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Counterfactuals, Dispositions, and the Causal Modalities

by
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Introduction

(i) Although the following essay attempts to deal in a connected way with a number of connected conceptual tangles, it is by no means monolithic in design. It divides roughly in two, with the first half (Parts I and II) devoted to certain puzzles which have their source in a misunderstanding of the more specific structure of the language in which we describe and explain natural phenomena; while the second half (Parts III and IV) attempts to resolve the more sweeping controversy over the nature of the connection between 'cause' and 'effect,' or, in modern dress, the logical status of 'lawlike statements.'

(ii) The essay begins with a case analysis of a puzzle, taken from recent philosophical literature, relating to the analysis of counterfactual conditionals, statements of the form "If that lump of salt had been put in water, it would have dissolved." The diagnosis of this puzzle, which occupies the whole of Part I, shows it to rest on a misunderstanding of the conceptual framework in terms of which we speak of what *things do* when *acted upon* in certain ways in certain kinds of circumstance. Although the puzzle is initially posed in terms of examples taken from everyday life, the logical features of these examples which, misunderstood, generate the puzzle, are to be found in even the more theoretical levels of the language of science, and the puzzle is as much at home in the one place as in the other. For the framework in which things of various kinds (e.g. matches, white rats) *behave* ('respond') in various ways (catch fire, leap at a door) when acted upon ('submitted to such and such stimuli') under given conditions (presence of oxygen, 24 hours of food deprivation) is far more basic than the distinctions between metrical and non-metrical concepts, molar and micro-things, {226} observable and unobservable properties, empirical generalizations and theoretical assumptions, which seem, at first sight, to introduce such a gulf between pre-scientific and scientific discourse.

(iii) If Part I is primarily 'critical' in its orientation, calling attention in only the most general terms to the above mentioned logical features of the framework presupposed by counterfactuals such as "If that match had been scratched, it would have lighted," and subjunctive conditionals such as "If that piece of salt were put in water, it would dissolve," Part IIattempts a constructive account which, though necessarily brief and schematic, highlights those features of this framework which seem to have caused the most trouble. Postponing for later treatment (Parts III and IV) the classical puzzle about the 'connection' between 'cause' and 'effect,' it explores the logic of expressions for *things*, for *kinds* of things, for the *causal* properties of things, as well as the distinction between *properties* and *states.* It offers an analysis of the relation between thing-kinds and the traits in terms of which we identify things as belonging to them which illuminates both the nature and, which is more important, the *limitations* of the explanations provided by generalizations of the form "Things of kind K behave thusly when such and such is done to them under such and such conditions." I have italicized the word "limitations" because it is, in my opinion, the considerations advanced at the end of Part II which provide the key to a correct interpretation of the role of theoretical explanations and the status of theoretical ('unobservable') entities.

(iv) The second half of the essay (Parts III and IV) is devoted to an attempt to disentangle and resolve the issues matted together in the centuries long debate between the 'constant conjunction' (or 'regularity') and the 'entailment' (or 'necessary connection') interpretations of 'causality.' Part III attempts a sympathetic reconstruction of the controversy in the form of a debate between a Mr. C (for Constant Conjunction) and a Mr. E (for Entailment) who develop and qualify their views in such a way as to bring them to the growing edge of the problem. Although it is primarily designed to pose the problem in a way which reflects the philosophical commitments and concerns of the participants in the great debate, Part III also develops some of the themes and distinctions which are put to use in the constructive analysis which follows in Part IV. In particular, it contains a brief discussion of the force of probability statements (section 60), an examination of {227} what it might mean to say that the world is 'in principle' describable without using either prescriptive or modal expressions (sections 79-80), and some remarks on the supposed 'metalinguistic' status of modal statements (sections 81-82).

(v) Of the fourth and final part of the essay I shall say only that it offers an account of lawlike statements and of the inductive reasoning by which we support them which shows, in my opinion, how the logical insights of Mr. E can be reconciled with the naturalistic, empiricist tradition defended (if in too narrow and oversimplified a form) by Mr. C.

I. Counterfactuals

1. In his important paper on counterfactual conditionals,* Nelson Goodman interprets his problem as that of "defining the circumstances under which a given counterfactual holds while the opposing counterfactual with the contradictory consequent fails to hold."† As examples of such opposing counterfactuals, he gives "If that piece of butter had been heated to 150 F, it would have melted," and "If that piece of butter had been heated to 150 F, it would not have melted."

2. After a quick survey of some varieties of counterfactual and related statements, he finds that "a counterfactual is true if a certain connection obtains between the antecedent and the consequent,"‡ and turns to the task of explaining this connection. He points out, to begin with, that "the consequent [of a counterfactual] seldom follows from the antecedent by logic alone,"§ and never in the case of the empirical counterfactuals with which he is primarily concerned. Nor, in the case of the latter, does the consequent follow from the antecedent alone by virtue of a law of nature. For "the assertion that a connection holds is made on the presumption that certain circumstances not stated in the antecedent obtain."

When we say

If that match had been scratched, it would have lighted, we mean that the conditions are such—i.e., the match is well made, is dry enough, {228} oxygen enough is present, etc.—that "That match lights" can be inferred from "That match is scratched." Thus the connection we affirm may be regarded as joining the consequent with the conjunction of the antecedent and other statements that truly describe the relevant conditions. Notice especially that our assertion of the counterfactual is *not* conditioned upon these circumstances obtaining. We do not assert that the counterfactual is true if the circumstances obtain; rather, in asserting the counterfactual we commit ourselves to the actual truth of the statements describing the requisite relevant conditions. (p. 17)

"There are," he concludes, "two major problems, though they are not independent and may even be regarded as aspects of a single problem . . . The first . . . is to define relevant conditions: to specify what sentences are meant to be taken in conjunction with the antecedent as a basis for inferring the consequent."* The second is to define what is meant by a law of nature. For

even after the particular relevant conditions are specified, the connection obtaining will not ordinarily be a logical one. The principle that permits inference of
That match lights
from
That match is scratched. That match is dry enough.
Enough oxygen is present. Etc.
is not a law of logic but what we call a natural or physical or causal law. (p. 17)

3. Goodman first takes up the problem of relevant conditions. He has implied, in the passages just quoted, that whenever we assert a counterfactual, we have in mind a specific set of relevant conditions, those conditions, indeed, which the relevant law of nature requires to obtain in order that we may infer "That match lights" from "That match is scratched." Instead, however, of focusing attention on these specific conditions, and exploring their bearing on the truth or falsity of the counterfactual, Goodman begins from scratch. Thus he writes,

It might seem natural to propose that the consequent follows by law from the antecedent and a description of the actual state-of-affairs of the world, that we need hardly define relevant conditions because it will do no harm to include irrelevant ones. (pp. 17-18)

points out that

if we say that the statement follows by law from the antecedent and {229} *all* true statements, we encounter an immediate difficulty: among true sentences is the negate of the antecedent, so that from the antecedent and all true sentences everything follows. Certainly this gives us no way of distinguishing true from false counterfactuals. (p. 18)

and embarks on the task of so narrowing the class of true auxiliary sentences that we can account for this difference. A compact but lucid argument, in which he introduces a series of restrictions on the membership of this class, leads him to the following tentative rule:

. . . a counterfactual is true if and only if there is some set S of true sentences such that S is compatible with C [the consequent of the counterfactual in question] and with ~C [the contradictory consequent], and such that A · S is self-compatible [A being the antecedent] and leads by law to C; while there is no set S′ compatible with C and with ~C, and such that A · S′ is self-compatible and leads by law to ~C. (p. 21)

4. It is at this point that Goodman explodes his bomb.

The requirement that A · S be self-compatible is not strong enough; for S might comprise true sentences that although *compatible* with A, were such that *they would not be true if A were true*. For this reason, many statements that we would regard as definitely false would be true according to the stated criterion. As an example, consider the familiar case where for a given match M, we would affirm

(i)

If match M had been scratched, it would have lighted

but deny

(ii)

If match M had been scratched, it would not have been dry.

According to our tentative criterion, statement (ii) would be quite as true as statement (i). For in the case of (ii), we may take as an element in our S the true sentence

Match M did not light,

which is presumably compatible with A (otherwise nothing would be required along with A to reach the opposite as the consequent of the true counterfactual statement (i)). As our total A · S we may have

Match M is scratched. It does not light. It is well made. Oxygen enough is present . . . etc.;

and from this, by means of a legitimate general law, we can infer

It was not dry.

And there would seem to be no suitable set of sentences S′ such that A · S′ leads by law to the negate of this consequent. Hence the unwanted counterfactual is established in accord with our rule.

"The trouble," Goodman continues, without pausing for breath,

is caused by including in our S a true statement which although compatible with A would not be true if A were. Accordingly we must exclude {230} such statements from the set of relevant conditions; S, in addition to satisfying the other requirements already laid down, must be not merely compatible with A but 'jointly tenable' or *cotenable* with A. A is cotenable with S, and the conjunction A · S self-cotenable, if it is not the case that S would not be true if A were. (pp. 21-22)

5. This new requirement, however, instead of saving the rule leads it to immediate shipwreck.

. . . in order to determine whether or not a given S is cotenable with A, we have to determine whether or not the counterfactual “If A were true, then S would not be true” is itself true. But this means determining whether or not there is a suitable S_1, cotenable with A, that leads to ~S and so on. Thus we find ourselves involved in an infinite regressus or a circle; for cotenability is defined in terms of counterfactuals, yet the meaning of counterfactuals is defined in terms of cotenability. In other words, to establish any counterfactual, it seems that we first have to determine the truth of another. If so, we can never explain a counterfactual except in terms of others, so that the problem of counterfactuals must remain unsolved. (p. 23)

As of 1947, Goodman, “though unwilling to accept this conclusion, [did] not . . . see any way of meeting the difficulty.”* That he still regards this difficulty as genuine, and the line of thought of which it is the culmination philosophically sound, is indicated by the fact that he has made “The Problem of Counterfactual Conditionals” the starting point of his recent re-examination† of the same nexus of problems. Indeed, Goodman explicitly tells us that the four chapters of which this new study consists, and of which the first is a reprinting of the 1947 paper, “represents a consecutive effort of thought on a closely integrated group of problems,”‡ and that this first chapter contains “an essentially unaltered description of the state of affairs from which the London lectures took their departure.”§

6. It is my purpose in the opening sections of this essay, devoted as it is to fundamentally the same group of problems, to show that Goodman’s puzzle about cotenability arises from a failure to appreciate the force of the verbal form of counterfactuals in actual discourse, and of {231} the general statements by which we support them; and that this failure stems, as in so many other cases, from too hasty an assimilation of a problematic feature of ordinary discourse to a formalism by reference to which we have succeeded in illuminating certain other features.

7. Let me begin by asking whether it is indeed true that in “the familiar case where for a given match M, we would affirm

(i)
If match M had been scratched, it would have lighted”

we *would* “deny

(ii)
If match M had been scratched, it would not have been dry.”

Goodman himself points out in a note that "Of course, some sentences similar to (ii), referring to other matches under special conditions may be true."* Perhaps he has something like the following case in mind:

Tom:
If M had been scratched, it would have been wet.
Dick:
Why?
Tom:
Well, Harry is over there, and he has a phobia about matches. If he sees anyone scratch a match, he puts it in water.

But just *how* is Goodman's "familiar case" different from that of the above dialogue? Why are we so confident that (ii) is false whereas (i) is true? Part of the answer, at least, is that we are taking for granted in our reflections that the only features of the case which are relevant to the truth or falsity of (ii) are such things as that the match was dry, that it was not scratched, that it was well made, that sufficient oxygen was present, that it did not light, "etc."† and that the generalization to which appeal would properly be made in support of (ii) concerns only such things as being dry, being scratched, being well made, sufficient oxygen being present, lighting, etc. For as soon as we modify the case by supposing Tom to enter and tell us (a) that if M had been scratched, Harry would have found it out, and (b) that if Harry finds out that a match has been scratched, he puts it in water, the feeling that (ii) is obviously false disappears.

8. In asking us to consider this "familiar case," then, Goodman, whether he realizes it or not, is asking us to imagine ourselves in a {232} situation in which we are to choose between (i) and (ii) knowing (a) that only the above limited set of considerations are relevant; and (b) that scratching dry, well-made matches in the presence of oxygen, etc. causes them to light. It is, I take it, clear that if we did find ourselves in such a situation, we would indeed accept (i) and reject (ii).

To call attention to all this, however, is not yet to criticize Goodman's argument, though it does give us a better understanding of what is going on. Indeed, it might seem that since we have just admitted that once we are clear about the nature of the case on which we are being asked to reflect, we would, in the imagined circumstances, accept (i) but reject (ii), we are committed to agree with Goodman that the criterion under examination is at fault. For according to it would not (ii) be true?

9. It is not my purpose to defend Goodman's tentative criterion against his criticisms. There are a number of reasons why it won't do as it stands, as will become apparent as we explore the force of counterfactuals in their native habitat. I will, however, in a sense, be defending it against the specific objection raised by Goodman. For it is

because he *misinterprets* the fact that we would accept (i) but reject (ii) that he is led to the idea that the criterion must be enriched with a disastrous requirement of cotenability. And once this fact is properly interpreted, it will become clear that while there is *something* to Goodman's idea that a sound criterion must include a requirement of cotenability, this requirement turns out to be quite harmless, to be quite free of regress or paradox.

10. But is it, on second thought, so obvious that even if we were in the circumstances described above, we would reject (ii)? After all, knowing that M didn't light, but was well made, that sufficient oxygen was present, etc. and knowing that M wasn't scratched but was dry, would we not be entitled to say,

(iii)
If it had been true that M was scratched, it would also have been true that M was not dry?

The fact that this looks as thought it *might* be a long-winded version of (ii) gives us pause.

If, however, we are willing to consider the possibility that (ii) is after all true, the reasoning by which Goodman seeks to establish that if the tentative criterion were sound, (ii) as used in our "familiar case" would {233} be true, becomes of greater interest. The core of this reasoning is the following sentence,

As our total A · S we may have
Match M is scratched. It does not light. It is well made. Oxygen enough is present . . . etc.
and from this, by means of a legitimate general law, we can infer
It was not dry. (pp. 21-22)

But although Goodman assures us that there is a "legitimate general law" which permits this inference, he does not take time to formulate it, and once we notice this, we also notice that he has nowhere taken time to formulate the "legitimate general law" which authorizes (i). The closest he comes to doing this is in the introductory section of the paper, where he writes,

When we say
If that match had been scratched, it would have lighted
we mean that the conditions are such—i.e., the match is well made, is dry enough, oxygen enough is present, etc.—that "That match lights" can be inferred from "That match is scratched." (p. 17)

11. Now the idea behind the above sentence seems to be that the relevant law pertaining to matches has the form

(x)(t) x is a match · x is dry at t · x is scratched at t · *implies* · x lights at t

(where, to simplify our formulations, the conditions under which matches light when scratched have been boiled down to being dry.) And it must indeed be admitted that if this were the "legitimate general law" which authorizes

If M had been scratched, it would have lighted

given *M was dry* and *M was not scratched,* there would be reason to expect the *equivalent* "legitimate general law"

(x)(t) x is a match · x does not light at t · x is scratched at t · *implies* · x is not dry at t

to authorize

If M had been scratched, it would not have been dry

given *M did not light* and *M was not scratched.*

Or, to make the same point from a slightly different direction, if we {233} were to persuade ourselves that the laws which stand behind the true counterfactuals of the form,

If x had been . . . it would have ---

are of the form,

$(x)(t)\ A(x,t) \cdot B(x,t) \supset C(x,t)$

we would, in all consistency, expect the equivalent laws

$(x)(t)\ A(x,t) \cdot {\sim}C(x,t) \supset {\sim}B(x,t)$
$(x)(t)\ B(x,t) \cdot {\sim}C(x,t) \supset {\sim}A(x,t)$

to authorize counterfactuals of the same form. And if we were to persuade ourselves that

[Given that M was dry, then, although M was not scratched,] if M had been scratched, it would have lighted has the form

[Given that x was A at t, then, although x was not B at t,] if x had been B at t, x would have been C at t

we would expect the equivalent general laws to authorize such counterfactuals as

[Given that M did not light, then, although M was not scratched,] if M had been scratched, it would not have been dry

and

[Given that M did not light, then, although M was not dry,] if M had been dry, it would not have been scratched.

12. But as soon as we take a good look at these counterfactuals, we see that something is wrong. For in spite of the fact that “M was not dry” can be inferred from “M was scratched” together with “M did not light,” we most certainly would not agree that—given *M did not light* and *M was not scratched*—

If M had been scratched, it would not have been dry.

What is wrong? One line of thought, the line which leads to cotenability takes Goodman’s “familiar case” as its paradigm and, after pointing out that we are clearly entitled to say

(1)
[Since M was dry,] if M had been scratched, it would have lighted

{235} but not

(2)
[Since M did not light,] if M had been scratched, it would not be dry,

continues somewhat as follows:

(1) is true
(2) is false
According to (1) M would have lighted if it had been scratched.
But (2) presupposes that M did not light.
Thus (1) being true, a presupposition of (2) would not have been true if M had been scratched.
So, (1) being true, a presupposition of (2) would not have been true if its ‘antecedent’

had been true.

Consequently, (1) being true, the truth of (2) is incompatible with the truth of its ‘antecedent’—surely a terrible thing to say about any conditional, even a counterfactual one . . .

and concludes, (A) that the falsity of (2) follows from the truth of (1); (B) in knowing that (1) itself is true we must be knowing that there is no true counterfactual according to which a presupposition (1) would not have been true if *its* [the new counterfactual’s] antecedent had been true; and, in general (C) that in order to know whether any counterfactual, Γ, is true we need to know that there is no true counterfactual, Γ′, which specifies that the S required by Γ′ would not have been the case if Γ’s antecedent had been true. In Goodman’s words, “. . . in order to determine whether or not a given S is cotenable with A, we have to determine whether or not the counterfactual ‘If A were true, then S would not be true’ is itself true. But this means determining whether or not there is a suitable S_1 cotenable with A, that leads to ~S and so on. Thus we find ourselves involved in a regressus or a circle . . .”

13. Now there are, to say the least, some highly dubious steps in the reasoning delineated above. I do not, however, propose to examine it, but rather to undercut it by correctly locating the elements of truth it contains. That there is *something* to the above reasoning is clear. The truth of (1) *does* seem to be incompatible with the truth of (2); and the falsity of (2) *does* seem to rest on the fact that if M had been scratched, it would have lighted.

Perhaps the best way of separating out the sound core of the above reasoning is to note what happens if, instead of exploring the logical relationship between the two counterfactuals (1) and (2), we turn {236} our attention instead to corresponding subjunctive conditionals not contrary to fact in a new “familiar case” which differs from Goodman’s in that these subjunctive conditionals rather than counterfactuals are appropriate. Specifically, I want to consider the “mixed” subjunctive conditionals,

(1′)
If M is dry, then if M were scratched, it would light,

and

(2′)
If M does not light, then if M were scratched, it would not be dry.

Is it not clear as in Goodman’s case that (1′) is true but (2′) false? Indeed, that the falsity of (2′) is a consequence of the truth of (1′)? Here, however, there is no temptation to say that (2′) is false for the reason that in order for it to be true a state of affairs would have to obtain which would not obtain if M were scratched. For (2′), unlike (2) does not require as a necessary condition of its truth that M does not light.

