Professor Baldwin evasions are exceedingly skilful, and the eruptions of polite invective which usually follow them exceedingly telling. But those who have followed this discussion with the purpose which I had in beginning it — the purpose of finding, if possible, the true explanation of the results of psychological experiments upon the duration of the simple reaction — will refer from his latest paper to mine, and read comparatively. I shall therefore assume that they have noted the importance of Professor Baldwin's admissions (e.g., p. 81), promises (e.g., p. 85) and qualifications (e.g., p. 89), and proceed at once to the special points emphasised in his argument[1].

1. As to the Leipsic procedure, I can only repeat deliberately what I have before deliberately stated: that, so far as my knowledge goes, no subject who has been found capable of reaction (of giving approximately the same response to the same stimulus in a series, say, of fifteen trials, after practice) has been neglected either in the parent or in any more recently established laboratory. It was Martius -- one of the contributors to the Leipsic theory -- who first analysed what is now known as the "central" form of the simple reaction, a form which is neither sensorial nor muscular. In the Cornell Study from which Professor Baldwin quotes the 'disposition view' are given the times of several observers who did not show the sensorial-muscular difference; and that although it is expressly stated that the object of the Study was not to examine and account for these divergences from the norm. In face of these and similar facts, the charge is made that I (and, I suppose -- else the matter would not be important -- the Leipsic school with me) think that certain results "ought to have been suppressed," and that certain cases "ought not to have been investigated"[2]! [p. 237]

2. I stated that there were many ways of testing memory type besides that of the reaction experiment. Professor Baldwin challenges me to produce my methods, remarking that he knows of none which are conclusive except those of introspection and pathology. I was referring to the normal mind when I made the statement; and as all psychological experiments on the normal mind, the reaction experiment included, follow or should follow the introspective method, I am afraid that a list of my methods will not broaden Professor Baldwin's knowledge.
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However, I recognise the justness of the challenge, and give the laboratory and other methods (co-ordinate with the reaction method as sub-forms of introspection) which I have found useful [3].

Methods of Investigating Memory Type. (1) I believe the best method for the determination of memory type to consist in the introspection of a trained observer at times when consciousness is, so to speak, off its guard. He must educate himself to take his mind unawares when he is remembering, or failing to remember. All sorts of rememberings -- cases referring to all the different sense departments -- must be noted. This, the most direct way in which introspection can be practised, is also, I think, the most fruitful. I have employed it for five (not 'one or two') years; and have only refrained from publishing my results in detail because, as I said in my previous paper, some facts are still obscure to me. (2) I have [p. 238] tried to get at memory type by questioning, with as absolute as possible avoidance of suggestion. This method can be usefully employed only where the subjects questioned have a general knowledge of psychology but are ignorant of the doctrine of memory type. Its results check and are checked by those of the foregoing and next following methods. (3) Questioning with suggestion is a method covering all such tests as Mr Galton's breakfast-table recollection. It has grave dangers, and must be used with great caution. I have tried to check it by what is called the "method of reproduction," -- the subject being required to reproduce his memory image in objective form; and by an error method,-- the memory image being compared with some objective standard. Neither check is very easy of application. But my results lead me to think that a method may be perfected, under this general head, which will be especially valuable for the estimation of the relative importance of the different memories in a given consciousness. (4) Another way of testing the relative importance of memories, or the fixity of a particular memory, is the following. A series of experiments on memory is made, with no directions to the subject as to the way in which he is to memorise. He is encouraged to be as full as possible in his introspective remarks. From these, checked by special experiments the experimenter ascertains the type of memory employed, a new set of experiments is then begun, in which the subject is told to remember in a particular way, different from the way of least resistance. The experimental results and the subject's introspection show whether the shift of type is successful, or only partially or sporadically possible, or impossible. (5) Sometimes two types are used in one and the same act of remembrance: introspection reveals the fact, but cannot say, under the ordinary conditions of memory, which type is the more indispensable. Experiments by the method of reproduction, checked by others with voluntary suppression, are again useful. (6) It is very Important to determine whether non-employment of a type is due to nature or habit and education. I am this year trying to get a reliable method of investigating the problem, and have obtained good preliminary results from two forms of the method of reproduction. (7) Another method of testing type in general I owe to Professor M. Washburn. Psychological experiments are often made under distraction: the subject is required to judge of the difference or likeness of impressions while he is adding numbers, etc. The mistakes made in this addition, etc., are indicative of type: if one sees the figures to be added, one's mistakes differ from those made by a subject who hears the numbers spoken as he adds them. (8) Mr A. Fraser has shown how a writer's memory type can be determined from his writings (Am. Journ. of Psych., IV., pp. 230ff.). This is the method which should replace 'surmise' in the case of Donders.

