

IV—By C. D. BROAD.

INTRODUCTION.—Before embarking on a philosophical discussion of Causation it is desirable to draw certain distinctions. I begin by distinguishing between *Causal Propositions* and *Principles about Causation*. By a “causal proposition” I mean any proposition which asserts of something that it is causally connected with something. Such propositions may be singular, *e.g.*, “The death of Harold at Hastings caused the defeat of the English Army”; or they may be universal, *e.g.*, “Friction causes rise of temperature.” The latter are called *Causal Laws*. By a “principle about causation” I mean a general principle about causal propositions. Examples would be: “Every event is causally determined,” “An effect and its cause must be manifestations of different determinate values of the same supreme determinables,” and so on.

Next I will distinguish three questions. (1) Can causal propositions be *analysed*; and, if so, what is the right analysis of them? (2) Are there any causal propositions which I *know* or have grounds for *rationally believing*? (3) Do I know, or have I grounds for rationally believing, that there are some true causal propositions? In future I will use the phrase “rationally cognize” for “know or have grounds for rationally believing.”

It is very easy to confuse the second and the third questions with each other, but they are quite different. If the second is answered in the affirmative, it follows that the third must be answered affirmatively also. But the converse of this does not hold. I might rationally cognize the proposition that there are some true causal propositions, and yet there might not be a single causal proposition which I rationally cognize. Suppose, *e.g.*, that it were a self-evident principle about causation that every event is causally determined. Then I should know that there must be some true causal propositions. But there might still be no causal propositions which I rationally cognize. This

example suggests that there is a fourth question to be added to our original three, viz., (4) Are there any intuitively or demonstratively *a priori* principles about causation? It is clear that there might be such principles even if there were no intuitively or demonstratively *a priori* causal laws.

Let us now consider the connexion between the first question and the second. It might be that, if causal propositions were analysed in a certain way, it would necessarily follow that there would be no causal propositions which I rationally cognize. If I feel certain that this analysis is correct, and I see this consequence, I ought to admit that there are no causal propositions which I rationally cognize. If, on the other hand, I feel certain that there are some causal propositions which I rationally cognize, and I see this consequence, I ought to reject this analysis, even though I cannot think of any alternative to it. But there is a third, and much more uncomfortable, possibility. I may feel quite certain that this is the right analysis so long as I do not notice that this answer to the first question would compel me to give a negative answer to the second. I may feel equally certain that the second question must be answered in the affirmative so long as I do not notice that such an answer would compel me to reject what seems to be the right analysis. If I am in this situation, the only honest attitude for me to take is that of complete doubt and suspension of judgment about *both* questions. Similar remarks would apply, *mutatis mutandis*, to the connexion between the first question and the third or the fourth.

INDUCTION AND CAUSAL ENTAILMENT.—I think that all the other symposiasts would claim to have rational cognition of some causal propositions, though I am not sure that they would all claim to know some causal propositions. *One* important line of argument, which is explicit in Prof. Stout's paper and is not questioned by Dr. Ewing or by Mr. Mace, may be fairly stated as follows. (i) In many cases past experience of a certain kind makes it rational for me to conjecture that a particular which I have not examined

will have a certain characteristic ψ if it has a certain other characteristic ϕ . (ii) This would be impossible unless such past experience made it rational for me to conjecture that there is a causal law connecting the occurrence of ϕ in any particular with the occurrence in it of ψ . (iii) If the "regularity analysis" of causal laws were correct, no past experience, however extensive or of whatever kind, would make it rational for me to conjecture that there is such a causal law. (iv) Therefore the regularity analysis of causal laws must be rejected. (v) There is one and only one alternative to the regularity analysis, viz., what I will call the "entailment analysis." (vi) Therefore the entailment analysis of causal laws must be accepted. Professor Stout then draws certain consequences from this, which the other two symposiasts do not admit to follow. For the present I shall ignore this further step, which is peculiar to Prof. Stout. I will consider the rest of the argument, which is, so far as I know, accepted by the other two symposiasts.

One preliminary comment is obvious. Prof. Stout should have shown us quite clearly that, if the entailment analysis is accepted, past experience of the right kind and amount *will* make it reasonable for me to conjecture that there is a causal law connecting the occurrence of ϕ in any particular with the occurrence in it of ψ . Suppose that this cannot be shown. Or suppose it can be shown that, even if the entailment analysis be accepted, past experience *will not* make it rational for me to conjecture that there is such a causal law. Then, if the rest of the argument is valid, only two alternatives are open. Either : (a) I was mistaken in thinking that past experience will ever make it rational for me to expect that a particular which I have not yet examined will have ψ if it has ϕ ; or (b) there must be some alternative to the regularity analysis beside the entailment analysis. This is not merely captious criticism. In the first place, McTaggart, who accepted the entailment analysis, professed to show that it does not provide a rational basis for inductive inference ; and his argument is certainly not *prima facie* unsound. Secondly, W. E. Johnson, who rejected the regularity analysis, did not accept the entailment

analysis Having lodged this preliminary protest, I will turn to clauses (ii) and (iii) of the argument.

It seems to me quite certain that clause (ii), as it stands, is false. If past experience of a certain kind and amount will ever make it rational for me to conjecture that a *considerable percentage* of the as yet unexamined instances of ϕ are instances of ψ , and will make it reasonable for me to expect that the next one that I meet will be a *fair sample* of the class of instances of ϕ , this will suffice to make it reasonable for me to expect that the next instance of ϕ will be an instance of ψ . It is not in the least necessary that the past experience should make it rational for me to conjecture that there is a *causal law* connecting the occurrence of ϕ with that of ψ . The artificial example of drawing counters from a bag, noting the colour, and conjecturing the colour of the next to be drawn, is enough to refute clause (ii). No one supposes that we have to assume that there is a causal law connecting the characteristic of being a counter in a certain bag with the characteristic of having a certain colour. Yet, if conjectures about the next instance can be justified in any case, they can most easily be justified in these artificial cases.