14. How, then, is the incompatibility of (2′) with (1′) to be understood? The answer is really very simple, and to get it, it is only necessary to ask ‘Why would we reject (2′)?’ For to this question the answer is simply that it is just not the case that by scratching dry matches we *cause* them, provided they do not light, to become wet. And how do we know this? Part of the answer, of course, is the absence of favorable evidence for this generalization; not to say the existence of substantial evidence against it. But more directly relevant to our *philosophical* puzzle is the fact that in our “familiar case” we are granted to know that scratching dry matches causes them to light. And if this generalization is true—and it must be remembered that we are using “x is dry” to stand for “x is dry and x is well made, and sufficient oxygen is present, etc.”—then the other generalization *can’t* be true. The two generalizations are, in a very simple sense, incompatible. For if scratching dry matches causes them to light, then the expression ‘scratching dry matches *which do not light*’ describes a kind of situation which cannot (physically) obtain. And we begin to suspect that Goodman’s requirement of cotenability mislocates the sound idea that (to use a notation which, whatever its shortcomings in other respects, is adequate for the purpose of making *this* point) if it is a law that

(x) Ax · Sx · ⊃ · Cx

{237} then it can’t—*logically* can’t—be a law that

(x) Ax ⊃ ~Sx

15. But we have not yet pinpointed Goodman’s mistake. To do so we must take a closer look at our reasons for rejecting (2′). We said above that we would reject it simply because it is not the case that by scratching dry matches we cause them to become wet. Perhaps the best way of beginning our finer grained analysis is by making a point about our two subjunctive conditionals (1′) and (2′) which parallels a point which was made earlier about counterfactuals (1) and (2).

Suppose that the “legitimate general law” which authorizes (1′) had the form

(x)(t) A(x,t) · S(x,t) · ⊃ · C(x,t);

would not (2′) be authorized by

(x)(t) A(x,t) · ~C(x,t) · ⊃ · ~S(x,t)?

and hence—in view of the logical equivalence of these two general implications—be true if (1′) is true? Clearly we must do some thinking about the form of the "legitimate general laws" which authorize subjunctive conditionals of the form

if x were . . . it would ---

and which stand behind contrary-to-fact conditionals of the form

if x had been . . . it would have ---.

This thinking will consist, essentially, in paying strict attention to the characteristics of subjunctive conditionals, counterfactuals and lawlike statements in their native habitat, rather than to their supposed counterparts in PMese.

16. We pointed out above that if we were asked why we would reject (2′) in the context in which it arose, we would say that it is not the case that scratching dry matches causes them to become wet, if they don't light. We now note that if (1′) were challenged we would support it by saying that scratching dry matches does cause them to light, or that matches light when scratched, provided they are dry, or, perhaps, that if a dry match is scratched it will light, or something of the sort. Is it proper to represent these statements by the form

(x)(t) F(x,t) · G(x,t) · ⊃ · H(x,t)?

If we leave aside for the moment the fact that there is something odd {238} about the expression "x is a match at t," and focus our attention on the other concepts involved, it does not take much logical imagination to see that while 'there is no law against' representing "x is scratched at t" by "Sc(x,t)", "x is dry at" by "D(x,t)", and "x light at t" by "L(x,t)", to do so is to obscure rather than make manifest the logical form of "If a dry match is scratched, it lights." For it is by no means irrelevant to the logic of this generalization that matches *begin to burn* when they are scratched. And it is a familiar fact that

*A*s *B* when *D*ed—provided the circumstance, *C,* are propitious,

concerns something *new* that A's *begin* to do when *changed* in a certain respect in certain *standing conditions*—which need not, of course, be 'standing still.'

17. I do not, by any means, wish to suggest that all empirical generalizations are of the above form. Clearly,

Eggs stay fresh longer if they are not washed, which authorizes the counterfactual

If this egg had not been washed, it would have stayed fresh longer,

is not of this form. But our problem, after all, is that of understanding just *why* it is clear that—in Goodman's "familiar case"—we would affirm

(i)
If M had been scratched, it would have lighted

but reject

(ii)
If M had been scratched, it would not have been dry,

and to do this we must get the hang of generalizations of the former kind.

18. Now, *being dry* is obviously not the same thing as *becoming dry,* nor *beginning to burn* as *burning,* and though we can imagine that someone might say "matches *burn* when scratched," this would, strictly speaking, be either incorrect or false—incorrect if it was intended to express the familiar truth about matches; false if it was intended to express the idea that matches *burn* when they are *being scratched* as iron rusts while exposed to moisture. (Having made this point, I can now rephrase it by saying that if it were correct to use "matches burn when scratched" in the former sense, this would simply mean that "burn" has an idiomatic use in which it is equivalent to "begin to burn".)

With this in mind, let us examine the apodosis of Goodman's (ii), {239} namely, ". . . it (M) would not have been dry." If we suppose that this is intended to have the force of "would have *become* wet", we can, indeed, assimilate

If M had been scratched, it would not have been dry

to the form

If x had been *D*ed, it would have *B*ed

of which (i) is such a straightforward example. For *becoming wet* would seem to be a legitimate example of *B*-ing.

But while there are true generalizations to the effect that doing certain things to matches in certain favorable circumstances causes them to become wet, none of them seem to involve scratching. Again, a match which *becomes* wet must have *been* *dry*, and approaching Goodman's "familiar case"—as we do—in the knowledge that scratching dry (etc.) matches causes them to light, we cannot consistently say both

[Since M was dry, etc.,] if M had been scratched, it would have lighted

and

[Since M was dry, etc.,] if M had been scratched, it would have become wet

unless we suppose that the circumstances in which scratching dry matches causes them to become wet (the *etc.* of the second 'since-' clause) differs in at least one respect from the circumstances in which scratching dry matches causes them to light (the *etc.* of the first 'since-' clause). And it is clearly no help—in the absence of this supposition— to add to the second counterfactual the proviso, "provided M does not light"; for this proviso, given the truth of the first counterfactual, is physically inconsistent with the conjunction of the antecedent of the second counterfactual with the 'since-' clause on which it rests.

19. If, therefore, we interpret " . . . it (M) would not have been dry" as " . . . it (M) would have become wet," we run up against the fact that a generalization is implied which is not only patently false, but *inconsistent*—given the stipulations of the case—with one which we know to be true. And this is, as we have already noted, the sound core of Goodman's cotenability requirement. Two counterfactuals cannot both he true if they imply logically inconsistent generalizations. If one counterfactual is true, no counterfactual which involves an antecedent-cum-circumstances {240} which is specified to be physically self-incompatible by the generalization implied by the first counterfactual can also be true. On the other hand, cotenability thus *understood* leads to no "infinite regressus or . . . circle," for while one has not confirmed a generalization *unless* one has *disconfirmed* logically incompatible generalizations, this does not mean that *before* establishing one thing one must *first* establish something else, *and so on.* For the process of confirming a generalization *is* the process of disconfirming logically incompatible generalizations.

20. Suppose, however—as is indeed obvious—that we are not to interpret ". . . would not have been dry" as ". . . would have become wet"; does another interpretation of (ii) lie within groping distance? The answer is Yes—but, as before, on condition that we are prepared to make certain changes in its wording. Let us begin this groping with an examination of—not (ii) but—the closely related counterfactual,

If M had been scratched without lightning, then it . . .

then it *what?* Should we say "... *would* not have been dry"? or "... *could* not have been dry"? Clearly the latter. The difference, in this context, between 'w' and 'c' is all important. It is the difference between

(A)
Matches *will not* be (stay) dry, if they are scratched without lighting

and

(B)
Matches *cannot* be dry, if they do not light when scratched.

(A) introduces, as we have seen, a new generalization into our "familiar case"—one which is inconsistent with

(C)
Matches *will* light when scratched, provided they are dry,

which is the generalization implied by (i). (B), on the other hand, far from being inconsistent with (C) would seem to be just another version of it.

And it is clear, on reflection, that (C) is the only 'will-' statement which expresses the fact that scratching dry matches causes them to light. Thus, we can say that

A dry match will light when scratched

but not

A match which does not light when scratched *will* not be dry.

{241} To be sure, we can say—with a little license—

A match which does not light when scratched *will be found* not to be dry

so that the above claim is not *quite* true. But the point being made is clear enough, and, in any case, we shall be examining the 'exception' in a moment.

21. We begin, therefore, to suspect that corresponding to generalizations of the form

*B*ing *A*s causes them to *D*—provided *C*

there is only one correctly formed counterfactual of the form "If x had been *Y*ed, it would have ..." namely

[Since C,] if *this A* had been *B*ed, it would have *D*ed

which is not to say that each such generalization might not authorize a number of counterfactuals having a *different* form. Beating about the bushes for other asymmetries pertaining to our familiar generalization about matches, we notice that while it tells us that scratching matches causes them to light, *it doesn't tell us the cause of matches not being dry*; and that while it enables us to explain the fact that a match lighted on a certain occasion by pointing out that it was scratched and was dry, *it doesn't enable us to explain the fact that a match was not dry by pointing out that it was scratched without lighting.*

On the other hand, the generalization does enable us to explain *how we know* that a given match was not dry. "I know that it wasn't dry, *because* it didn't light when scratched." "M *can't* have been dry, because it was scratched, but did not light." "*Since* M was scratched, but did not light, it can't have been dry." "M was scratched without lighting, *so* it wasn't dry." All these point to the hypothetical

(M was scratched without lighting) *implies* (M was not dry),

and, indeed, to the general hypothetical

The fact that a match is scratched without lighting *implies* that it was not dry.

22. I have already pointed out how misleading it is to characterize the "legitimate general law" which authorizes the counterfactual

If M had been scratched, it would have lighted

{242} as a "principle which permits the inference of

That match lights

from

That match is scratched. That match is well made. Enough oxygen is present. Etc."*

For the fact that if there is a principle which authorizes the inference of S_3 from $S_1 \cdot S_2$, there will also be a principle which authorizes the inference of $\sim S_2$ from $S_1 \cdot \sim S_3$ leads one to expect that the same general fact about matches which, in Goodman's "familiar case," supports the above counterfactual, will also support

If M had been scratched, it would not have been dry, an expectation which is the ultimate source of the puzzlement exploited by Goodman's paper.

23. This is not to say that it is *wrong* to interpret our generalization about matches as a "season inference ticket." It is rather that the connection between the generalization and the counterfactual, "If M had been scratched, it would have lighted," rests on features of the generalization which are not captured by the concept of a season inference ticket, and which, therefore, the logical form of a general hypothetical does not illuminate. Thus, while

(m)(t) m is scratched at t · m is dry at t · *implies* · m lights at t[†]

does, *in a sense,* have the force of "dry matches light if scratched," or "scratching dry matches causes them to light," this mode of representation must be supplemented by a commentary along the lines of the above analysis, if its relation to "If M had been scratched, it would have lighted" is to be understood; while if our familiar fact about matches is assimilated without further ado to the form

(x)(t) A(x,t) · B(x,t) · C(x,t) . . . · ⊃ · L(x,t)

all chance of clarity has been lost.

24. We have connected the fact that scratching a match is *doing* something to it, with the fact that we expect

. . . if M had been scratched . . .

{243} to be preceded, at least tacitly, by an expression referring to *standing conditions,* thus

[Since M was dry,] . . .

and to be followed by an expression referring to a *result*, thus

. . . it would have lighted.

It is important to bear in mind that the distinction between the *standing conditions,* the *doing* and the *result* is an *objective* one. It is not relative to a particular way of formulating the general fact that dry matches light when scratched. The equivalent formulas

$(x)(t)\ D(x,t) \supset \cdot\ Sc(x,t) \supset L(x,t)$
$(x)(t)\ {\sim}L(x,t) \supset \cdot\ Sc(x,t) \supset {\sim}D(x,t)$
$(x)(t)\ {\sim}L(x,t) \supset \cdot\ D(x,t) \supset {\sim}Sc(x,t)$

do *not* give us three different ways of cutting up the above fact about matches into a standing condition, a doing and a result or *consequence*; although, *in a purely logical sense,* "~L(x,t)" may be said to formulate a 'condition' under which "(x)(t) Sc(x,t) ⊃ ~D(x,t)" holds, and "Sc(x,t)" and "~D(x,t)" respectively to be the 'antecedent' and the 'consequent' of this implication.

The fact that "D(x,t)" formulates a 'condition' in a sense in which "~L(x,t)" does not, is, though obvious, the key to our problem. For it is just because "If M were scratched . . ." and "If M had been scratched . . ." are expressions for something's being done to something (in a certain kind of circumstance) that we expect them to be followed, not just by a 'consequent,' but by (expressions for) a *consequence,* and also expect the context to make it clear just what conditions or circumstances are being implied to obtain. There is, however, a manner of formulating this same content which does not evoke these expectations, and which *does* focus attention on specifically logical relationships. Consider, for example, the following conditionals,

If it were the case that M was scratched without lighting, it would be the case that M was not dry.
If it had been the case that M was scratched without lighting, it would have been the case that M was not dry.

Clearly, to wonder what else would have to be the case, if it were the case that M was scratched without lighting, is not the same thing as to wonder what the *consequence* of striking a match would be, given that it failed to light.

{243} Does this mean that in our familiar case, we would accept

If it had been the case that M was scratched, it would have been the case that M was not dry (or would not have been the case that M was dry)

although we reject

If M had been scratched, it would not have been dry?

The answer is *almost* Yes. We are getting 'warmer,' though there is still work to be done. I shall introduce the next step by discussing examples of quite another sort.

25. Consider the following (where n is a number, perhaps the number of planets):

(1)
If n were divisible by 3 and by 4, it would be divisible by 12
(2)
If n were divisible by 3, then if n were divisible by 4, it would be divisible by 12
(3)
Since n is divisible by 3, if n were divisible by 4, it would be divisible by 12
(4)
Since n is divisible by 3, if n had been divisible by 4, it would have been divisible by 12

and then the following (where n is, say, the number of chess pieces on a side):

(5)
If n were not divisible by 12, but divisible by 3, it would not be divisible by 4
(6)
If n were not divisible by 12, then if n were divisible by 3, it would not be divisible by 4
(7)
Since n is not divisible by 12, if n were divisible by 3, it would not be divisible by 4
(8)
Since n is not divisible by 12, if n had been divisible by 2, it would not have been divisible by 4.

The crucial step, in each series, is from the first to the second, i.e. from (1) to (2), and from (5) to (6). (1) and (5) are clearly true. What of the others? What, to begin with, shall we say about (2)? The point is a delicate one. At first glance, it looks quite acceptable, a sound inference ticket. But would we not, perhaps, be a bit happier if it read

(2′)
If n were divisible by 3, then if it were *also* divisible by 4, it would be divisible by 12?

What of (3)? It calls attention to the argument,

n is divisible by 3
So, if n were divisible by 4, it would be divisible by 12.

{245} (Call the conclusion of this argument C_1.) The principle of the argument is the complex hypothetical (2). And the question arises, How does this argument differ from one that would be authorized by (1)? Consider the argument

n is divisible by 3
So, if it were *also* divisible by 4, it would be divisible by 12.

There is clearly a sense in which the conclusion of this argument (call it C_2) is more cautious than that of the preceding argument. For C_2 carries with it a reference to, and a commitment to the truth of, its premise, which is lacking in C_1. The difference can be put by noting that while C_1, as a conclusion, may imply that one has *come to know that* 'n is divisible by 12' can be inferred from 'n is divisible by 4' *by virtue of knowing that* n is divisible by 3; it does not imply that *what* one has come to know is {that 'n is divisible by 12' can be inferred from 'n is divisible by 4' *given that n is divisible by 3*}. And by not implying this, indeed by implying that what one has come to know is that 'n is divisible by 12' can *without qualification* be inferred from 'n is divisible by 4,' it is false. To infer from p the legitimacy of the inference *from q to r,* is not the same thing at all as to infer from p the legitimacy of the inference *from q to r, given p.*

26. In the symbolism of modal logic, there is all the difference in the world between 'p · q · < · r' and 'p < (q < r)' even though the corresponding formulas in the system of material implication are equivalent. The former authorizes the argument

I.
p
So, q ⊃ r

but not, as does the latter

II.
p
So, q < r

And while argument II defends the subjunctive conditional

If q were the case, r would be the case

argument I does not. The only resembling subjunctive conditional defended by the assertion *that p,* and an appeal to 'p · q · < · r' is

If q were the case *as well as p,* r would be the case

{246} and while this latter carries with it the assertion *that p,* it does not do so in the same way as does

[Since p is the case,] if q were the case, r would be the case.

For this points to argument II with its stronger conclusion.

27. Turning back to our two lists of conditional statements about numbers, we can now see that the counterfactual corresponding to (1) is not (4), but rather

(4′)
Since n is divisible by 3, if it had *also* been divisible by 4, it would have been divisible by 12

On the other hand, (4) does correspond to (2). Again, (6) has the same force as (5) only if it is interpreted as

(6′)
If n were not divisible by 12, then if it were *also* the case that it *was* divisible by 3, it would not be divisible by 4.

and (5) authorizes (8) only if it is interpreted as

(8′)
Since n is not divisible by 12, if it had *also* been the case that it *was* divisible by 3, it would not have been divisible by 4.

28. It is worth noting, in this connection, that it is not only in cases like our match example, where we are dealing with particular matters of fact, that a generalization may enable us to explain *how we know* a fact, without enabling us to explain the fact itself. Thus, while

If n is divisible by 3 and 4, it is divisible by 12

enables us to explain the fact that a certain number is divisible by 12 ("It is divisible by 12 *because* it is divisible by 3 and by 4."). It does not enable us to explain the fact that a certain number is not divisible by 4, though it does enable us to explain how we happen to know that the number is not divisible by 4. ("I know that it is not divisible by 4, because though it is divisible by 3, it is not divisible by 12." "He knows that it is not divisible by 4, because he knows that though it is divisible by 3, it is not divisible by 12." "It can't be divisible by 4, because, though it is divisible by 3, it is not divisible by 12.") It would simply be a mistake to say, "It is not divisible by 4, because, though divisible by 3, it is not divisible by 12."

One is tempted to put this by saying that just as one explains a particular matter of empirical fact by 'showing how it comes about,' and not, simply, by subsuming under the 'consequent' of a general hypothetical, {247} the 'antecedent' of which it is known to satisfy, so one explains such a fact as that a certain number is divisible by 12, or not divisible by 4, not simply by subsuming it under the ‘consequent’ of any old mathematical truth under the ‘consequent’ of which it can be subsumed, but only by applying a mathematical truth which, so to speak, takes us in the direction of the ‘genesis’ of the property in question in the mathematical order, i.e. which starts us down (or up?) the path of what, in a neatly formalized system, would be its ‘definition chain.’ Certainly “n is divisible by 12, because it is divisible by 3 and by 4” has something of this flavor, while “n is not divisible by 4, because though divisible by 3, it is not divisible by 12” does not. But to say anything worthwhile on this topic, one would have to say a great deal more than there is space for on this occasion.

29. Now the moral of these mathematical examples is that the counterfactual most resembling Goodman’s (ii) which *is* authorized by our (simplified) generalization about matches, is, explicitly formulated,

(ii′)
Since M did not light, if it had *also* been the case that it was scratched, it would have been the case that it was not dry,

and it would be correct to boil this counterfactual down to

If it had been the case that M was scratched, it would have been the case that M was not dry

only if the context makes it clear that there is a tacit *also* in the statement, and indicates in what direction the additional presupposition is to be found.