3. Professor Baldwin wrote of the subjects of his Study as follows (italics mine). "The reagents were, besides the writers (B. and S.), Mr Faircloth (F.), a student who had had only the experience gained from the practical work in this subject of the course in Experimental Psychology. His reactions were ready and unconfused, and from all appearances he was a normal and more than usually suitable man for such work. The fourth, Mr Crawford (C.), is an honour student in this subject in . His reactions were taken in the course of another investigation, and being so few in number, they are included only because they give a certain case of a capable reagent whose sensory is shorter than his motor reaction. [p. 239] We hope to test him further." I read this to mean that the authors believed their two reagents to be reliable subjects, but were a little doubtful as to the extent of their practice. Hence I said: "The greatest reliance is placed upon the times of B. and S." · It was an instance of the psychologist's fallacy: had I written the paragraph, I should have meant what I took it to mean. I am sorry that I misunderstood the writers[4].
4. I come to the matter of Professor Baldwin's own reaction times. In his *Senses and Intellect* he remarks, in general terms, that he had anticipated Lange's discovery of the sensorial-muscular difference. Lange found that the difference averaged one-tenth of a second (*Phil. Stud.*, IV., 494; Wundt, *Phys. Psych.*, 4te Aufl., II., 311). Many subsequent experiments have confirmed this result (e.g., those published in the *Phil. Stud.*, VIII., 144; and those of the Cornell Study before alluded to), and it is now generally accepted by 'the Leipsic people' as the normal difference between the two forms (Wundt, *loc. cit.*; Kuelpe, *Outlines*, 408, 410). If Professor Baldwin anticipated Lange, his times must have shown an original difference of some 85 to 115s. If they did not, he did not anticipate Lange.

The differences between the times given for himself in his Study are, as I said in my earlier paper, 29, 7, 12 and 46s. No one of these is anything like the sensorial-muscular difference. The 7 and 12 are times no larger than the average *m.v.* of the muscular reaction (about 10s); an *m.v.* of 30s is not uncommon in the case of the sensorial form; and 46 would be a typical "central" difference. Either Professor Baldwin is mistaken in thinking that he anticipated Lange, or his times have changed since he wrote his *Senses and Intellect*. S.'s differences are 51, 40, 79 and 40s. Taken as absolute times, these would all be "central," though one shows an approximation to the true sensorial-muscular difference. I do not think, however, that the differences can be treated in this way, since neither B. nor S. gave what would be ordinarily regarded as a muscular reaction. The times are 171, 149, 164, 138; 195, 184, 158, 179s. These are all, in my opinion,-- and I believe that [p. 240] those familiar with chronometrical results will agree with me,-- more or less "central" or mixed reactions. The muscular reaction to sound averages 120s.

5. Professor Baldwin resents my method of appraising his theory. I confess that, when I am trying to form a theory of certain phenomena or to estimate a theory already set up, I like to have the facts 'catalogued,' ticketed and weighted. Professor Baldwin objects to bringing facts together: he distributes them sparsely in a matrix of theory,-- like the infrequent plums in school plum-cake. Then, if the critic complains of the quality of the compound, he says: But I have plenty more plums in the pantry. How does that help the present consumer?

