3

Causal Regularities[1]

ONE of Hume's legacies is the regularity theory of causation. In his definitions, which aim at reform rather than analysis of our ordinary concepts, he equates causation as it really exists in the objects with regular succession. Such regularity is, as we saw in Chapter 1, well fitted to play the role of necessity$_3$, that is, of something that would license causal inference but not *a priori*, not from a knowledge of the individual cause or effect on its own.

It was argued in Chapter 2 that an initial analysis, at least, of our ordinary causal concepts could be given in terms of certain conditional statements, especially certain counterfactuals; in asserting singular causal sequences we are talking, in part, not only about what has occurred but about what would or would not have occurred had things been different,

[1] The ideas underlying this chapter were originally presented in 'Causes and Conditions', *American Philosophical Quarterly*, ii (1965), 245–64. I have tried to take account of a number of discussions of this article, both published and unpublished, notably the criticisms made by Jaegwon Kim in 'Causes and Events: Mackie on Causation', *Journal of Philosophy*, lxviii (1971), 426–41. In particular, I have tried to clarify my account by maintaining, as I did not in the original article, a firm distinction between types of events ('generic events', 'properties') and the individual events that instantiate them, more or less as recommended in Part IV of Kim's article.

The account of eliminative induction used in this chapter was first presented in my article 'Mill's Methods of Induction', in *Encyclopedia of Philosophy*, ed. Paul Edwards, vol. 5, pp. 324–32. A fuller statement of it is given in the Appendix.

The ultimate credit for much of what is said here must go to J. S. Mill, but in recent years a number of philosophers have, independently of one another and using different approaches, produced improved accounts which are essentially alike even in respects in which they could not be derived from Mill. I have noticed particularly those of Konrad Marc-Wogau, 'On Historical Explanation', *Theoria*, xxviii (1962), 213–33, and of Michael Scriven, in a review of Nagel's *The Structure of Science*, *Review of Metaphysics*, xvii (1964), 403–24, and in 'The Logic of Cause', *Theory and Decision*, ii (1971), 49–66, but there are, I believe, more than a few others. Such a convergence of independent approaches suggests that we may be getting near to the truth.

that is, about some merely possible situations and events. We have, I suggested, two ways of doing this, a primitive one and a sophisticated one. The primitive one relies on imagination and analogy, but the sophisticated one uses general propositions, which sustain the counterfactual conditionals. If we have inductive reasons—reasons that carry us beyond the supporting observations—for believing that all situations of a certain kind develop in a certain way, we find plausible the counterfactual conditional statement that if this situation had been of that kind it too would have developed in that way. Regularity statements, if inductively supported, will sustain the conditionals which an initial analysis of causal statements brings to light. The meaning of causal statements is given by the conditionals, but their grounds may well include the corresponding regularities.

It is therefore appropriate to inquire how far a regularity theory will go as an account of causation as it exists in the objects. Regularity has at least the merit that it involves no mysteries, no occult properties like necessity$_2$. It is true that an unqualified regularity, holding for unobserved as well as observed instances, obviously cannot be observed: but we can say quite explicitly what it would be. And there is no need to introduce the mystery of a special *sort* of regularity, a 'nomic universal', to account for the ability of causal laws to sustain counterfactual conditionals.[2] Yet there is some obscurity in the notion of regular succession. Hume's account, as we noted, is careless and imprecise: if the regularity theory is to be given a fair trial we must begin by describing more accurately and in more detail the forms of regularity which might count as the whole or as part of causation in the objects.

Mill's account is a great improvement upon Hume's: he explicitly recognizes a number of important complications. 'It is seldom, if ever, between a consequent and a single antecedent that this invariable sequence subsists. It is usually between a consequent and the sum of several antecedents; the concurrence of all of them being requisite to produce, that is, to be certain of being followed by, the consequent.'[3] We may put this more

[2] I have argued this point in *Truth, Probability, and Paradox*, Chapter 3, pp. 114–19, but it is considered further in Chapter 8 below.

[3] *System of Logic*, Book III, Ch. 5, Sect. 3.

formally. There are certain *factors*—that is, types of event or situation—which we can symbolize as *A*, *B*, *C*, *etc.*, and the effect (Mill's 'phenomenon') *P* occurs whenever some conjunction of factors occurs—say the conjunction of *A* and *B* and *C*, which we shall symbolize as *ABC*—but not when only some of these conjuncts are present. All *ABC* are followed by *P*, but it is not the case that all *AB* are followed by *P*, and so on. (The references to 'sequence', to 'following', of course mean that the 'consequent' occurs, in each individual instance of the causal sequence, fairly soon after all the 'antecedents' are assembled, and in the appropriate spatial region. There is some looseness in these notions, which may reflect a real inadequacy in the regularity theory, but let us postpone this objection and assume that we understand well enough what counts as sequence.) Mill also points out that there can be what he calls a plurality of causes.

