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The Given Element in Empirical Knowledge

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## THE GIVEN ELEMENT IN EMPIRICAL KNOWLEDGE

SINCE I have already said in print how I would propose to deal with our present topic, and my colleagues on this program have made references to that view, let me here omit any attempted summary and try instead to emphasize those basic considerations which, as I see it, dictate this conception of an incorrigible datum-element underlying empirical beliefs which are justified.

Empirical knowledge—if there be any such thing—is distinguished by having as an essential factor or essential premise something disclosed in experience. That is a tautology. To express this tautological fact circumstantially and circumspectly can be a matter of some difficulty; but, if anyone should deny what we so attempt to state, he must impress us as philosophizing by the Russian method of the big lie, and argument with him might well be useless. It is this essential factor in knowledge which comes from experience which I would speak of as ‘the given.’

But since experience and the functioning of it as the basis of empirical knowledge is something open to the inspection of all of us, each in his own case, how comes it that we tell such different tales about it? The account which I have offered has frequently met with dissent; and this dissent with respect to something which, if correctly stated, should be obvious gives me pause. If those who so find fault held a rationalistic theory, I might offer myself the excuse that they philosophize in the interest of an unsound major premise. But the greater number of my critics have been as firmly empiricistic in their professed convictions as myself. That is just what puzzles me most, because I seem to find only two alternatives for a plausible account of knowledge: either there must be some ground in experience, some factuality it directly affords, which plays an indispensable part in the validation of empirical beliefs, or what determines empirical truth is merely some logical relationship of a candidate-belief with other beliefs which have been accepted. And in the latter case any reason, apart from factualities

afforded by experience, why these *antecedent* beliefs have been accepted, remains obscure. Even passing that difficulty, this second alternative would seem to be merely a revival of the coherence theory of truth, whose defects have long been patent.

There undoubtedly is some logical relation of facts — or more than one — to which the name 'coherence' might aptly be given. And there is equally little doubt that such logical and systemic relationships are important for assuring credibility — once a sufficient number of antecedent and relevant facts have been otherwise determined. But no logical relationship, by itself, can ever be sufficient to establish the truth, or the credibility even, of any synthetic judgment. That is one point which logical studies of the last half century have made abundantly clear. Unless the beliefs so related, or some of them, have a warrant which no logical principle can assure, no logical relation of them to one another constitutes a scintilla of evidence that they are even probable.

Let us assume that the whole of the truth has even that strongest type of coherence illustrated by a system of geometry. The statements of the system (postulates and theorems together) are so related that, if we should be doubtful of any one of them, the other statements of the system would be sufficient to assure it with deductive certainty. But that relationship, as we know, is insufficient to determine any truth about the geometric properties of actual things. If Euclid is thus coherent, then so too are Riemann and Lobachevsky; though given any denotation of the geometric vocabulary, these three geometries are mutually incompatible systems. If the truth about our space is ever to be ascertained, something disclosed in experience must be the final arbiter. Since this is the case for geometric truths, which cohere by the strong relations of deductive logic, *a fortiori* it must be the case for empirical truth at large, for the determination of which we must so often rely upon induction, which affords a probability only, on the supposition that our premises are certain or that they have some antecedent probability on other grounds.

In brief, we have nothing but experience and logic to determine truth or credibility of any synthetic judgment. Rule out datum-facts afforded by experience, and you have nothing left but the logically certifiable. And logic will not do it.

Such argument by elimination is admittedly not final, and I would

not rest upon that but would appeal additionally to the facts of life. However, I would ask my critics where they stand on this point. Have they repudiated a fundamental requirement of any empiristic theory? Are they rationalists who think to extract from logical considerations alone some sufficient ground for empirical beliefs? Or are they really skeptics who dislike to acknowledge that fact in so many words? Or do they find some third alternative which I have overlooked?

One class of those who disagree have made their point of objection clear; it concerns my supposition that what is given in experience is incorrigible and indubitable. Empiricists generally are agreed that nonperceptual synthetic knowledge rests finally on knowledge which is perceptual, and so find the root problem in the nature of perception. Practically all empiricists recognize that some items of perceptual cognition are less than indubitable; perception is subject to illusion and mistake. They differ among themselves as to whether all perceptions, or some only, are subject to doubt. Mr. Moore, for example, regards such convictions as "This is my hand" (under appropriate circumstances) as subject to no doubt. But many, perhaps most of us, can find differences of degree only in the valid assurance of perceptual judgments: we recognize that most of them have what may be called 'practical certainty' but think that none of them is theoretically and validly certain. Those of us who come to this conclusion are then confronted with the following question: Is there, either antecedent to and supporting the perceptual belief in objective fact, or in the perceptual experience itself, an element or factor which is the basis of the perceptual judgment but is not, like this judgment of objective fact, subject to theoretical doubt?

