and writers are apt to be more or less biased in favor of their own countrymen, giving frequent occasion for remarks on their ignorance of what is going on outside and on their general insularity, the system prevalent among us is directly the contrary, at least in astronomy, and, to a certain extent, in the allied sciences. The way in which this bias displays itself is so well illustrated by the notes in question that we may be pardoned for taking them as a text for some remarks.

Among the great wants of astronomy for half a century past has been a standard system of positions of the principal fixed stars, which should serve as points of reference in defining the positions of other stars and of the heavenly bodies in general. The first step toward this end was taken by Dr. Auwers about 1870, and consisted of a determination of the corrections necessary to reduce the principal modern catalogues of stars to a homogeneous mean system; that is to say, to a system which should be as nearly as possible self-consistent, and express the mean result of all the determinations of positions made in each region of the heavens. But this work, though most ably performed and marking an epoch in astronomy of precision, was defective in not rigorously taking account of the proper motions of the stars. Hence, Dr. Auwer's system was valid only near a central epoch, say about 1840 or 1850. That he did not make it permanently valid was doubtless due to the fact that at that time the older observations, especially those of Bradley, had not been reduced with sufficient rigor to determine the proper motions. It was, therefore, a fitting complement of his work that he set about the thorough re-reduction of Bradley's observations at Greenwich with the mural quadrant, during the years 1750-1757.

About 1878 was published Boss's system of declinations, which appeared in a quarto volume of some 200 pages. A careful examination of this work showed that it stood unequalled in the thoroughness with which all the material was collected and worked up; in the completeness with which the errors of the older adopted

values of the astronomical constants were corrected, and in the rigor with which the entire discussion was carried through and the results presented.

A year or two after the appearance of Boss's work, the new system for the *Astronomische Gesellschaft*, constructed by Dr. Auwers, was published. A very slight examination of this work would show that its superiority to that of Boss was at least open to question. The weakest point was that the proper motions depended entirely on the observations of Bradley with the old mural quadrant, which was known to be subject to errors the amount of which did not admit of determination. But this defect did not prevent the general adoption of the foreign system by American astronomers, even in the case where the other would have been most eminently appropriate, the official work of boundary surveys.

There is one final and conclusive arbiter of all questions concerning the accuracy of predicted motions in the heavens. This arbiter is subsequent observation. Let us wait a sufficient length of time and see on which system the positions of the stars are most accurately predicted. In certain features of the system and in certain regions of the heavens the two works differed so widely that a very few years of accurate observations would suffice to settle the question.

About twenty years have elapsed since the last observations on which either of the two works was based. Within that time four catalogues of stars have appeared, founded on observations made at the respective observatories of Pulkowa and Greenwich, prepared with all the refinements of recent science, and therefore superior to any before made. In these results, combined with such conclusions as can be drawn from the best previous observations, we have the basis of a comparison which is found in the number of the *Astronomical Journal* quoted in the note found in the last number of SCIENCE. Without going into technical details, it will suffice to say that there are six separate and independent features in which the respective systems differed most largely. These six features, tested by the four modern authorities just quoted, showed the following average errors or

difference between Boss's prediction and observations in different regions of the heavens, near the epoch of 1880:

—0''.02	+0.02
—0.03	+0.02
+0.03	0.00

It was then shown that, carrying back these six special points of difference between the two catalogues to the epoch of Bradley's observations, the actual differences between the two were larger than any likely deviation of Boss from the truth. In the most marked case the difference consisted in ten discrepancies, all in the same direction. Another very marked instance occurs in a region of the heavens including the northern part of the constellation Andromeda. In this region were found ten stars in the A. G. catalogue. The Polkowa catalogue of 1895, the most carefully prepared that astronomy has yet had at its command, showed that every one of these ten stars was in error in the same direction, that direction being the same in which they differed from the Boss system, and by amounts which could not be reasonably attributed to errors of the Pulkowa observations.