It is not true that one effect must be connected with only one cause, or assemblage of conditions; that each phenomenon can be produced only in one way. There are often several independent modes in which the same phenomenon could have originated. One fact may be the consequent in several invariable sequences; it may follow, with equal uniformity, any one of several antecedents, or collections of antecedents. Many causes may produce mechanical motion: many causes may produce some kinds of sensation: many causes may produce death. A given effect may really be produced by a certain cause, and yet be perfectly capable of being produced without it.[4]

In our symbolism, this means that we may have, say, not only 'All *ABC* are followed by *P*' but also 'All *DGH* are followed by *P*'. Now the conjunction of these two propositions is equivalent to 'All (*ABC* or *DGH*) are followed by *P*'. A plurality of causes is tantamount to a disjunctive antecedent, as an assemblage of conditions is to a conjunctive one: allowing for both, we have a disjunction of conjunctions. Now suppose that there is a finite set of assemblages of conditions that produce *P*, say *ABC*, *DGH*, and *JKL*. It may well be that *P* occurs only when at least one of these conjunctions has occurred soon before in the right region. If so, all *P* are preceded by (*ABC* or *DGH* or *JKL*). (There is, of course, no logical necessity that this should be so. Events might occur in a disorderly way: *P* might sometimes

[4] Ibid., Book III, Ch. 10, Sect. 1.

occur without there having occurred, just before in the right region, any assemblage of conditions which is always followed by *P*. But at present I am considering cases where this is not so. That is, we may have a pair of (roughly) converse universal propositions, 'All (*ABC* or *DGH* or *JKL*) are followed by *P*' and 'All *P* are preceded by (*ABC* or *DGH* or *JKL*)'.

In discussing such forms of regularity, it will be convenient to use the terms 'necessary condition' and 'sufficient condition' in senses different from, though related to, the senses in which these phrases were used in Chapter 2. There, a necessary condition, for example, was related to a counterfactual conditional; '*X* was a necessary condition for *Y*' meant 'If *X* had not occurred, *Y* would not', where '*X*' and '*Y*' stood for particular events. But we are now using letters to stand for types of event or situation, and '*X* is a necessary condition for *Y*' will mean that whenever an event of type *Y* occurs, an event of type *X* also occurs, and '*X* is a sufficient condition for *Y*' will mean that whenever an event of type *X* occurs, so does an event of type *Y*.

Then in the case described above the complex formula '(*ABC* or *DGH* or *JKL*)' represents a condition which is both necessary and sufficient for *P*: each conjunction, such as '*ABC*', represents a condition which is sufficient but not necessary for *P*. Besides, *ABC* is a *minimal* sufficient condition: none of its conjuncts is redundant: no part of it, such as *AB*, is itself sufficient for *P*. But each single factor, such as *A*, is neither a necessary nor a sufficient condition for *P*. Yet it is clearly related to *P* in an important way: it is an *insufficient* but *non-redundant* part of an *unnecessary* but *sufficient* condition: it will be convenient to call this (using the first letters of the italicized words) an *inus* condition.[5]

Mill includes in his assemblages of conditions *states*, that is, standing conditions, as well as what are strictly speaking *events*. He also stresses the importance of factors which we should naturally regard as negative, for example the absence of a sentry from his post. It may be the consumption of a certain poison conjoined with the non-consumption of the appropriate

[5] This term, 'inus condition', was introduced in 'Causes and Conditions', having been suggested by D. C. Stove; the term 'minimal sufficient condition' was used by K. Marc-Wogau in 'On Historical Explanation', *Theoria*, xxviii (1962).

antidote which is invariably followed by death. If a certain type of event is symbolized as *C*, then not-*C*, or $\bar{C}$, will be the absence of any event of that type. It may be that although *AB* alone is not sufficient for *P*—because *ABC* is regularly followed by not *P*—$AB\bar{C}$ is sufficient for *P*. Mill is reluctant to call such a negative condition as $\bar{C}$ a cause, but speaks instead of the absence of counteracting causes; if $\bar{C}$ is needed as a conjunct in the minimal sufficient condition $AB\bar{C}$, then *C* itself is a counteracting cause.[6]

