

Indeed, we suspect it might be difficult to show in any way that any two branches of knowledge should be allowed to throw no light on one another. Far less can calling one question scientific and another metaphysical warrant Prof. James in "consequently rejecting" certain conclusions, against which he has nothing better to object. Nor is it in the least true that physicists confine themselves to such a "strictly positivistic point of view." Students of heat are not deterred by the impossibility of directly observing molecules from considering and accepting the kinetical theory; students of light do not brand speculations on the luminiferous ether as metaphysical; and the substantiality of matter itself is called in question in the vortex theory, which is nevertheless considered as perfectly germane to physics. All these are "attempts to explain phenomenally given elements as products of deeper-lying entities." In fact, this phrase describes, as well as loose language can, the general character of scientific hypotheses.

Remark, too, that it is not merely nor chiefly the "soul" and the "transcendental ego," for which incomprehensibles he has some tenderness, that Prof. James proposes to banish from psychology, but especially *ideas* which their adherents maintain are direct data of consciousness. In short, not only does he propose, by the simple expedient of declaring certain inquiries extra-psychological, to reverse the conclusions of the science upon many important points, but also by the same negative means to decide upon the character of its data. Indeed, when we come to examine the book, we find it is precisely this which is the main use the author makes of his new principle. The notion that the natural sciences accept their data *uncritically* we hold to be a serious mistake. It is true, scientific men do not subject their observations to the kind of criticism practised by the high-flying philosophers, because they do not believe that method of criticism sound. If they really believed in idealism, they would bring it to bear upon physics as much as possible. But in fact they find it a wordy doctrine, not susceptible of any scientific applications. When, however, a physicist has to investigate, say, such a subject as the scintillation of the stars, the first thing he does is to subject the phenomena to rigid criticism to find whether these phenomena are objective or subjective, whether they are in the light itself, or arise in the eye, or in original principles of mental action, or in idiosyncrasies of the imagination, etc. The principle of the uncritical acceptance of data, to which Prof. James clings, practically amounts to a claim to a new kind of liberty of thought, which would make a complete rupture with accepted methods of psychology and of science in general. The truth of this is seen in the chief application that has been made of the new method, in the author's theory of space-perception. And into the enterprise of thus revolutionizing scientific method he enters with a light heart, without any exhaustive scrutiny of his new logic in its generality, relying only on the resources of the moment. He distinctly discourages a separate study of the method. "No rules can be laid down in advance. Comparative observations, to be definite, must usually be made to test some preëxisting hypothesis; and the only thing then is to use as much sagacity as you possess, and to be as candid as you can."

53 (9 July 1891) 32-33

JAMES'S PSYCHOLOGY.—II

The Principles of Psychology.

By William James, Professor of Psychology in Harvard University. [American Science Series, Advanced Course.] Henry Holt & Co. 1890. 2 vols., 8vo, pp. xii + 689, and vi + 704.

We have no space for any analysis of the contents of this work, nor is that necessary, for everybody interested in the subject must and will read the book. It discusses most of the topics of psychology in an extremely unequal way, but always interesting and always entertaining. We will endeavor to give a fair specimen of the author's critical method (for the work is essentially a criticism and exposition of critical principles), with a running commentary, to aid a judgment. For this purpose we will select a short section entitled "Is Perception Unconscious Inference?" Perception in its most characteristic features is, of course, a matter of association in a wide sense of that term. If two spots of light are thrown upon the wall of a dark room so as to be adjacent, and one of these is made red while the other remains white, the white one will appear greenish by contrast. If they are viewed through a narrow tube, and this is moved so that the red spot goes out of view, still the white one will continue to look green. But if the red light, now unseen, be extinguished and we then remove the tube from the eye, so as to take a new look, as it were, the apparent greenness will suddenly vanish. This is an example of a thousand phenomena which have led several German psychologists to declare that the process of perception is one of reasoning in a generalized sense of that term.

It is possible some of the earlier writers held it to be reasoning, strictly speaking. But most have called it "unconscious inference," and unconscious inference differs essentially from inference in the narrow sense, all our control over which depends upon this, that it involves a conscious, though it may be an indistinct, reference to a genus of arguments. These German writers must also not be understood as meaning that the perceptive process is any more inferential than are the rest of the processes which the English have so long explained by association—a theory which until quite recently played little part in German psychology. The German writers alluded to explain an ordinary suggestion productive of belief, or any cognition tantamount to belief, as inference conscious or unconscious, as a matter of course. As German writers are generally weak in their formal logic, they would be apt to formulate the inference wrongly; but the correct formulation is as follows:

A well-recognized kind of object, M, has for its ordinary predicates P₁, P₂, P₃, etc., indistinctly recognized.

The suggesting object, S, has these same predicates, P₁, P₂, P₃, etc.

Hence, S is of the kind M.

