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, Glasenapp 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 consciously 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-

erence. But no one ever did or ever will count a group of horses, for instance, by first conceiving of an artificial * unit horse and then matching it with each actual horse in turn—which 'measuring' the group of horses must mean if it means anything."

The whole point here is under what circumstances does one, not a mathematician or for mathematical purposes, count a group of horses. The answer is something of the following sort, it seems to me: One counts when one wishes to find out how many horses he has caught in a day's hunt, whether the same number has been driven back at night that were taken out in the morning; how much money is to be got in selling them, it having been settled that each horse is to fetch the same sum, etc., etc.; how one ranks as a chieftain, or a soldier, compared with others, etc., etc. In other words, one not having arrived at the *abstract* interest of the mathematician (and certainly the child to be educated has not) counts only *when* there is some value to be ascertained, and counts *by* setting off something which, for present purposes is a sample unit of value, *e. g.*, a horse, then equating the total value to the number of such units. Taking the matter in its development then, (and not at the stage of the mathematician when abstracts have already become concretes) enumeration is always to define value, *i. e.*, to measure.

If the book referred to did not recognize the distinction between this sort of measuring and the technical sort it should certainly be condemned. But one of the points emphasized is that the former is an imperfect sort of measurement; that we don't really know, *e. g.*, what the possession of 60 horses amounts to till we know what one horse is worth, and so measuring proper (measuring with measured units) is substituted for mere counting, *i. e.*, measuring with undefined units of value.

2. It is said that number is not ratio. If one is using ratio to denote a certain idea, and not a technical abstraction of the mathematicians, I do not see how this statement is to be reconciled with Prof. Fine's own account of enumeration. "To count a group of things on the fingers is merely by assigning one of the fingers to each one of the things to form a group of fingers which stand in a *relation of 'one-to-one correspondence to the group of things.'*" * And again, "When we say of two groups of things that they are equal numerically, we simply mean that for each in the second there is one in the first, and for each thing in the first there is one in the second, in other words that the groups may be brought into a *relation of one-to-one correspondence.*" What does the phrase italicized mean, save the idea of ratio? If this way of stating it had only been known to me when the book reviewed was written, I should gladly have utilized it to indicate precisely the point we were trying to make—the implicit presence of the ratio idea in every number.

Psychologically there is, of course, a difference in the mental attitude in recognizing a thing as 'one,' as unity, as a whole, an individual, and recognizing it as '*a* one,' a unit. The primary problem the educator has to face, if he is to rationalize the teaching of arithmetic, is the discovery of this difference. The answer given is that 'one' (qualitative individuality or unity) becomes '*a* one,' a unit when it is *used* to *measure* value; and that, in turn, the need for this use arises when the thing is no longer taken as an adequate end, but as a means to be adjusted to some further end. *E. g.*, once more, when a man is *wholly* occupied in riding or hunting, or feeding a horse, when that absorbs his whole interest, he never takes the numerical view; when he wants to know how much of a horse owner he is, and how far this horse contributes to that end, he necessarily takes it. The question then is whether 'one' ever becomes '*a* one,' save as it is put into a 'relation of one-to-one correspondence?'

3. Prof. Fine remarks that 'the one postulate of arithmetic is that distinct things exist.' The mathematician may perhaps be reminded that this postulate is precisely one of the chief problems of the psychologist. Given a certain num-

* Whence and wherefore this artificial? The point to be proved involves nothing about an 'artificial' unit, but only a unit of reference, and that surely a horse is. But even if the term were relevant in the argument the question would arise whether the use of an artificial unit or of a measured unit is the essence of technical measurement; whether, indeed, a foot is, psychologically, more artificial than a horse.

* Italics mine.

ber of things already recognized as distinct, and it is a very simple matter to go ahead and enumerate them, though even that must have a psychology *motivation*. But the whole tendency of contemporary psychology is to take a psychical continuum as its datum, and find distinctness (the property at the basis of number) as the outcome of a process of differentiation. The identification of this process, the ascertaining of the circumstances under which it arises, the mode of its operation—this is the thing which the psychologist wants to know about number, and is the thing the educator must know to secure the conditions under which the child shall form the number concepts easily and efficiently. The theory of the book, 'Psychology of Number,' viz., that the differentiation and enumeration of units arises through the progressively accurate adjustment of means to end, may be right or wrong, but its error can hardly be established, I take it, by a mathematical view which considers number only as it is after it is fully developed, and has become so familiar as to be itself a complete object to the mind. Without pretending to a knowledge of numerical theory which I do not possess, I may say that it seems to me that the work done by Gauss is at precisely the opposite pole from that which the educator needs from the psychologist, *i. e.*, Gauss was attempting to reduce to its ultimate simple numerical generalizations the developed mathematical structure. Dr. McLellan and myself were engaged upon the much humbler task of finding out what sort of a mental condition creates a demand for number, and how it is that number operates to satisfy that demand.