A further complication, for which Mill does not provide, is the recognition of a causal field in the sense explained in Chapter 2. The 'antecedents' and 'consequents' will not, in general, be events that float about on their own, they will be things that happen to or in some subject or setting. In discussing the causes of death, for example, we may well be concerned with the dying of human beings who have been living in an ordinary environment. If so, human beings and this environment together constitute the field. We shall not then regard the facts that these are human beings, or that they are in this ordinary environment, as causal factors: these will not figure as conjuncts in such a condition as $AB\bar{C}$. The causal field in this sense is not itself even part of a cause, but is rather a background against which the causing goes on. If we sum up such a field as *F*, and allow for the various points made by Mill, we arrive at the following typical form for a causal regularity:

In *F*, all ($AB\bar{C}$ or $DG\bar{H}$ or $JK\bar{L}$) are followed by *P*, and, in *F*, all *P* are preceded by $AB\bar{C}$ or $DG\bar{H}$ or $JK\bar{L}$).

For some purposes this may be simplified to

All *F* ($AB\bar{C}$ or $DG\bar{H}$ or $JK\bar{L}$) are *P* and all *FP* are ($AB\bar{C}$ or $DG\bar{H}$ or $JK\bar{L}$).

That is, some disjunction of conjunctions of factors, some of which may be negative, is both necessary and sufficient for the effect in the field in question. But what then is the cause? Mill says that 'The cause . . . philosophically speaking, is the sum total of the conditions positive and negative',[7] in other words such a conjunction as $AB\bar{C}$; but if we go as far as this there is no good reason why we should not go further and equate 'the

[6] *System of Logic*, Book III, Ch. 5, Sect. 3. [7] Ibid.

cause, philosophically speaking' rather with the complete disjunction of conjunctions, such as ($AB\bar{C}$ or $DG\bar{H}$ or $JK\bar{L}$). It is this that is both necessary and sufficient for the effect (in the field) as causes have often been assumed to be. And when speaking from a regularity point of view, it will be convenient to call this the *full cause*. But what is ordinarily called a cause, or what is referred to by the subject of a causal verb, is practically never anything like this; rather, in general causal statements, like 'The consumption of more than such-and-such a dose of aspirin causes death', the cause is a factor such as is represented, say, by 'A' in our formula, and in singular causal statements like 'Taking this dose of aspirin caused his death' the cause is an instance of some such type of event as is represented by 'A'. That is, what is typically called a cause is an inus condition or an individual instance of an inus condition, and it may be a state rather than an event.

A regularity theory that is to have any chance of being defended as even a partial description of causation in the objects must deal in regularities of this complex sort. And such a theory has considerable merits. It seems quite clear that there are many regularities of succession of this sort, and that progress in causal knowledge consists partly in arriving gradually at fuller formulations of such laws. Also, even these complex regularities could play the role of necessity_3, they could license the sorts of causal inference that Hume thought so important. If ($AB\bar{C}$ or $DG\bar{H}$ or $JK\bar{L}$) is both necessary and sufficient for P in F, and this is known or believed, then if an instance of $AB\bar{C}$ in F is observed, an inference to the conclusion that an instance of P will follow is in order, while if an instance of P in F is observed, and there is reason to believe that neither $DG\bar{H}$ nor $JK\bar{L}$ (as a whole) has occurred at the right time and place, an inference to the conclusion that $AB\bar{C}$ has occurred and therefore that an instance of A has occurred is in order. Complex regularities still license inference from cause to effect and from effect to cause.

Moreover, they will sustain the various kinds of conditionals that come to light in the analysis of causal concepts. First, and most important, is the one which states that the individual cause-event was necessary in the circumstances for the effect, for example 'If he had not eaten of that dish, he would not have died'. Such a conditional, I have argued,[8] can be understood as saying 'Suppose that he did not eat of that dish; then (within the scope of that supposition) he did not die' or 'In the possible situation where he did not eat of that dish, he did not die'. Now if there is some regularity of something like the form suggested above—though no doubt with many more disjuncts—giving the 'full cause' of death of human beings in an ordinary environment, and one of the minimal sufficient conditions, say $A\bar{B}\bar{C}$, is the conjunction of consuming at least such-and-such an amount of a certain poison (which was in the dish in question, but nowhere else) with not taking the appropriate antidote and not having one's stomach evacuated within a certain time, and this minimal sufficient condition was realized on this occasion but none of the other minimal sufficient conditions was realized, then from the supposition that on this occasion he did not eat of that dish it follows that A did not occur on this occasion; from this, together with the second half of the regularity, of the form 'All FP are ($A\bar{B}\bar{C}$ or etc.)', and the information that none of the other disjuncts, summed up here as 'etc.', occurred on this occasion, it follows that P did not occur in F on this occasion; such an inference justifies the assertion, within the scope of the supposition that he did not eat of that dish, that P did not occur, that is, that he did not die, and hence sustains the conditional 'If he had not eaten of that dish he would not have died'.

