

The 'Type-Theory' of Reaction  
By J. Mark Baldwin (1896)

Classics in the History of Psychology

An internet resource developed by  
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ISSN 1492-3713

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First published in *Mind*, 5, 81-90.

Posted December 2001

In the Oct. No. of *Mind* Professor Titchener devotes some pages to a very discriminating examination of the recent 'Study' of mine in *The Psychological Review* (May, 1895) in which I stated in some detail a theory -- announced some time earlier -- to explain the variations shown by different reagents in the time of their reactions. His statement of the question is so full and his quotations of my statement of it so generous that I need not now do more than refer the reader to his article, or to mine, for the preliminaries. I may also waive all discussion as to the method of science in general and the nature of proof -- matters of a kind that we either agree upon or would probably continue to disagree upon. All such machinery out of the way -- and I cannot help thinking that Professor Titchener sometimes allows the dust of his machinery to obscure his vision -- I may be allowed to state a point or two, first on his article, and afterwards on my theory.

1. The first point made is this: that I was wrong in calling the 'disposition' or 'Anlage' view a 'theory.' That, certainly, is true; and I claim, as Professor Titchener grants my right to, that my theory goes farther, in attempting to give a psychological explanation of reaction rather than a simple statement of fact.

2. Professor Titchener's explanations regarding what he calls the Anlage of the reagent, and the quotations from the works of others on the same point, still seem to me, in spite of the 'four-fold root of sufficient reason' which he presents in numerical order, to be open to my original charge of *circulum in probando*. He says, first, that, in Lange's words, "there are certain persons who are incapable of reacting consistently in the sensorial or muscular way." This I not only admit, but expect as a natural circumstance, if the truth be what my theory says it is. The man of the sensory type, my case of F, for example, complained of just this difficulty: he found himself almost incapable of reacting in the muscular way, being a musician and a man of the auditory type. Is it better to explain this man's condition, first finding out about him all that we can, or to drive him out of the laboratory? Then, under the same heading, Professor Titchener cites Wundt's version of the same incapable man in these words: "there are individuals [p. 82] who are entirely incapable of any steady concentration of the attention." This I also admit -- the asylums are full of them -- and I also admit that they are better out of the laboratory. But this is a very different class from those persons described by Lange; and it is just the confusion of the two kinds of people that makes Mr Titchener's whole position a false one. I find that my case F, if I am patient and do not turn him out too hastily, shows a remarkable power of concentration of his attention upon sounds: he can beat all the laboratory besides at that. And in other directions his attention is very fine. He is, in fact, a high-stand man in his university-work generally. So he is in no sense one of Wundt's class who are incapable of any steady concentration of the attention. On the contrary, he can concentrate his attention splendidly, provided we allow him to do it his own way. Assuming then that Wundt stated just what he meant, I quite agree with him; provided his usage go no farther than his words. But coming to the question of usage, in the Leipzig laboratory and speaking only by the book, we find these words in Professor Titchener's article in Wundt's *Studien*.

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After saying that his results ought to be published: "Weil die Zahlen auf einer strengen Durchführung des zwischen den sogenannten sensorischen und muskulären Reactionen existierenden Unterschieds beruhen, und daher theils Abweichungen von den früher erhaltenen Zahlen aufweisen, theils zur Erklärung der innerhalb dieser vorhandenen Unregelmässigkeiten dienen können," he goes on to report: "Mitarbeiter in diesem Theil der Untersuchung sind neun Herrn gewesen. Sichere Resultate habe ich jedoch nur von zweien ausser mir selbst gewinnen können." (Phil. Studien, VIII. s. 138.)[\*]