Why, then, it may be asked, should we not conclude that (i) itself is simply a shorter version, appropriately used in certain contexts, of

(i′)
Since M was dry, if it had *also* been the case that M was scratched, it would have been the case that M lighted?

(Clearly it is not a shorter version of

Since M was dry, if it had *also* been scratched . . .

for *this* would imply that something else must be *done* to the match besides scratching it, to make it light.)

The answer should, by now, be obvious. It is part of the logic of generalizations of the form

Xing Y's causes them to Z, provided . . .

{248} that when we say

If *this* Y had been Xed, it would have Zed,

it is understood that it is because *this* Y was in certain (in principle) specifiable circumstances that it would have Zed if it had been Xed. In other words, the fact that it is proper to say, simply

If M had been scratched, it would have lighted

rests on the relation of the statement to the *objective* distinction between *standing conditions*, what is *done* and its *result.*

30. If, however, the framework of our discussion has been adequate for the purpose of dispelling the specific perplexities generated by Goodman's formulation of his first problem, we must, before we turn our attention to his second problem, namely, that of the 'connection' between the antecedent and the consequent of a law of nature construed as a general hypothetical, build this framework into some sort of overall constructive account of the logical form of what, for the time being, we shall lump together as 'causal generalizations in actual usage.'

II. Thing-Kinds and Causal Properties

31. I shall begin this constructive account of causal generalizations with some remarks which grow quite naturally out of the first part of this essay. Suppose we have reason to believe that

Φ-ing Ks (in circumstances C) causes them to ψ

(where K is a kind of thing—e.g., *match*). Then we have reason to believe of a particular thing of kind K, call it x_1, which is in C, that

x_1 would ψ, if it were Φ-ed.

And if it were Φ-ed and did ψ, and we were asked “Why did it Ψ?” we would answer, “Because it was Φ-ed”; and if we were then asked, “Why did it Ψ when Φ-ed?” we would answer “Because it is a K.” If it were then pointed out that Ks don’t always Φ when Ψ-ed, we should counter with “They do if they are in C, as this one was.”

31. Now, there is clearly a close connection between

x_1 is (water-) soluble

and

If x_1 were put in water, it would dissolve.

So much so, that it is tempting to claim, at least as a first approximation, {249} that statements of these two forms have the same sense. I believe that this claim, or something like it, would stand up under examination—indeed, that the prima facie case in its favor is so strong that to defend it is simply to weed away misunderstandings. Unfortunately, “simply” to weed away misunderstandings is not a simple job. For most of them spring from misguided efforts to fit causal discourse into an overly austere, indeed procrustean, empiricism. And it will not be until the conclusion of the fourth and final section of this essay—in which I shall attempt to clarify certain fundamental issues pertaining to scientific inference—that we will, I believe, be in a position to accept the ‘obvious’ with good philosophical conscience.

32. Perhaps the simplest way to come to grips with puzzles about causal properties is to represent the analysis of concepts like ‘(water-) soluble’ by the schema

$D(x,t) =_{Df} \Phi(x,t)$ *implies* $\Psi(x,t)$

(where ‘Φ(x,t)’ and ‘Ψ(x,t)’ are informally construed as the counterparts, respectively, of ‘x is scratched at t’ and ‘x lights at t’) and then ask “What is the force of the term ‘implies’ in this context?” For this question calls attention to the fact that to attribute a property of the sort we are considering to an object is to be prepared *in that context* to infer ‘it ψs’ from ‘it is Φ-ed.’ And if the phrase “in that context” poses, in a sense, our original problem all over again, there may be some gain in the reformulation.

Now, any answer to this question must account for the fact that we think of *being Φ-ed* as the *cause* of *Ψ-ing*. The pellet of salt dissolved *because* it is put in water, just as, in our earlier example, the match lights *because* it is scratched. This suggests that to attribute to an object a ‘disposition’ of which the ‘antecedent’ is *being Φ-ed* and the ‘consequent’ *Ψ-ing*,is to commit oneself to the idea that there is a general causal fact, a law, shall we say, which related *being Φ-ed* to *Ψ-ing*.

On the other hand, it is perfectly clear that this law is not to the effect that *Φ-ing anything* causes it to Ψ, i.e. that

(x)(t) Φ(x,t) *implies* Ψ(x,t)

For when we say of a piece of salt that it is soluble, we are certainly not committing ourselves to the idea that everything, e.g. a stone, dissolves in water. And, indeed, the argument of the first part of this essay has made it clear that if, when we attribute a 'disposition' to an object, {250} we are committing ourselves to the existence of a general fact involving *Ψ-ing* and *being Φ-ed*, this general fact has the form *Φ-ing Ks (in C) causes them to Ψ.*

And reflection on the fact that when we attribute a 'disposition' to an object we think of being *Φ-ed* as the cause of *Ψ-ing* calls attention to the fact that words like "dissolves", "ignites", etc. are words for *results*. To say of something that it has *dissolved* is to say more than that, having been placed in water, it has disintegrated and disappeared, but is recoverable, say, by evaporation. It is to imply that it has disintegrated *because* it was placed in water. It is no accident that alongside such a word as "soluble" we find the word "dissolves". And this, in turn, suggests that it is no accident that alongside such a word as "soluble" there is the fact that we know such general truths as that *salt dissolves in water*.

33. Let us, therefore, work for the time being with the idea that when we ascribe a 'disposition' to a thing, we are committing ourselves to the idea that there is a general fact of the form

Whenever and wherever a thing of kind K is Φ-ed (in favorable circumstances) it Ψs.

And let us limit our discussion to those cases in which the things referred to are correctly classified by a thing-kind word in actual usage, and in which the causal properties in question are similarly enshrined in discourse. In other words, let us examine the logic of 'disposition terms' in a framework which abstracts from the fact, so annoying to logicians, that human discourse is discourse for *finding things out* as well as for expressing, in textbook style, what we already know.

34. Let us suppose, then, that to ascribe the causal property D(x,t) to x_1 *now,* where (it is also supposed that)

D(x,t) $=_{\mathrm{Df}}$ Φ(x,t) *implies* Ψ(x,t) is to commit oneself to the idea that x_1 belongs to a kind of thing, *K,* and is in a kind of circumstance, *C,* such that if one knew *which* kind was K, and *which* kind was C, one would be in a position to reason

x_1 is K, and is in C
So, if it were *also* the case that $\Phi(x_1, \text{now})$, it would be the case that $\Psi(x_1, \text{now})$

{251} thus,

x_1 is a pellet of salt . . .
So, if x_1 were put in water, it would dissolve.

Now, the "also" in the above reasoning schema reminds us that the first part of this essay has made it clear that

If x were Φ-ed, it would Ψ

is correctly transcribed into the technical language of logic by neither an *unqualified*

$\Phi(x_1, \text{now}) \supset \Psi(x_1, \text{now})$

which is obvious, nor by

$\Phi(x_1, \text{now}) \longrightarrow \Psi(x_1, \text{now})$

where '⟶' is the *basic* symbol for 'causal implication.' The root idea behind modal connectives is *inferability,* and, as we saw, once we turn from ordinary discourse to logistical formulations, the above subjunctive conditional must, in the first instance (i.e. neglecting, for the moment, all the other respects in which such formulations are misleading), be represented by the schema

. . . if it were *also* the case that $\Phi(x_1, \text{now})$, then it would be the case that $\Psi(x_1, \text{now})$

where the *also* makes it clear that the above attempt to represent the conditional by an unqualified modal statement simply won't do.

35. On the other hand, our analysis has also made it clear that the force of a subjunctive conditional is, at bottom, a modal force. It rests on such inference-authorizing general truths as 'salt dissolves in water' (or, to mention an example of a kind which is excluded from the restricted framework of the present discussion,' (In the northern hemisphere) floating needles point to the northernmost regions of the earth)'. And though we shall not come to grips with the 'causal modalities' until the concluding

section of this essay, it is not unreasonable to assume, provisionally, that these general truths are properly represented, in the technical language of modal logic, by the form

$$(x)(t) : K(x) \cdot C(x,t) \cdot \longrightarrow \cdot \Phi(x,t) \supset \Psi(x,t)$$

If so, it would be natural to suggest that

If x_1 were Φ-ed, it would Ψ

as presupposing a general fact of this form, should be transcribed by {252} an implication symbol which has a modal force without being the basic symbol for causal implication. It might, for example, be represented by the use of a shorter arrow, thus

$$\Phi(x_1, \text{now}) \rightarrow \Psi(x_1, \text{now}).$$

36. But before we follow up this suggestion, let me call attention to the fact, which has undoubtedly been noticed, that the above general hypothetical contains the expression 'K(x)', and not, as might have been expected, the expression 'K(x,t)'. This formulation embodies the fact that where 'K' is a thing-kind word, it is misleading to represent both 'x is a K' and, say, 'x is red' by the same form, 'F(x,t)'. The point is simply that *being a K* is logically related to the self-identity of a thing, x, at different times, in a way in which *being red* is not. It might be put by saying that where 'K' is a thing-kind word in a given context of discourse, if at *any* time it is true of x that it is a K, then if x were to cease to be a K, it would cease to be x, i.e. would cease to be (full stop). Or, to put it less paradoxically, *being a K* is not something that a thing *is at a time*, though it may be *true at a time* that it is a K. "Can't we say that x was a *child* at t, and subsequently a *man* at t′?" But 'child', as Aristotle saw, is not a thing-kind term. *Child* and *grown-up* are not sub-kinds of *man* (human being), as *man* and *dog* are sub-kinds of *animal.*

How we classify objects depends on our purposes, but within a given context of discourse, the identity of the things we are talking about, their coming into being and ceasing to be, is relative to the *kinds* of that context.

37. I shall have more to say about thing-kind words in a moment. But first let us put the suggestion of section 36 to work in the analysis of 'disposition terms.' With the introduction of the symbol '→' we would seem to be in a position to answer the question "What kind of implication belongs in the schema

$D(x,t) \; =_{Df} \; \Phi(x,t)$ *implies* $\Psi(x,t)$?"

by simply rewriting it as

$D(x,t) =_{Df} \Phi(x,t) \rightarrow \Psi(x,t)$.

But if we do so, we must not forget that it is only because we have informally been construing '$\Phi(x,t)$' as 'x is Φ-ed at t' and '$\Psi(x,t)$' as {253} 'x Ψs at t' that we can go from

$\Phi(x_1, now) \rightarrow \Psi(x_1, now)$

to

If x_1 were Φ-ed, it would Ψ

as contrasted with

If it were *also* the case that $\Phi(x_1, now)$, it would be the case the $\Psi(x_1, now)$.

In other words, the informal commentary with which we have been surrounding our use of logistical expressions is essential to their correct interpretation as a transcription of causal discourse. This commentary is associated with a division of the functions which appear in this transcription into four categories with a different category of sign designs for each category (thus, 'K_1' 'K_2' . . . 'K_n'; 'C_1' 'C_2' . . . 'C_n'; 'Φ_1' 'Φ_2' . . . 'Φ_n'; 'Ψ_1' 'Ψ_2' . . . 'Ψ_n') such that expressions for kinds of things are transcribed by a 'K', expressions for kinds of circumstances by a 'C', expressions—roughly—for something done to a thing by a 'Φ' and expressions for what it does in return by a 'Ψ'. Thus, the form 'F(x,t)' has a different *logic* depending on whether 'F' is representing a 'C', a 'Φ', or a 'Ψ'. And, indeed, if we read 'F(x,t)' as 'x is F at t,' we should not be under the illusion that "at t" means the same whether 'F' is a 'C', a 'Φ', or a 'Ψ'; or, for that matter—if we were to elect to represent 'x is a K' by 'K(x,t)'—a 'K'.

Thus it will not do simply to propose

$D(x,t) =_{Df} f_2(x,t) \rightarrow f_3(x,t)$

as the technical transcription of

x is soluble at t *if and only if,* if x were placed in water at t, x would (begin to) dissolve at t

stipulating only that expressions of the form

$f_1(x_1,t_1) \rightarrow f_2(x_1,t_1)$

imply the truth of a statement of the form

$(x)(t)\ f_3(x,t) \cdot f_1(x,t) \rightarrow f_2(x,t)$

and the truth of

$f_3(x_1,t_1)$.

For, in the absence of such additional stipulations as have been associated {254} with our division of descriptive functions into four categories, the above transcription procedures would generate Goodman's paradox.

38. Now we could, of course, abandon the attempt to capture the force of such subjunctives as

If M were scratched, it would light

in our technical language, and limit ourselves instead to conditionals of the form

If p were the case, q would be the case,

abandoning the device of contextual implication, and putting all implications into the direct content of what is said. This would mean that the counterpart of

If this were put in water, it would dissolve

would no longer be

$\Phi(x_1, \text{now}) \rightarrow \Psi(x_1, \text{now})$

but rather something like

$(\exists K)(\exists C) :: K(x_1) \cdot C(x_1,t_1) :. (x)(t) : K(x,t) \cdot C(x,t) \cdot \longrightarrow \cdot \Phi(x,t) \supset \Psi(x,t)$

i.e., there is a kind of thing and a circumstance such that x_1 is of that kind and is now in that circumstance, and such that if anything of that kind is ever in that circumstance, then if it is Φ-ed, it Ψs. For the only way in which the contextual implications of the above subjunctive conditional could become part of the direct content of what is asserted, is by the use of existential qualification over thing-kind and circumstance variables.*

On the other hand, if our technical language does distinguish between the four classes of function, there is no reason why we should not introduce a symbol, '→', together with the contextual stipulations described above. The issue is partly a matter of what we want our technical language to do. If our aim is the limited one of *rewriting* ordinary causal property and subjunctive conditional discourse in a symbolism sprinkled with 'Φ's, 'Ψ's, etc., then this purpose is readily achieved by introducing as many categories of, and logics for, symbolic expressions as are necessary to reproduce the complexity of ordinary usage. If, on {255} the other hand, our aim is in some sense to *analyze* or *reconstruct* ordinary usage, then, instead of simply creating, so to speak, a symbolic *code* for ordinary discourse, we will seek to introduce these special categories of expression with their special 'logics,' in terms of a smaller number of initial categories and a basic framework of logical principles. this effort, presumably, would be guided, not so much by abstract considerations of formal elegance, as by reflection on the scientific use the produce is to have. Of course, once the appropriate derivative categories had been introduced, it would then be possible to introduce the symbol '→' as before. This time, however, our transcriptions of causal discourse would be far more than a simple rewriting in logistical symbols.

39. Now it might be thought that the task of constructing our four categories of function, K, C, Φ, and Ψ out of more primitive descriptive functions, including, among others, a function 'f(x,t)'* (e.g. 'Red(x,t)'), is a straightforward one, if not downright easy. That this is not the case; that this task is not only not easy, but that it may spring from a misconception, will emerge, I believe, in the following paragraphs.

40. We pointed out above that when one makes explicit the presuppositions of a statement to the effect that a certain object, x, has a certain causal property D (of the kind we are considering) at time t, thus, '$D(x_1,t_1)$', one gets something like

$$(\exists K)(\exists C) :: K(x_1) \cdot C(x_1 t_1) :. (x)(t) : K(x,t) \cdot C(x,t) \cdot \longrightarrow \cdot \Phi(x,t) \supset \Psi(x,t)$$

Bearing in mind our stipulations of an ideal universe of discourse containing a *fixed* and *known* and *named* variety of thing-kinds, and a *fixed* and *known* and *named* variety of causal properties, let us use this form as a means of calling attention both to certain distinctions among causal properties, and to certain additional questions of philosophical interest.

41. It is clear, to begin with, that the above formula imposes serious restrictions on the sort of thing that is to count as a 'dispositional property.' The first thing to note is that it captures only one of the uses of the terms we are considering, for we not only speak, as above, of an individual thing as having a certain causal property; we also ascribe causal properties to thing-kinds.

When we say of salt, for example, that it is (water-) soluble, we are {256} clearly not ascribing this property to the thing-kind *at a time*. This would, indeed, be logical nonsense. The statement that salt is soluble has the form 'D(K)' and not 'D(K,t)'. How, then, shall we represent the connection between the two type levels? Shall we say

$$D(K) \equiv (x)\, K(x) \textit{ implies } D(x)$$

or

$$D(K) \equiv (x)(t)\, K(x) \textit{ implies } D(x,t)?$$

Actually, of course, these two formulas are equivalent, provided we stipulate that

$$D(x) \equiv (t)\, D(x,t).$$

But the question calls attention to the fact that not only do we say "salt is soluble" rather than "salt is soluble at t," we also say "This is soluble" rather than "This is soluble at t," or, to put it somewhat differently, "This is soluble" does not have the force of "This is soluble now." This suggests that the schema

$$D(x,t) \equiv \Phi(x,t) \rightarrow \Psi(x,t)$$

does not do justice to the logic of words for causal properties in ordinary usage. Just as something is of a kind, *period*, and not of a kind at a time, so something has a causal property, *period*.* As a matter of fact, this seems to be the heart of the distinction between *properties* (causal or otherwise) and *states*. Expressions for states have the form 'F(x,t)'; those for properties the form 'F(x)'. And what would correspond in ordinary usage to the 'D(x,t)' of the above schema is '(the state of) being such that if it were Φ-ed, it would Ψ.' Thus, *being magnetized* is a state.† We implied above that not all properties are causal properties. The point will be developed shortly. It must now be added, though the point is less likely to be controversial, that not all states have the form '(the state of) being such that if it were Φ-ed, it would Ψ.' The state of *being red* at a certain time would seem to be a good example of what might be called an "occurrent" as contrasted with a "causal" state.

{257} 42. Now if the above remarks are sound, they highlight anew the central role in causal discourse of thing-kind concepts. For *this* is soluble, *period*, rather than soluble-at-t, just because we are thinking of *this* as belonging to a soluble thing-kind. But to make these remarks stick, we must draw certain distinctions. For, to begin with, it might be said that *malleable* is a causal property, and yet a thing can be malleable at one time but not malleable at another. The rough and ready answer to this objection is that the term 'malleable' is ambiguous, and that in one sense of 'malleable', malleability may be a causal property of *iron*, while in another sense it may be a state of *this* (piece of) iron, which was not (in this sense) malleable a moment ago. But to spell this out calls for some remarks on the notion of a *capacity*.

43. It is not my purpose to botanize causal characteristics, to draw, for example, the familiar distinctions between ‘active’ and ‘passive’ powers, between quantitative (metrical or non-metrical) and non-quantitative causal properties, or between causal characteristics of various levels (illustrated by the distinction between being *magnetized* and being *magnetizable*). For the philosophical perplexities with which we are concerned arise when the attempt is made to understand even the most ‘elementary’ members of this family. Thus, our immediate purposes will be achieved by reflection on the distinction between a *disposition* (as this term is currently used) and a *capacity.*

As a first approximation, this distinction can be put by saying that to say of a certain kind of thing that it has the capacity to Ψ, ΓΨ, is to say that *there is* a combination of a *circumstance,* C, and a *something done,* Φ, which results in the Ψ-ing of that kind of thing. Thus, roughly,

$$\Gamma\Psi(K) =_{Df} (\exists C)(\exists \Phi) :. (x)(t) : K(x) \cdot C(x,t) \cdot \longrightarrow \cdot \Phi(x,t) \supset \Psi(x,t)$$

This reference to circumstances calls to mind the fact that on our account of dispositions to date, to ascribe a disposition to a thing is to imply that the thing *is actually in* a favorable circumstance, the *C* of ‘$(x)(t)\ K(x) \cdot C(x,t) \cdot \longrightarrow \cdot \Phi(x,t) \supset \Psi(x,t)$.’ It might not have been in C (or in any other favorable circumstance, C′), in which case a presupposition of the ascription would not obtain. This means, however, that we must reconsider the schema

$$D(K) \equiv (x)(t)\ K(x)\ \textit{implies}\ D(x,t)$$

which tells us that if K has D, then any thing of kind K is always in a {258} circumstance such that if it were Φ-ed, it would Ψ. This suggests that what we want, instead, is the function ‘$D_c(K)$’, where

$$D_c(K) \equiv (x)(t)\ K(x) \cdot C(x,t) \cdot \text{implies } D(x,t)$$

which amounts to the concept of being disposed to Ψ if Φ-ed *when the circumstances are C.* Only if things of kind K would Ψ if Φ-ed in any circumstances whatever, would it be proper to scribe to K a property of the form D(K) as contrasted with $D_c(K)$.