Secondly, although I argued in Chapter 2 that we do not always require that an individual cause should be sufficient as well as necessary in the circumstances for its effect, I had to use rather odd indeterministic examples in order to discriminate between necessity and sufficiency in the strong sense. Most individual causes, it appears, both are and are taken to be sufficient in the circumstances in the strong sense: we can say that if in the circumstances he had not been going to die, he would not have eaten of that dish. This counterfactual conditional is sustained, analogously with the previous one, by the *first* half of the regularity 'All F ($A\bar{B}\bar{C}$ or etc.) are P' together with the information that both B and C were absent on this occasion; for these together with the supposition that he did not die entail first that none of the disjuncts occurred on this

[8] *Truth, Probability, and Paradox*, pp. 92–108.

occasion, then that A did not occur (since $\bar{B}C$ did), and hence that he did not eat of that dish. So 'He did not eat of that dish' can be asserted within the scope of the supposition that he did not die, and this is equivalent to the required counterfactual.

Thirdly, it is commonly held that a causal claim entails that if a sufficiently similar antecedent had occurred on other occasions (when in fact it did not) a similar consequent would have occurred. This means, in this case, that if $A\bar{B}C$ had occurred in F on some other occasion, P would have, and it is clear that this counterfactual too is sustained, in much the same way, by the first half of the suggested regularity.

On the other hand, although such complex regularities may hold in the objects, we must admit that they are seldom, if ever, known in full. Even in a matter of such intimate and absorbing interest as the death of human beings, we cannot confidently assert any complete regularity of this kind. We do not know all the causes of death, that is, all the different closely preceding assemblages of conditions that are minimally sufficient for death. And even with any one cause, we do not know all the possible counteracting causes, all the factors the negations of which would have to be conjoined with our positive factors to make up just one minimal sufficient condition. Causal knowledge progresses gradually towards the formulation of such regularities, but it hardly ever gets there. Causal regularities *as known* are typically incomplete; they have rather the form

All F ($A \ldots \bar{B} \ldots$ or $D \ldots \bar{H} \ldots$ or $\ldots$) are P, and all FP are ($A \ldots \bar{B} \ldots$ or $D \ldots \bar{H} \ldots$ or $\ldots$)

where the dots indicate further as yet unknown conjuncts and disjuncts that have still to be filled in. What we know are certain *elliptical* or *gappy* universal propositions. We do not know the full cause of death in human beings, but we do know, about each of a considerable number of items, that it is an inus condition of death, that, as we ordinarily say, it may cause death.

The same knowledge that is expressed by such elliptical universal propositions can be expressed, alternatively, by propositions in which second-order existential quantifications precede the universal ones. Thus the knowledge that A is an inus condition of P in F may be formulated thus:

For some X and for some Y all $F(AX$ or $Y)$ are P, and all FP are $(AX$ or $Y)$.

The suggestion that causal regularities as known are commonly of this sort may provoke two questions. Of what use is such exiguous information? And how is knowledge of such a curious form acquired? We can show, however—surprising though this may be—that such knowledge can be of great value, and that it may be discovered in just those ways in which we do most commonly acquire causal knowledge.

First, this information still permits causal inferences (in both directions), but makes them more tentative. Knowing that something of the form $(AX$ or $Y)$—where A is known but X and Y are not—is both necessary and sufficient for P in F, we may well have reason to believe that the X, whatever it may be, is often present; if so, we can infer from an observed occurrence of A that P is fairly likely to follow. X will, of course, include the negations of all the counteracting causes, whatever they may be; and we may well have reason to believe that though there may be many as yet undiscovered counteracting causes they are absent, and their negations therefore are present, on this occasion. There may be many undiscovered antidotes to this poison, but this victim is unlikely to take any of them. Again, without being able to specify Y in any great detail, we may have some reason to believe that it is likely to be absent on this particular occasion. Y, of course, may be partly known; it may be known that some of the other disjuncts it covers are of the forms BZ, CW, etc., where B and C, say, are known, though Z and W are not. If so, we can check that these disjuncts at least are absent by discovering that B and C are; that is, we can check that none of the other known causes (that is, inus conditions) of P in F is present on this occasion. And then we can infer, still tentatively but with some probability, from the occurrence of P on this occasion that A has also occurred. Typically we infer from an effect to a cause (inus condition) by eliminating other possible causes.