Now, does Mr Titchener mean to say that these three alone of the nine were capable of any 'steady concentration of the attention'? If not so, then where are the six? Are the six 'incapable of introspection,' as another of Professor Titchener's authorities is quoted to have put it? I happen to know about some of the six, and can say that the average ability of the patrons of the Leipsic laboratory is not as low as this procedure would seem to indicate. So Professor Titchener is not following Wundt's formula, of exclusion; he is rather following his own and Lange's formula, and by it excluding all who are 'incapable of reacting consistently in the sensorial or muscular way.' If one-third of mankind are to be taken to prove that a result is a universal principle, the rest being deliberately excluded because they cannot get the result that the one-third do, then what conclusions could not be proved in well-managed psychological laboratories? It would be interesting -- indeed it would be the only possible justification of the procedure -- to have the partial results which the other two-thirds did give, with the criticism of them on the ground of which they were thrown out.

3. Mr Titchener then says that my charge that the "Leipsic [p. 83] school 'rules out' results which do not accord with the Leipsic theory, but are nevertheless constant and regular results, is altogether unfounded" -- quoting passages again from Leumann and Külpe to the effect that due regard should be had to individual differences among reagents. The only results ruled out, he says, 'are those which are wholly irregular and inconstant.' To this I have two replies to make. First, I may ask: if this be true, why does not Mr Titchener accept the results of Flournoy, Cattell, and myself, which show tables of cases whose reactions were as regular and constant as the Leipsic results, but which fail to show the sensorial-muscular relation which the Leipsic school believe in. I shall say a word more on this question of relative accuracy of result farther on. And second, Professor Titchener overlooks one of the essential factors in the case -- the factor in the case, to wit, that relative regularity and constancy may be just the thing we are observing. Results may be regularly irregular: and that is just the contrary case to the one which he looks exclusively for, i.e., the case of results which are regularly regular. In ruling out all results which are irregular, the Leipsic school beg the question. In matters of the attention it is evident that steadiness, uniformity, ease of fixation, is the opposite of hesitation, now-good-now-bad, easy-then-difficult, effects. And it is just a part of the phenomenon that my theory attempts to bring to recognition, that the case in reaction is exactly this normal and common kind of variation. Irregularity, therefore, may arise from difficulty in getting the required image or content held up for attention. And I think that the Leipsic school have to recognise and act upon the same principle as soon as they come to ask for the slightest shadow of explanation of their own distinction between the two kinds of reaction. In short, to put my position briefly on this point, I should say that irregularity of result might occur -- and we actually have cases of it on each side -- in either kind of reaction, and if one should determine beforehand to rule out all cases of such irregularity of the muscular kind, then he might find one-third of his cases remaining to serve as basis of a formulation exactly the opposite of that held by the Leipsic school.

I have, further, to thank Professor Titchener for quoting the passage from Külpe to the effect that "if a person is incapable of any vivid ideation of a sense impression, he will give the appropriate direction to his attention by the formation of a corresponding judgment, or by help of the organic sensations arising from the strain set up in the organ of sense or of movement, or perhaps by visual ideas of the stimulus or of the required movement. But it is probable that certain differences in the determination of reaction times are largely referable to the differences in the form of expectation." This is my view. It is only another way of saying that these things should be taken into account, and that all variations in individuals should be counted. Professor Flournoy's case is especially valuable as enabling us to follow up one of the variations [p. 84] which Külpe hints at; and my research into the variation between 'visual motor' and 'kinaesthetic motor' reactions is a deliberate attempt to clear up one of these distinctions. Külpe wrote in the same passage: 'so far there has been no accurate discrimination of all these forms of muscular and sensorial preparation.' How then, I may ask, can he say beforehand that the muscular form will turn out in each case to be shorter than the sensorial? One of the merits of the 'type-theory' is just that it gives us natural lines of advance along which to

direct these further investigations.

