

VIII.—NOTES.

DR. CATTELL ON "ELEMENTS OF PHYSIOLOGICAL PSYCHOLOGY".¹

The generous praise which Dr. Cattell bestows in the October No. of MIND upon my *Elements of Physiological Psychology*, as a whole, receives my grateful recognition. Many of his criticisms of the details do not admit of reply by argument; for they concern matters like the amount of space, method of treatment, weight in evidence, to be assigned to particulars, and therefore raise such questions as every author must solve for himself in a practical way. For example, it may be open to debate, theoretically, whether a treatise aiming to cover the entire ground of physiological psychology in an elementary manner should devote any space to a description of the nervous system, and, if any, how much space it should thus devote. To such a question the reply must be: All depends upon what the author wishes to do. There can be little doubt that not one in a hundred of the readers of any work on this subject, even including the experts of different kinds, possesses, or can easily obtain, the material for forming that clear and symmetrical picture of the nervous mechanism which an understanding of its relations to the mind requires.

There is one class of Dr. Cattell's strictures, however, which appeal to me for a reply. His review is characterised by the frequent complaint of "confusion" in my treatment of individual topics. Now my own researches and reflections have been so elaborated, and my conclusions lie so clear in my own mind, that I am persuaded the appearance of confusion is due to some infelicity of expression on my part, or else the confusion is not *my* confusion. I wish, then, briefly to examine this class of Dr. Cattell's strictures.

In the first place, Dr. Cattell accuses me of confusion amounting to a "sheer contradiction," because I hold both that the nervous system must be considered as a mechanism and that there may be, and is, a causal connexion between this mechanism and the mind. In advocating the view that the nervous system is a mechanism, he is pleased to regard me as a "follower of Lotze". Now I can by no means claim so distinguished a title as this; but it seems rather strange that if I am to be regarded as a follower of the German philosopher in holding one of these tenets, I should not be regarded as equally his follower in holding the other tenet; for Lotze certainly advocated the view that the mind and the system of molecules which constitutes the central nervous mechanism are causally related to each other.

But in truth there is no incompatibility between these two tenets, and no confusion involved in holding them both. On the contrary, the marks of manifold confusion of the most antiquated kind are likely to become evident whenever anyone sets out to argue that there can be, or is, no causal relation between body and mind. It is then we hear the principles of a mediæval metaphysics virtually affirmed in the name of modern science. 'Only like can act on like;' no action is possible except through 'contact' of extended beings; the only causation is through the 'transmission' of so-called physical energy under the principle of mechanical equivalents, &c. These are some of the assumptions which

¹ This communication, which should have had immediate insertion, failed by accident to come to hand in time for the January No.—ED.

bring confusion into the treatment of this subject. But these are certainly not the assumptions which I advocate.

The interacting molecules of a living nervous system without doubt constitute a molecular mechanism. They are a system of moving material beings, which at every instant must be regarded as conditioning each other. But they certainly do not constitute a 'closed' system. If they did, there could be no such thing as 'irritating' or 'exciting' the system either by external or internal stimuli. A half-dozen perfectly elastic billiard-balls thrown down upon a table with perfectly elastic cushions would, in all their subsequent motions, constitute a physical mechanism. But what if any of the balls are from time to time struck by a cue? They do not for that reason cease to constitute a mechanism; but they do cease to constitute a 'closed' mechanical system. In other words, if we are to account for the behaviour of the balls, we have now to take the blows of the cue into the account. Neither does the effect of the many forms of constantly active stimuli, both internal and external, upon the nervous system render it any less a mechanism than it would be without this effect. It does, however, make it impossible to account for the action of this mechanism without taking the action of beings lying outside of it into our account.

And now the question arises: Is the nervous system a mechanism absolutely 'closed' to all causal action from the mind? Everything in the way of actually observed fact concerning the relations of the two kinds of phenomena—phenomena of the nervous system and mental phenomena—would encourage us to answer, No. But we are told, on the alleged authority of a certain form of a mechanical theory of the entire universe that, in spite of all appearances, we must answer, Yes. Why? I should be glad to know. Because action of mind on matter is mysterious, unimaginable, &c.? But so is every kind of action: action of material molecule on material molecule not the least so. Is it, then, because we must assume not only that all causal action is according to uniform modes or laws (which I readily grant), but also that all causal action is only between material molecules under the law of the conservation and correlation of physical energy? In other words, is it because the action of mind and brain on each other cannot be like the action of the billiard-balls under the stroke of the cue? But it seems to me that those who maintain the latter view may excuse us from assenting to them (under penalty, I suppose, of being found guilty of confusion), until it has been shown more clearly how the behaviour of the nervous mechanism under ordinary physical stimuli is to be expressed in terms of the action of the cue on the billiard-ball.