My own answer to this question is affirmative. When I perceive a door, I may be deceived by a cleverly painted pattern on the wall, but the presentation which greets my eye is an indubitable fact of my experience. My perceptual belief in a real door, having another side, is not an explicit inference but a belief suggested by association; nevertheless the *validity* of this interpretation is that and that only which could attach to it as an inductive inference from the given visual presentation. The given element is this incorrigible presentational element; the criticizable and dubitable element is the element of interpretation.

The arguments which have been offered in criticism of this view are literally too numerous to be mentioned here. Some of them have

been of the casual variety which may be advanced without reference to any attempted full account of empirical knowledge. The objections of Goodman and Reichenbach, however, are not of that sort but are made in the interest of alternative views which are complex and worked out. Neither of them has had time to do more than suggest his alternative conceptions; and I shall have time only to suggest where, as it seems to me, some of the critical issues lie.

I hope I shall not give offense if I say that Reichenbach's view impresses me as being an unabridged probabilism; a modernized coherence theory with two immense advantages over the older one so named. First, he makes provision for observation-statements, though he insists that these should be in objective ('physical') language, and that they are both dubitable and corrigible. And second, he substitutes for the vague relation, historically called 'coherence,' meticulously described relations of probability-inference.

First, as to observation-statements: Let us suppose that I look over yonder and report that I see a horse. You (being epistemologists) may reply that you find my report ambiguous; that statements of the form "I see an X" are assertions of objective fact if and only if the constants substitutable for 'X' are understood to be confined to expressions denoting physical entities, but that statements of this form are in the protocol or expressive idiom if and only if the expressions substitutable for 'X' are understood to be designations of *appearances*. In the one case — you observe — I have made a dubitable assertion of an existent horse; in the other case, I have merely reported a specific given presentation which, whether dubitable or not, at least asserts no real horse as being present. This protocol statement, in its intended meaning — so I would claim — will be true just in case I am not lying or making some verbal mistake in the words I use. I am unable to see that Reichenbach's denial of this second and expressive idiom, is other than a dogmatism. (Even his 'phenomenal language' seems not to coincide with what I deem essential for any formulation of the given in experience.) I would, moreover, emphasize that the near absence of any restricted vocabulary or syntax for expressive statements is an unimportant matter for empirical knowledge itself: no one needs verbal formulation of his own present experience in order to be aware of it; and obviously, nobody else's protocols are indubitable to us. Protocol expression is as inessential to what it expresses as a cry of

fear is to the fearful apparition which may cause it. It is for purposes of epistemological discussion that the notion of protocol statements is principally needed; though there are, of course, statements so intended, and the requisite idiom is one which finds exemplification in natural language.

Let us pass these points, however, and take it that the observer of the horse has formulated his observation in objective ('physical') language, and that what he reports is dubitable and only probable. Reichenbach himself refers to the difficulty which then arises (attributing the objection to Russell): a statement justified as probable must have a ground; if the ground is only probable, then there must be a ground of it; and so on. And to assess the probability of the original statement, its probability relative to its ground must be multiplied by the probability of this ground, which in turn must be multiplied by the probability of its own ground, and so on. Reichenbach denies that the regressive series of probability-values so arising must approach zero, and the probability of the original statement be thus finally whittled down to nothing. That matter could be discussed for the rest of the afternoon or longer; it makes a difference whether one is talking about determined probabilities on known grounds, or merely what are called 'a priori probabilities.' However, even if we accept the correction which Reichenbach urges here, I disbelieve that it will save his point. For that, I think he must prove that, where any such regress of probability-values is involved, the progressively qualified fraction measuring the probability of the quaesitum will converge to some determinable value other than zero; and I question whether such proof can be given. Nor do I think that the difficulty can be removed by his 'argument from concatenation.' It is true that, by the rule of inverse probabilities, we may proceed in either direction, determining the probability of a 'consequence' from the probability of a 'ground,' or of a 'ground' from a 'consequence.' But what I would emphasize is that, as Reichenbach mentions, you cannot take even the first step in either direction until you are prepared to assign numerical values to the 'antecedent probabilities' called for by the rule. These must literally be determined *before* use of the rule will determine the probability of anything. And, if the answer be given that these can be determined by another use of the rule, the rebuttal is obvious: in that case you must make that *other* use of it *before* you can make *this* one. An interminable progres-

sus or regressus need not defeat theoretical purposes provided you are on the right end of it—the end from which its members are successively determinable. But in the kind of case here in point, one is always on the wrong end of any segment of the series, always required to determine something else first before one can determine what one wants to determine. The supposition that the probability of anything whatever always depends on something else which is only probable itself, is flatly incompatible with the justifiable assignment of any probability at all. Reichenbach suggests that the craving for some certainty here is a retained trace of rationalism; my countersuggestion would be that it is the attempt to retain a trace of empiricism.

