A Critique of Psychophysic Methods.

I READ with care the comment by Dr. Boas upon my article in the *American Journal of Psychology*, and carry away from it the impression that there is less difference of opinion between us than Dr. Boas supposes. The question is not one of fact, but of interpretation. We all admit that there is a psychophysic fact for which the word 'threshold' is a good name; but the important question is, How shall we theoretically understand the conception, and what place shall we allow it in the development of an experimental psychology? Fechner makes it rank as by all means the most important factor in psychophysics, and is willing to sacrifice Weber's law before yielding the supreme and fundamental fact of the threshold. He is led to this view by the method of the 'just observable difference,' and by the neglect of the other two methods. This entire structure I regard as reared upon an illogical basis, and a psychophysics based upon the mathematical methods as very different and much sounder than the other. The threshold as a practical, empirical fact, I not only fully admit, but even suggest methods of further developing its utility; but its theoretical importance with reference to the establishment of a psychophysic law I regard as almost *nil*, its true importance lying in another direction. This, I trust, defines my position clearly. A single illustration may not be out of place. Dr. Boas says that a balance has a threshold, and I accept the comparison. This threshold is something to be eliminated, and that balance is the finest that has the least of this characteristic. The theoretical balance upon which mechanics works out its principles has no threshold. But apart from this, I think the physicist will agree with me that it leads to more useful and scientific conceptions to regard every particle that is placed upon the pan of the balance as producing an effect alike in kind, and differing only in degree from that produced by a mass sufficient to turn the balance. There is no point where a new factor enters, and the turning of the balance is a merely empirical fact. Returning to the psychophysical methods, I should state the case thus: it is generally admitted that the basis of the method of the "right and wrong cases," as of the "average error," ultimately rests upon the fact that the probabilities of my making errors of various degrees follow the path traced by the probability curve. This is the fundamental fact of the entire science of psychophysics. Now, this curve is a *continuous* one, and has no break in it, no point characterized by any special peculiarity, no threshold in any true sense.

A word as to my misrepresenting the views of my opponents. The important point is, not what the upholders really do say, but what logically follows from the position they take. If they do not say what I attribute to them, it is because they are inconsistent; and I have guarded myself against this misunderstanding by at times stating, and elsewhere unmistakably implying, that I was dealing with the logical consequences of the threshold theory, and not with that particular portion of it that its adherents happened to employ.

The second point in Dr. Boas's criticism is a real difference of opinion between us. He thinks "doubtful" answers should be admitted in experimentation: I most emphatically object to them. In my paper I regarded the objections to allowing such answers as so necessarily following from the theory of the "right and wrong cases" method, that a full statement of the reasons was superfluous. Any one of half a dozen reasons is enough to show the impropriety of the "doubtful" answers. For instance: it is admitted that the methods should be as comparable, one with the other, as possible. Now, the method of the "average error" depending upon the same principle as that of the "right and wrong cases," allows no doubtful answers. Again: there is no reason for singling out "doubtful" answers as any thing peculiar. Why not make a special rubric of unusually confident answers? And if we do, as Dr. Boas suggests, make a threshold where doubtful answers no longer occur, that threshold will vary so much in different individuals, etc., that it will invalidate a large share of the results. And what shall I say when some one else proposes a threshold for another degree of confidence, say, the point where one is sufficiently sure of the correctness of one's answer to risk money upon it, and so on, *ad infinitum?* If you mean that this subjective feeling is worth taking account of, I fully concord, and will welcome the skilful observation of this feeling as an important contribution to psychophysics.

JOSEPH JASTROW.

Baltimore, March 12.

On the Sense of Taste.[1]

AT the Philadelphia meeting of the American Association we presented a paper upon the 'Delicacy of the Special Senses,' — a topic upon which we have since continued our investigations from time to time.[2]

The method pursued in the following experiments was as follows: —

Solutions of known strength were made of the substances to be tasted; then, by successive dilutions, several series of solutions were made from these, each one in the series being of one-half the strength of the preceding one. The bottles containing these solutions, and several bottles of water, were placed without regard to order, and the person to be experimented upon was requested to separate them into their proper groups by tasting them. In each series the last solution was so dilute as to be beyond recognition. All unrecognized solutions were classified as water.

We chose for our tests the following typical substances. The strength of the initial solution of each is given below.

1. (Bitter) quinine, one part in 10,000 parts of water.
2. (Sweet) cane-sugar, one part in 10 parts of water.
3. (Acid) sulphuric acid, one part in 100 parts of water.
4. (Alkaline) sodium bicarbonate, one part in ten parts of water.
5. (Saline) sodium chloride, one part in 100 parts of water.