In brief, I do not for a moment admit that Dr. Cattell's charge of confusion is at this point well founded. There is no confusion or incompatibility between the view that the nervous system is a mechanism and the view that this system stands in certain causal relations to the mind. Confusion arises, and that without easily assignable limit, when the attempt is made to explain all the uniformities of the occurrence of phenomena, mental as well as physical, as mere resultants of the causal action of physical elements under the law of the conservation and correlation of physical energy. But this is not merely a mechanical theory of the nervous system. It is the materialistic theory of the relations of mental phenomena to that system. The latter theory should never be confused with the former. I cannot believe that my book has fallen into this confusion.

In this connexion I may, perhaps, best refer to the surprise which Dr. Cattell expresses at the sentence in which I do not, as he says,

"define" energy, but simply state what we seem compelled to understand by it—*viz.*, "that which moves or tends to move the elementary atoms, or their aggregations into molecules and masses". Possibly, if I (as here) remove the comma which the printers slipped in between the word "aggregations" and the word "into," Dr. Cattell will remove the exclamation-point which he placed after the entire sentence.

Dr. Cattell also finds fault for its confusion with my theory of perception. Its "fallacy" consists, he thinks, in holding to "the assumption of a mind with a mysterious power of creating unity of consciousness out of sensation-atoms". This is not at all the way in which I should consent to have my view expressed. And here again any confusion which may possibly be pointed out in such a view is not my confusion. I am what Prof. James called, in the October No. of *MIND*, a "psychical stimulist," as regards the origin of space-perceptions. That is, I hold that the space-form which objects of sense certainly have is *not* the result of a mere summation (whether by addition or multiplication, to use Dr. Cattell's misleading figure; since what is multiplication but a form of addition?) of non-spatial sensational elements. At the same time, I also hold that experimental analysis shows conclusively that many, if not all, of the sensational elements which enter into the presentations of sense do not originally possess the spatial quality which the results of their synthesis (the presentations of sense) certainly have. Therefore, I have argued, these spatial qualities are the results of the synthetic reaction of mind, according to its own laws of behaviour. Now Dr. Cattell may not accept or like this theory of perception; but I do not understand how he can rightly speak of it as necessarily fallacious or confused. As Prof. James shows in the article already referred to, such is virtually the view arrived at by far the greater majority of all investigators of sense-perception, whether they start from the philosophical or the experimental point of view. And I would undertake to show that Prof. James, with all the room he leaves to be filled by the mental acts of "identification," "summation," "imagination," "correction," &c., is something of a "psychical stimulist" himself.

Again, Dr. Cattell (very inconsiderately, I think) accuses me of "confusion amounting almost to contradiction" because, in one place (p. 391), I state the simple fact that objects of sense appear before the mind as *out* and *spread-out*, and in another place (p. 455) declare that this does not happen by way of copying off ready-made things which exist *extra-mentally* just as they are afterwards perceived. But all this amounts to saying that the objects of sense are *mental* constructions,—a statement which Dr. Cattell seems to approve. Since they *are* mental constructions, the qualities of being 'out' and 'spread-out' are not copied off from extra-mental things, but are imparted to the objects as the form in which the mind constructs them. Once more, I do not object to Dr. Cattell's holding any other view of perception which he thinks himself competent to defend; but I by no means confess to his charge of confusion and contradiction.

Dr. Cattell reiterates this charge of confusion against the chapter of my book on Feelings and Bodily Motions, although he is kind enough to say that the chapter was "evidently written with extensive knowledge of the German and English literature concerned with the subject". He gives to me, as well as to his other readers, scarcely any token, however, as to what this confusion consists in. All I can gather is that I am judged to have fallen again into my sad habit of getting confused, because I speak of "feeling with its colour-tone of pain or pleasure," and of an "involuntary act of will". As to the first point, I can only conjecture

that Dr. Cattell may be a follower of Herbart in his own theory of feeling. But certainly I have clearly, though briefly, pointed out the confusion of the whole subject in which the Herbartian theory involves us. As to the propriety of speaking of "an involuntary act of will" I have myself expressed doubt, but have consented to use the term, for want of a better, to indicate those "forced" acts of attention with which physiological psychology is so familiar.

Another instance of the facility with which Dr. Cattell discovers obscurities, and so feels impelled to dissent from its views, I find in his statement that my book holds the classification of tastes to be an easy matter. But, says Dr. Cattell, "no combination of sweet, sour, bitter and salt will give vanilla or chocolate, nor can the taste of lemon and sugar be analysed into sour + sweet". Now what the book says is this, that "most of the different kinds of tastes admit of being considered as compounds of a few simple sensations of this sense with each other and with sensations of smell, touch, common feeling and muscular sense" (p. 814). The ordinary classification I myself pronounce "loose," and I elsewhere (p. 354) hold that most of the complex tastes—strangely enough, instancing "chocolate" as one—cannot be wholly resolved into the simple kinds of gustatory sensations. Moreover, I also state that the modification of the acid of the lemon by the sugar is not a mere case of plus and minus, but that the explanation of the new sensation is in compound cerebral processes; the mixture takes place in the brain.