This is hypothetical inference in form. The first premise is not actually thought, though it is in the mind habitually. This, of itself, would not make the inference unconscious. But it is so because it is not recognized as an inference;

the conclusion is accepted without our knowing how. In perception, the conclusion has the peculiarity of not being abstractly thought, but actually seen, so that it is not exactly a judgment, though it is tantamount to one. The advantage of this method of explaining the process is conceived to be this: To explain any process not understood is simply to show that it is a special case of a wider description of process which is more intelligible. Now nothing is so intelligible as the reasoning process. This is shown by the fact that all explanation assimilates the process to be explained to reasoning. Hence, the logical method of explaining the process of association is looked upon as the most perfect explanation possible. It certainly does not exclude the materialistic English explanation by a property of the nerves. The monist school, to which the modern psychologists mostly belong, conceives the intellectual process of inference and the process of mechanical causation to be only the inside and outside views of the same process. But the idealistic tendency, which tinctures almost all German thought not very recent, would be to regard the logical explanation as the more perfect, under the assumption that the materialistic explanation requires itself ultimately to be explained in terms of the reasoning process. But Prof. James is naturally averse to the logical explanation. Let us see, then, how he argues the point. His first remark is as follows:

"If every time a present sign suggests an absent reality to our mind, we make an inference; and if every time we make an inference, we reason, then perception is indubitably reasoning."

Of course, every psychological suggestion is regarded as of the general nature of inference, but only in a far more general sense than that in which perception is so called. This should be well known to Prof. James, and he would have dealt more satisfactorily with his readers if he had not kept it back. Namely, perception attains a virtual judgment, it subsumes something under a class, and not only so, but virtually attaches to the proposition the seal of assent—two strong resemblances to inference which are wanting in ordinary suggestions. However, Prof. James admits that the process *is* inference in a broad sense. What, then, has he to object to the theory under consideration?

"Only one sees no room in it for any unconscious part. Both associates, the present sign and the contiguous things which it suggests, are above board, and no intermediary ideas are required."

Here are two errors. In the first place, "unconscious inference" does not, either with other logicians or with the advocates of the theory in question, mean an inference in which any proposition or term of the argument is unconscious, any more than "conscious inference" implies that both premises are conscious. But unconscious inference means inference in which the reasoner is not conscious of making an inference. He may be conscious of the premise, but he is not conscious that his acceptance of the conclusion is inferential. He does not make that side-thought which enters into all inference strictly so called: "and so it would be in every analogous case (or in most cases)." There is no doubt, therefore, that ordinary suggestion, regarded as inference, is of the unconscious variety. But

Prof. James further forgets his logic in hinting, what he soon expresses more clearly, that such an inference is to be regarded as a mere "immediate inference," because it has no middle term. We might suppose he had never heard of the *modus ponens*, the form of which, A and B being any proposition, is

If A, then B;

But, A;

Hence, B.

Those who think a light is thrown upon the ordinary process of suggestion by assimilating it to reasoning, assimilate it to the *modus ponens*. The proposition "If A, then B," is represented by the association itself, which is not present to consciousness, but exists in the mind in the form of a habit, as all beliefs and general propositions do. The second premise A is the suggesting idea, the conclusion B is the suggested idea.

Already quite off the track, our author now plunges into the jungle in this fashion:

"Most of those who have upheld the thesis in question have, however, made a more complex supposition. What they have meant is that perception is a *mediate* inference, and that the middle term is unconscious. When the sensation which I have called 'this' is felt, they think that some process like the following runs through the mind:

'This' is M;

but M is A;

therefore 'this' is A."

Those who have upheld the thesis are not in dispute among themselves, as represented. They make no supposition throughout not admitted by all the world. To represent any process of inference now as a *modus ponens*, now as a syllogism with a middle term, is not necessarily taking antagonistic views. As for the syllogism given, it is the weakest mode of supporting the thesis, far more open to attack than the form first given above. But Prof. James makes no headway, even against this. He says:

"Now there seem no good grounds for supposing this additional wheelwork in the mind. The classification of 'this' as M is itself an act of perception, and should, if all perception were inference, require a still earlier syllogism for its performance, and so backwards *ad infinitum*."

Not one of the authors whom we have consulted makes the M entirely unconscious; but Prof. James says they do. If so, when he insists that "this is M" is an act of perception, he must mean some ultra-Leibnitzian *unconscious* perception! Has he ever found the German authors maintaining that that kind of perception is inferential? If not, where is his *regressus ad infinitum*? What those authors do say is that M, and with it the two premises, are thrown into the background and shade of consciousness; that "this is M" is a perception, sometimes in the strict sense, sometimes only in that sense in which perception embraces every sensation. They do not hold sensation to be inferential, and consequently do not suppose a *regressus ad infinitum*. But even if they did, there would be no *reductio*

ad absurdum, since it is well known to mathematicians that any finite interval contains an infinite number of finite intervals; so that supposing there is no finite limit to the shortness of time required for an intellectual process, an infinite number of them, each occupying a finite time, may be crowded into any time, however short.