Nevertheless, I think that this step in the argument can be defended. Even in the artificial cases of bags and counters past experience justifies conjectures about the percentage of ϕ 's which are ψ only on certain assumptions. We must assume that the ϕ 's which were ψ will remain so, and that those which were not ψ will not become so. We must assume that the examined ϕ 's were a fair sample of the whole contents of the bag ; that the bag is not so large that there are some parts of it which we cannot reach ; that the ϕ 's which are ψ do not specially stick to our hands ; and so on. Now, in the first place, some of these assumptions are justified only if we have already established certain laws of nature by induction. And, in the second place, some of them certainly break down when we try to extend the argument from the artificial case to the investigation of nature. It is certain that I cannot have observed, any ϕ 's which do not yet exist or have not yet happened ;

that I cannot have observed any which were very remote in past time or very distant in space ; and so on. So defenders of clause (ii) might fairly say : " Unless you can establish certain *causal laws* by induction you cannot justify some of the assumptions which you have to make in order to apply induction to artificial cases like bags of counters. And even then you cannot apply to nature the *statistical* inductive arguments which you can legitimately apply to the bags of counters ; for the assumptions needed for such arguments clearly break down when applied to nature. Therefore, either inductive evidence will justify you in believing certain *causal laws of nature* or it will not justify you in believing *any* propositions either about nature or about artificial systems like bags of counters." Let us henceforth take clause (ii) in this amended form.

We can now pass to clause (iii) in the argument, viz., that, if the regularity analysis of causal laws were correct, no past experience, however extensive and however regular, would justify me in believing any suggested causal law. It is not necessary at the moment to go elaborately into the refinements with which the regularity analysis would have to be stated if it is to avoid certain *prima facie* objections to it. For our immediate purpose it will be enough to state it in the following rough outline. Any causal law is a statement of the form : " 100 per cent. of the instances of ϕ which have been, are, or will be, in the history of the universe, respectively have been, are, or will be, instances of ψ ." I shall shorten this into the more manageable form : " 100 per cent. of the ϕ 's in nature are ψ ."

Now it is quite certain that, if my *only* premise is " I have observed N ϕ 's, and 100 per cent. of them were ψ ," there is no valid form of argument by which I can either prove or render probable the conjecture that 100 per cent. of the ϕ 's in nature are ψ . But there are two very important points to be noticed here. (a) If this be granted, it follows *a fortiori* that there is no valid form of argument by which, from the same *single* premise, I could prove or render probable that the presence of ϕ in anything *entails* the presence in it of ψ . The proof of this is simple, and, so far as I can see,

quite conclusive. It is as follows. The proposition "The presence of ϕ in anything is inconsistent with the absence of ψ in it" entails, and is not entailed by, the proposition "100 per cent. of the ϕ 's in nature are ψ ." The former is therefore a logically more sweeping proposition than the latter. Now it is logically impossible that evidence which *will not* justify one in accepting as certain or probable the *less* sweeping of two propositions *will* justify one in accepting as true or probable the *more* sweeping of the two. If L is a true causal law, and the entailment view of causal laws is correct, it follows that the proposition which the regularity view offers as the analysis of L is *true*, though it is not the *analysis* of L. On the other hand, if the regularity view of causal laws is correct, it does not follow that the proposition which the entailment view offers as the analysis of L is true. Hence evidence which would not suffice by itself to prove or render probable the proposition which the regularity view takes to be the analysis of L must *a fortiori* be insufficient by itself to prove or render probable the proposition which the entailment view takes to be the analysis of L.

(b) Of course it remains possible that I may rationally cognize another premise P, such that the conjunction of P with the proposition "I have observed N ϕ 's, and 100 per cent. of them were ψ " would justify me in conjecturing that the presence of ϕ in anything entails the presence of ψ in it. But then it also remains possible that I may rationally cognize another premise Q, such that the conjunction of Q with the proposition "I have observed N ϕ 's, and 100 per cent. of them were ψ " would justify me *directly* in conjecturing that 100 per cent. of the ϕ 's in nature are ψ .

I propose henceforth to substitute for the phrase "the presence of ϕ in anything entails the presence in it of ψ " the shorter phrase " ϕ conveys ψ ." And I propose to substitute for "100 per cent. of the ϕ 's in nature are ψ " the shorter phrase " ϕ is *always accompanied by* ψ " This being understood, the discussion proceeds as follows.

Anyone who wants to use the argument which we are examining ought to substitute for clause (iii) the following set of four clauses. (iii, a) The empirical premise that

100 per cent. of the N ϕ 's which I have observed have been ψ does not, *by itself*, justify me in conjecturing that ϕ is always accompanied by ψ . (iii, *b*) *A fortiori* this empirical premise, *by itself*, does not justify me in conjecturing that ϕ conveys ψ . (iii, *c*) I *do not* rationally cognize any proposition Q , such that the conjunction of Q with my empirical premise *would* justify me in conjecturing that ϕ is always accompanied by ψ but *would not* justify me in conjecturing that ϕ conveys ψ . (iii, *d*) I *do* rationally cognize a certain proposition P , such that the conjunction of P with my empirical premise *would* justify me in conjecturing that ϕ conveys ψ and *would* therefore *a fortiori* justify me in conjecturing that ϕ is always accompanied by ψ .

My position, so far, is that I accept (iii, *a*) and insist that (iii, *b*) follows from it. The question for me turns, therefore on (iii, *c*) and (iii, *d*). In order to show the reader what sort of propositions I have in mind when I talk of P and Q , I will ask him to consider the two following propositions. (1) "Every characteristic of any particular is *conveyed* by some other characteristic (simple or compound) of that particular." (2) Every characteristic of any particular is *an invariable companion* of some other characteristic (simple or compound) of that particular." Proposition (1) entails, but is not entailed by, Proposition (2). But this is quite compatible with Proposition (1) being *self-evident* to a certain person and Proposition (2) *not* being self-evident to him. Let us suppose that Proposition (1) is in fact self-evident to me, whilst Proposition (2) is not. If I found Proposition (1) self-evident, I should know that in each of the N instances of ψ which I have observed there must have been *some* characteristic or set of characteristics (not necessarily the same in all) which conveys ψ . Since all the N observed instances of ψ were also instances of ϕ , it might perhaps be legitimate to conjecture that it was ϕ which conveyed ψ in all the N observed instances. If so, it would follow that it is equally legitimate to conjecture that *any* instance of ϕ will be an instance of ψ . If Proposition (2) is *not* self-evident to me, I could not use it in a similar way to justify *directly* the conjecture that ϕ is invariably accompanied by ψ . But

suppose that Proposition (2) *were* self-evident to me. Then I should know, as an immediate consequence of it, that in each of the N instances of ψ which I have observed there must have been *some* characteristic or set of characteristics (not necessarily the same in all) which is always accompanied by ψ . Since all the N observed instances of ψ were also instances of ϕ , it might perhaps be legitimate to conjecture that ϕ is a characteristic which is always accompanied by ψ . I should then have reached the same conclusion *directly*, instead of reaching it *indirectly* as a consequence of a previous conclusion about conveyance.