The attempt was made to include other substances, as aromatics, in the test; but it was soon found that the odor betrayed their presence without the aid of the sense of taste.

Other investigators have added astringents as a sixth class, but these substances are so often recognizable by odor, color, or some special taste not purely astringent, that it was thought best not to include them.

Tests by the method above described were made upon 128 persons; 82 being male, and 46 female observers.

The following table shows the amount of each substance which could be detected by the average observer: —

Substances.	Male Observers detected.	Female Observers detected.
Quinine . . .	1 part in 392,000	1 part in 456,000
Sugar . . .	" " 199	" " 204
Acid	" " 2,080	" " 3,280
Soda	" " 98	" " 126
Salt	" " 2,240	" " 1,980

From the above results the following conclusions may be drawn: —

1. The sense of taste is vastly more delicate for bitter substances than for any others. It is possible to detect quinine in a solution that is only $\frac{1}{2000}$ the strength of a sugar solution, and we have previously shown (*loc. cit.*) that quinine is only $\frac{1}{10}$ as bitter as strichnine.

2. The order of delicacy is, bitter, acid, salt, sugar, and alkali.

3. The sense of taste appears to be more delicate in women than in men. This is true in the case of all the substances excepting salt. As we had found a similar difference in favor of female observers in an earlier and independent set of experiments, which agreed in every essential particular with the results of the present test, we do not regard it as an accidental difference, or as likely to disappear in more extended investigations.

Marked differences in the delicacy of the sense of taste of different individuals were met with in the course of these experiments.

[1] Paper read at the New York meeting of the American Association for the Advancement of Science, August, 1887.

[2] See Relative Bitterness of Different Bitter Substances, by E. H. S. Bailey and E. C. Franklin, in *Proceedings of the Kansas Academy of Sciences*, 1885; Relative Sweetness of Sugars, by E. H. S. Bailey, in *Report of Kansas Board of Agriculture*, 1884; The Sense of Smell, by E. L. Nichols and E. H. S. Bailey, in *Nature*, XXXV. p. 74.

There were persons who could place in the proper class, solutions containing one part of quinine in 500,000, and other substances in correspondingly high dilution, while some failed to detect solutions of more than three times the above strength. In how far this was due to education, we are unable to say. Among the men examined were many who have been accustomed to handling and recognizing drugs and medicines, and yet even these were frequently surpassed by female observers who had no such training.

In some previous experiments upon the sense of smell, of which an account appeared in *Nature* (*loc. cit.*), we noted almost as marked superiority on the part of male observers.

In a few cases, the ability to detect a dilute sweet was accompanied by a lack of ability to detect dilute bitters. This peculiarity was, however, far from being a general one.

As quinine is so largely used as a medicine, especially in the Western States, it was thought that its habitual use might dull the sense of taste for this particular substance. Among the observers subjected to our experiments, the use or disuse of quinine seemed to have had no especial influence.

The experiments just described suggested several interesting questions upon which we were unable to enter. How many, for instance, of these substances, taken of equal delicacy-strength, could be detected together in a mixture, in what order would they be detected, and by what portion of the tongue or organs of taste? Would all observers recognize them in the same order as to time? What would be the influence of the temperature of a solution tasted, upon the delicacy of the sense of taste?

As to the degree of accuracy with which our results give the average delicacy of the human sense of taste for the substances in question, we are led to believe from their substantial agreement with determinations based upon the previous set of experiments already alluded to, alike in the matter of absolute delicacy, of relative delicacy, for the various substances used, and of relative sensitiveness of male and female observers, that they are but slightly influenced by individual idiosyncrasies, and may be regarded as fairly representative.

E. H. S. Bailey.
E. L. Nichols.

On New Facts relating to Eozoon Canadense.

In the February number of the *Geological Magazine* there is an interesting article by Sir J. W. Dawson, 'On New Facts relating to Eozoon Canadense.' In paragraph 9, 'Continuity and Character of the Containing Deposits,' there are some remarks respecting the stratigraphy of the Archæan or older crystalline rocks of Canada upon which I wish to make a few comments.