44. The next point I wish to make can best be introduced by considering an example which takes us somewhat away from the ‘ideal’ universe of discourse in which we have been operating. It takes its point of departure from the fact that according to our original account of disposition terms, to ascribe a disposition to an object at a certain time, thus, “$D(x_1,t_1)$”, is to imply the existence of a general fact such that *if one knew it,* and *if one knew* that x_1 had been Φ-ed at t_1, one would be in a position to reason

x_1 was a K
x_1 was in C at t_1
x_1 was Φ-ed at t_1
So, x_1 Ψ-ed at t_1

And this raises the question, Are we not often in a position to ascribe with reason a disposition to an object, although we are not in a position to subsume the object under a generalization of the appropriate form—that is to say of the form, 'Ks Ψ when Φ-ed in C'?

Suppose, for example, that the only chemical substances we have so far found to dissolve in water are salt, sugar and a few others, and that they all share the property of forming large white crystals. And suppose that by a chemical reaction which we can repeat at will, we produce large white crystals which consist neither of salt, sugar, nor any of the other substances in our list of soluble chemicals. Suppose, finally, that in the absence of any reason to the contrary we conclude that this new substance (do we also *conclude* that it is a substance?) is soluble.

Now, one way of interpreting the above reasoning is by saying that from the idea that all known soluble substances share the property of forming large white crystals, we drew the inductive conclusion that the product of this chemical reaction, as also having the property of forming large white crystals, (probably) belongs in its turn to *a* soluble {259} thing-kind, though we did not yet know *what* soluble thing-kind (*what* substance). We were not, the interpretation would continue, in a position to reason,

This is a K
So, if it were put in water, it would dissolve

though we were in a position to reason,

This has the property λ
Things having the property λ (probably) belong to a soluble thing-kind
So, if this were put in water, it would (probably) dissolve.

(where λ is the property of forming large white crystals).

'But,' it might be asked, 'instead of concluding that if something has the property λ, then it (probably) belongs to a soluble thing-kind, i.e. that

$(x)\lambda(x)$ implies (in all probability) $(\exists K)\, Sol(K) \cdot K(x)$

why not conclude that λ *itself* is a soluble thing-kind?' The suggestion is, in other words, that the proper conclusion of the above inductive inference, instead of being that having the *property* λ implies that *there is* a thing-kind K, such that

$$(x)(t)\ K(x) \cdot \longrightarrow \cdot\ \Phi(x,t) \supset \Psi(x,t)$$

should rather be, simply, that λ is *itself* the *thing-kind* in question, a thing-kind of which salt, sugar, etc., are sub-kinds. Our thing-kind generalization would be

$$(x)(t)\ \lambda(x) \cdot \longrightarrow \cdot\ \Phi(x,t) \supset \Psi(x,t)$$

and 'Sol(λ)' would be the counterpart of 'Sol(salt)'.

45. The answer is, in principle, straightforward. Words for thing-kinds not only embody a great deal of empirical knowledge, which is obvious, but they have quite a different role in discourse from that of expressions for properties, e.g., 'forming large crystals', or 'being white'. We might be tempted to locate this difference between, say, 'salt' and 'λ' by saying that while they both stand for properties, the property of being *saline* fits into a scheme of classification which organizes our chemical knowledge into a perspicuous whole. And, of course, it is true that thing-kind words do go along with ways of *classifying* things. But this is only part of the story, and is quite misleading if taken for the whole.

To bring this out, let us suppose someone to ask, "Can we not {260} imagine that λ might have been the identifying property of a thing-kind; and if so, would we not then be in a position to say 'x_1 is (a sample of) λ' as we now say 'x_1 is (a sample of) salt'?" But the change from 'x is λ' ('x is large crystal forming') to 'x is a λ' gives the show away. For if 'λ' were to acquire *this* use, it would no longer be the same term, as when John the miller became John Miller, the word "miller" acquired a new use.*

46. Words for thing-kinds have a special logic which is ill-represented by the schema

$$K(x) =_{Df} (t)\ P_1(x,t) \cdot P_2(x,t) \cdot \ldots P_n(x,t)$$

(where 'P' is a neutral term which ranges over both 'causal' and 'non-causal' characteristics.) There is, indeed, a sense in which a thing-kind word 'means' a certain set of characteristics; but, as elsewhere, the term 'means' is unilluminating. One must get down to cases and see just how words for thing-kinds are related to words for causal properties and to words for such items as *color, shape,* and *number of legs.*

Now, the basic flaw in the above schema is that it assimilates the logic of thing-kinds to that of complex properties. The point is not simply that thing-kind concepts are *vague* in a way which makes inapplicable the model of a set of separately necessary jointly sufficient defining criteria. For while there are important differences between thing-kind and property expressions with respect to the applicability of this model, the difficulty of finding separately necessary and jointly sufficient criteria is not limited to expressions for thing-kinds. Indeed, the problem of vagueness had been discussed for some time in terms of such examples as ‘bald’, before the pervasiveness of the problem became apparent. The point is the more radical one that the relation of a thing-kind word to the criteria for belonging to that kind of thing is different in principle from the relation of words for characteristics of things to the criteria for the presence of these characteristics. “Lemon” and “bald” may both be vague, but they are so in radically different ways.†

{261} 47. One way of attempting to put this point is by saying that words for kinds of thing continue, in an important sense, to have the same meaning in spite of significant changes in their so-called defining traits, whereas words for characteristics do not. Thus, one might begin by claiming that a shift in the criteria for baldness would amount to the substitution of a new concept for the old one. Suppose, for example, that evolution diminishes man’s initial endowment of hair; might not the word “bald” come to be so used that one would have to have less hair than today in order to be called bald? And would not the word have changed its meaning? This, of course, is much too simple. Yet the very way in which it is too simple throws light on thing-kind words. For the term ‘bald’ is not a cold descriptive term; bald people are not merely people with a ‘small’ amount of hair, nor is it simply a matter, say, of having a small proportion of the original endowment (in which case there need have been no change of meaning to begin with). The logic of ‘bald’ involves the idea that being bald is not the sort of thing one would choose; and this *theme* is a *continuing theme* in its use.

48. This idea of a *continuing theme* illuminates the ‘meaning’ of thing-kind words. It is the role of these words in explanation which accounts for the fact that it can be reasonable to say “That wasn’t really gold” in spite of the fact that the object in question was correctly called according to the criteria used at the time the claim that is being disputed was made. And the statement that the object *really* wasn’t gold is not to be construed as a queer way of saying that the word “gold” no longer means exactly what it did. It is the *regulative* connection of thing-kind words with the schema

Ks Ψ when Φ-ed, in appropriate circumstances

which guides them through the vicissitudes of empirical knowledge.

This feature of the tie between a thing-kind word and the criteria by which one identifies members of the kind throws new light on the logic of general truths concerning thing-kinds. If thing-kind words were adequately represented by the schema

$K(x) =_{Df} (t) P_1(x,t) \cdot P_2(x,t) \ldots P_n(x,t)$

then general truths of the form

$(x) K(x) \cdot \textit{implies} \cdot (t) \Phi(x,t) \supset \Psi(x,t)$

{262} would, viewed more penetratingly, have the form

$(x)(t) P_1(x,t) \cdot P_2(x,t) \ldots P_n(x,t) \cdot \textit{implies} \cdot \Phi(x,t) \supset \Psi(x,t)$

Now, if $P_1(x,t) \cdot P_2(x,t) \ldots P_n(x,t)$ were ‘occurrent’ rather than ‘causal’ characteristics, there would be relatively little in all this to puzzle us. But it is clear on reflection that the criteria for belonging to a thing-kind are by no means limited to non-causal characteristics; and once we realize this, we begin to be puzzled.

For, we wonder, how is it to be reconciled with the idea, on which we earlier laid such stress, that when a soluble object dissolves in water, the fact that it is put in water *causes* it to dissolve? We took this at the time to mean that while there is no general causal fact to the effect that *anything* put in water dissolves, the soluble object has *some* character such that it is a general fact that anything having this character which is *also* put in water, dissolves. If we now ask, Can this character be *another* causal property or set of causal properties, or include a causal property? we are strongly tempted to say No—partly because we are tempted to think of the fact that the object has this additional character as a *part cause* (and hence of the fact that it is put in water as *really* only itself as a *part cause* of the dissolving), and then to wonder how a causal property can be a *cause*; and partly because we smell the beginnings of a circle. If, on the other hand we try to fall back on the idea that the general fact in question is of the form

$(x)(t) O(x,t) \cdot \textit{implies} \cdot \Phi(x,t) \supset \Psi(x,t)$

(where O neither is nor includes a causal characteristic), we are confronted by the brute fact that *we don’t know any such general facts.*

49. Now part of the solution to this puzzle consists in recognizing that even if the fact that the object was put in water is not the complete explanation of the fact that it dissolved, the putting in water was not, for this reason, only a *part cause* of the dissolving. Thus, when we explain the fact that a piece of salt dissolved in water by calling attention to the fact that it *was* a piece of salt, we are not implying that being a piece of salt is a *part cause* (along with being put in water) of the dissolving. More, indeed, must be known of an object than the mere fact that it was put in water, in order to infer that it dissolved. But such thing-kind generalizations as “Salt dissolves in water”

include this *more* not by specifying additional part causes, but by restricting their scope to identifiable kinds of thing, in identifiable kinds of circumstance.

{263} 50. But the philosophically more exciting part of the solution consists in distinguishing between the causal properties of a certain kind of thing, and *the theoretical explanation of the fact that it has these causal properties*. For while causal generalizations about thing-kinds provide perfectly sound explanations, in spite of the fact that thing-kinds are not part-causes, it is no accident that philosophers have been tempted to think that such a phenomenon as salt dissolving in water must "at bottom" or "in principle" be a "lawfully evolving process" *describable in purely episodic terms*. Such an "ideal" description would no longer, in the ordinary sense, be in *causal* terms, nor the laws be *causal* laws; though philosophers have often muddied the waters by extending the application of the terms 'cause' and 'causal' in such wise that any law of nature (at least any nonstatistical law of nature) is a 'causal' law.*

It would be a serious mistake to think that a mode of explanation, in particular, ordinary causal explanation, which enables us to give satisfactory answers to one family of questions, cannot be such as *by its very nature* to lead us on to new horizons, to new questions calling for new answers of a different kind. The plausibility of the 'positivistic' interpretation of theoretical entities rests on a failure to appreciate the way in which thing-kind generalizations by *bunching* rather than *explaining* causal properties point beyond themselves to a more penetrating level of description and explanation; it rests, that is, on a failure to appreciate the promissory note dimension of thing-kind expressions. The customary picture of the relation of 'observational' to 'theoretical' discourse is upside down. The 'primacy' of molar objects and their observable properties is *methodological* rather than *ontological*. It is the ultimate task of theory to re-create the observational frame in theoretical terms; to make available *in principle* a part of itself (at a highly derived level, of course) for the observational role—both perceptual, and, by containing a micro-theory of psychological phenomena, introspective.†

51. We said above that the picture of the world in terms of molar things and their causal properties (a) points beyond itself to a picture of the world as pure episode, and (b) leads, by its own logic, to the {264} introduction of unobserved entities. It is important to see that these two 'demands,' though related, do not coincide. For micro-theories themselves characteristically postulate micro-*thing-kinds* which have fundamentally the same logic as the molar *thing kinds* we have been considering. And if they *do* take us *on the way* to a process picture of the world, they do not take us all the way. For even if a 'ground floor' theory in terms of micro-micro-things were *equivalent* to a pure process theory by virtue of raising no questions concerning the causal properties of these micro-micro-things to which it could not provide the answer, it would not for that reason *be* a pure process theory. For the logical form of

a *thing* theory is, after all, characteristically different from that of a theory whose basic entities are spatio-temporally related *events*, or overlapping *episodes.*

52. The conception of the world as pure process, which is as old as Plato, and as new as Minkowski, remains a regulative ideal; not simply because we cannot hope to know the manifold content of the world in all its particularity, but because science has not yet achieved the very concepts in terms of which such a picture might be formulated. Only those philosophies (New Realism, Neo-Thomism, Positivism, certain contemporary philosophies of common sense and ordinary usage, etc.) which suppose that the final story of "what there is" must be built (after submitting them to a process of epistemological smelting and refinement) from concepts pertaining to the perceptible features of the everyday world, and which mistake the methodological dependence of theoretical on observational discourse for an intrinsically second-class status with respect to the problems of ontology, can suppose the contrary.

53. Important though these broader implications of our analysis may be, to follow them up would take us far beyond the scope of this essay. Our task is to quarry some of the stones for this more ambitious enterprise. Let us, therefore, conclude the present section of this essay with some remarks on the distinction between the *identifying traits* and the *properties* of thing-kinds.

Suppose, to begin with, that a certain kind of thing, K_1, has a certain causal characteristic, P_1, which is not one of its identifying traits. One might be tempted to think that the assertion that K_1 has P_1 is equivalent to the assertion that anything having the traits by which K_1 is identified, has P_1. If our argument to date is sound, however, this idea would {265} be incorrect—as assimilating thing-kind expressions to expressions for complex characteristics. On the other hand, it *is* quite true that the *momentary cash value* of the idea that K_1 has P_1 is the idea that the traits by virtue of which K_1 is identified are conjoined with P_1. It is important to note, however, that not *all* the identifying traits of a thing-kind need be directly relevant to its possession of a given causal characteristic. Indeed, we are often in a position to formulate generalizations which, prima facie, have the form

$$(x)\ P_n(x) \cdot \ldots P_{n+m}(x) \cdot \longrightarrow \cdot\ (t)\ \Phi(x,t) \supset \Psi(x,t)$$

where $P_n \ldots P_{n+m}$ constitute a proper subset of the identifying traits of a thing-kind, and where this group of traits may be found in several thing-kinds. The existence of generalizations having (prima facie) this form encourages the mistaken idea that thing-kind generalizations are *special cases* of such generalizations, cases in which the set of traits in question has acquired the status of a thing-kind concept by virtue of being given a place in a classificatory scheme.

The truth of the matter is rather that the generalizations represented above exist *within* a framework of identifiable thing-kinds, and that far from it being the case that generalizations pertaining to thing-kinds are a special case of generalizations pertaining to sets of characteristics, the latter are abstractions from generalizations pertaining to thing-kinds, and are, implicitly, of the form,

(x)(K) :. K(x) *implies* $P_n \ldots P_{n+m}(x)$: *implies* : (t) $\Phi(x,t) \supset \Psi(x,t)$

54. Finally, what of the case where a causal property is one of the identifying traits of a thing-kind? What are we to make of the formula

(x)(t) K(x) · C(x,t) : *implies* · $\Phi(x,t) \supset \Psi(x,t)$

(where *Ψ-ing when Φ-ed (in C)* is an identifying trait of K)? We seem to be confronted with a dilemma. Either the remaining identifying traits of K imply the presence of this trait—in which case there is no point to including it among the identifying traits—or they do not, in which case how can a generalization of the above form be anything other than a tautology? But surely when we suggested that

If x_1 were Φ-ed, it would Ψ

points beyond itself to

F(x_1, now), and (x_1, now)
{266} *So* if it were *also* the case that Φ(x_1, now) it would be the case that Ψ(x_1, now)

we were thinking of the relation between *F and Ψ-ing when Φ-ed* as synthetic and empirical!

It might be thought that the solution to this puzzle is to be found in the idea that the *remaining* identifying traits imply (perhaps only with high probability) that objects having them Ψ when Φ-ed (in C), so that our reasoning, instead of the above, is (roughly)

x_1 is (K - P_1) [where P_1 is the property of Ψ-ing when Φ-ed (in C)]
and x_1 is now in C
So, if it were also the case that Φ(x_1, now), it would be the case that Ψ(x_1, now).

But it does not seem to be true that the identifying traits of a thing-kind need be highly correlated with each other. What *is* true, and the key to the answer, is that *as a complete group* they belong together by virtue of their correlation *with non-identifying* *traits.* They belong together, that is to say, by virtue of the fact that the thing-kind they identify has causal properties which belong to it *as a matter of empirical fact.*

The ‘empirical’ element, then, in the connection between *being a K* and *Ψ-ing when Φ-ed (in C)*, where Ψ-ing when Φ-ed (in C) is an identifying trait of K enters in with what might be called the ‘methodological distance’ between thing-kind words and identifying traits to which we have called attention, a distance which enables them to play their characteristic role, both in the organization of empirical knowledge at a time, and in the search for new knowledge. It is this methodological dimension which enables a reference to being a K in circumstances C to authorize a subjunctive conditional, even though, as far as *cash value* is concerned, ‘(x)(t) K(x) · C(x,t) · *implies* · Φ(x,t) ⊃ Ψ(x,t)’ is a tautology.

III. Causal Connection: The Dialectic of the Controversy

55. I propose next to discuss a number of questions which, though they must inevitably arise in any discussion of subjunctive conditionals or causal properties, have been either postponed outright or answered most provisionally in the preceding sections of this essay. The general {267} theme of these questions, as well as the appropriate apperceptive mass of philosophical perplexity, can readily be called to mind by referring to the perennial debate as to whether or not ‘causality’ is to be construed as a ‘necessary connection’ of logically distinguishable states of affairs.

56. It will be useful to begin by reflecting on the general pattern of this controversy. It has tended to organize itself around two ‘polar’ positions. Each is a ‘straightforward’ answer to the question, What is the relation between the two kinds of state of affairs, A and B, which we have in mind when we speak of them as causally connected? These answers are, respectively, (1) “The relation consists in a constant conjunction of A with B”; (b) “The relation is a logical relation of entailment—in an inclusive but proper sense of this phrase—between A and B.” Needless to say, once these ‘straightforward’ answers have been given, each is immediately surrounded with a host of qualifications and ‘clarifications’ designed to counter the standard objections from the opposite camp.*

Now, the only way to resolve the perplexities embodied in this controversy is to ‘join’ it, while maintaining—to adopt and adapt a metaphor from aesthetics—an appropriate ‘philosophical distance.’ And perhaps the easiest way to put ourselves into the picture is to ask, Why would anyone be moved to say that causal connection is a quasi-logical relation of entailment?

First, why entailment? Mr. Entailment's answer is simple enough. "Because when we say that A and B are causally related, we mean that if a case of A were to exist, a case of B *would* exist; we mean that there are cases of B *because* there are cases of A; in short, we *reason* from the existence of a case of A to the existence of a case of B. And if this is what we have in mind, does it not amount to the idea that the existence of an A-situation *entails* the existence of a B-situation?" To which Mr. E adds, "The sheer idea that there have been and will be no A-situation unaccompanied by B-situations, while it does, indeed, imply that *anything identical with one of the past, present or future A-situations*—granted there be such—would have a B-situation as its {268} companion, simply does not imply that if an A-situation *were* to occur, it *would* have a B-situation as its companion."