I am not, of course, saying that either of these propositions (1) and (2) *is* self-evident, or that the first is and the second is not. Nor am I saying that the suggested arguments which use them as premises are valid. I am merely asking the reader to make certain suppositions on these points, in order that he may understand what I have in mind in clauses (iii, *c*) and (iii, *d*) of my amendment to clause (iii) of the argument which we are examining. It is now evident that anyone who uses this argument is bound to do two things. (*a*) He must indicate to us some general principle about the *conveyance* of characteristics in nature, which we can rationally cognize, and which, in conjunction with suitable empirical premises, will justify us in conjecturing that a certain characteristic (simple or compound) conveys a certain other characteristic. (*b*) He must show that we do not rationally cognize any general principle about the *invariable accompaniment* of characteristics in nature, which, in conjunction with suitable empirical premises, will justify us in conjecturing that a certain characteristic (simple or compound) is invariably accompanied in nature by a certain other characteristic. My position about this is that I agree as to (*b*), but am still anxiously awaiting enlightenment about (*a*).

I will now restate the argument in an amended form, and will indicate what I accept in it and what seems to me doubtful. (1) *No* fact of the form : " I have observed N instances of ϕ and they have all been instances of ψ " will justify me in making a conjecture of the form " A certain unobserved

instance of ϕ is probably an instance of ψ ” unless *some* facts of the first form will justify me in making conjectures of the form : “ Probably ϕ is always accompanied by ψ .” This clause has to be stated in the above rather complicated way in order to allow for the fact that one may be justified in strongly expecting that the next counter to be drawn from a bag will be red, in view of the observation that several have been drawn and have all been red, and yet may not be justified on the same evidence in conjecturing that all the counters in the bag are red. I accept clause (1), when thus stated.

(2) No fact of the form : “ I have observed N instances of ϕ and they have all been ψ ” will *suffice by itself* to justify a conjecture of the form : “ Probably ϕ is always accompanied by ψ .” Some additional premise is needed ; and, if I am to make use of it, I must rationally cognize it. I accept clause (2).

(3) I do not rationally cognize any proposition which, in conjunction with an empirical premise of this form, *would* justify me in making a conjecture of the form : “ Probably ϕ is always accompanied by ψ ,” but *would not* justify me in making a conjecture of the form : “ Probably ϕ conveys ψ .” Speaking for myself, I admit clause (3).

(4) Empirical facts of the form under consideration *do* sometimes justify me in making conjectures of the form : “ A certain unobserved instance of ϕ is probably an instance of ψ .” I do not feel at all certain of this clause when I reflect on its implications, though I cannot help constantly acting as if I believed it.

(5) Therefore I *do* rationally cognize some proposition which, in conjunction with empirical facts of this form, *would* justify me in making conjectures of the form : “ Probably ϕ conveys ψ .” I do not feel at all certain of this conclusion. It is difficult enough to state any proposition which *would* do what is wanted of it if I *did* rationally cognize it. Something like Keynes’s Principle of Limited Variety would seem to have the best credentials for the post. It is still more difficult to believe that one rationally cognizes any proposition which would do what is wanted of it.

Thus my position may be summed up as follows. I accept the first three clauses ; and I admit that they, in conjunction with the fourth, *entail* the fifth. I therefore admit that the fourth *implies* the fifth. But the fifth seems so doubtful that the only result is to make me feel doubts, which I might not otherwise have felt, about the fourth. If someone should now say to me : "After all, you may rationally cognize some proposition of the kind alleged in the conclusion of the argument, although you have never succeeded in disentangling it and getting it clearly stated," I should, of course, agree that this is possible. But the more heartily I agreed the more inclined I should be to go back on my acceptance of clause (3). My ground for admitting that I do not rationally cognize any principle which *would* justify me in conjecturing that ϕ is always accompanied by ψ but *would not* justify me in conjecturing, on the same empirical evidence, that ϕ conveys ψ , is simply that *I cannot think of* any principle which would answer these conditions and is rationally cognized by me. It is not that I can positively see that there *could not be* a rationally cognizable principle which fulfilled the positive and the negative condition. I think that Prof. Stout and Dr. Ewing would perhaps claim to see this. If so, the fact that they cannot formulate the principle which the conclusion asserts that they must rationally cognize would not cast any doubt on clause (3). It would therefore not tend to invalidate the argument for the conclusion that they do in fact rationally cognize such a principle. But suppose that one's *only* ground for accepting clause (3) is failure to formulate any principle answering to the conditions, and that there is no positive insight that the conditions *could not* be fulfilled. Then the admission that I might rationally cognize a principle without being able to disentangle it and formulate it weakens my ground for accepting clause (3) just as much as it strengthens the conclusion against an obvious *prima facie* objection.

This completes my discussion of the argument from the validity of inductive inferences to the entailment view of causation. It seems to me to be a very important argument,

for the following reason. Suppose that all the premises were accepted ; and that the conclusion, which undoubtedly follows from them, were drawn. Then we should have done something much more important than merely showing that the regularity view of causal propositions is inadequate. Direct inspection and reflexion might, perhaps, convince many people that the regularity analysis fails to state what they have in mind when they are thinking of a causal law. But a person who admitted this might answer : " Very well. But we have not the least *reason to believe* any causal law in any sense of that phrase but the sense which it would have if the regularity analysis were correct. Anything more, or anything different, that we may have in mind when we think of causal laws is, so far as one can tell, just baseless superstition which we must hand over to the genetic psychologist for explanation." Now, if the argument which we have been discussing were accepted, this answer could not be made. The argument would show, not only that there is something involved in the notion of " causal law " which the regularity view ignores, but also that we have *reason to believe* certain causal laws in a sense of " causal law " which the regularity view fails to analyse.

REGULARITY ANALYSIS AND ENTAILMENT ANALYSIS.—It seems to me fairly certain on inspection that I do not mean by " causal laws " propositions of the form " ϕ is always accompanied by ψ " limited by conditions about spatio-temporal and qualitative continuity and decked out with psychological frillings. And it seems to me fairly certain on inspection that I do mean by " causal laws " propositions which involve the assertion that one proposition in some sense entails another, *i.e.*, that the truth of one is in some sense incompatible with the falsity of the other. Whether I have any reason to believe that there are causal laws, and whether there are any causal laws which I have reason to believe, are, of course, two quite different questions from the question : " What do I mean by a *causal law* ? " And they are left quite open by the negative and the positive statements which I have just made in regard to the latter question. I

propose at present to confine myself to some comments on the two alternative types of answer to this question about meaning or analysis.