May I conclude by referring to the practical point involved? The trained mathematician as such is, of necessity, interested in the further use of certain finished psychical products. As a mathematician any reference to the preliminary development of these products can only disturb and divert him. But the problem for the pupil *is how to get the standpoint of the mathematician;* not how to use certain tools, but how to make them; not how to carry further the manipulation of certain data, but how to get meaning into the data. This is ultimately a psychological question, not a mathematical one, although it has to be translated over into mathematical terms and processes; and none is so well fitted to do it as the mathematician, provided only he will project himself far enough backward in the scale of development to realize the problem. The point does not conclude with primary instruction. Our text-books of algebra, geometry and high analysis are almost entirely written from the standpoint of an elegant and logical exposition of the matter as it stands to the trained mathematician. They are very nice for one who doesn't need them any longer. The first books written from the standpoint of one who is still coming to consciousness of the meaning of his concepts will, perhaps, seem foolishness to the trained mathematician, but they will mark the dawn of a new day to the average student. I venture the statement that (putting aside the few with the inborn mathematical instinct) higher and secondary mathematics is to the majority of students a practical riddle with no definite *intellectual* content in itself. What meaning it possesses it has got by way of attained practical facility in solving problems; or through its applications to other sciences or to engineering. It will hardly be denied that the educational value of mathematics is not realized until its concepts and methods have a definite intellectual meaning and content of their own. Can this be secured, save as the methods of instruction follow the evolution of the process out of its cruder psychical forms to the more finished?

I shall be more than satisfied to have made many blunders on the mathematical side if only I do not offer myself up in vain as a spectacle; if only more competent psychologists take up the matter, and if only mathematicians may descend from their acquired mathematical plane and endeavor to rethink the psychical conditions and steps through which their present magnificent apparatus has grown out of primitive, non-mathematical or crudely mathematical forms up to its present high estate. If the psychologist will risk some blundering around among the mathematical concepts, and the mathematician will recognize the relevancy of the psychological demand, and venture a little blundering upon that side, both parties may not only come to an understanding, but mathematical teaching may get what it to-

day so largely lacks, some relationship to the psychical needs and attitudes of those under instruction.

JOHN DEWEY.

UNIVERSITY OF CHICAGO, February 6, 1896.

DOES THE PRIVATE COLLECTOR MAKE THE BEST MUSEUM ADMINISTRATOR?

THE concluding portion, section K, of Dr. Goode's recent paper on the Classification of Museums, is devoted to a consideration of private cabinets and collectors, and to the major portion of the propositions therein laid down all can heartily subscribe. There is, however, one among them to which I can not fully assent, at least so far as museums of natural history are concerned, and that is, that "The person who has formed a private collection can most successfully manage one for the use of the public."

It must be confessed that this doubt largely rests upon theory, but an acquaintance with some collectors makes it seem probable that it is, after all, well founded.

A considerable amount of collecting is done with no purpose in view other than that of accumulating specimens, but, on the other hand, a private collection may be formed with a definite purpose and along certain lines. In the one case the collector certainly shows no unusual fitness for a position in a museum, while in the other he is interested in his collection for what he can get out of it himself and not for the benefit it may be to others, and this is exactly the opposite view to that which should be held by an officer in a public museum. This is not saying that such is the point of view universally assumed by museum curators, but it is certain that the success of a public museum depends on the extent to which it is adopted. Again a private collector is, from the nature of the case, apt to be one-sided, to lay too much stress on one group to the exclusion of others, and thus to lack the evenness of balance which should be one of the characteristics of the 'museum man.' This one-sidedness frequently takes the form of undue preference for rare or costly specimens, attaching an undue importance to the specimens themselves rather than what is to be got out of them.

Moreover the care and arrangement of a private study series and of a public study series, and, above all of an exhibition series, are entirely different things and require a totally different treatment. A private series may be ill-arranged and poorly labeled, but the owner knows each specimen, its history and whereabouts. A public study series should, on the contrary, be so arranged and so labeled that any student may consult it and make notes upon it, while in an exhibition series the specimens should be so chosen that, while each conveys some information, all form a harmonious whole.

A private collector may know his own needs, but he would not know or would not care for the needs of the public, and he would carry to a public museum the taste for accumulation, or for research, which probably led to the formation of his own collection. Accumulation is a good thing, but it needs to be properly directed in order to be of public service, while there is probably no greater drawback to the public efficiency of a museum officer than too great devotion to original research, as this leads not only to lack of care for material which has served its turn, but to a very decided lack of interest in the public which must be reached through the exhibition series.

This criticism is by no means to be construed into a criticism of the private collector; the value of his work and the influence of his collections are immense; it is simply a denial of the proposition that because a man has formed a private cabinet he is therefore best fitted to administer a public museum.

F. A. LUCAS.

WASHINGTON, D. C.

SCIENTIFIC LITERATURE.

Lehrbuch der Entwicklungsgeschichte des Menschen und der Wirbelthiere. OSCAR HERTWIG. Jena, Gustav Fischer. 1895. Pp. xvi + 612.

This excellent work now appears in a fifth edition, in which many improvements have been made. Prof. Hertwig is especially distinguished both for his comprehension of the problems of morphology and for the lucidity of his explanations, so that his text-book has long been accepted as a valuable treatise both for students and for advanced workers, and has been accorded the distinction of translation into several languages. A very admirable