Why quasi-logical (or, as we put it above, logical in a broad, if proper sense)? Again Mr. E has a ready answer. "Actually it might better be called 'natural' or 'physical' entailment, for while any entailment is a logical relation, we can distinguish within the broad class of entailments between those which are, and those which are not, a function of the specific empirical contents between which they obtain. The latter are investigated by general or formal logic (and pure mathematics). Empirical science, on the other hand, to the extent that it is a search for *laws,* is the search for entailments of the former kind. (Putative) success in this search finds its expression in statements of the form 'It is (inductively) probable that A physically entails B.'"

57. Mr. C(onstant Conjunction), as is typical in debates of this kind, opens his case with an attack. "Whatever else, if anything, it may mean to say that A 'physically entails' B, it is surely a necessary condition of the truth of 'A physically entails B' that A be constantly conjoined with B. In short,

E_p(A,B) *logically implies* (x) Ax ⊃ Bx*

It would, however, I am prepared to admit, be a mistake simply to *equate* lawlike statements with statements affirming constant conjunctions. Something more must, indeed, be said. It is this *more* which finds unhelpful expression in your phrase 'physical entailment'.

"To begin with," continues Mr. C, "the statement 'A-situations cause B-situations' differs from the technical formula '(x) Ax ⊃ Bx' in that it carries with it the contextual implication that the conjunction of A with B is not simply a matter of there being no As. There may be secondary senses of 'law' in which a statement may formulate a law and yet, strictly speaking, be vacuously satisfied, but they have a derivative status which is to be explicated in terms of their relation to non-vacuous conjunctions. More important is the fact that a basic lawlike statement carries with it the further implication that *it is accepted on inductive grounds*, and, in particular, that it is not accepted on the ground that all the As which (in all probability) ever will have existed *have been examined and found to be B.* The statement, in short, *sticks its neck* {269} *out.** It is this neck-sticking-out-ness (to use a Tolmanism) which finds its expression in the subjunctive mood. Thus, when I say 'If this were an A-situation, it would be accompanied by a B-situation,' this statement not only gives expression to the idea that A is constantly conjoined with B, but does so in a way which has these contextual implications."

58. Mr. C concludes his initial say with the following two comments on Mr. E's contention : (a) "I am ill at ease in this vocabulary of 'entailments,' 'necessary connections,' and, to tell the truth, modal expressions generally. It does seem to me, however, that if one is going to use this vocabulary, one should *use* it, and not pick and choose. Isn't an entailment the sort of thing one *'sees?'* Doesn't one describe an inference in these terms by saying that the person who (correctly) drew the inference not only believed the premises, but *'saw'* that the premises entailed the conclusion? But ever since the collapse of extreme rationalism, philosophers of your stripe have hastened to admit that one doesn't 'see' these 'physical entailments,' but merely concludes that (probably) *there is* a relation of physical entailment between, say, A and B. Surely, however, an entailment which can't be *'seen'* is an entailment which can't do entailment work, which can't serve the purpose you want it to serve, namely that of authorizing inference." (b) "If, on the other hand, you are prepared to say, in the context of discovering empirical laws, that we do come to 'see' physical entailments, you must face up to the fact that we 'see' formal entailments by understanding the meanings of the terms of our discourse. Yet it is obviously not by reflecting on the meanings of empirical terms that we discover empirical laws."

Both of these points are telling ones, and together they appear to confront Mr. E with an insuperable dilemma: *either* physical entailments can't be 'seen'—in which case they are not *entailments*; or they can be 'seen'—in which case they are not *empirical.*

59. As far as the first horn of this dilemma is concerned, I think that we, from our 'philosophical distance,' must agree that it won't do at all to claim that we make inferences of the form

x_1 is A
So, x_1 is B

{270} which are not enthymematic forms of

(x) x is A ⊃ x is B
x_1 is A
So, x_1 is B;

add that the validity of such inferences is to be explicated in terms of a 'physical entailment' between A and B; *and then deny that we can 'see' that A entails B.* If one is going to explain our thinking in causal matters by using the idea of physical entailment, one must do more than defend the idea that "there are" such entailments; *one must make plausible the idea that these entailments play a role in causal reasoning analogous to the role of 'formal' entailments in less problematic forms of inference.* This, of course, does not mean that one is precluded from speaking of *unknown* (not *unknowable*) physical entailments, any more than the fact that we can 'see' formal entailments precludes us from speaking of *unknown*formal entailments.

60. Now, Mr. E *might* reply to all this—and I say 'might' because we are on the point of taking over the argument—as follows: "To conclude, 'It *is* (inductively) *probable* that A physically entails B' is simply to conclude (*inductively*) that A physically entails B. If Jones concludes (inductively) that A physically entails B, *and we think him right in so doing,* we shall say that he has *come to see* that A physically entails B, thus borrowing for metaphorical use from perceptual discourse the *endorsing* form 'x sees that p'."

This hypothetical reply, while it won't do as it stands, is a step in the right direction. It has, to begin with, the fundamental merit of recognizing that to say "(So) *probably* p" is not to attribute a relational property to a state of affairs, i.e. that of 'standing in the probability relation' to certain implied facts. It is to assert *that p,* but contextually imply that one has good reasons of an empirical kind for asserting it. In other words, "Probably p" *says that p,* but *indicates* something about the grounds on which one says that p.

And if this is the case, the fact that our knowledge of the laws of nature is expressed by the form

(In all probability) p

has not the slightest tendency to show that what we are *really* knowing is facts of the form

Prob(p,e)

{271} i.e., *p is probable on evidence e,* and that it is facts of this form which are our reasons for asserting *that p.* Our reason for asserting *that p* is *that e*, *not* that prob(p,e). And the knowledge expressed by

(In all probability) p,

where it *is* properly endorsed as knowledge, is not the knowledge *that prob(p,e),* but the knowledge *that p*. And this fundamental point about the phrase ‘in all probability’ is untouched by the fact that ‘p’ may be, in a suitably broad sense, a statistical proposition. Nor does it require us to deny that there are such facts as *that prob(p,e),* as is readily seen once one recognizes the ubiquitous role in discourse of the context “. . . it is a fact that . . .” and its cousin “. . . it is true that . . ..” Consider the sentence

It is true that debts ought to be paid, but Jones is in such a tight spot that . . ..

Do we really think that this should be edited with raised-eyebrow quotation marks to read

It is ‘true’ that . . .?

61. Thus, if laws of nature are, as Mr. E insists, of the form

Being A physically entails being B,

the fact that the knowledge of such a law would be properly expressed by

(In all probability) being A physically entails being B

does not mean that what is known is ‘the probability of the existence of a relation of physical entailment between being A and being B.’ To say that Jones knows that A causes B, and add that this knowledge is, of course, inductive, is not to say that Jones knows a probability fact about A causing B, but simply to say that his knowledge that A causes B is *probable knowledge,* i.e. the kind of knowledge we get in empirical science, as contrasted with the kind of knowledge we get in, say, pure mathematics.

On the other hand, Mr. E *was* going a bit fast when he went from the sound idea that the inductive character of scientific knowledge is compatible with the claim that it is the physical entailments themselves which are known, to the idea that it is compatible with the claim that these entailments are *‘seen.’* It simply does not seem proper to speak of an entailment as ‘seen’ when the knowledge of the entailment is probable knowledge in the sense characterized above. Thus, if it were {272} true that entailments had to be ‘seen’ and not merely *known* in order to authorize inferences, subjunctive conditionals, etc., Mr. E would not yet be out of the woods.

Yet that some progress has been made is indicated by the fact that if we were asked, Would not the *knowledge* that A entails B authorize the inference from ‘this is A’ to ‘this is B’ even if the entailment were physical entailment, and the knowledge that it obtains the probable knowledge of inductive science? the only thing that would keep us from answering “Yes” is doubt about the very idea, presupposed by the question, that there is such a thing as the ‘probable knowledge of physical entailments’; indeed, that there is such a thing as physical entailment.

62. Thus, even if Mr. E’s thesis has been reformulated in such a way as to meet some of the objections leveled against it by Mr. C, we have not yet come to grips with the most searching challenge of all, namely, Why introduce the concept of physical entailment at all? Mr. C, it will be remembered, argued

1. that whatever *else* it may be, the idea that A physically entails B is, *at least in part*, the idea that A is constantly conjoined with B;
2. that while it would indeed be incorrect to say that ‘A causes B’ simply amounts to ‘A is constantly conjoined with B,’ the latter *with certain qualifications* will do the job;
3. that these qualifications amount, at bottom, to the idea that ‘A causes B’ says that

$$(x)\ Ax \supset Bx$$

and *implies* that the latter is asserted on inductive grounds.

Of these three contentions, the first would seem to be common ground, for Mr. E and his colleagues are typically prepared to grant without further ado that to say that *being A* physically entails *being B* is equivalent to saying that

It is physically necessary that A be constantly conjoined with B

or, in current logistical symbolism,

$$\{(x)\ Ax \supset Bx\}$$

where ‘ ’ has the sense of ‘it is causally necessary that’, and where ‘ $\{(x)\ Ax \supset Bx\}$’ logically implies ‘$(x)\ Ax \supset Bx$’.* From this point of view, the difference between Messrs. E and C with respect to the second {273} of the above three contentions is simply *which* 'qualifications' are the right ones. And from this point of view, the outcome of the controversy hinges, therefore, on the third contention.

63. How are we to come to grips with it? As far as I can see, the best way is to begin by noting that Mr. C is, by implication, granting what is surely the case, namely that *in the absence of the contextual implications which he has introduced,* '$(x)\ Ax \supset Bx$' *pure and simple* cannot with any plausibility be claimed to authorize either 'If anything *were* A, it *would be* B,' or 'If x_1 *were* A, it *would be* B,' though it clearly would authorize subjunctive conditionals which might easily be confused with the above, namely

If anything *were* identical with one of the things which *are* (have been or will be) A, it would be B

and

If x *were* identical with one of the things which *are* (have been or will be) A, it would be B.

The principle which relates the latter conditionals to '$(x)\ Ax \supset B(x)$' is the Leibnitz-Russell principle

$$x = y \cdot \supset \cdot (\Phi)\ \Phi x \supset \Phi y$$

a principle which authorizes such reasonings as

Tom is tall
So, if I were Tom, I would be tall

64. It can readily be seen that where K is a class given by a listing of its members, thus

$$(x)\ x \in K \ =_{Df}\ x = x_1 \vee x = x_2 \vee \ldots x = x_n$$

and where the function 'Φx' has the definition

$$\Phi x \ =_{Df}\ \Phi' x \cdot x \in K$$

where, in other words, the class of the things which are Φ contains a reference to a denotatively specified set of individuals, the proposition

$$(x)\ \Phi x \supset \Psi x$$

while it does, indeed, authorize the subjunctive conditional

If anything were Φ, it would be Ψ

it does so only because the latter has the sense of

{274} If anything *were* identical with one of the things which *are* Φ, it would be Ψ.

It is tempting to conclude with Popper(14) that where the class of Φ is an *open* class, i.e. a class which is not defined in terms of a denotatively specified set of individuals, then

$(x)\ \Phi x \supset \Psi x$

authorizes not simply

If anything *were* identical with one of the things which *are* (have been or will be) Φ, it would be Ψ.

but

If anything *were* Φ, it *would be* Ψ

as well.

65. But how are we to test the claim that 'A causes B' can be construed as '$(x)\ Ax \supset Bx$' with or without the stipulation that the former contextually implies that the speaker has inductive grounds for his statement? The proper move would seem to be a thought experiment. Thus, Mr. C's claim amounts to the idea that it would be a 'howler' to affirm that $(x)\ Ax \supset Bx$, implying, in so doing, that one has good inductive grounds for making the statement, while denying in the same breath that if *anything were A it would be B.* If, therefore, we can construct in imagination a situation in which it would be quite proper to do this, the claim will have been refuted.

Suppose that we had good grounds for believing that all planets revolving around a central sun rotate in the same direction as they revolve, provided that the planets and the sun around which they revolve have had a common origin. *Suppose* also that we had good grounds to believe that it was extremely improbable that a body could join a solar system from outside without gravitating directly to the sun; in short that it was extremely unlikely that a planet could have originated from outside. *Suppose,* however, that we had good grounds for believing that a material object of whatever size which revolved around another object, rotating as it does so, *need not* rotate in the same direction as it revolves. *Suppose*, finally, that no planet has ever been observed to rotate in a direction other than that in which it revolves.

Given all the above suppositions, and postponing for the moment any question as to their legitimacy and mutual consistency, would we not be in a position to say

(In all probability) all planets *in point of (contingent) fact* rotate in the same direction as they revolve

{275} but deny that

(In all probability) if anything *were* a planet, it *would* rotate in the same direction as it revolves?

that is, in the language of the controversy, with 'Px' for 'x is a planet' and 'Rx' for 'x rotates in the same direction as it revolves', to affirm

(In all probability) (x) Px ⊃ Rx

but reject

(In all probability) (x) if it *were* the case that Px, it *would be* the case that Rx?

though not, of course, reject

(In all probability) if anything *were* identical with one of the planets which *have existed or ever will exist*, it would rotate in the same direction as it revolves.

On the other hand, since one of our suppositions is that we have good grounds for believing it to be extremely improbable that a body could come into a solar system without gravitating directly to the sun, we *could* say (could we not?) that

(In all probability) *under the circumstances which (in all probability) obtain*, i.e. that all planets have originated and will originate within their solar systems, if anything *were* a planet, it *would* rotate in the same direction as it revolves

for this mobilizes the two ideas (1) that

(In all probability) all planets *which originate within their solar system* rotate *of physical necessity* in the same direction as they revolve

in a way which is analogous to that in which

[Since $(p \supset q)$ is the case] if p were *also* the case, r would be the case

mobilizes '$p \supset q$' and '$(p \cdot q) < r$'.

66. We shall shortly see that things are not quite so simple; indeed that our thought experiment simply will not do as it stands, for reasons which the reader may already have spotted. But let us work with it for a moment before submitting it to criticism, as it will enable us to bring into our argument themes from many current discussions of the distinction between 'necessary' and 'accidental' constant conjunctions.

67. There are, however, two difficulties, one minor, one of major import, which should at least be looked at before we continue. To take the minor point first, it is clear that if the only way in which {276} we could come to know that

$$\{(\exists x)\ Px \cdot {\sim}Rx\}$$

i.e., 'It is physically possible that $(\exists x)\ Px \cdot {\sim}Rx$', were by inferring this from

$$(\exists x)\ Px \cdot {\sim}Rx$$

we could never be in a position to say both

$$(x)\ Px \supset Rx$$

and

$$\{(\exists x)\ Px \cdot {\sim}Rx\}$$

Evidence which would warrant the affirmation of a constant conjunction would *ipso facto* warrant the denial that exceptions were physically possible; and the idea that '

$\{(x)\ Ax \supset Bx\}$' had the force of '(In all probability) $(x)\ Ax \supset Bx$' would have survived our thought experiment for the simple reason that the latter would be internally inconsistent.

In general, if

(In all probability) $(x)\ Ax \supset Bx$

had the form

Observed As have, without exception, been B
So, (in all probability) $(x)\ Ax \supset Bx$

and

(In all probability) $\{(\exists x)\ Ax \cdot {\sim}Bx\}$

had the form

$(\exists x)\ Ax \cdot {\sim}Bx$

So, $\{(\exists x)\ Ax \cdot {\sim}Bx\}$

one could not simultaneously have inductive reasons for affirming both a constant conjunction and the (physical) possibility of an exception.

68. On the other hand, to mention a point of major significance it must be admitted that where 'A' and 'B' are *unlocalized* predicates, predicates, that is, which do not contain covert references to particular times, places or things, there are, to put it mildly, serious difficulties about the idea that one could have reason to assert both

$\{(\exists x)\ Ax \cdot {\sim}Bx\}$

and

${\sim}(\exists x)\ Ax \cdot {\sim}Bx$

{277} Nor do these difficulties depend on the idea that, in general,

(In all probability) $\{(\exists x)\ Ax \cdot {\sim}Bx\}$

has the form

$(\exists x)\ Ax \cdot {\sim}Bx$

So, $\{(\exists x)\ Ax \cdot {\sim}Bx\}$

For the time being I shall simply look these difficulties in the face and walk on, as I shall also do in the case of the related problem of how the supposition (whatever its analysis) that 'It is extremely likely that a body entering a solar system from without would gravitate directly to the sun' where this does *not* mean 'It is inductively probable that *all* bodies entering a solar system from without would gravitate directly to the sun' could authorize

(In all probability) *all* planets have *as a contingent matter of fact* originated within their solar system, and will continue to do so

as contrasted with

(In all probability) *most* planets have originated (or will originate) within their solar system.

69. Let us suppose, therefore, that Mr. C does not, for the moment, seize on these weaknesses in our thought experiment. Let us suppose, indeed, that, for the moment, he finds our thought experiment convincing, and abandons the simple idea that

$(x)\ Ax \supset Bx$

has the force of

(In all probability) $(x)\ Ax \supset Bx$.

It would be a mistake, however, to conclude that having left this bastion, he has no option but to join forces with an already chastened Mr. E. Indeed, he has one more qualification up his sleeve which might do the trick without amounting to full surrender.

What this qualification might be is suggested by the argument of the preceding section. After all, even if the use of modal language in connection with empirical phenomena arouses one's philosophical anxieties, the fact remains that in our unperplexed moments we *do* speak of this or that as a 'necessary consequence' of something else, as 'physically possible,' etc. It is only sensible, therefore, to look to the inter-relationships of modal expressions for a clue to the presuppositions of lawlike {278} statements. Of these inter-relationships, the following are the most promising:

(1) $\{(\exists x)\ Ax \cdot {\sim}Bx\} : < : {\sim} \quad \{(x)\ Ax \supset Bx\}$

(2) $\{(x)\ Ax \cdot Cx : \supset {\sim}Bx\}$

$: < : \quad \{(\exists x)\ Ax \cdot Cx \cdot {\sim}Bx\}$

$: < : \quad \{(\exists x)\ Ax \cdot {\sim}Bx\}$

$: < : {\sim} \quad \{(x)\ Ax \supset Bx\}$

(1) immediately suggests the idea that to assert that (in all probability) A causes B is to imply that one has good inductive reason for claiming that

(In all probability) $(x)\ Ax \supset Bx$

and does *not* have good inductive reason for claiming that

(In all probability) $\{(\exists x)\ Ax \cdot {\sim}Bx\}$.

It correspondingly suggests that to assert that (in all probability) A is constantly conjoined with B, but deny that A causes B, is to imply that although one has good inductive ground for claiming that

(In all probability) $(x)\ Ax \supset Bx$

one *also* has good reason for claiming that

(In all probability) $\{(\exists x)\ Ax \cdot {\sim}Bx\}$

But this suggestion is clearly of little use to Mr. C as it stands, for by explaining the claim that the constant conjunction between A and B is or is not *necessary* in terms of the idea that exceptions are or are not *possible*, it does nothing to ease his anxieties about the causal modalities generally. On the other hand, if we replace, in each case,

'(in all probability) $\{(\exists x)\ Ax \cdot {\sim}Bx\}$' by '(in all probability) $(\exists x)\ Ax \cdot {\sim}Bx$', on the ground that if it is true that

$$\{(\exists x)\ Ax \cdot {\sim}Bx\} : < : {\sim} \quad \{(x)\ Ax \supset Bx\}$$

((1) above) it is equally true that

$$(\exists x)\ Ax \cdot {\sim}Bx : < : {\sim} \quad (x)\ Ax \supset Bx$$

the suggestion, thus modified, fails to accomplish its mission; for to assert that (in all probability) A has B for its constant companion, but deny that A causes B obviously cannot be to imply that there is sufficient inductive reason to warrant the assertion of *both* the constant conjunction *and the existence of exceptions.* While to assert that A causes B is to deny not just the *existence* of exceptions, but the (physical) *possibility* of exceptions.