(1) I have an uncomfortable feeling that the most impressive arguments *for* either kind of analysis are the objections *against* the other kind. The regularity analysis seems unpalatable on inspection, and difficult to reconcile with the supposed validity of inductive arguments. So we are inclined to favour the entailment analysis until we look into it. When we do so we find perhaps that it does very little towards helping to justify inductive inference. And we certainly find that it is not very plausible to identify causal entailment with either of the two kinds of entailment which are commonly admitted to occur, viz., the purely formal necessary connexion between the premise and the conclusion of a valid deductive argument, and the conveyance of extension by shape or of certain geometrical properties by certain others. Yet we not unnaturally hesitate to join W. E. Johnson in postulating a special kind of connexion between attributes, which is "less necessary" than ordinary conveyance and "less contingent" than constant accompaniment; to use phrases which are, perhaps, as absurd as they look, and yet do seem to express what one feels to be needed. And so, to avoid the unpalatability of one form of the entailment view and the possible nonsense of the other form of it, we may be inclined to favour the regularity view. "How happy could I be with neither," as Macheath might have said if he had had to philosophize about causation.

(2) The other symposiasts have not explicitly distinguished between laws of coexistence of attributes in a substance and laws of sequence of events. It did not seem to be necessary to draw this distinction in discussing the connexion between induction and causal entailment, but it is desirable to draw it now.

Laws of co-existence, on the regularity view, are of the form: "Every continuant in nature which has ϕ also has ψ ." On the entailment view they are of the form: "The presence of ϕ in any continuant is incompatible with the absence of ψ from it."

Laws of sequence, on the regularity view, would be roughly of the following form. “(a) There have been, are now, or will be events which are manifestations of the characteristic ϕ in circumstances of the kind C. (b) Corresponding to any such manifestation of ϕ , there has been, is now, or will be (according to whether it is past, present, or future) one and only one event which is a manifestation of a certain other characteristic ψ and which stands in a certain relation R to that manifestation of ϕ . (c) If e and e' be any two manifestations of ϕ in circumstances of the kind C, then the manifestation of ψ which corresponds to e, in accordance with the last clause, will be a different event from the manifestation of ψ which corresponds to e'.” This looks rather complicated, but I am sure that nothing less complicated will do. The relation R always involves immediate temporal sequence of the ψ -event on the ϕ -event with which it is correlated. In the case of purely physical causation it involves spatial coincidence or adjunction of the two events. In the case of purely mental causation it involves, perhaps, the concurrence of the two events in a single mind.

Laws of sequence, on the entailment view, would involve a proposition of the following form. “(a) If e is any manifestation of ϕ in circumstances of the kind C, it necessarily follows that there is one and only one event which is a manifestation of a certain other characteristic ψ and which stands in a certain relation R to e. (b) If e and e' be any two manifestations of ϕ in circumstances of the kind C, it necessarily follows that the manifestation of ψ which corresponds to e, in accordance with the last clause, will be a different event from the manifestation of ψ which corresponds to e'.” The same remarks apply to R as were made in the immediately previous paragraph.

(3) These statements of the two kinds of law, as they would be on the two types of analysis, bring out one point very clearly. The suggested analogy between admitted cases of non-formal entailment, in geometry, *e.g.*, and the alleged cases of causal entailment breaks down almost completely for laws of sequence. All the admitted instances

or non-formal entailment are instances of what I call "conveyance" of one characteristic by another. They are all concerned with the *co-existence* of attributes in *substances*. Laws of sequence, on the entailment view, would all involve entailment between *instantial* propositions. For they would all assert, *inter alia*, that the *occurrence* of an event of one kind entails the *occurrence* of another event of a certain other kind. There is not the least analogy between such entailment and the conveyance of one characteristic of a substance by another.

(4) This leads me to think that, even if some form of the entailment view be true of laws of sequence, the form of it which is suggested by both Prof. Stout and Dr. Ewing is too stringent to be at all plausible. Dr. Ewing says: "It would be possible in principle, with enough insight, to see what kind of effect must follow, from examination of the *cause alone* without having learnt by previous experience what are the effects of similar causes." (My italics.) Prof. Stout makes similar assertions. Now, even if some form of the entailment view were true, this extreme consequence would not follow. Let us grant that a person who had observed a number of different manifestations of ϕ to be immediately followed by as many different manifestations of ψ , correlated each to each with the former, might be able to see that *any* manifestation of ϕ *must be* immediately followed by a correlated manifestation of ψ . It does not follow that, if only he were acute enough, he could see this *before* he had observed and reflected upon at least one instance of the sequence. Unless ψ and R are involved in the analysis of ϕ , as black is in that of negro, it seems obvious that he might have observed a manifestation of ϕ at a time when he could have had no idea of ψ or of R, and therefore at a time when he could not even have entertained the suggestion that manifestations of ϕ must be immediately followed by R-correlated manifestations of ψ . And yet, when experience had put him into a position to understand and to contemplate this proposition, and had suggested it to him, he might be able to see that it must be true. In the case of the conveyance of one attribute of a substance by

another attribute of it, the situation which I have envisaged could hardly arise. But, in the case of sequences, it obviously might arise.

(5) If we are to be fair to the regularity view, we must recognize that it could, and presumably would, distinguish between ultimate and derivative laws. Derivative laws are laws which follow as necessary consequences from one or more other laws. There are several different ways in which this could happen, and it will be worth while to enumerate some of the more important. (i) Suppose it is a law that ϕ is always accompanied in nature by ψ , and suppose it is a law that ψ is always accompanied in nature by χ . Then it necessarily follows that ϕ is always accompanied in nature by χ . This will be a derivative law as compared with the two which together entail it. (ii) Suppose it is a law that ϕ is always accompanied in nature by ψ . And suppose it is a necessary proposition that anything which had ψ would have ω . Then it necessarily follows that ϕ is always accompanied in nature by ω . This will be a derivative law as compared with the law which, in conjunction with the necessary proposition, entails it. The following are the two most obvious examples. (a) ψ might be a determinate under the determinable ω . Or (b) ψ might be a conjunctive characteristic of the form $\lambda \mu$; and it might be possible to see or to prove that anything which had *both* λ and μ would necessarily have ω , though not everything that had λ or everything that had μ would necessarily have ω . (iii) Suppose, as before, that there is a law that ϕ is always accompanied in nature by ψ . And suppose it is a necessary proposition that anything which had χ would have ϕ . Here again the two most obvious examples would be (a) if χ were a determinate under the determinable ϕ , or (b) if χ were a conjunction of two characteristics, λ and μ , about which we could see directly or prove that anything which had *both* of them would necessarily have ϕ . Here two different cases can arise, which it is important to distinguish.