The author does not indicate what are, in his opinion, "the extravagant statements respecting the older crystalline rocks now being made," nor by whom they have been made. Neither does he state what portion of the Laurentian system is referred to under the term 'Middle Laurentian,' nor where he has recently examined it. I am not aware that Sir W. Logan ever used the term 'Middle Laurentian.' As regards the 'continuity of the great limestones' over certain areas, and their intimate association and interbedding with the gneisses, both orthite and anorthite, it has, so far as I know, never been questioned. In some cases, however, the limestones are very irregular, and occur in longish, more or less lenticular bands interleaved with the gneisses, often in such a manner as to suggest an origin posterior to that of the gneisses, or, rather, to that of the strata from which they have been produced. It is, I think, more than probable that original sedimentation of calcareous matter, and subsequent segregation, have both operated in producing the phenomena now observed in connection with these great limestone belts, the latter somewhat analogous to that which has produced the great 'quartz belts' in the Nova Scotia goldfields.

I must entirely dissent from the views expressed by the author in correlating any of the so-called Upper Laurentian anorthosites of the vicinity of St. Jerome, or elsewhere, with the Huronian rocks west of Lake Superior. The massive anorthosites, as I have elsewhere stated, are clearly intrusive, and the surrounding gneisses and limestones do not pass beneath them; and there are no grounds whatever for regarding them as an unconformable Upper Laurentian series. On p. 4, 'Report of Progress, Geological Survey of Canada' 1879–80,' I wrote, "If the foregoing determinations by Mr. Vennor, which are given in his own words, are correct, they seem very conclusively to prove, what I have already stated to be my opinion, that the labradorite or Norian rocks of Hunt do not constitute an Upper Laurentian formation, but occur in part as unstratified intrusive masses, and in part as interstratifications with the orthoclase gneisses, quartzites, and limestones of the Laurentian system." It is satisfactory to find that Sir William Dawson is now disposed to admit that the "great masses of labradorite may be intrusive;" but when these are eliminated, nothing remains of the Upper Laurentian as defined in any of these areas, from the Moisie River to St. Jerome; and unless the interstratified anorthite gneisses are made Upper Laurentian, the term, so far as the Norian or labradorite rocks of the areas named are concerned, must be abandoned, and I would reiterate what I wrote in 1884 ('Descriptive Sketch of the Physical Geography and Geology of Canada, 1884'):—

"As regards the so-called Norian or Upper Laurentian formation, I have no hesitation in asserting that it has as such no existence in Canada, its theoretical birthplace. Wherever these Norian rocks have been observed, they are either intimately and conformably associated with the ordinary orthoclase and pyroxene gneisses, or they occur as intrusive masses when they present no gneissoid or bedded structure. They clearly cut the surrounding gneiss, and are probably due to volcanic or other igneous agency in the Laurentian age."

Considerable further investigation since the above was written has entirely supported the view then expressed.

Alfred R. C. Selwyn.

Ottawa, March 13.

Queries.

30. Poisonous Jelly-Fish. — Last summer, while bathing on the Maine coast, I had what was to me a novel and not very enjoyable experience. While swimming I happened by accident to kick some sort of an animal. For an instant the feeling that passed over my feet was like a slight electric shock. Of course, I turned to see what the animal was, and, from the glimpse which I had, I should call it a red jelly-fish. For three or four hours after, my feet were slightly inflamed and very painful, the feeling being like that caused by a burn. Afterwards I learned that a certain kind of jelly-fish was said by fishermen thereabouts to be poisonous. Can you tell me through your columns about this animal, what it is, and how it stings, shocks, or poisons? What is the remedy for its poisons?

Zoö.

Boston, Mass., Feb. 29.

Answers.

30. Poisonous Jelly-Fish. — The above doubtless refers to the effects of our common large red jelly-fish (*Cyanea arctica*). Many jelly-fishes have the power of stinging soft-skinned animals, and in this way ordinarily kill and secure their prey; but there are only a few species that have nettling threads powerful enough or long enough to sting the human skin. On our New England coast the only ones that are able to sting thus are the *Cyanea*, referred to above, and the Portuguese man-of-war (*Physalia*); but the latter is not common, and is rarely, if ever, found on the shore north of Cape Cod. The *Cyanea* stings many persons very severely, especially if the tentacles come in contact with a tender part of the skin, as the face, lips, eyes, or between the fingers, and of course on any part of the body that is ordinarily covered; but in my experience they will not sting the palms of the hand. The sensation is much like that of the sting of a nettle ordinarily; but in some cases, or with some persons particularly sensitive to the poison, it results in numbness, swellings, and subsequent eruptions, and even ulcerations. The *Physalia* stings much more severely than *Cyanea*, and is able to cause temporary paralysis of the arm or leg; and in some experiments it has been found to act in such a way as to affect the heart: perhaps in a severe case it might even cause paralysis of the heart. The nature of the poison is unknown, but it must be very powerful, for the quantity is minute.

A. E. V.

New Haven, March 10.