{279} 70. The second relationship, (2), is more promising. This time the suggestion is the twofold one that (a) to assert that (in all probability) A is constantly conjoined with, but does not cause, B, is to imply that while one has good inductive reasons for claiming that

(In all probability) (x) Ax ⊃ Bx

one knows about a character C such that one is entitled to say

(In all probability) (x) Ax · Cx : ⊃ ~Bx

though not, of course, on the grounds that one has observation knowledge to the effect that

(∃x) Ax · Cx · ~Bx

It correspondingly suggests (b) that to assert that (in all probability) A causes B is to imply that one has good inductive grounds for claiming that (in all probability) (x) Ax ⊃ Bx, and knows of no characteristic C, such that one is entitled to say

(In all probability) (x) Ax · Cx : ⊃ ~Bx.

71. Now the first part, (a), of this suggestion involves the obvious difficulty that since, *ex hypothesi,* A and B are *constantly* (if contingently) conjoined, the generalization which, according to it, is contextually implied, namely, '(in all probability) (x) Ax · Cx : ⊃ ~Bx', is implied to be *vacuously* true. But if this implied generalization concerning the characteristic C amounts, simply, to

(In all probability) ~(∃x) Ax · Cx · Bx

it is difficult to see how the idea that 'A is constantly, but contingently, conjoined with B' implies that we know of a characteristic C of which *this* is true, and illuminates the force of the assertion; while if we replace '(in all probability) (x) Ax · Cx : ⊃ ~Bx' by ‘(in all probability) {(x) Ax · Cx : ⊃ ~Bx}’ we are once again explicating the force of one modal expression (‘(physically) possible’) by another (‘(physically) necessary’).

72. As for part (b) of the suggestion, I shall limit myself to pointing out that when we claim that A is not merely *constantly conjoined* with, but *causes,* B, we seem to be implying not simply that *we know of* no characteristic C such that we are entitled to say that

(In all probability) (x) Ax · Cx : ⊃ ~Bx

(though we are, of course, implying *at least* this) but that *there is no {280} such characteristic.* I limit myself to this remark because the discussion initiated by our ‘thought experiment’ has served its purpose—which was not, it will be remembered, to *prove* anything, but to introduce certain themes, and it is time to approach our problem from new directions.

73. Let us leave Mr. C for a moment and return to our chastened Mr. E, a Mr. E who is willing, indeed, eager, to go as far as he can to meet his critics; if only he can preserve the ‘core’—however small it may be—of his original position. Instead of continuing to press him back step by step, however, I shall present him without further ado in his ‘last ditch.’ As a matter of fact, I shall begin by having him retreat *too* far, as, indeed, he might be expected to do under the momentum of the argument.

74. Like Mr. C, he now seeks to illuminate the idea that A causes B in terms of the contextual implications of causal statements. Unlike Mr. C, however, he interprets the significance of the causal modalities *not* in terms of the reasoning

I.

. . .
So, (in all probability) (x) Ax ⊃ Bx

II.

(x) Ax ⊃ Bx
Ax
So, (of *logical* necessity) Bx

but in terms of the reasoning

I′.

. . .
So, (in all probability) if anything *were* A it *would be* B

II′.

Ax_1
So, (of *physical* necessity) Bx_1

In short, he takes the causal modalities at their face value.

On the other hand, while he takes ‘being A *physically* necessitates (entails) being B’ as having the same sort of job as ‘being C logically necessitates (entails) being D’, he is no longer moved to say *in either case* that ‘necessity is a relation between properties.’ Not that he is moved to deny this; for he is well rehearsed in the rubric “You can say that . . . is a --- if you like, but ***”; and he has learned that insight may be divided from itself in such clashes as those between ‘There are abstract entities,’ ‘There is a unique meaning-relation of aboutness or reference,’ and ‘Obligation is a unique togetherness of a person, a situation, a possible action’ on the one hand, and ‘There really {281} are no such things’ on the other. He has also learned that to pull this insight together, it is necessary to explore the way in which many dimensions of discourse come together in the use of expressions ‘under analysis.’ (This depth-psychological expression is, as frequently noted, a useful metaphor; a philosophical analysis is barely under way when ‘surface’ techniques of definition—a family which includes ‘definition in use’—have bee brought to bear on the problem.)

75. In keeping with the above, Mr. E beings his last ditch stand by saying that the statement ‘Being A physically entails being B’ (which we shall abbreviate, from now on, to ‘A P-entails B’) contextually implies that the speaker feels himself entitled to infer that something is B, given that it is A.

76. But why, it may be asked, does he say ‘feels,’ and not that . . . the speaker *believes* himself entitled to infer that something is B, given that it is A? And could we not extort from him the ‘admission’ that the speaker believes this *because* he believes that A P-entails B? If so, the enterprise is doomed from the start.

77. Now, if Mr. E were to replace ‘feels’ by ‘believes’, he would, of course, have to point out that ‘believes’ in this context does not imply ‘does not know’. For if it did, he would be back on the road to making physical entailments the sort of thing one ‘believes is there’ but cannot *know*. On the other hand, it would be misleading to say that the statement ‘A P-entails B’ contextually implies that the speaker *knows* that he is entitled to infer that something is B, given that it is A, not only because ‘knows’ implies, among other things, that we would agree with the statement—which could be gotten around by putting it in inverted commas as representing something the speaker would claim—but

because, as Cook Wilson emphasized, an unqualified statement may express a mere 'thinking without question' that something is the case. And even if the statement '(*In all probability*) A P-entails B' could correctly be said to imply that the speaker knows (or 'knows') that he is entitled to infer that something is B, given that it is A, the same could scarcely be said of '(*Probably*) A P-entails B' and '(*It is more probable than not*) that A P-entails B.'

78. Suppose, then, Mr. E were to replace 'feels' by 'thinks'. Could he be forced into the stultifying 'admission' that if the speaker thinks himself entitled to infer that something is B, given that it is A, it is *because* he thinks that A P-entails B? Mr. E is quite unmoved by this line of {282} questioning, for he has up this sleeve the idea that thinking oneself entitled to draw this inference is somehow *the same thing* as thinking that A P-entails B—though this does not mean, he hastens to add, that one can simply *equate* 'A P-entails B' with 'One is entitled to infer that something is B, given that it is A.'

In any event, what Mr. E actually said was that 'A P-entails B' contextually implies that the speaker *feels* himself entitled to infer that something is B, given that it is A. And what he has in mind is the idea, to be found in many recent discussions of prescriptive discourse, that instead of the *feeling* that one ought to do A consisting of a *thinking* that one ought to do A *plus* a tendency to be moved to do A from this thought, the thinking that one ought to do A is a way of being moved to do A. Not, as early emotivism had it, because *thinking that one ought to do A* isn't really *thinking*, but *merely* a matter of having a 'pro-attitude' towards the doing of A (or an 'anti-attitude' towards the not-doing of A), but because 'to have the concept of obligation' is *both* to *reason* from and to the idea that people ought to do certain things, and to *intend* that they do them. And it is both of these *not* because it is each of them separately, but because *thinking* that people ought to do certain things *is intending that they do them*, where the intentions embody a certain pattern of *reasoning*.*

79. Now, once it is granted—and the point cannot be argued here—that empiricism in moral philosophy is compatible with the recognition that 'ought' has as distinguished a role in discourse as descriptive and logical terms, in particular that we *reason* rather than 'reason' concerning *ought,* and once the tautology 'The world is described by descriptive concepts' is freed from the idea that the business of all non-logical concepts is to describe, the way is clear to an *ungrudging* recognition that many expressions which empiricists have relegated to second-class citizenship in discourse, are not *inferior,* just *different.*

Clearly, to use the term 'ought' is to prescribe rather than describe. The naturalistic 'thesis' that the world, including the verbal behavior of those who use the term 'ought'—and the mental states involving the concept to which this word gives expression—can, 'in principle,' be described {283} without using the term 'ought' or any other prescriptive expression, is a logical point about what is to count as a description *in principle* of the world. For, whereas in ordinary discourse to state what something *is,* to describe something as Φ (e.g., a person as a criminal) does not preclude the possibility that an 'unpacking' of the description would involve the use of the term 'ought' or some other prescriptive expression, naturalism presents us with the ideal of a *pure* description of the world (in particular of human behavior), a description which simply says what things *are,* and never, in any respect, what they *ought* or *ought not* to be; and it is clear (as a matter of simple logic) that neither 'ought' nor any other prescriptive expression *could* be *used* (as opposed to *mentioned*) in such a description.

80. An essentially similar point can be made about modal expressions. To make first hand use of these expressions is to be about the business of *explaining* a state of affairs, or *justifying* an assertion. Thus, even if to state that p entails q is, in a legitimate sense, to state that something is the case, the primary use of 'p entails q' is not to state that something is the case, but to explain *why q,* or justify the assertion *that q.* The idea that the world can, in principle, be so described that the description contains no modal expression is of a piece with the idea that the world can, in principle, be so described that the description contains no prescriptive expression. For what is being called to mind is the ideal of a statement of 'everything that is the case' which, however, serves, *through and through, only* the purpose of stating what is the case. And it is a logical truth that such a description, however many modal expressions might properly be used in *arriving* at it, or in *justifying* it, or in showing the *relevance* of one of its components to another, could *contain* no modal expression.

81. It is sometimes thought that modal statements do not describe states of affairs in the world, because they are *really* metalinguistic. This won't do at all if it is meant that instead of describing states of affairs in the world, they describe linguistic habits. It is more plausible if it is meant that statements involving modal terms have the force of *prescriptive*statements about the use of certain expressions in the object language. Yet there is more than one way to *'have the force of'* a statement, and a failure to distinguish between them may snowball into serious confusion as wider implications are drawn. Is 'p entails q' *really* the same as a statement to the effect, say, that one ought not to commit {284} oneself to 'not-q' unless one abandons 'p'? But one can know that Turks, for example, ought to withdraw '. . .' when they commit themselves to '---' without knowing the language, whereas the statement 'p entails q' contextually implies that the speaker not only knows the language to which 'p' and 'q' belong, but, in particular, knows how to use 'p' and 'q' themselves. A related point may illuminate the situation. The semantical statement

'Rot' (in German) means *red*

is not *really* the statement

'Rot' (in German) translates into 'red' (in English)

for the simple reason that whereas both statements are in English, and hence cannot be made without knowing English, the former cannot properly be made unless the speaker understands not only English, but in particular, the English word 'red'—and presupposes that the person spoken to does also—whereas the latter can quite properly be made by—and to—an English-speaking person who does not know how to use the word 'red'. One is tempted to put this by saying that in the semantical statement 'red' is functioning both as 'red' and as ''red''. But when one reflects on the fact that it translates into French as

'Rot' (en Allemand) veut dire *rouge*

one wonders if it is helpful to say that it *mentions* the English word 'red' at all.

Shall we say that modal expressions are metalinguistic? Neither a simple 'yes' nor a simple 'no' will do. As a matter of fact, once the above considerations are given their proper weight, it is possible to acknowledge that the idea that they are metalinguistic in character over-simplifies a fundamental insight. For our present purposes, it is sufficient to say that the claim that modal expressions are 'in the metalanguage' is not too misleading if the peculiar force of the expressions which occur alongside them (represented by the 'p' and the 'q' of our example) is recognized, in particular that they have a 'straightforward' translation into other languages, and if it is also recognized that they belong not only 'in the meta*language,*' but in discourse about *thoughts* and *concepts* as well.*

82. The above remarks may serve to reconcile us somewhat to Mr. E's suggestion that 'thinking (or feeling) oneself entitled to infer that something is B, given that it is A' is *somehow* the same thing as 'thinking that A P-entails B.' "Well, then," it may be said, "if all Mr. E wishes to claim is that on occasion we 'know' ourselves 'entitled to infer' that something is B, given that it is A, what is all the fuss about?" *The fuss is about the inverted commas around 'know' and 'entitled to infer'.*

We have learned the hard way that the core truth of 'emotivism' is not only compatible with, but absurd without, *ungrudging* recognition of the fact, so properly stressed (if mis-assimilated to the model of describing) by 'ethical rationalists,' that ethical discourse as *ethical discourse* is a mode of rational discourse. It is my purpose to argue that the core truth of Hume's philosophy of causation is not only compatible with, but absurd without, *ungrudging* recognition of those features of causal discourse as a mode of rational discourse on which the 'metaphysical rationalists' laid such stress but also mis-assimilated to describing.

IV. Toward a Theory of the 'Causal' Modalities

83. It is in the spirit of the concluding paragraphs of Part III that Mr. E now suggests that the distinction between 'A is constantly conjoined with, but does not cause, B' and 'A causes B' is to be interpreted in terms of the idea that statements of the second form imply that *simply from the fact that something is A* one is entitled to infer that it is B, whereas statements of the first form, whatever else they may do, imply that one is *not* entitled to infer *simply from the fact that something is A* that it is also B. He insists, of course, that the idea that one is entitled to infer that something is B *simply* from the fact that it is A is *exactly that,* and not 'at bottom,' as Mr. C would have it, the idea of the enthymeme obtained from

$(x)\ Ax \supset Bx$
Ax_1
So, Bx_1

by 'suppressing' the major premise even with the proviso that the major premise has been accepted on inductive grounds, and, perhaps, that it {286} is related in certain ways to other inductively supported propositions of this or related forms. In short, Mr. E insists that the 'season inference ticket'

If anything *were* A, it *would be* B

is actually an *inference ticket,* and not, so to speak, a *letter of credit* certifying that one has a major premise and a *formal* inference ticket at home. And this means that he conceives of induction not in terms of the model

. . .
So, (in all probability) $(x)\ Ax \supset Bx$

but, rather, the model

. . .
So, (in all probability) if anything *were* A, it *would be* B.

He conceives of induction as establishing principles *in accordance with which* we reason, rather than major premises *from which* we reason.

84. It is a simple consequence of this analysis that Mr. E finds a looser connection between the statement ‘A causes B’ and the idea that there is good inductive reason to suppose that A and B are constantly conjoined than did Mr. C in the later stages of his argument. For, although ‘If anything *were* A, it *would be* B’ is the sort of thing which is properly justified by inductive argument, it is, on the entailment view, even in its least metaphysical form, an oversimplification to say that the claim to have been reached by inductive reasoning is part of the force of the statement ‘A causes B.’

As a parallel Mr. E might offer the fact that although moral principles are indeed the sort of thing that is properly justified by relating their espousal to the general welfare, it would be most misleading to say that we simply *mean* by ‘If one has promised to do something, then, *ceteris paribus,* one ought to do it’ something that has this sort of justification. Not that it would be ‘sheer error’ to say this, for it points to an important truth; the trouble is that it gives too simple a picture of the reasonableness of the appeal to general welfare in morals.

85. It is now high time that I dropped the persona of Mr. E, and set about replying to the challenge with which Mr. C ended his first critique of the entailment theory. This challenge, which Mr. E has simply ignored, presents the view I propose to defend with its crucial test. “How,” it runs, “unless we view causal arguments as enthymemes, {287} can the terms ‘implies’, ‘necessitates’, ‘impossible’, etc., with which they are sprinkled, have other than a *metaphorical* use in these contexts, in view of the fact that the paradigm cases in which these expressions occur are cases in which to know entailments is simply to understand the language by which they are expressed?”

86. But, to begin with, even in the case of ‘analytic inference’ the situation is not quite as simple as this challenge supposes. For it would seem quite possible to understand an entailment statement in mathematical discourse without knowing its truth value. To this the reply would presumably be that to understand the meaning of mathematical expressions is, in part, to know certain procedures; and that while one can indeed understand a given segment of mathematical discourse with- out knowing all the entailments which one might come to know by following the appropriate procedures, one comes to know such entailments as one does come to know simply by virtue of the fact that one has understood the expressions involved, i.e., has known (and followed) these procedures. If this *is* the reply, then the challenge would appear to consist, at bottom, of the idea that the paradigm cases of entailment are cases to which *armchair* procedures are appropriate.

Thus understood, the challenge is met by *drawing a distinction* and by *reflecting,* in the spirit of the later Wittgenstein, *on the idea of 'meaning.'* The distinction is between the *antecedent* 'meanings' of 'A' and 'B' in terms of which one formulates the evidence which points to a certain inductive 'conclusion' (actually the decision to espouse the inference ticket 'If anything were A, it would be B') and what one *subsequently* 'understands' by these terms when one uses them in accordance with this decision. The point of this distinction is that while one does not inductively establish that A P-entails B by armchair reflection on the *antecedent* 'meanings' of 'A' and 'B', to establish by induction that A P-entails B is to *enrich* (and, perhaps, otherwise modify) the use of these terms in such wise that to 'understand' what one now 'means' by 'A' and 'B' is to know that A P-entails B.

If to establish by induction that A causes B were to establish that (in all probability) (x) Ax ⊃ Bx, perhaps as a member of a set of inductive conclusions, there would be little reason to say that to establish by induction that A causes B is to decide on empirical grounds to *give a new use* to 'A' and 'B'. If, however, it is, as I am arguing, a matter of deciding to adopt a new principle of inference, then there is every {288} reason to say that to establish by induction that A causes B is to modify the use of 'A' and 'B' and, indeed, to modify it in such a way that these terms can properly be said to have acquired a new 'meaning.'

Here two warnings are in order: First, the new 'meanings' do not involve a change in explicit definition. B has not, in *this* sense become 'part of the meaning of' 'A'. Yet the new role played by 'B' and 'A' does warrant the statement that the 'meaning' of 'A' involves the 'meaning' of 'B'; for they are now 'internally related' in a way in which they were not before.

Second, the relation between the new and the old 'meanings' of 'A' and 'B' is a *logical* rather than a purely historical one; as long, that is, as the espousal of this new inference ticket retains its character as a scientific decision. For in spite of the fact that in science as in life "You can't go home again," and one never quite returns to old 'meanings' (though the historian of science can unearth them), scientific terms have, as part of their logic a 'line of retreat' as well as a 'plan of advance'—a fact which makes meaningful the claim that in an important sense A and B are the 'same' properties they were 'before.' And it is this strategic dimension of the use of scientific terms which makes possible the *reasoned* recognition of what Aldrich (2) has perceptively called "renegade instances," and gives inductive conclusions, in spite of the fact that, as principles of inference, they relate to the very 'meaning' of scientific terms, a corrigibility which is a matter of 'retreat to prepared positions' rather than an irrational 'rout.' The motto of the age of science might well be: *Natural philosophers have hitherto sought to understand 'meanings'; the task is to change them.*

87. If the above argument is successful, it explains how one can grant that 'knowing that A P-entails B' has the connection with 'understanding the meaning of "A" and "B" ' which Mr. C requires of 'knowing that one state of affairs entails another,' without being committed to the absurdities of classical rationalism. In doing so it explains why one feels uncomfortable about saying that one *sees* that one state of affairs P-entails another. For the metaphor of *seeing* and *coming to see,* taken from a paradigm case of *non-inferential* knowledge, is clearly inappropriate where, instead of exploring implications within a *status quo* of 'meanings,' one is reasoning one's way into a decision to change the *status quo.* (As a matter of fact, we have recently come to appreciate that the extent to which even the discovery of new logico-mathematical {289} entailments is a matter of working within the framework of antecedent usage was greatly exaggerated by classical philosophies of mathematics.)