(α) We might know that there are instances of χ in nature. Then we could at once infer the law that χ is

always accompanied in nature by ψ . This law would be derivative, and would be of precisely the same kind as the other laws, ultimate or derivative, which we have so far considered. We may describe all the laws which we have so far considered as "instantial laws." By this I mean that they are not of the purely negative form : "There are no particulars in nature which have ϕ and lack ψ " ; but they are of the form : "There are particulars in nature which have ϕ , and none of them lack ψ ." If the regularity view be accepted, all *ultimate* laws of nature must be instantial, and many derivative laws will be instantial. Now it has been said that there are non-instantial laws of nature. *E.g.*, it is a law that, if two perfectly elastic bodies were to collide, the total kinetic energy of the system would be the same before and after the impact ; and this is not an instantial law, since there are no perfectly elastic bodies in nature. It has sometimes been made an objection to the regularity view that it leaves no room for non-instantial laws of nature. I will now pass to the second possible case, and I will show how far and in what way the regularity view can deal with non-instantial laws.

(β) We might not know that there are instances of χ in nature, or we might positively know that there are not. At the same time we may see that it would be impossible for anything to be an instance of χ without being an instance of ϕ . Under these circumstances we should be inclined to assert the law : "If anything were χ it would be ϕ ." What would this mean on the regularity view ?

It could not be identified with the instantial universal proposition : "There are instances of χ in nature and none of them lack ϕ ," for this is known to be false if it is known that there are no instances of χ in nature. It could not be identified with the purely negative proposition : "There are no particulars in nature which have χ and lack ϕ ," for this is a mere truism if it is known that there are no particulars in nature which have χ . Lastly, it could not be identified with the proposition : "The presence of χ in any particular would be incompatible with the absence of ψ

from it," for this would commit us to the entailment view. What then does it mean on the regularity view?

The answer seems fairly plain. On the regularity view to say: "If anything had χ it would have ψ " has the following meaning. It means that there is an instantial law of nature, or a set of such laws, such that it, or they, in conjunction with the *supposition* that there are instances of χ in nature, *formally entails* the proposition: "There are instances of χ in nature, and none of them lack ψ ." Take, *e.g.*, the proposition that kinetic energy would be unaltered in total amount by any collision between perfectly elastic bodies. What it means, on the regularity view, is this. There are instantial laws of nature (*viz.*, the Conservation of Momentum and the special laws about impact) which, when conjoined with the supposition that there are perfectly elastic bodies and that they sometimes collide, formally entail the instantial universal: "There are cases of collision between perfectly elastic bodies, and in none of them is there any change in the total kinetic energy of the system." The conclusion is false and one of the premises is false, but this is irrelevant. What we are concerned to assert is that this false conclusion is a necessary consequence of the conjunction of a certain false instantial supposition with certain true instantial laws of nature.

It is clear, then, that upholders of the regularity view can distinguish between ultimate and derivative causal laws, and that they can give a plausible interpretation to derivative non-instantial laws. On this point the differences between the upholders of the regularity view and the upholders of the entailment view are the two following. (i) On the regularity view the ultimate laws will be brute facts; whilst they will be intrinsically necessary propositions, true in all possible worlds, on the entailment view. (ii) On the regularity view all ultimate laws will be instantial; whilst, on the entailment view, there might be laws which were non-instantial and yet ultimate.

Plainly both parties could set before them the intellectual ideal of trying to find a minimal set of ultimate laws which would account for all the observed facts up to date, and they

would both feel legitimate intellectual satisfaction in proposition as they reduced this minimal set more and more. If the regularity view is true, insight is being gained, in the negative sense that the number of independent brute facts to be accepted is being reduced, and in the positive sense that one is seeing necessary connexions between facts which are themselves contingent. On the entailment view, and on it only, a further kind of positive insight is conceivable, and it is therefore conceivable that an additional intellectual satisfaction could be enjoyed. For, on this view, the ultimate laws of nature would be intrinsically necessary propositions, "holding in all possible worlds," and therefore it is conceivable that they might become self-evident to us, like the axioms of pure mathematics.

(6) So far we have confined our attention to laws of the crudest kind, viz., those which assert merely of one *determinable* characteristic ϕ that it is invariably associated with a certain other *determinable* ψ . Suppose that there are n determinates, $\phi_1, \phi_2, \dots, \phi_n$, under the determinable ϕ , and that there are n determinates, $\psi_1, \psi_2, \dots, \psi_n$, under the determinable ψ . Then the more refined kind of laws assert that every instance of any given determinate ϕ_i under ϕ is an instance of a certain one determinate under ψ . They assert that, if ϕ_r and ϕ_s are two different determinates under ϕ , then the determinate under ψ which invariably accompanies ϕ_r is different from the determinate under ψ which invariably accompanies ϕ_s . When the determinates under ϕ and under ψ are measurable magnitudes, laws reach a further degree of refinement. A law then asserts further that there is a certain one mathematical function, characteristic of ϕ and ψ , such that the number which measures any determinate ψ_i under ψ is this function of the number which measures the determinate ϕ_i which ψ_i invariably accompanies.

A law expressible by a mathematical equation of the form $\psi = F(\phi)$ would have to be stated as follows on the regularity view. "(i) Every determinate under ϕ is invariably accompanied by a certain one determinate under ψ . (ii) The determinate under ψ which invariably

accompanies any *one* determinate ϕ_r under ϕ is different from the determinate under ψ which invariably accompanies any *other* determinate under ϕ . (iii) There is a certain one mathematical operation F , such that, if ϕ_r be any determinate under ϕ , and ψ_r be the determinate under ψ which invariably accompanies ϕ_r , then the number which measures ψ_r can be obtained by performing the operation F on the number which measures ϕ_r ." The same law, on the entailment view, would be stated by substituting throughout the word "conveys" for the phrase "is invariably accompanied by," and the phrase "is conveyed by" for the phrase "invariably accompanies."

Now there are two remarks to be made about such laws.