4. When, therefore, Professor Titchener says that my "demand for a statement of the origin and meaning of the 'disposition' is a demand for the impossible," I have only to cite certain practical considerations to meet his views as to the intrinsic obscurity of nurture, of heredity and education,' as far as this topic involves those things. Is not the fact that F is a musician, something of an explanation of his auditive 'disposition'? Is not the fact that a man having certain defects of vision has also difficulty in giving visual attention, in so far a reason for his long visual reaction? Is there not now a mass of pathological evidence proving that movement of a limb may be impossible if visual, auditory, or other types of attention cannot be brought into play? And is not this in so far the ground of a theory of the variations which these men show when they are well? In short, is not the pathological theory which I have used in working out the 'type-theory' of reaction just a theory of the variations produced by 'nurture, heredity, and education'? But even if, theoretically, 'dispositions' are obscure, we should be sure that we have 'caught the rabbit' before we decide that he is not worth cooking; and this is the task which the 'type-theory' sets itself -- to investigate the so-called 'dispositions' and find out what they really are.

Professor Titchener then goes on to examine the evidence upon which my theory rests. I may say before taking up the points which he makes, that I by no means admit the implication that I have anywhere stated all the evidence in what I may call the form of a catalogue -- as he is fond of doing; on the contrary, the article he quotes is mainly the report of a research, and the general considerations are very schematic. I hope later to do more justice to the evidence as a whole. So I shall now only comment on the evidence as he states it, not as I should state it.

1. He objects to my cases on the ground that they were not tested as to their type. Now, in spite of Mr Titchener's assertion that 'there are many methods of testing type,' I may say that I do not know of any that are conclusive except those of introspection and pathology. I believe that in most cases a very safe conclusion can be reached by questioning the subject in a variety of ways, i.e., by using the method of introspection. This I have done with my cases, and it is only a phase of the incompleteness of my article, when looked at from a 'catalogue' point of view, that I did not state it. Professor Titchener is quite right in asking for it; and [p. 85] later I shall furnish it. He would do psychology a service, however, if he would publish some of the 'many methods of testing type, apart from the reaction method.'

2. He says of my results: "four persons reacted to sound. Two of them, B and S, carried out the investigation of which the present 'Study' is a report: presumably, therefore, they had the type theory in mind throughout. Whether the other two reacted with or without knowledge, we are not told. The greatest reliance is placed upon the times of B and S." Of this I have again two things to say: first, that the research was carried out largely in Toronto at the time when I (B) still accepted the Leipsic distinction as a general one; and my present theory was arrived at only after I had subsequently secured the results reported in the table of F, and largely on the basis of that table, which forced me to alter my former view. This shows for itself in the tables, in both my case and that of S -- he too had no such theory when he gave the reactions -- for we are the very two who do not contradict the sensorial-muscular distinction! What Mr Titchener means by saying 'the greatest reliance is placed upon the times of B and S' passes my comprehension. As also any ground he may have for the unhandsome charge that I have changed my reaction-times since I wrote my book on Senses and Intellect. It looks to me like a case either of the extremest carelessness as to self-contradiction, or of 'bluff.' Of course I do not accuse him of the latter: but why strain to make a point which is contradicted by the table which he himself constructs out of mine? It can only deceive the non-elect. My results still show the Leipsic distinction as they always did; so do Mr Shaw's (S). Mine have only changed in that the distinction is less marked than it used to be; and this I go the trouble to explain in the same article as probably due to habit and practice -- as my theory again seems at least not to contradict. The times of B and S, therefore, are very neutral to the discussion: that of F and, as far as examined, that of T, are the ones on which "greatest reliance" is placed -- of all which I have myself investigated.