One would suppose the conclusion so obvious as to need no statement and admit of no question. Fifteen years of the most refined observations show a continuing agreement of the Boss system with observations which is most extraordinary, and which cannot possibly be shared by the other. This evidence, however, fails to convince the writer of the note. He claims that the results 'throw no new light on the subject.' If astronomers differ as to the question whether the approach to perfect agreement with observation above shown is conclusive, the question would seem to be forever incapable of decision.

Again, in the case of ten separate stars in which the deviations of the Bradley observations were all in the same direction, the writer remarks: "So we can hardly escape the conviction that our whole conclusion may be vitiated by a large error in a particular star."

Here it would seem that the astronomers must have recourse to legal advice to settle their dispute. Only a member of the legal profession can decide whether the concurrent evidence of ten

independent witnesses, all testifying to the same fact, may be 'vitiated' by one of them being very much mistaken. It is to be regretted that the writer of the note does not tell us just how far the one erroneous star must have been wrong in order to vitiate the result. The corresponding testimony of the ten Pulkowa observations upon another group of ten stars may be left out of consideration, because this conclusion might be vitiated in the same way.

S. NEWCOMB.

THE PERTURBATIONS OF 70 OPHIUCHI.

PROF. JACOBY'S review in a recent number of this journal (p. 197) is eminently fair in spirit; it is incomplete, and therefore I fear it will be misleading. It is a mistake to say that my work on the perturbations of 70 Ophiuchi is supported by the American observations, but contradicted by those made at the same time in Europe. On the contrary, the deviation from Schur's orbit and the work of the American observers is confirmed by the measures of all the best observers abroad. Thus the deviation appears unmistakably in the observations of Bigourdan, Callandreau, Schiaparelli, Glasnapp and Knorre. Since publishing the paper in *American Journal* 363, measures have been received from several of the above observers, and there is absolutely no doubt of the substantial accuracy of the American observations. Among the European observers Schur and Ebell (a student at Berlin) alone find no deviation, but Schur's measures are very discordant, and he admits (A. N. 3324) that they are of little value; while Ebell's measures show discrepancies on the several nights amounting to over ten degrees in angle.

Hence it is evident that all the best observations, both American and European, confirm the deviation from Schur's orbit and point to the existence of the dark body as the cause of this unexpected phenomenon. My researches on the orbits of 40 binary stars, which are now practically complete, will probably remove all doubt as to the propriety of using the distances in such investigations. Indeed the discovery of the perturbations in 70 Ophiuchi by using both angles and distances, after Schur had con-

sciously rejected the distances which would have given him the discovery, is a striking illustration of the evil of orthodoxy in scientific procedure.

T. J. J. SEE.

THE UNIVERSITY OF CHICAGO, February 11, 1896.

PSYCHOLOGY OF NUMBER.

TO THE EDITOR OF SCIENCE—*Sir*: As Prof. Fine in his review of McLellan's and Dewey's *Psychology of Number* (January 24, 1896) raised a question of considerable importance to educators and to psychologists, permit me to add a few words to the discussion, first thanking the reviewer for the generally appreciative tone of his article.

1. The question of principle raised is whether or no counting is measuring, whether or no integral number has a metric origin or purpose, and involves the idea of ratio. Now measurement is a word both of a more general and a more technical sense. That, in the most technical mathematical sense, counting is not measurement, is clearly recognized in the book referred to. But as it is held that in the larger sense of the term it is a process of measuring, and that the technical mode of measurement is an outgrowth, psychologically, of the broader and looser sense, this disclaimer amounts, perhaps, to little.

Starting from the larger sense, it is held that number has its psychological genesis in the felt need for valuation, and that its function (psychologically once more) is to serve the purposes of valuation. Now counting seems to me indubitably one mode of defining the value of a previously unvalued mental whole, and in that sense to be a mode of measurement. Any process of defining value is, I should say, a form of measurement in the broad sense of that term. Counting implies first a mental whole; secondly, the breaking up of that whole into distinct parts; third, the use of one (any one, not some one) of these parts as a unit; fourth, the measurement of the amount or value of the original whole, through equalizing it to a certain definite number of the selected unit.

But Prof. Fine says: "In however loose a sense the word may be used, 'measuring' at least involves the conscious use of a unit of ref-