(a) It seems very doubtful whether there is any interpretation which can be put on clause (i) by the regularity view which would not make that clause either false or trivial. If it is interpreted instantially, it implies that there are in nature instances of *every* determinate under ϕ . Now in many cases the number of determinates under ϕ is enormous, and perhaps even infinite. It is extremely doubtful whether every possible pressure or temperature or volume has had or will have an instance in nature. Yet no one would think that this was any reason for doubting a well-established formula connecting the pressure, the volume, and the temperature of gases. If, on the other hand, it is interpreted non-instantially and yet in accord with the regularity view, it becomes trivial as regards any determinate under ϕ which has no instances in nature. If there are no particulars in nature which have the determinate ϕ_r , it follows of course that there are no particulars in nature which have ϕ , and lack a certain determinate ψ_r under ψ . But this is entirely trivial. We want to be able to say: "If there *were* a particular which had ϕ in the form ϕ_r (which there is not), it *would* have ψ in the form ψ_r (though no particular in fact does)". The entailment view can give a meaning to such statements which does not reduce them to trivialities. So far as I can see at present, the regularity view cannot.

(b) It is extremely difficult to suppose that we rationally cognize any principle which, in conjunction with suitable

empirical premises, *would* justify us in believing a *functional regularity* but *would not* justify us in believing a corresponding *functional entailment*.) I therefore agree with Prof. Stout that, unless there are laws of functional *entailment* and we have grounds for believing some of them on empirical evidence, we have no ground for believing any law of functional *regularity* on empirical evidence. On the other hand, even if there are laws of functional entailment, I could have no right to believe any of them on empirical evidence *alone*. I should have no ground to believe any of them unless I rationally cognize some principle which, in conjunction with suitable empirical premises, would justify such a belief. Unfortunately I cannot formulate any such principle which I could claim, with the least conviction, to be sufficient for this purpose and to be rationally cognized by me. My conclusion is as follows. Either (a) I do rationally cognize some principle which, in conjunction with suitable empirical premises, would justify me in believing certain laws of functional *entailment*, although I cannot elicit or formulate any such principle ; or (b) no empirical evidence, however regular, varied, and extensive, gives me the slightest ground for believing a law even of functional *regularity*. I should tend, *prima facie*, to reject (b), as contrary to common-sense and to my own unquestioning convictions when not philosophizing about induction. But, when I realize that rejecting (b) entails accepting (a), I become more and more doubtful as to what I ought to hold.

ARE ANY CAUSAL PROPOSITIONS COGNIZED "A PRIORI"?—We must begin by defining our terms. To say of a proposition p that it is "known *a priori* by M" is equivalent to the following statement. "(a) The proposition p is a necessary one. (b) M knows that it is necessary ; either by direct inspection of it, or by seeing that it is a necessary consequence of certain other propositions which he sees by direct inspection to be necessary." Now the proposition p might be either *primary*, i.e., not about any other proposition, or it might be *secondary*, i.e., about some primary proposition q. If p is secondary, it might be of the form : "The proposition q is more probable than not, given the datum h." If I

know this secondary proposition *a priori*, and I am acquainted with the datum *h*, I may be said to "have *a priori* grounds for believing *q*." We can now define the statement: "*M* has *a priori* cognition of the proposition *x*." It means: "*M* either knows *x a priori* or has *a priori* grounds for believing *x*."

We have seen that it is impossible that any *causal law* should be rationally cognized by induction unless certain *principles*, which we have not managed to formulate, are rationally cognized. I think it is evident that these principles, if rationally cognized at all, must be cognized *a priori*. But the question remains whether any *causal proposition* is an object of rational cognition to any human mind. The connexion between this question and the questions which we have been discussing in the earlier part of the paper is the following. If the entailment view of causal propositions is correct, they are propositions of such a kind that they might conceivably be known *a priori*, though it is, of course possible that none of them is in fact known *a priori*. For, on the entailment view, they are necessary propositions; and therefore it is conceivable that someone might be able to see or to prove the necessity of some of them. If the regularity view of causal propositions is correct, they are propositions of such a kind that they could not conceivably be known *a priori*. For, on that view, they are contingent propositions.

Having cleared up these preliminary matters, I will now make the following remarks on the question at issue.

(1) We are always liable to think that we have *a priori* knowledge of a synthetic proposition when really we are having such knowledge only of a trivial analytic proposition which we have confused with the former owing to some trick of language. The following would be an example. It might be said that I know *a priori* that it is impossible for me to be now remembering an experience *e* unless I formerly had the experience *e*. Now this is true in the sense that the phrase "remembering *e*" is commonly used in such a way that no experience of mine would be called a "memory of *e*" unless I had formerly had the experience *e*. But, in

that sense, it is analytic and trivial. If, on the other hand, it is taken to mean that I could not have had an experience which is psychologically indistinguishable from a memory of *e* unless I had experienced *e* beforehand, the proposition is synthetic and interesting, but is almost certainly not known *a priori*.

(2) Prof. Stout's claim to know *a priori* certain psychological causal propositions connected with inference seems to me very interesting. If he claimed to know *a priori* that the proposition : " I am now believing *p* and seeing that it entails *q* " causally entails the proposition " I shall believe *q* in the immediate future," several objections might be made. I might lose consciousness in the immediate future or be struck dead. Or the effect might be that I begin to doubt *p* or to doubt whether I saw that *p* entails *q*. Lastly, it seems to me conceivable that, if none of these things happened, I might still avoid believing *q* if it were very distasteful to me. But I think that the claim can be stated in a way which will avoid these objections. Suppose we put it as follows : " So long as I am having the experience of believing *p*, of seeing that *p* entails *q*, and of considering whether *q* is true, it is impossible that I should be having the experience of disbelieving *q*." When the proposition is put in this form, it does seem to me to be self-evident ; and, so far as I can see, it is not merely analytic. It is, of course, a proposition asserting *simultaneous* causation ; whether an equally plausible example of an apparently self-evident and synthetic proposition involving causal *sequence* could be produced I do not know.

(3) Dr. Ewing holds that there are " degrees of *a priori* intelligibility." He is content to claim that we can see " apart from experience " that some kinds of sequence (among mental events, at any rate) would be " more intelligible than others." Prof. Stout says that insight into necessary connexions among natural processes " is in general, and perhaps always, only partial and imperfect." But it is no more so, he adds, " than, from the nature of the case, it ought to be in view of our inevitable ignorance of what actually takes place in causal process."