But if it is inappropriate to speak, in scientific contexts, of *seeing* physical entailments, it by no means follows that it is inappropriate to speak of *knowing* that one state of affairs physically entails or necessitates another. And it is the fact, to which we have already called attention, that empirically minded philosophers who have developed interpretations of causality along entailment lines (among others, C. D. Broad and W. C. Kneale) have taken for granted that entailments which can't be *'seen'* can't be *known* which has given their views such an air of paradox and put them unnecessarily on the defensive. There is, indeed, an implicit contradiction in the idea of explaining scientific reasoning in terms of un*know*able relations of entailment which one *opines* to be *there.* Mr. C was certainly entitled to expostulate, 'If *our* science doesn't require us to know them, why put them there to be known by the angels? It is, as I see it, a primary virtue of the account we have been developing, that it lacks this unwelcome feature of classical entailment interpretations of causality.

88. It is high time that we reminded ourselves that our controversy between Messrs. E and C has been carried on in a very rarified atmosphere. Among other things, it has been formulated in terms of a supposed distinction between physically contingent and physically necessary constant conjunctions, where both are *truly universal* in scope; i.e. contain no reference, overt or covert, to particular places, times or objects. In a certain sense, this was unavoidable. The initial stages, at least, of an attempt to resolve a classical controversy must take 'theses' and 'antitheses' as it finds them, and not change the subject. Yet the story is not complete until some effort has been made to tie these abstractly formulated issues to the features of actual discourse, scientific and pre-scientific, which gave rise to them. But before discussing the 'universality,' in the above sense, of scientific generalizations, I shall touch all too briefly on a topic which, though at first sight it serves only to *supplement* our account of inductive inference, will actually lead us to the more penetrating account of lawlike statements as material rules of inference.

I have in mind the problem of interpreting those inductions of which the conclusion, instead of having the form '(In all probability) All As are B,' have instead the form '(In all probability) the proportion of {290} As which are B is n/m.' For, it may well be asked, unless you are going to interpret all 'statistical' inductive conclusions (in a suitably broad sense of 'statistical') as belonging to the category of 'accidental,' as opposed to 'nomological' generalizations, you face the problem of explaining how statistical nomologicals can be interpreted as physical entailments or analogous to physical entailments.

89. Can we find anything in the field of statistical induction which resembles

. . .
So, (in all probability) A P-entails B [i.e. that something is B may reasonably be inferred, given that it is A]?

The obvious suggestion is

n/m of observed As have been (found to be) B
So, (in all probability) K is a restricted class of As P-entails that n/m of its members are B* [i.e. that n/m of the members of K are B may reasonably be inferred, given that K is a restricted class of As].

(By speaking of K as a 'restricted' class of As, I mean that K is a finite class of As the membership of which is specified in terms of a place and/or a time and/or an individual thing.)

There are many respects in which this formulation would have to be tidied up and qualified to meet the demands of a theory of primary statistical induction. In so far as they are of a kind which must be taken into account by any such theory, I can safely leave them to the reader. There is, however, one objection which is, at first sight, fatal to the above suggestion. Suppose that of *m* hitherto observed cases of A, *n* have been found to be B. According to the theory under consideration, we argue

. . .
So, (in all probability) that K is restricted class of As P-entails that n/m of the members of K are B.

Suppose, however, that we subsequently examine a new restricted class of A, K_1, and find that n′/m′ of the members, m′ in number, of K_1 {291} are B. We then reason, according to the theory, and taking into account this new evidence,

$n + n' / m + m'$ of the observed cases of A have been B
So, (in all probability) K is a restricted class of As P-entails that $n + n' / m + m'$ of the members of K are B.

But have we not just examined a restricted class of As, namely K_1, and found the proportion of Bs to be, *not* $n + n' / m + m'$, but, rather, n' / m'? How can it be reasonable to say that

(In all probability) that K is a restricted class of As P-entails that $n + n' / m + m'$ of the members of K are B

when we know that *not* $n + n' / m + m'$, but n' / m', of the members of K_1, which we have just examined, are B? After all, can we reasonably say that p entails q, when we know that p is true and q false?

90. Before we attempt to answer this objection, let us note that if it *could* be answered, and if the above account is essentially correct, then statistical induction, like the induction of logically universal laws, would yield principles *in accordance with which* we would move from *empirical* proposition to *empirical* proposition. In other words, we would not be faced with the problem of how to get from 'limits of frequencies' in an infinite reference class, or even from frequencies in classes which are stipulated to be finite but of unspecified numerosity, to empirical propositions concerning the ratio with which a quaesitum property occurs in a restricted class of objects having the reference property. For the view we are proposing so formulates the conclusions of primary statistical inductions that they concern the inference of finite, restricted empirical matter of fact from finite, restricted empirical matter of fact.

91. But what of the objection? We must begin by granting that if the conclusions of primary statistical induction were of the form

. . .
So, (in all probability) that K is *any* restricted class of As P-entails that n/m of its members are B

the objection would be unanswerable. When, however, we reflect that the *point* of induction is to give us a rational grip on *unobserved* or, better, in a suitably broad sense, *unexamined* cases, a ray of hope appears. Suppose, then, we try

. . .
So, (in all probability) that K is any *unexamined* restricted class of As P-entails that n/m of its members are B.

{292} At first sight this formulation is open to the objection that it makes a physical entailment of the fact of being a restricted class, K, of As *hinge on K's being unexamined.* This strikes us, at first sight, as of a piece with saying that causal laws are of the form '*unexamined* As cause B.' Actually, however, the situation is not as bad as it looks, for the first two parts of this essay have made it abundantly clear that the word 'cause' as actually used has a meaning which is *not* captured *in toto* by the notion of physical entailment. Thus, if we have permitted ourselves in these last two sections to use such locutions as 'A causes B' and to treat them as equivalent (from Mr. E's standpoint) to 'A physically entails B,' this has been dialectical license. Once, however, we bear in mind that not even in the conclusion of non-statistical inductions, as we have formulated them, is 'physically entails' intended to have the force of 'causes', but to have the force, roughly, of 'entails in a way which essentially involves the specific empirical subject matter to which reference is made', the above paradox vanishes, as is readily seen if, instead of the above, we write

. . .
So, that n/m of the members of K are B *may reasonably be inferred,* given that K is an unexamined restricted class of As.

And, indeed, concerning what sort of classes does one wish to draw *inferences,* if not *unexamined* ones, unexamined, that is, in the respect under consideration?

Statements of the form 'The *chance* that x_1 is B is n/m' imply that the individual in question, whether a thing (e.g. a planet, a marble) or an event (e.g. an election, the drawing of a marble), is being considered as a member of a restricted class K of, say, As, where there is reason to think (perhaps because one has examined them, perhaps by the 'use' of a statistical inference ticket) that the proportion of Ks which are B is n/m. The statistical inference ticket itself, namely 'That the proportion of Bs in K is n/m can reasonably be inferred, given that K is an unexamined, restricted class of As' corresponds, therefore, to the statement '(In all probability) the chance that *an* A is B is n/m.'

That the proportion of Bs in a specific restricted class of As is n/m is, of course, either true or false as a matter of empirical fact. On the other hand, the question as to the applicability of the terms 'true' or 'false' to the conclusions of inductive inferences is considerably more complex. What can be said is that it is undoubtedly the fact that it {293} would at the very least be *misleading* to say of the conclusion of yesterday's carefully and correctly drawn inference that it was false, on the ground that negative evidence turned up this morning, which gives aid and comfort to the idea that the conclusions of carefully and correctly drawn inductive inferences are logically true, and hence to the conception that probability is a relation of the form *prob(p,e).* *

92. Whether or not this account of primary statistical induction would substantially survive all criticism, I think it has some merit. And if it is sound (in its general lines) it provides welcome reinforcement for the idea that the 'conclusions' of primary non-statistical inductions are decisions to espouse inference tickets. But more than this it provides us, as we shall see in a moment, with an important clue to the evaluation of the idea (so dear to Mr. C's heart) that the conclusions of primary non-statistical induction either *are* or *include* statements about all things, everywhere and every-when.

This can best be brought out by imagining the following objection to be brought against our treatment of Mr. C: "It is simply absurd to suppose that where 'A' and 'B' contain no overt or covert reference to particular times, places or individuals, one could have reason to suppose that

$$\{(\exists x)\ Ax \cdot {\sim}Bx\}$$

without having reason to suppose that

$(\exists x)\ Ax \cdot {\sim}Bx$.

After all, *all* things *everywhere* and *every-when* is a pretty large order, and there is an air of unreality about challenging Mr. C to explain the 'fact' that one could have reason, on occasion, to believe in physical possibilities that are *never* actualized. Take the example around which you built your 'thought experiment' (section 65). The idea was, if I understand it, that we might be entitled to believe that $(x)\ Px \supset Rx$ is a constant but (physically) contingent conjunction, because we might be entitled to explain this constant conjunction in terms of the *two* ideas, (1) that as *a (physically) contingent matter of fact* all planets have arisen and will arise from inside their solar system; and (2) that as a matter of *physical necessity* all inside-originating planets have the {294} property P. In other words, the idea was that we might be entitled to assert that planets which originate from outside, *although there happen to be none*, need not rotate as they revolve.

"But," the objection continues, "unless I am very much mistaken, the idea that we could be entitled to believe, on inductive grounds, *both* that

It is physically possible for planets to originate from outside their solar system

and that

Nowhere and no-when does any planet so originate simply won’t do. And if so, you have not really challenged Mr. C’s interpretation of ‘A causes (or physically necessitates) B’ as an *unrestricted* generalization of the form

Everywhere and every-when A is conjoined with B

as contextually implying that the speaker has inductive grounds for his assertion.”

93. We are now in a position to concede that this objection rests on a sound insight; one, however, which gives neither aid nor comfort to the constant conjunction interpretation of causality. The truth it sees, and which must be conceded, is that one, indeed, could not have inductive reason to assert both

(In all probability) $(x)\ Ax \supset Bx$

and

$\{(\exists x)\ Ax \cdot \sim Bx\}$

where the scope of the former statement is *unrestricted* in the sense indicated.* What point could there be to a distinction between statements {295} of unrestricted constant conjunction and lawlike statements, if one is never in a position to assert the physical possibility of exceptions, while denying that they actually occur?

94. Or, to put it somewhat differently, what point can there be to an extension to the universe as a whole (everywhere and every-when) of the distinction we can readily draw, in more restricted contexts, between uniformities which depend on the presence of an additional factor, and those which do not? After all, in evaluating the uniformities found in restricted contexts, we can point beyond them in certain cases, the ‘physically contingent cases,’ to the fact that the uniformity is absent when the circumstances are different. This procedure, however, makes no sense when extended to the universe as a whole, everywhere and every-when.

95. What shall we say? Actually, the clue to the answer is to be found in what might be thought to be the very strength of the objection. “This procedure makes no sense *when extended to the universe as a whole, everywhere and every-when.”* For Mr. C has been construing the primary induction of non-statistical laws to the effect that A causes B, as a limiting case of *coming to know that the members of a restricted class of As are, without exception, B;* the limiting case, that is, in which the relevant class is the class of all As in the universe as a whole, every-when and everywhere. This, however, if our argument to date is sound, is simply not so. He has been reinforced in this mistake by the idea that the primary induction of non-statistical laws is a limiting case of the

primary induction of statistical laws. But while *in a certain sense* this latter idea is quite correct, *it is so only it primary statistical induction is construed as we have been construing it.* These two points hang neatly together, for on our account the idea that the induction of non- statistical laws is a special case of statistical induction amounts to the idea that the former is to be represented by the schema

Observed As have been found, without exception, to be B
So, (in all probability) that K is an *unexamined, restricted* class of As P-entails that the proportion of Bs in K is 1.

or

. . .
So, that the proportion of Bs in K is unity can reasonably be inferred, given that K is an unexamined, restricted class of As.

And, of course, the difference between this case, and the case of primary {296} statistical induction is that if a hitherto unexamined restricted class, K_1, of As turns out, on examination, to include As which are not B, no future induction which includes the members of K_1 among its data can hope to establish once again that the proportion of Bs within an unexamined restricted class K can reasonably be inferred to be unity (as opposed to 'can reasonably be inferred to approximate unity' or 'can reasonably be inferred to be the closer to unity, the larger the membership of K'). It is in this sense, and in this sense only that new evidence is more threatening to non-statistical than to statistical primary induction.

96. To the objection that to take this line is to formulate causal laws in terms of what is entailed by the fact that K is an *unexamined* class of A's, whereas we surely think that (in general) the fact of being unobserved is irrelevant to the *effects* things have, it is sufficient to point out once again that we are analyzing not the specific force of 'causes', but the logical character of modal expressions in scientific explanation, and that to put 'P-entails' in the above context does not require us to say that 'A causes B on *condition that it is unobserved,'* but only that 'the idea that A P-implies B commits one to the reasonableness of the inference with respect to any *unexamined,* restricted class K of As, that its members are, without exception, B.'

97. Viewed in this light, our problem no longer appears as that of distinguishing within the class of statements of the form

$(x)\ Fx \supset Gx$

between those which formulate laws and those which formulate ‘accidental but unrestricted conjunctions.’ For if our interpretation of inductive reasoning is correct, it is just a mistake to suppose that lawlike statements *are* statements of the form ‘$(x)\ Fx \supset Gx$’ at all, *with or without contextual implications*. It is even misleading to say that they imply statements of this form, though we did not demur when Mr. C made this claim at an early stage of his argument. The relation between

A P-entails B

and the function

$\sim[x \text{ is } A \cdot \sim(x \text{ is } B)]$

which we are tempted to put by saying that

A P-entails B : $<$: $(x)\ Ax \supset Bx$

or by representing ‘A P-entails B’ by ‘ {$(x)\ Ax \supset Bx$}’, where, in each {297} case ‘$(x)\ Ax \supset Bx$’ formulates a statement about everything everywhere and every-when consists rather in the fact that our decision to espouse the inference ticket which is the lawlike statement, would be logically undermined were we to find *anywhere* or *anywhen* a restricted class of As, the members of which were not uniformly B.

My point, of course, is not that we couldn’t use ‘$(x)\ Ax \supset Bx$’ to represent lawlike statements. It is, rather, that given that we use this form to represent general statements which do not have a lawlike force, to use it *also* to represent lawlike statements is to imply that lawlike statements are a *special case* of non-lawlike statements. It is therefore particularly important to note that I am not claiming that all restricted generalizations, i.e. generalizations of which the subject term is a *localized* term, are unlawlike. It may, indeed, be true that all unrestricted generalizations *which we can have reason to assert* are lawlike. But not all lawlike statements which we can have reason to assert are unrestricted generalizations. And the logical form of even a restricted law-like generalization is obscured rather than clarified by representing it by ‘$(x)\ fx \supset gx$’.

98. It would be incorrect to say that any statement of the form ‘All . . . are ---’ is a mere abbreviation for a conjunction of singular statements. Yet some statements of this form, those which, as we informally say, commit themselves only to what is the case, are *in principle* completely confirmable by confirming each of a finite set of singular statements. This does not mean, of course, that to do the latter *is the same thing* as to

do the former; for, as was pointed out above, these ‘all-’ statements are not shorthand for the conjunction of these singular statements. To illustrate, we can in principle completely confirm such statements as ‘All the people in this room are tall’ (made on a particular occasion) by confirming each of a set of singular statements to the formulation of which the original statement is (in the circumstances in which it was made) a guide. But, once again, the conjunction of these singular statements does not have the force of the original statement.

Now it is such ‘all-’statements as these (let us call them *descriptive* ‘all-’statements) which authorize only ‘subjunctive identicals’ as contrasted with subjunctive conditionals proper. Thus, ‘All the people in this room are tall’ authorizes ‘If anyone were (identical with) one of the people who are in this room, he would be tall’; but not ‘If anyone {298} were in this room he would be tall.’ To be sure, if we knew that *being tall* was, for one reason or another, a necessary condition of getting into the room, then we would know that if anyone were in the room, he would be tall; but the correct way to imply that if anyone were in the room he would be tall is *not* by saying ‘All the people in this room are tall,’ but rather by saying ‘All the people in this room are, of necessity, tall.’ And to say this is not to claim that it is a law that being in the foam entails by itself being tall, but simply that the state of affairs asserted by the descriptive ‘all-’statement can be shown to be a necessary consequence of certain other facts about the situation.

The fact that descriptive ‘all-’statements can be *reasonably* believed to be true without complete confirmation—that they can, in a broad sense, be established by *inductive* reasoning—has given aid and comfort to the idea that lawlike statements are a special class of descriptive ‘all-’ statements. To have this idea, however, is, among other things, to be puzzled as to why, where our knowledge that all the men in this room are tall is, in a broad sense, ‘inductive,’ we can’t say ‘If anyone were in this room, he would be tall’ or ‘. . . it would be the case that he was tall.’ The explanation can’t consist in the fact that the ‘all-’statement is restricted in its scope to less than the whole universe, for statements can be so restricted and yet authorize subjunctive conditionals proper (as opposed to counter-identicals, e.g. ‘If anyone were identical with one of the persons who are in this room, he would be tall.’) Nor is the explanation that the reasoning which supports the statement is of the same sort as that by which we support lawlike statements, but that the grounds are *weaker,* not *strong enough* to support a lawlike statement. The explanation is rather that the ‘inductive’ reasoning—the ‘crime detection’ reasoning—by which we support restricted ‘all-’statements which are not lawlike is *not* the same sort of thing as the reasoning by which we support lawlike statements, as can be readily seen by comparing the reasoning by which, from a Holmesian variety of clues, we might establish that all the people who walked down a certain muddy road yesterday were farmers, with the following, dated 1500,

All observed free-floating needles in these latitudes which have been stroked by stone S have, without exception, pointed to the North
So, that the members of K will point to the North can reasonably be inferred given that K is an unexamined, restricted {299} class of free-floating needles which have been stroked by stone S.

To make the same point the other way around, if the reasoning by which a restricted 'all-'statement (e.g. 'All the people in that room are tall') has been established *were* of the form

All of the people in that room whose height has been ascertained have proved, without exception, to be tall
So, (in all probability) all the people in that room are tall

or *were* a straightforward derivation of this 'all-'statement by specification from a more general 'all-'statement which had been established by an argument of the above form, then 'All the people in that room are tall' *would be* the (misleading) formulation of a lawlike statement. For while lawlike statements may be accepted without reason, or reasons which do not have the form of an inductive argument in the narrow or 'primary' sense, all 'all-'statements which are accepted on inductive grounds in the narrow or 'primary' sense are, *however restricted in their scope they may be,* without exception lawlike. It is because we already know so much about people, rooms, size, etc. antecedent to raising the question 'Are all the people in the next room tall?' that we would reject the above argument as a bad one. And while the argument by which, in a given case, the statement 'All the people in that room are tall' is supported *might,* as we have seen, be such as to authorize 'If anyone were in that room, he would be tall (i.e. it would be the case that he was tall),' it *need* not be.*

To sum up, lawlike statements are not a special case of descriptive 'all-'statements. In particular, they are not descriptive 'all-'statements which are unrestricted in scope, i.e. not localized by reference to particular places, times, or objects. Indeed, do we ever make descriptive 'all-'statements about the whole universe everywhere and every-when? As philosophers we can imagine ourselves doing so; but the idea that we are doing so everytime we make an unrestricted lawlike statement is a product of bad philosophy.