3. Now as to accuracy of result -- the point which comes up next. Professor Titchener criticises my tables as to certain results which show variation, quoting only the figures for B and S. 'These variations,' says he, 'call for special explanation.' Yes, they do; and I can give it. But as I have said, these are the two cases which have no great bearing on the discussion -- a kind of citation which, if I were criticised by one whose standing I did not know, I should say showed incompetency or playing

to the galleries. The two cases which are important to my argument and which go with those of other observers to prove the 'type-theory' are those of F and C, as I may again repeat. In the case of F the difference between the sensorial and muscular reactions is 40s and in that of C it is 25s. Is it competent argumentation, in view of these figures, to say: 'Professor Baldwin argues from time-differences (22, [p. 86] 18, 21s),' with no shadow of reference to the other cases, especially after declaring, without any accuracy, that I placed 'greatest reliance upon the times of B and S.' The only possible point in my article to which such criticism would apply is the distinction between 'visual motor' and 'kinaesthetic motor' reactions, where I do use the results of B and S. But that is quite another topic; and while to have confused the two may, in a measure, excuse Professor Titchener's error, it is, I am bound to say, most unfortunate. For in that case, how can Professor Titchener go on to say: "Nevertheless it must be admitted that the tables show some striking results, and that the construction of the type-theory out of them is very ingenious." This would seem to show that the writer of the sentence did see the bearings of the times of F and C after all, and yet did not cite them in his quotation of figures.

4. Flournoy's case. Professor Titchener gives the details of this case sufficiently. He dismisses it with these words: "All that 'they [i.e. the Leipsic school] would say is that the 'physical possibility' to react muscularly is not, in [our] laboratory experience, a feature of the normal or average mental constitution. Consequently, the mind so constituted cannot be drawn upon to furnish norms of reaction: however interesting its workings may be in other connections." This summary exclusion of cases has been spoken of above. So far from disposing of the case it shows, in my mind, the plainest confession of inability to do anything with it. It amounts to saying: 'this case was investigated; it ought not to have been investigated: the results were published; they ought to have been suppressed.'

Other cases are then taken up, i.e. those of Professor Cattell, from whom a letter is cited quoting his two reagents J and D. Cattell says that D supports the type-theory, and that J gives no difference between the two kinds of reaction -- a fact which, of course, fails to support the Leipsic distinction. Professor Cattell then gives a case (unpublished) of a reagent who gave a slower reaction for sound than for light while distracted 'by not knowing where the sound was.' When this cause of distraction was removed 'his reaction (to sound) became much quicker and more regular.' Cattell says this case 'supports your (Titchener's) point of view'; and Professor Titchener, on the ground of this common phenomenon of distraction of attention, dismisses the evidence from Professor Cattell's cases with the phrase 'honours are divided.' Professor Cattell, on the other hand, in the same letter declares in favour of the type-theory in these words: "My own idea is that an unusual direction of the attention lengthens the reaction time, and that when the reaction has been much practised it becomes reflex." If Professor Titchener can get any comfort from the unpublished case mentioned, it is well, but to me it seems to be quite easy of explanation. The person is uncertain what he is to attend to in certain respects, and so cannot attend quickly or well; as soon, [p. 87] however, as this cause of uncertainty is removed, he can. There is no question here as between types of attention; it is rather a question of good attention and bad attention. And the result is what the type-theory says it is: with the attention bad, the reaction was long; with attention good, it was short. The case is too meagre to be of any value except as a tendency case -- were it not that Professor Titchener uses it again below, forgetting all the proper demands made earlier in his paper for exact figures. As to the Donders case -- it is pure surmise one way end the other; I cited it in my other paper only as showing the length that the Leipsic people are willing to go with their distinctions.

As to additional cases from which the author says I do not claim support, it is equally true that I make no reference to them, again not writing a 'catalogue': the main reason that I did not 'claim' certain other cases recorded in the literature of the topic, was that I thought the cases cited were sufficient.