It seems to me that this notion of “degrees of *a priori* intelligibility” or of “partial and imperfect insight into necessary connexions” needs more explanation than it has received. I am going to try to clear it up. Let us call any instance in which a number of conditions $c_1 c_2 \dots c_n$ are simultaneously fulfilled in a certain relation S to each other a “concurrence” of these conditions. Let us suppose that any concurrence of $c_1 c_2 \dots c_n$ entails the simultaneous or immediately subsequent occurrence of one and only one event of a certain kind e which stands in a certain relation R to that occurrence. Let us further suppose that, if C and C' are two different concurrences of these conditions, the R -correlated e -event whose occurrence is entailed by C is different from the R -correlated e -event whose occurrence is entailed by C' . Finally, let us suppose that there is no selection from $c_1 c_2 \dots c_n$, such that the propositions enumerated above are true of this selection. Then I shall say that $c_1 c_2 \dots c_n$ are a “Smallest Sufficient Condition” of e . I shall say that any one of these conditions, or any selection consisting of several of them, is a “Relatively Necessary Condition” of e .

Now it is possible that there might be a number of alternative Smallest Sufficient Conditions of e . If there is any condition common to all the Smallest Sufficient Conditions of e , I shall call it an “Absolutely Necessary Condition” of e . It is quite possible that there should be no Absolutely Necessary Condition of e . On the other hand, it is possible that there might be several Absolutely Necessary Conditions of e . If so, I shall call the most numerous set of conditions common to all the Smallest Sufficient Conditions of e “The Greatest Absolutely Necessary Condition” of e . If e should have only one Smallest Sufficient Condition, every factor in this will be an Absolutely Necessary Condition of e , and e ’s only Smallest Sufficient Condition will be identical with e ’s Greatest Absolutely Necessary Condition.

Now even if e has *several* alternative Smallest Sufficient Conditions, and if it has *no* Absolutely Necessary Condition, it might still be possible to arrange e ’s Relatively Necessary

Conditions in what I will call an "order of dispensability." Suppose that e has N alternative Smallest Sufficient Conditions, and has no Absolutely Necessary Condition. Suppose that a certain condition c_1 is present in m of these, and that a certain other condition c_2 is present only in k of them, where k is less than m . Then there would be a perfectly good sense in saying that whilst c_1 and c_2 are *both* relatively, and *neither* absolutely, necessary conditions of e , c_2 is a "more dispensable" condition of e than c_1 is. To be an *absolutely* necessary condition of e is the same as to be a condition of e which has *zero* dispensability.

Now what is called "The Law of Universal Causation" may be stated as follows. "(i) Every *kind* of event has one or more Smallest Sufficient Conditions. (ii) Every *occurrence* of an event of a given kind e is due to one and only one occurrence of one and only one of the Smallest Sufficient Conditions of e . (iii) If E and E' are two *different* occurrences of an event of a given kind e , then the occurrence to which E is due and the occurrence to which E' is due are *different* occurrences of the same or of different Smallest Sufficient Conditions of e ." This is a principle about causation which many people would claim to find self-evident, even if they did not claim to find any causal proposition self-evident. I have defined "Smallest Sufficient Condition," and the other notions which are correlated with it, in terms of the entailment view of causation. But it would be possible, no doubt, to define them in terms of the regularity view; and a person who accepted the regularity view might find the Law of Universal Causation self-evident just as much as a person who accepted the entailment view. On the other hand, a person who accepted the entailment view, and who further claimed to find certain causal propositions self-evident, might nevertheless not find the Law of Universal Causation self-evident.

We must now apply these considerations to the question of "degrees of insight" into causal connexions. Let us assume that a person accepts the entailment view of causation and finds the Principle of Universal Causation, when stated in terms of entailment, self-evident. The former

assumption is certainly true of all the other symposiasts ; they have not made any explicit statement on the second point, but I think it is safe to assume that Prof. Stout and Dr. Ewing, if not Mr. Mace, do find the Principle of Universal Causation self-evident when stated in terms of entailment.

Consider the following five propositions. (i) "c is a relatively necessary condition of e." This is equivalent to the proposition : "There is *at least one* Smallest Sufficient Condition of e which contains c as a factor." (ii) "c is an absolutely necessary condition of e." This is equivalent to the proposition : "*Every* Smallest Sufficient Condition of e contains c as a factor." (iii) " $c_1 c_2 \dots c_m$ is the Greatest Absolutely Necessary Condition of e." This is equivalent to the proposition : " $c_1 c_2 \dots c_m$ are all contained in every Smallest Sufficient Condition of e, and *no other* factor is contained in every Smallest Sufficient Condition of e." (iv) " $c_1 c_2 \dots c_n$ is a Smallest Sufficient Condition of e." This is equivalent to the proposition : "Each of the conditions $c_1, c_2, \dots c_n$ is a relatively necessary condition of e, and their concurrence in a certain relation R to each other is a sufficient condition of e." (v) " $c_1 c_2 \dots c_n$ is the only Smallest Sufficient Condition of e." This is equivalent to the proposition : "Each of the conditions $c_1, c_2 \dots c_n$ is an absolutely necessary condition of e, and their concurrence in a certain relation R to each other is a sufficient condition of e."

It is evident that (i) is a weaker proposition than (ii) and that (ii) is weaker than (iii). It is also evident that (iv) is weaker than (v) and that (iii) is weaker than (v). I do not think that any direct comparison can be made between (iv) and either (ii) or (iii). But it follows immediately that (i) is the weakest and (v) is the strongest of all these propositions.

The mildest possible claim to *a priori* knowledge of causal laws would be the claim to know *a priori* some propositions of the form (i). This claim is certainly made by Prof. Stout and Dr. Ewing. The boldest claim would be the claim to know *a priori* some propositions of the

form (v). I do not think that any of the other symposiasts make this claim. An important intermediate claim would be to know *a priori* some propositions of the form (iv). This would involve knowing *a priori* some propositions of the form (i). For, if I know *a priori* that $c_1 c_2 \dots c_n$ is a Smallest Sufficient Condition of e , I *ipso facto* know, with regard to each of the factors, that it is a relatively necessary condition of e . I am not sure whether Dr. Ewing claims to know *a priori* any proposition of the form (iv). But the example of the psychological law about inference would seem to imply that Prof. Stout claims to know some propositions of this form *a priori*.