The type-theory has been written about in a medical weekly, a philosophical bi-monthly, a psychological bi-monthly, and a book. Now we are told that its presentation is not yet complete. I did not, of course, know this when I criticised it. Nevertheless, I do not regret the criticism: since it may prevent overhasty acceptance of an attractive hypothesis, and may impel Professor Baldwin to show his full hand to the psychological public.

Something might be said, I think, from the ethical standpoint, of this piecemeal doling-out of a scientific theory. Had Professor Baldwin's article left me a shred or two of moral character, I might have made bold to say it.

6. A few minor differences remain to be cleared up. I deal with them in a foot-note. [p. 241]

In conclusion, I cannot but express my regret that Professor Baldwin should have seen fit to write a dialectical and personal rejoinder to my criticism, without furnishing new facts or reasons for the absence of facts in earlier publications. A good deal of his reply, and therefore of this answer to it, might have been disposed of in private correspondence. Until the promised support is brought up, the theory remains what it has been,-- a very happy idea, or ingenious analogy, apparently natural and probable, but (so far as published) based upon an altogether insufficient substrate of fact.

I also regret Professor Baldwin's attitude to the "Leipsic people." He is a professor of experimental psychology; he must know the literary history of reaction theories,-- he must know how much patient work the "Leipsic people" have done, for how many years,-- how much the different theorists differ, and how the central theory has advanced,-- how the theory compares with other theories, and how adequately it covers the ground of ascertained fact. Yet he nowhere meets the Leipsic theory as a theory, but only questions its norms; he sets its authors
contemptuously aside, as if to have worked at Leipsic meant a biassed view of psychology in
general; he charges "Wundt, Kuelpe," et id genus omne in the present instance with "a flagrant
argumentum in circulo," and attributes to them an unscrupulous rejection of results which make
against their circulus;-- when some of these results are published by their own "people," and
some even in their own organ! I have tried to write moderately in this and my previous paper,
and have no wish to emulate Professor Baldwin in the matter of name-calling at the last
moment. But I cannot think that his attitude to a long line of predecessors in the field is either
scientifically or ethically defensible.

E. B. TITCHENER

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Footnotes

[1] I give one instance of the way in which Professor Baldwin can parry an objection. In
his Psych. Rev. Study he identified the 'disposition view' with the Leipsic theory. I urged that
the 'view' was not a theory at all; and that the type theory had to meet, not it, but the Leipsic
theory proper,-- something quite different. He now says, in effect: I grant that the view is not a
theory; but that leaves my theory in a better position than ever, since it is a theory.
To which
course, reply that the rejoinder is formally correct, but that the objection holds as strongly as it
held before, inasmuch as no comparison of the type theory with the Leipsic theory has been
carried out.

[2] Nine gentlemen took part with me in my Leipsic Study. I published the results
obtained from Dr Meumann, Mr H. C. Warren and myself. There are consequently seven (not
six) to be accounted for. One devoted almost all his time to the apparatus. One was called
away on military service early in the course of the investigation; the series which I have from
him promise well. One found the apparatus too complex, and its management too tedious, and
withdrew from the research group. One gave such curiously slow reactions that they were
hardly reactions at all. I was advised by Professor Wundt to continue work with him, but he left
the laboratory for a reason which I cannot recall. One was found to be colour-blind, and left my
group for another in consequence. I have many series from him, which may be useful some
day to compare with those taken from other colour-blind persons. One was unanimously --
himself included -- referred to the category of incapables in this department of work. It would
have been interesting to study his irregularity (here I heartily agree with Professor Baldwin): but
that was not the object of my inquiry. It would have demanded simple experiments in many
sense spheres: I was desirous of making complicated experiments in one. The last participator
was the 'odd man' of the group: a very useful personage, liable to be called upon at short notice
to replace an absentee as experimenter or subject, in order to prevent interruption of the work.
His results were good; but they were too scanty to be published, and were not intended for
publication

Only one of the seven, then, was rejected on the ground of incapacity: though others
might have been, had they continued with me. And it is surely evident that irregularities cannot
be explained till we have norms whereby to explain them; i.e., that it was more important to
proceed with the original research than to turn aside to examine the single case. This is to me
so obvious, that I almost wonder whether Professor Baldwin and myself are not using the term
"reaction experiment" in two totally different senses,-- such as those indicated by Dr Rivers,
Journ. of Mental Science, Oct. 1895.