Even more crudely put: the probabilistic conception strikes me as supposing that if enough probabilities can be got to lean against one another they can all be made to stand up. I suggest that, on the contrary, unless some of them can stand alone, they will all fall flat. If no nonanalytic statement is categorically assertable, without probability qualification, then I think the whole system of such could provide no better assurance of anything in it than that which attaches to the contents of a well-written novel. I see no hope for such a coherence theory which repudiates data of experience which are simply given — or no hope unless a postulate be added to the effect that *some* synthetic statements are probable a priori; the postulate, for example, that every perceptual belief has *some* probability just on account of being a perceptual belief.<sup>1</sup>

There is time only for very brief comment on one other point. Both Goodman and Reichenbach would impose a requirement of consistency — or 'inductive consistency' — on protocols. This goes along with their supposition that what protocols report is dubitable and corrigible. Briefly and inadequately, there is no requirement of consistency which is relevant to protocols. A protocol is a report of given appearances, of experience as such. Looking out over this audience, I see in one place two heads on one neck. When I lift my own head a bit, I see only one head there. But that is no reason to alter my first protocol and deny this apparition of two heads. I do not, of course, believe the two apparent heads to be actual. It is at *that* point that the requirement of inductive consistency comes in. But the critique by which I avoid that

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<sup>1</sup> This was suggested to me by Professor Paul Henle — though not as a supposition which he would adopt.

conclusion as to objective fact is criticism of a suggested interpretation — of a perceptual *belief* — and not a criticism of what the protocol reports. What it further indicates is only the desirability of some objective explanation of this apparition. The careless observer's protocols, the insane man's direct experience, and the content of the dreamer's dream must not be corrected or eliminated in the interest of consistency; to do that would be simple falsification of facts of experience. The problem of empirical knowledge at large is the problem of an account of objective facts which will accord with the occurrence of the experiences reported in all truthful and verbally accurate protocols. That is one test of adequate empirical knowledge. And the capacity of the objective account to explain any puzzling and apparent incongruities of experience is a further such test. To call a given experience an illusion, or a dream, or a careless observation, is to indicate the kind of objective fact which will explain it — just as the laws of optics and the fact of my looking through the edge of my glasses explains my apparition of two heads. We must not forget that experience is all that is given to us for the purposes of empirical knowing, and that such knowledge of objective facts as we achieve is simply that body of beliefs which represents our over-all interpretation of experience. If we could not be sure of our experience when we have it, we should be in poor position to determine any objective fact, or confirm the supposition of one, or assign any probability to one.

I regret not to make the further and detailed comments which Goodman's paper merits. Disbelieving that my conception of an indubitable given element in experience can be maintained, he suppresses further criticisms he might have, in the interest of a possible pragmatic reformulation of statements describing experience.

Putting it oversimply one may say that what he proposes is the interpretation of observation-statements in terms of the forward looking import of what they lead us to expect. But that proposal is, I fear, a little more pragmatic than I dare to be. However plausibly such reformulation could be carried out, it would fail to satisfy me because of a conviction I have concerning the task of epistemological study; the conviction, namely, that a principal business of epistemology is with the *validity* of knowledge. And validity concerns the character of cognition as warranted or justified.

In order to be knowledge, empirical judgment must not only have

predictive import of what will verify or confirm it; it must also be distinguished from a merely lucky or unlucky guess or hazard of belief by having some justifying ground. And in the nature of the case, what so justifies an empirical judgment cannot be something future to it and presently uninspectable but must lie in something antecedent to or compresent with it. Where it is perceptual cognition which is in question, the point is that the interpretation of experience — the perceptual belief — *is* significant of the future and verifiable, but, in order that this belief have *validity*, that which functions as the ground of it must be present and given.

That is precisely the point with which I am here principally concerned. It is on account of that point that I have felt it necessary to depart from or to supplement other pragmatic theories. And it is on account of that point that I could not accept Goodman's pragmatic proposal: by interpreting empirical findings in terms of what is future to them, it would invite confusion of the ground of knowledge which is there and given with what is not there but anticipated. It is also on that same account that I must disagree with various other current theories, put forward as empirical, which fail to recognize the datum-element of experience. In terms of such conceptions — so I think — no explanation of the validity of knowledge is forthcoming or even possible, and the holders of them can escape the skeptical conclusion only by failing to look where they are going.

I consider skepticism something worse than unsatisfactory; I consider it nonsense to hold or to imply that just any empirical judgment is as good as any other — because none is warranted. A theory which implies or allows that consequence is not an explanation of anything but merely an intellectual disaster.

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