Now the growth of insight into causal connexions, which Prof. Stout talks about, might take two forms. It might be *extensive*, *i.e.*, we might get to know *a priori* more propositions of a certain form than we knew before. Or it might be *intensive*, *i.e.*, we might pass from knowing *a priori* only propositions of a weaker form to knowing *a priori* certain propositions of a stronger form. Even if we do not do this, we might pass to knowing *a priori* that certain propositions of a stronger form are more and more highly probable. I am not sure whether Prof. Stout would claim that insight grows intensively as well as extensively. And, if he claims that it grows intensively, I am not sure whether he would claim that this intensive growth of insight is of the first kind or that it is only of the second.

(4) Propositions of the form : “ c is a relatively necessary condition of e ” might be rationally cognized in two different ways, which might be called “ the *direct* way ” and “ the *indirect* way ” respectively. And the direct way might take two different forms. (i) It might be that, without ever having observed a transaction in which an occurrence of e was due to a Smallest Sufficient Condition in which c was a factor, I could know *a priori* that c is a relatively necessary condition of e . If so, I should know this in the direct way. (a) Supposing this to be possible, I might be able to know *a priori* that c is a relatively necessary condition of e merely by reflecting on c and without needing to have observed an occurrence of e . Dr. Ewing seems to claim such know-

ledge. (b) It might be that I should need to have observed occurrences of *e* in order to give me the idea of *e*, but that, when I reflect on *both* *c* and *e*, I can know *a priori* that *c* is a relatively necessary condition of *e*. This seems to me to be a much more reasonable claim. These are the two forms of the direct way of rationally cognizing that *c* is a relatively necessary condition of *e*.

(ii) Suppose I observe a certain occurrence of *e*, and suppose I know the Law of Universal Causation. Then I know that this occurrence of *e* must have been simultaneous with or immediately successive to a certain one occurrence of one or other of *e*'s Smallest Sufficient Conditions, and that this occurrence of *e* must stand in a certain relation *R* to this occurrence of this Smallest Sufficient Condition of *e*. Suppose I know that the relation *R* involves spatial adjunction or coincidence between the occurrence of an event and the occurrence of its Smallest Sufficient Condition in the case of physical events, and that it involves occurrence in the same mind in the case of mental events. If *e* is an event of a physical kind, suppose that a certain condition *c* was fulfilled in the immediate neighbourhood of this occurrence of *e* just before it happened. And suppose that, so far as I know, the situation in the immediate neighbourhood of this occurrence of *e* immediately before it happened differed from the situation in this neighbourhood a little while ago *only* by the fulfilment of *c*. Then it would be reasonable to conjecture that *c* is a relatively necessary condition of *e*. This could never be more than a reasonable conjecture. For, even if I *know* that only circumstances in the immediate neighbourhood of a physical event can be conditions of its occurrence, I could never *know* that the observed fulfilment of *c* immediately before the occurrence of *e* was the *only* change that had taken place in the immediate neighbourhood of this occurrence of *e* immediately before *e* happened.

This is what I mean by the "indirect way" of getting rational cognition of a proposition of the form: "*c* is a relatively necessary condition of *e*." It certainly cannot be said to give *a priori* knowledge of any causal proposition.

But it can be said that it gives us rational belief in certain causal propositions, which *is* based partly on a *priori* knowledge of causal principles and *is not* reached by problematic induction.

CAUSATION AND CONATION.—I propose to say very little on this topic, partly because I have had to say so much about the other parts of our subject, and partly because I find myself in almost complete agreement with the remarks which Dr. Ewing and Mr. Mace have made about Prof. Stout's theory. What I wish to add is this.

In defining the notion of "Smallest Sufficient Condition," and the other notions connected with it, we had to mention two kinds of relation, S and R. S is the relation which a number of simultaneously fulfilled conditions must have to each other if they are to be factors in a single occurrence of a single Smallest Sufficient Condition. It might be called a "Co-operative Bond." R is the relation in which each occurrence of e stands to one and only one occurrence of some one of e's Smallest Sufficient Conditions; each different occurrence of e being correlated by R with a different occurrence of some one or other of e's Smallest Sufficient Conditions. It might be called a "Consecutive Bond." It is plain that the Co-operative Bond involves more than mere spatial coincidence or adjunction in the case of purely physical conditions, and that it involves something more than mere occurrence in one and the same mind in the case of purely mental conditions. Similar remarks apply to the Consecutive Bond.

Now any ideas that we may have of co-operative bonds and consecutive bonds must presumably be derived from instances in which we were acquainted with an occurrence of a Smallest Sufficient Condition followed immediately by the occurrence of the event which it caused. Plainly one's own conative processes are the most striking and important instances of this kind of process with which we are acquainted. It is therefore highly plausible to hold that we derive from our acquaintance with them the idea of a number of factors co-operating or conflicting with each other and thus forming a single Smallest Sufficient Condi-

tion. And it is plausible to hold that we derive from the same source the idea of a certain event being *the* event which is determined by a certain occurrence of a certain set of co-operating and conflicting simultaneous conditions.

It is from the qualities and relations and changes of particulars with which we are acquainted that we must ultimately derive all the ideas by which we think of things and processes with which we are not acquainted. The ideas of spatial characteristics, of extensible qualities, and of motion and qualitative change, which we ascribe to physical objects, are derived ultimately from the spatial characteristics, the sensible qualities, and the sensible motion and qualitative change, which are manifested to us by the *sensa* that we sense. Similarly, when we think of physical things and processes as causal factors which co-operate and conflict and thus constitute total causes of certain physical events, the ideas which we use must ultimately be derived from instances of co-operation, conflict, and consequence with which we are acquainted. And the most striking, if not the only, instances with which we are acquainted are our own conative processes, whether successful or thwarted. Therefore, when we think of the external world under dynamical categories, we are no doubt using conceptions derived ultimately from our acquaintance with our own conative processes ; just as, when we think of the external world under spatial and kinematic and qualitative categories, we are using conceptions derived ultimately from our acquaintance with visual, tactual, auditory, and other *sensa*. Both procedures are psychologically inevitable ; and, if *either* is epistemologically justifiable, there is no reason to think that the first is so and the second is not.

On the other hand, we must remember how extremely remote a concept, which is ultimately derived from certain features in objects with which we are acquainted, may be from the sensible or introspectable characteristic which is its ultimate source. Contrast, *e.g.*, the notion of a generalized non-Euclidean N-dimensional manifold with the visual field and the spatial characteristics which it presents to our

inspection. The co-operative and consecutive bonds and their terms, which are involved in purely physical causation, may be as remote from those which we find in our own conative processes as the generalized non-Euclidean N-dimensional manifolds of the abstract geometer are from the visual fields which are the ultimate source of his spatial concepts.