[3] Is it illogical, as Professor Baldwin implies, to state that there are many methods
of testing type, and yet that the elucidation of type is difficult? There are many methods of learning
Greek.

In Nature of , a reviewer says: "Surely we all know what is the particular language of
our own translation of experience." If we did, all the method-work -- reaction and other -- would
be needless.
[4] Just as, I am sure, Professor Baldwin will be sorry that he jumped to an interpretation of the sentence in my Leipsic Study, which turns out to be very largely wrong. I must be more accustomed to making mistakes than Professor Baldwin is; for I find it impossible in that case to work myself up to the height of moral and intellectual indignation from which he looks down upon my misreading here.

My presumption that the writers were working definitely upon the type theory from the outset was based on the statement that one of the "questions set for research" was that of "the differences of reaction times for different individuals under identical conditions."

In the paragraph in which he insists that the greatest reliance was not placed upon the times of B. and S., Professor Baldwin writes that these times are "very neutral to the discussion." Yet they receive quite detailed treatment in his Study in the examinations following the two Tables. Why?

[5] Professor Baldwin says that his times "have only changed in that the distinction is less marked than it used to be; and this I go [to] the trouble to explain in the same article as probably due to habit and practice." In my copy of the Study there is not a word of this explanation. The change in the author's times is not once referred to. A general statement is made about habit towards the end of the Study; I commented on it on p. 514 of my criticism. It does not contain any the most remote trace of personal reference.

[6] (1) "How can Kuelpe say beforehand that the muscular form will turn out in each case to be shorter than the sensorial?" If Professor Baldwin will read Kuelpe's Studien articles,-- or if he will even read on for a single page of the "Outlines," from the place of my quotation,-- he will find Kuelpe's answer to this question. (2) "Is not the fact that F is a musician something of an explanation of his auditive 'disposition'?" Not necessarily; not i.e., if other musicians do not show auditive dispositions in their reactions. It is just here that facts are so useful,-- or so obstinate. (3) Defect of vision might, certainly, lengthen: reaction time. I do not see that this helps to explain the reaction itself. (4) The rest of the paragraph which, has called forth these last two remarks is obscure to me, m spite of many readings. The type theory would hardly be a theory of the geistige Anlagen which it presupposes, even if it fitted all the reaction facts. It surely posits memory type; it does not state the conditions under which one or other type may be looked for. I fail to see, therefore, how its application can be 'an investigation of the so-called 'dispositions' to find out what they really are.' The Study, indeed, dismisses this problem (p. 78): it is evident, we read, that attention is now motor, now sensory, differing in individuals with type,-- "apart quite from the question as to how one of other state of things comes to be as it is in any one case." At the same time, I admit that the incomplete statement of the theory may account for its obscurity on this point, and shall await the complete presentation before offering further criticism. (5) I quoted Professor Cattell's letter, because he allowed me to publish it under his name. I did so altogether unhesitatingly, because Professor Cattell has taken part in the discussion of the validity of Lange's distinction (readers of the Studien will know how rigidly his adverse criticisms were 'suppressed' by Professor Wundt), and because every jot of direct evidence for or against the type theory was important to me. When the 'exact figures' and their analysis are published Professor Cattell's cases will, undoubtedly, carry greater weight than they can in outline form. The same is true of Professor Baldwin's cases: "I fear that those mean variations which "are too complex to be of any value" will still be asked for by the cataloguing psychologists. (6) M. Inaudi's case tells heavily against the type theory, as published, for the reasons given on p. 513 of my earlier paper.
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