So much then for the 'evidence for the type-theory.' I submit that it is strengthened by Mr Titchener's examination of it. And there is, besides, the great mass of evidence drawn from the pathology of the motor functions, and from the general principles of habit and relative accommodation of the attention, which are stated at some length in my article. All this held is untouched by the examination of our author, although it is there that -- apart from the actual cases reported -- I lay 'greatest reliance.' But Mr Titchener is not yet done; he next cites 'evidence against the type-theory.' And what he cites he himself describes as 'these two negative instances' -- i.e. of himself, and of Binet's case of M. Inaudi. As to Professor Titchener's case, as he reports it from his impressions of his own mental life, he simply shows, with quotations from my book on Mental Development also in

support of it, that type differs in the same individual for different functions, and 'shifts' with education for the same function. Both of these points I admit; and I have put both of them in evidence in the book quoted: but how do they bear against the type-theory of reaction? They do not. The reason it is a type-theory is just that it allows for such variations; and it matters not whether the variation, in any case, be in a person or in a function. And indeed, the very ground of origin of types is to be found in education, which must necessarily apply to single functions. But I do not think that the little practice that one may give himself in a year or two, or in the case of one function or two, is likely to alter the general type of his reactions; that goes in most cases deeper down in the habits of one's life. This is all that Professor Titchener's case shows, and even then are we not taking very general statements for figures? Why has not Professor Titchener tested himself for type by some of his 'many methods'? He seems to forget those 'many methods' when he now says: 'the elucidation of a memory type is by no means an easy matter.'

The other case, that of M. Inaudi, is to my mind unavailable. [p. 88] Inaudi is a prodigy of mathematics, investigated by Binet and found to be dependent upon hearing in his calculations. Professor Titchener draws the inference, and it seems that Binet did also, that he should give a remarkably short auditory reaction compared with his other sensorial times. This he did not, when investigated, and so he is now cited as evidence against my theory. Of course I reply as Mr Titchener supposed I should, that this does not show anything about his muscular reaction. And further it is quite too abnormal a case to show anything about the relation of the different kinds of sensory reactions to each other. This arithmetical work on the part of such prodigies is not to be accounted for as due to habit, practice, training of the attention, &c., the usual ground of type distinctions; it is rather a variation of an obscure kind, some sort of a twist of which we know really nothing, and in it Professor Titchener ought to recognise an Anlage if there ever was one, and promptly rule it out of the laboratory. I quite agree with M. Binet in saying in the passage which Mr Titchener quotes: "It must not be supposed that M. Inaudi is an auditive outside of his professional exercises in calculation. He is an auditive for calculation, i.e., for one partial, special, sharply defined memory." It seems to me quite likely -- if this freaky calculating gift be amenable to any rules -- that for this function his muscular reaction would be longer than the sensory. But for his other senses it seems to me also probable that he was reacting all the time in a muscular way. And even though M. Inaudi gave all his reactions with muscular attention as Professor Titchener supposes, how does that in any way 'tell heavily against the type-theory'? That theory does not say that no one shall react in that way if he want to. In that case one would only have to suppose that Inaudi's reactions of the two kinds to sound were about equal and both very short. This is supported by the lack of conclusive evidence that he was much more auditive than motor, even in his calculating.

After all this rather tiring discussion, in which there is on both sides too much hair-splitting, hypothetical interpretation of cases, and conjecture as to what a reagent 'ought' to do on this view or on that, I find relief in turning to one or two of the larger bearings of the subject. They may be taken to be a further statement of aspects of the general position now sufficiently well characterized by the phrase 'type-theory.' At the same time, I desire to thank Professor Titchener for the careful consideration he has given to my point of view.

1. It is not a necessary corollary from the type-theory that a subject be of the same type in his reactions with the hand to sounds, sights, &c. that he is in his speech. I think, as I said in my earlier article, that this is oftener so than not; and it was this thought that first led me to look to the general doctrine of types for an explanation of the variations in different persons' times. We find that speech itself may vary in its type very remarkably in the same individual from one language to another, especially when the [p. 89] conditions of learning have been fairly consistent and of long duration. The case described by Ballet, and my own sense of relative contrast in type as between my use of French and German[1], are instances of this. And the pathological instances of damage to the brain which incapacitates the patient from using one language while another may remain intact--together with many interesting minor variations--tend to furnish evidence in the same direction. It should not surprise us, therefore, if it should finally become evident, in any subject, that a hand-function, such, say, as hand-writing, was most readily stimulated by some other centre in the brain than that which serves for the 'cue' to speech; giving in the same person one type for writing and another for speech. I am concerned to say this here since in the same article Professor Titchener holds me somewhat strictly to the complete parallelism between speech, on the one hand, and hand-functions on the other, interpreting my statements that way -- with some right to, certainly, from the partial statements of my earlier papers.