The Professor concludes:

"So far, then, from perception being a species of reasoning, properly so called, both it and reasoning are coördinate varieties of that deeper sort of process known psychologically as the association of ideas, and—"

We break the sentence, which goes on to something else, in order to remark that "a species of reasoning properly so called" must be a slip of the pen. For otherwise there would be an *ignoratio elenchi*; nobody ever having claimed that perception is inference in the strict sense of conscious inference. Instead of "a species of reasoning properly so called," we must read "reasoning in a generalized sense." Remembering also that Prof. James began by insisting on extending the controversy to association in general, we may put association in place of perception, and thus the conclusion will be; "so far from association being reasoning in a generalized sense, reasoning is a special kind of association." Who does not see that to say that perception and reasoning are coördinate varieties of association, is to say something in entire harmony with the thesis which Prof. James is endeavoring to combat? To resume:

"—physiologically as the law of habit in the brain. To call perception unconscious reasoning is thus either a useless metaphor or a positively misleading confusion between two different things."

Here the section ends, and in these last words, for the first time in the whole discussion, the real question at issue is at length touched, and it is dismissed with an *ipse dixit*. There is no room for doubt that perception and, more generally, associative suggestion, may truthfully be considered as inference in a generalized sense; the only question is whether there is any use in so considering them. Had Prof. James succeeded in establishing his *regressus ad infinitum*, he would have refuted himself effectually, since it would then have been shown that an important consequence, not otherwise known, had been drawn from the theory. As it is, he says nothing pertinent either pro or con. But a little before, when an unconscious predication was called perception, was this perception "properly so called"? And if not, was calling it by that name a "useless metaphor," or was it a "positively misleading confusion between two different things"?

53 (13 August 1891) 129

Vorlesungen über die Algebra der Logik.

Von Dr. Ernst Schröder. Leipzig: Teubner. 1890. Vol. 1, Pp. 717.

This review of Schröder's first volume is unassigned in Haskell's *Index to The Nation*, vol. 1. This leaves open the possibility that it is a review by Peirce, based on certain internal signs such as the reference to Peirce's work and that of O. H. Mitchell.

Ernst Schröder (1841-1902) was a German mathematician and logician. As a young man, Schröder studied physics and chemistry with such famous men as Bunsen, Kirchhoff, and Hesse. From 1870 until 1874, he held the post of professor of mathematics and natural sciences at the Pro- und Realgymnasium at Baden-Baden. For the two years following 1874, he taught mathematics at the Technische Hochschule at Darmstadt, from which he moved in 1876 to the Technische Hochschule at Karlsruhe, his final academic post.

The Algebra of Logic has here received an admirable setting forth at the hands of Dr. Schröder. The book is doubtless too large and too diffuse, but it is chiefly intended for a German audience (the subject has been hitherto neglected in that country), and Germans are not frightened away by voluminous reading. The doctrine is almost uniformly sound, and, what is of chief consequence, the arguments in favor of admitting the subject among the branches of human learning are well calculated to convey conviction. The arguments which have been advanced on the other side have sometimes been of a very curious nature. For instance, Mr. Bradley, in his 'Principles of Logic,' scouts it because it does nothing for reasoning that is not syllogistic—for example, for such reasoning as this: A is north of B, E = C, therefore A is north of C. In the first place, it is not true that reasoning of this kind is not included in an Algebra of Logic. The formal definition of the primary copula is simply that it is transitive—that is, that it is subject to the single condition that when A stands in a certain relation to B, and B stands in that same relation (or a limiting case of it) to C, then A stands in that same relation to C. Any relation whatever which fulfills this condition is already included in the Algebra of the primary copula—the copula, that is, which represents, in the first instance, words "all . . . are . . ."

But in the second . . . Algebra of Logic covered syllogism only, no one could doubt . . . d to perform without it the extremely complicated pieces . . . ng which it can work out by purely mechanical process . . . any instances of reasoning of this sort can be got from rea . . . As Dr. Schröder points out, it is not strange that this kind . . . seldom attempted at a time when it was almost impossible of accom . . . Mr. McColl has already made a useful application of the theory to the determination of the new limits of several integrals upon a change in the order of integration.

Dr. Schröder makes constant acknowledgment, in very graceful terms, to the work of Mr. Charles S. Peirce and his school. He rightly considers that Boole's contributions to the subject possess, at present, only an historical interest. He seems to us to attribute rather less value than is due to the method of Mr. O. H. Mitchell as described in the 'Studies in Logic by Members of the Johns Hopkins University.'

Dr. Schröder's book is the only one, in any language, in which the subject can be properly approached by one who takes it up for the first time. We learn that a Spanish logician has undertaken a translation of it. For an English-speaking public, a somewhat different presentation of the subject would be preferable.