Dr. Cattell furthermore thinks that I have through several chapters confused the doctrine of the Specific Energy of the Nerves with the fact that nerves connect special-sense-organs and muscles with special brain-centres. But again the confusion is not mine. The doctrine of the specific energy of the nerves I have stated, and touched upon in several places, but I believe always, with one exception, in such a way as to avoid all possibility of the confusion he finds. That one exception occurs in my summary of the conclusions respecting the localisation of cerebral function. There I say that all the results of investigation emphasise two great laws, one the law of Specific Energy and the other the law of Habit. It did not occur to me that any careful reader could suppose that in insisting upon the great and general principle of specific energy, as exemplified in the cerebral nervous mechanism, I should be thought of as confusing this use of the term with J. Müller's theory of "the specific energy of the nerves". It is perhaps worth notice in passing that Dr. Cattell thinks I am not justified in stating, and in italics: "*Sensibility seems, then, to be the predominating function of the right hemisphere, as motion is of the left*". He entirely overlooks the fact, however, that I am here giving a summary of Exner's conclusions.

Finally, Dr. Cattell more than intimates that, did he not refrain from discussing the more purely speculative part of my work, he should be compelled to point out other instances of confusion. I can only wish that, either by him or by some other critic, they might be brought to my notice. It might then appear whether the confusion is really mine or belongs to the false traditional opinions which are wont to be carried into the consideration of the relations between the body and the mind, and of the nature of the mind as made known through those relations.

My critic is good enough to apologise, apparently, for some of my failures by saying that the preparation of a book on physiological psychology is "a task of the utmost difficulty". This is indeed true. I make no claim to have overcome all the difficulties, or to have dealt with them successfully. But I feel confident that I have at least avoided being myself confused on the points regarding which Dr. Cattell

complains of my confusion. I will close by saying that, in my judgment, the greatest difficulty which physiological psychology has had to encounter hitherto consists in the fact that it has been, with few exceptions, pursued by students lacking in psychological insight and broad philosophical training.

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LEIBNIZ AND HOBBS.

The recent discovery in the University Library at Halle of a large number of letters from the unwearied hand of Leibniz—surely the most epistolary of all great thinkers—does not thus far prove to have much philosophical importance. Dr. L. Stein, editor of the new *Archiv für Gesch. der Phil.*, has in the first two numbers of that review given a careful account of all the autographic letters found, to the number of 101; and the utmost that can be said of them is that they help to deepen, if that were necessary, the impression of Leibniz as a man to whose breadth and variety of intellectual interests there was no bound, but who yet could pursue with the utmost tenacity special scientific objects of his own,—as here the perfecting of his reckoning-machine, entrusted, from about 1700 (long after its first invention), to a Helmstädt mathematical professor, R. C. Wagner, his chief correspondent in the collection. There is promise, indeed, that in the next number of the *Archiv* some other of the Halle letters—but these only copies, though not before published—will be made to yield matter of philosophical interest, as touching the question of the scope and value of history of philosophy. Meanwhile it may be noted that the discovery at Halle is not the only addition that has just been made to our knowledge of Leibniz' amazing activity as a letter-writer. There has recently appeared vol. iii. of the division given to 'Correspondence' in the stately collection of *Die philosophischen Schriften von G. W. Leibniz* (Berlin, Weidmann), made since 1875 by C. J. Gerhardt, editor before of *L.'s Mathematische Schriften*. This volume was kept back while vols. iv.-vi. of 'Works' were being issued from 1880. Apparently, though the editor says nothing, some kind of supplement must still be in view, outside of the original scheme; various things remaining unaccounted for within either division, as, for example, the well-known correspondence with Samuel Clarke. With all his merits and his unique claims to the gratitude of Leibniz-students, Gerhardt, it must be said, has not in all respects chosen the happiest way of presenting the fruits of his research; in particular, he might have been more forward with the reasons for some of his action in the past, and now he might have been less silent as to his actual intentions. There can, however, be no question as to the philosophical interest and value of the new, and hardly less of the corrected, matter which, in all his volumes (of 'Works' as well as 'Correspondence'), he has, with extraordinary labour, been able to bring forth from the recesses of the Royal Library at Hanover. In his latest volume—to go no farther back—at least one important interchange of letters (with Jacquelot, pp. 442-82) is made known for the first time; while other correspondences, more or less imperfectly printed before (some in merest fragment), are now set out with all desirable fulness and care. Among these are three: (1) with Thomas Burnett of Kemnay, a Scottish friend of Locke's; (2) with Cudworth's daughter, Lady Masham, the comforter of Locke's declining years; (3) with Pierre Coste, the French translator (in England) of Locke's *Essay*,—which throw so much new light on the relations of the German to the English philosopher that another