2. An important requirement, which Professor Titchener has not brought up against the type-theory, is yet to be fulfilled; and I hope to go into the consideration of it and the point mentioned immediately above when I publish the further experimental results which are accumulating in my laboratory. The requirement is this: should not any theory of the variations in the relative lengths of the two sorts of reaction in different individuals give some kind of an account of the great disproportion between the number of cases which give a shorter muscular, as against those which give a shorter sensorial, reaction-time? Professor Titchener may find it difficult to formulate such a requirement, since it would seem to commit him to the recognition of some instances of the latter. But those of us who believe in testing everybody, and in making the differences themselves fruitful data for theory, are bound to recognize the disproportion spoken of, although, for myself, I think when more laboratory workers take persons just as they come, the relative numbers will probably be more evenly adjusted.

Yet, as far as this disproportion does exist, as it appears to, I think it really bears out the analogy of reactions generally with speech. The discussions recently published on so-called 'internal speech' turn, it will be remembered, not on the question as to whether there are the same number of cases of persons sensory as motor in their speech; but rather on the question whether all men are not motor. As I have put the question elsewhere, for convenience in grouping the evidence pro and con, 'are the kinæsthetic memory centres intrinsic to speech,' or not?[2] There is a school of physiologists and psychologists, represented by Stricker of Vienna, who go so far as to deny that any persons can speak without the [p. 90] incipient stimulation of the motor organs involved. They seem to me to be for that discussion about in the position that the Leipsic people are for the discussion of reaction. And while the case for speech seems to be going clearly against them on pathological grounds, yet they have by far the larger number of cases. The literature seems to show a great disproportion of cases in favour of the motor aphasias: and that fact has seemed to keep back the recognition of the sensory cases. Those who are familiar with the literature of 'aphasia will, I think, agree that the type-theory has had this obstacle to contend with. So, while I may not stop here to make good the indications now noted of the state of the facts in regard to aphasia, perhaps sufficient has been said to show that, far from being a difficulty to the type-theory of reaction that the disproportion of cases is as it is, it rather seems to extend and strengthen the analogy with the mechanism of speech.

P.S. Since writing and despatching the article above, I have received a letter from Professor James R. Angell of the University of Chicago which promises further experimental confirmation of the type-theory. He says, under date of Nov. 9, 1895 : " It may interest you, in connection with Titchener's criticism of your theory for reaction time peculiarities, to know that at the very time your article appeared, I had all ready a considerable body of experiments remarkably similar to yours from which I had drawn conclusions absurdly like your own. I decided to postpone publishing until I could supplement them with more detailed work. I hope to get the thing into print before long. It seems to substantiate entirely the general principle underlying your view, although introducing some minor modifications."

J. MARK BALDWIN.

#### Footnotes

[\*] Classics Editor's note: "Because the numbers depend on a rigorous execution of the existing difference between the so-called sensory and motor [muscular] reaction, and therefore indicate deviations from earlier obtained numbers, partially they can serve for the explanation of their irregularities,.... "Collaborators in this part of the study were nine gentlemen. I could obtain definite results, besides from myself, only from two". [Thanks to Dr. Thomas Teo for his assistance in this translation.]

[1] See my *Mental Development: Methods and Processes*, pp. 435, 461 note. Ballet's case is to be found In his *Le langage intérieur*, p. 62.

[2] *Philos. Review*, July 1893, p. 386, incorporated in *Mental Development*, Chap. XIV.

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