99. A related point can be made about statistical laws. Thus, suppose {300} that on grounds of analogy or extrapolation, as contrasted with direct induction from planetary observations, we reasoned

. . .
So, that, say, 99/100 of the members of K originate inside their solar system may reasonably be inferred, given that K is an unexamined restricted class of planets.

Granted that our statistical knowledge about planets, thus interpreted, does not entail that there are any planets, does it not, perhaps, entail that if there are any, then some 1/100 of the number of all planets everywhere and every-when have succeeded in joining their solar systems from without? Is it not *incompatible* with the idea that there never has been nor never will be a planet which succeeds in joining its solar system from without?

The answer, in terms of our analysis, is that there is, indeed, such an incompatibility; not, however, because the statistical law is a statement of the form

The proportion of Bs among As in the past, present and future of the universe is n/m

or its more sophisticated variants (Reichenbach, Von Mises) designed to make sense of the relative frequency of a quaesitum property in an infinite reference class, but because to accept the law is to be prepared to infer, with respect to any unexamined restricted class of planets, that the proportion of its members which originate from outside the solar system to which they belong is 1/100.

100. In the case of both statistical and non-statistical primary inductions, then, our account has freed us from the idea, disturbing even to those who have believed themselves committed to it, that if induction is to be reasonable, it must be reasonable to move from 'observed As have been found to be B in the proportion n/m (where n may equal m),' to ' (In all probability) everywhere and every-when the proportion of As which are B is n/m.'

101. Does any place remain, after all this has been said, for the idea of a *physically contingent* but *unrestricted* constant conjunction? The answer is that even if one is never, can—logically—never be in a position to say

. . .
So, (in all probability) A is *as a matter of physically contingent fact* constantly accompanied by B

{301} one may well be in a position to say

. . .
So, there need not have been nor need there ever be exceptions to the conjunction of B

with A, in spite of the fact that (in all probability) there *are* exceptions, and that (in all probability) A does not P-entail B.

We must here, as elsewhere, draw a distinction between what we are committed to concerning the world by virtue of the fact that we have reason to make a certain assertion, and the force, in a narrower sense, of the assertion itself. Idealism is notorious for the fallacy of concluding that because there must be minds in the world in order for *us* to have reason to make statements about the world, therefore there is no sense to the idea of a world which does not include minds; the idea, that is, that things might have been such that there were no minds. Surely there is a parallel fallacy in the argument that since we can have no reason to suppose that the relationship between A and B is contingent, which is not a reason for supposing there to be As which are not B, there is no sense to the idea that things *might* have so worked out that A was constantly but contingently conjoined with B!

And just as it throws light on the status of mind in the universe to point out that it makes sense to speak of a universe which contains no minds; so it throws light on the concept of a law of nature to point out that it makes sense to speak of a universe in which there are uniformities which, although physically contingent, are without exception. Surely, to be in a position to say

. . .
So, (in all probability) A · C P-entails B; whereas A · C′ P-entails ~B

or, more simply,

. . .
So, (in all probability) A is P-compatible with both B and ~B

is to be in a position to say that, *although in the nature of the case we have reason to think that they have not done so,* things might have so worked out that A was constantly conjoined with B (or, for that matter, with ~B). And where our reason for thinking that A is compatible with both B and ~B is *not* that we have observed As which are B and As which are ~B, but consists, rather, in considerations of extrapolation and analogy, we are in a position to say that, although in the nature of the case we have reason to think they have not done {302} so, things *may actually be* such that A *is* constantly conjoined with B (or ~B).

102. To take the causal modalities at their face value, that is to say, to interpret statements concerning what is physically necessary or possible or impossible as belonging to the object language of scientific (and everyday) discourse, which statements, however intimately they may be related to such metalinguistic statements

as they may, *in some sense,* imply, are nevertheless not themselves 'really' metalinguistic, is certainly to court serious philosophical perplexity. Even a dyed in the wool empiricist might be willing to go along with the idea that specific statements of the form 'A P-entails B' are non-descriptive statements which contextually imply that the speaker feels entitled to infer that something is B, given that it is A; it is when he is confronted with statements of the form

There is a property which P-entails B

or, above all, by such statements as

There exist causal connections which have not yet been discovered

and

For every kind of event E *there is* a kind of event E′ such that the occurrence of E′ P-entails the contiguous occurrence of E

that his anxiety is likely to reach serious proportions.

It is as though someone who had taken the early emotivist line in ethics had been carefully talked into the idea that 'ought' is a perfectly good concept, though not a descriptive one, and that 'Everybody ought to keep promises' contextually implies a wish, on the speaker's part, that promise keeping were a universal practice, and was then confronted with such statements as

There are obligations which have not yet been recognized

and

Some of the things we think of as obligations are not obligations.

103. It is therefore important to realize that the presence in the object language of the causal modalities (*and* of the logical modalities *and* of the deontic modalities) serves not only to express *existing* commitments, but also to provide the *framework* for the thinking by which {303} we reason our way (in a manner appropriate to the specific subject matter) into the making of *new* commitments and the abandoning of old. And since this framework essentially involves quantification over predicate variables, puzzles over the 'existence of abstract entities' are almost as responsible for the prevalence in the empiricist tradition of 'nothing-but-ism' in its various forms (emotivism, philosophical behaviorism, phenomenalism) as its tendency to assimilate all discourse to describing.

This is not the occasion for an exploration of the problem of 'abstract entities.'* It is, however, essential to my purposes to urge that an ungrudging recognition of the role in discourse of such contexts as

$(\exists\Phi) \ldots \Phi \ldots$

is the very foundation of sound philosophy. Empiricists have tended to be reasonably comfortable with such statements as

The German word *'rot' means the quality red*

by virtue of construing them, correctly, as having *something like* the force of

The German word 'rot' has—in an appropriate sense—the same role or use as our *one-place predicate 'red'.*

and, in general, by thinking of the rubric 'there is a quality, relation, property, etc., such that . . .' as a framework device which uses what Wittgenstein (in the *Tractatus*) called 'formal concepts' to represent *in a language* certain features of its use, features the formulation of which would belong in a metalanguage. It is when the empiricist is confronted with such statements as

There are qualities which no one has yet experienced
There are numbers for which no designation exists in our language

not to mention the examples of the preceding paragraph, that he is tempted either to regress to a more primitive empiricism, or to cross the lines and join forces with the more empirically minded metaphysicians.

104. It has been, on the whole, a presupposition of contemporary empiricism that the range of variables is to be interpreted in terms of {304} *existing* conceptual resources. This presupposition found its most explicit expression in the formulas (Wittgenstein, 20)

$(x)\ fx = fa \cdot fb \cdot fc \ldots$
$(\exists x)\ fx = fa \vee fb \vee fc \ldots$

Built on a distinction between expressions which *name* (the 'a', 'b', 'c', etc. of the above formulas) and expressions which, *though they may present the appearance of names,* are to be analyzed as 'shorthand' for definite descriptions, this presupposition amounted to the idea that our language refers to the members of a certain category of entity, in this case particulars—which, to free the logical point I am making from phenomenalistic associations, I shall suppose to be physical objects—by virtue of (a) the names it contains, and (b) by virtue of the descriptive phrases of the form 'the x which is R to a' which can be formulated in it. And while reference to a *particular* physical object is, indeed, by name or by description, the idea that reference to physical objects *in general* exists by virtue of names supplemented by definite descriptions, where the variable x in the description is interpreted in the spirit of the above formulas can readily be seen to involve the paradoxical consequence that the range of describable objects, and hence the range of reference of the language (with respect to physical objects) coincides with the range of *named* objects.

It is obvious, indeed, that only in an 'ideally complete' language which did contain names for all physical objects could there be a *cash value* equivalence between '(x) fx' and 'fa · fb · fc . . .' We are tempted to put this by saying that *as matters stand* the use of variables and the device of quantification gives language a direct reference to physical objects which supplements the reference provided by names. To do so, however, is to become puzzled—particularly if, following Quine, we are tempted to construe all designations of individuals as descriptions.

105. The solution of this puzzle lies in the fact that the logic of variables and quantification involves not only the *momentary* crystalized content of the language at a *cross section* of its history, but also its character as admitting—indeed demanding—modification, revision, in short, development, in accordance with rational procedures. In the case of variables the values of which are descriptive constants, these rational procedures can be summed up in the single word 'Induction.' But the point is of more general import, as can be seen by reflecting {305} on the logic of number variables in the context of the history of mathematics since, say, 1600.

106. The above remarks on variables are designed to introduce the concluding theme of this essay. For they make it clear that the force of such expressions as 'There is a characteristic such that . . .', i.e.

$(\exists\Phi) \ldots \Phi \ldots$

is likewise misunderstood if they are interpreted as mobilizing the *existing* stock of 'primitive descriptive predicates' of our language at this cross section of its history. Rather they embody the idea of the predicates it will be reasonable to *introduce* (or to *discard*) as well as of those which express our imperfect achievements to date.

But it is this fact which enables us to make sense of the idea that

There are causal connections which have not yet been discovered.

For we have not yet fully faced the question: What can be meant by speaking of the 'existence' of an *unknown entailments?* It is the willingness to accept this way of talking which—in the absence of the distinction we have been drawing—leads directly to rationalistic metaphysics. It is the unwillingness to accept rationalistic metaphysics which—in the absence of the distinction we have been drawing—leads directly to Humean metaphysics.

107. Is the idea that every change has a cause a super-hypothesis? An induction from inductions? Or is it, perhaps, an a priori truth? It is certainly not the latter if we mean by an a priori truth the sort of thing that *could* be established by induction but is *fortunately* exempt from having to run the inductive gauntlet because of our progress at rational intuition. For the 'Causal Principle' isn't the sort of thing that *could* be established by induction. It isn't a hypothesis about the world—not because it is about nothing, but because it no more has the business of describing than do specific causal propositions. Not that it is like specific causal propositions, only more abstract; its force, as we shall see, is of quite another kind.

The first thing to see, is that it is a logical truth that there can be no *descriptive* statement which stands to 'Every event has a cause' as 'This A is B / 'That A is B,' etc. stand to 'All A is B.' That is to say, there can be none if our analysis of lawlike statements is correct. And if so, then the idea of an inductive argument of which the conclusion is {306} '. . . *So*,(in all probability) every event has a cause' is logical nonsense.

And once one abandons the idea that the causal principle is a super- description of the world, one is no longer confronted by the need to choose between the alternatives, (a) that it is an induction from inductions (thus implicitly committing ourselves to the regularity analysis of lawlike statements), and (b) that it is a rational intuition. And we find ourselves in a position to acknowledge the truth in the claim that we know a priori—i.e. other than by induction—that every change has a cause. For not all *knowing* is knowing how to describe something. We *know* what we *ought to do* as well as what the circumstances *are.*

The indicative sentence "Every change has a cause" and its more sophisticated counterparts, bring every descriptive term which has or will be introduced into the language of science within the scope of the question '*What* is its cause?' And while it is, indeed, true that circumstances can be imagined in which it would be unreasonable to continue the search for causes in a certain domain of problems, it would be simply incorrect to express the decision to abandon this search by means of the sentence '(In all probability) such and such a kind of event has no cause.' It would, of course, be equally incorrect to express the decision to press on in all areas of science, based on success after success in past investigations, by the sentence, '(In all probability) every kind of event has a cause.' The *economic* (in the broadest sense) issue 'to continue or not to continue?' must not be confused with an issue concerning the causal principle. To say that a certain kind of event has no cause is not to express a pessimistic view of the outcome of an investigation; if it didn't express philosophical confusion, it would be to abdicate.

108. For the causal principle gives expression to features of our language (indeed, of our mind) which are independent of success or failure, of optimism or pessimism, of the economics of intellectual effort. Among other things, it gives expression to the fact that although describing and explaining (predicting, retrodicting, understanding) are *distinguishable,* they are also, in an important sense, *inseparable.* It is only because the expressions in terms of which we describe objects, even such basic expressions as words for the perceptible characteristics of molar objects* locate these objects in a space of implications, that they *describe* at all, rather than merely label. The descriptive and the explanatory resources of language advance hand in hand; and to abandon the search for explanation is to abandon the attempt to improve language, *period.*

Once the development of human language left the stage when linguistic changes had *causes,* but not *reasons,* and man acquired the ability to reason about his reasons, then, and this is a logical point about having the ability to reason about reasons, his language came to permit the formulation of certain propositions which, incapable of proof or disproof by empirical methods, draw, in the heart of *language militant,* a picture of *language triumphant.* Kant's conception that reason is characterized by certain regulative ideals contains a profound truth which empiricism has tended to distort into the empirical psychology of the scientific enterprise.

February 24, 1957

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NOTES:

p. 227:

* Nelson Goodman, "The Problem of Counterfactual Conditionals," *Journal of Philosophy*, 44:113-28 (1947); reprinted in his book, *Fact, Fiction and Forecast* (Cambridge, Mass.: Harvard Univ. Press, 1955), pp. 13-34. Page references in the following are to *Fact, Fiction and Forecast.*

† Op. cit., p. 14.

‡ Ibid., p. 16.

§ Ibid., p. 16.

p. 228:

* Ibid., pp. 16-17, passim.

p. 230:

* Ibid., p. 23.

† *Fact, Fiction and Forecast.* This book, of which the first part is a reprinting of the 1947 paper, contains the University of London Special Lecture in Philosophy for 1953.

‡ Op. cit., p.7.

§ Ibid., p. 9.

p. 231:

* Ibid., p. 33 (note 7 to p. 21).

† Goodman's "etc." should not mislead us. Its scope, though vague, is limited by the context, and does not include, positively or negatively, every *possible* circumstance, e.g., the presence of a pyrophobe.

p. 242:

* *Fact,Fiction and Forecast*, p. 17.

† I have used the so-called 'tenseless present'—a typical philosophical invention—to simplify the formulation of this general hypothetical, without, in this context, doing it too much violence.

p. 254:

* For a careful analysis of dispositional concepts along these lines, see Burks (6). The above account, however, was independently developed at the Minnesota Center for Philosophy of Science memoranda during 1953-1954.

p. 255:

* Or, perhaps, 'f(x,s,t)'—thus 'x is red at place s and time t.'

p. 256:

* For a related distinction, see Bergmann (3).

† Yet it would be an oversimplification, of course, to say that the form 'F(x,t)' represents exactly the force of ordinary expressions to the effect that a certain thing is in a certain state. For when we say, for example, of a certain object that it is red, or magnetized, we imply that it has been and will continue to be red or magnetized for an unspecified period; otherwise we would say, "It is red (or magnetized) *now*" or ". . . for the moment."

p. 260:

* For a further discussion of thing-kind expressions which construes them as common names of the individuals belonging to the kinds, and compares the irreducibility of a

thing-kind to its 'criterion characteristics' with the irreducibility of proper names to the definite descriptions which are the criteria for their application, see my "Form and Substance in Aristotle: an Exploration," *Journal of Philosophy,* forthcoming, fall 1957.

† For basic discussions of vagueness and 'open texture,' see Kaplan (10), Kaplan and Schott (11), Pap (13), Waismann (19), and Wittgenstein (21).

p. 263:

* Cf. Collingwood (8), Feigl (9), and Russell (22).

† For an elaboration of this theme, and a more complete discussion of the nature and status of theories, see my essay "Empiricism and the Philosophy of Mind," in Volume I of *Minnesota Studies in the Philosophy of Science,* particularly sections 39-44 and 51-55.

p. 267:

* The most recent, and, in many respects, the best, presentation of the entailment side of the controversy is to be found in Kneale (12); but see also Broad (7). For equally effective representations of the regularity analysis, see Ayer (1), Braithwaite (4), Reichenbach (16, 17), and Weinberg (23).

p. 268:

* the form '$(x)\ Ax \supset Bx$' will be used for simplicity of formulation, leaving the reference to time as something to be 'understood'; to say nothing of the complexities discussed in the first two parts of this essay.

p. 269:

* Cf. Goodman's opening characterization of lawlike statements in the second part of his paper on counterfactuals in *Fact, Fiction and Forecast,* pp. 24ff., particularly p. 26.

p. 272:

* For a lucid presentation of the fundamentals of a logic of the causal modalities, see Burks (5); also Reichenbach (15, 17) and von Wright (19).

p. 282:

* The above remarks are necessarily brief. For an expanded account, along the above lines, of statements of the form 'Jones thinks that x ought to do A,' see my essay

"Imperatives, Intentions and the Logic of 'Ought'," *Methodos,* 8, 1956; also R.M. Hare, *The Language of Morals,* (Oxford: The Clarendon Press, 1952).

p. 284:

* For further discussion of the rubric "'. . .' means ---" and for an interpretation ofthe framework of thoughts and concepts as a quasi-theoretical framework of which the role in discourse pertaining to the explanation of overt human behavior is logically prior to its role in 'self-awareness' or 'introspection,' see my essay "Empiricism and the Philosophy of Mind," in Volume I of *Minnesota Studies in the Philosophy of Science,* especially sections 30-31; also the Appendix on Intentionality and the Mental at the end of the present volume.

p. 290:

* A more realistic account would have, instead of 'n/m', 'a proportion approximating to n/m', and would give some indication of how the closeness of the approximation relates to the 'size' of the evidence and to other features of the inductive situation. But I am leaving out of account all 'secondary' inductions, for I am attempting to put my finger on a logical point which relates to the very concept of inductive inference.

p. 293:

* This paragraph and the one which immediately precedes it were stimulated by a helpful discussion with Michael Scriven of the compatibility of the above treatment of statistical induction with the view, which he wished to defend, that statements about the likelihoods of individual events are empirically true or false.

p. 294:

* This statement must be qualified, once the theoretical dimension of scientific reasoning is taken into account, by mentioning such possible exceptions are represented by the following: Suppose that we knew on theoretical grounds that the universe was one 'heart beat' of expansion and contraction, and that a certain kind of event B could take place, given an event of kind A, only when and if the universe went through a certain pattern P in its expansion. Suppose, further, that whether or not the universe goes through this pattern depends on the initial state of the universe, the theory defining a class of alternative initial states some of which do, some of which do not, eventuate in P. Suppose, finally, that observation provides reason to believe that the universe did go through P. We conclude that it is a *physically contingent fact* that A was constantly conjoined with B, and will continue (if vacuously) to be so, but that it is a matter of

physical necessity that A was and will be constantly conjoined with B in the presence of P.

p. 299:

* This paragraph was added after Mr. Gavin Alexander correctly pointed out that the distinction between the ‘crime detection’ reasoning—inductive ‘in a broad sense’—by which we support restricted ‘all-’statements which are not lawlike, and the reasoning by which we support even restricted lawlike statements, is so central to my argument that it should not be referred to cavalierly as something that “can readily be seen.”

p. 303:

* I have discussed this problem in an essay, “Empiricism and Abstract Entities,” to appear at a future date in *The Philosophy of Rudolf Carnap,* (New York: Tudor, forthcoming).

p. 306:

* For an elaboration of this point, see my essay “Empiricism and the Philosophy of Mind,” in the first volume of *Minnesota Studies in the Philosophy of Science,* particularly sections 35-38.