Since we can often infer, with probability, an effect from a cause, we can similarly infer, in corresponding circumstances, the absence of a cause from the absence of an effect. And since we can often infer, with probability, a cause from an effect, we can similarly infer the absence of an effect from the absence of a cause. Consequently the gappy universal propositions, the incompletely known complex regularities, which contribute to such inferences will still sustain, with probability, the counterfactual conditionals that correspond to these inferences, that is, statements of the forms 'If A had not occurred, P would not' and 'If P had not been going to occur, A would not'. And the gappy universal in allowing inference from cause to effect will equally sustain the subjunctive conditional that if this cause occurred again in sufficiently similar circumstances, so would the effect, while leaving it open and unknown just what such sufficient similarity requires. Gappy universals, then, still sustain all the types of conditionals commonly associated with causal statements.

Moreover, exiguous though it is, this information allows us to engage, though of course with something less than complete confidence, in the production and the prevention of effects by way of causes. If X, whatever it may be, is often present, we may hope, by introducing an instance of A, to bring about a P. Equally, by eliminating a known cause, A, of P, we have done something to prevent the occurrence of P, and of course if there are several such known causes, say A, B, and C—that is, if the necessary and sufficient condition has the form (AX or BZ or CW or . . .)—then we do the best we can to prevent P by preventing the occurrence of A or B or C; just what X, Z, and W may be is then of no practical importance. And this, surely, is how in many fields intelligent practice goes on. We operate with, or on, factors about which we know only that they can, and perhaps often do, help to produce certain results. We take what precautions we can, but failure does not necessarily show that a plan was ill-conceived; and there is equally such a thing as undeserved success.

Secondly, the elliptical character of causal regularities as known is closely connected with our characteristic methods of discovering and establishing them: it is precisely for such gappy statements that we can obtain fairly direct evidence from quite modest ranges of observation. Of central importance here is what Mill called the Method of Difference; but we can improve on his formulation of it.

This is one of the set of methods of eliminative induction. Like any other such method, it can be formulated in terms of an assumption, an observation, and a conclusion which follows by a deductively valid argument from the assumption and the observation together. To get any positive causal conclusion by a process of elimination, we must assume that the result—the 'phenomenon' whose cause we are seeking—has *some* cause. While we can make some progress with the weaker assumptions that it has only a sufficient cause, or only a necessary cause, the most significant results emerge when we assume that there is some condition which is both necessary and sufficient for the occurrence (shortly afterwards and in the neighbourhood) of the result. Also, if we are to get anywhere by elimination, we must assume a somehow restricted range of possibly relevant causal factors, of kinds of event or situation which might in some way help to constitute this necessary and sufficient condition. Let us initially include in our assumption, then, a list of such *possible causes*, *possibly* relevant factors, say A, B, C, D, E, etc.—though as we shall see later, we do not in fact need a *list*. Even if we had specified such a list of possibly relevant factors, it would in most cases be quite implausible to assume that the supposed necessary and sufficient condition we are seeking is identical with just one of these factors on its own; we expect causal regularities to involve both assemblages of conditions and a plurality of causes. The plausible assumption to make, therefore, is merely that the supposed necessary and sufficient condition will be represented by a formula which is constructed in some way out of some selection from the list of single terms each of which represents a possibly relevant factor, by means of negation, conjunction, and disjunction. However, any formula so constructed is equivalent to some formula in disjunctive normal form—that is, one in which negation, if it occurs, is applied only to single terms, and conjunction, if it occurs, only to single terms and/or negations of single terms. So we can without loss of generality assume that the formula for the supposed necessary and sufficient condition is in disjunctive normal form, that it is at most a disjunction of conjunctions in which each conjunct is a single term or the negation of one, that is, a formula such as '($AB\bar{C}$ or $G\bar{H}$ or J)'. Summing this up, the assumption that we require will have the form:

For some Z, Z is a necessary and sufficient condition for the phenomenon P in the field F, that is, all FP are Z and all

FZ are P, and Z is a condition represented by some formula in disjunctive normal form all of whose constituents are taken from the range of possibly relevant factors A, B, C, D, E, etc.

Along with this assumption, we need an observation which has the form of the classical difference observation described by Mill. This we can formulate as follows:

There is an instance, I_1, in which P occurs, and there is a negative case, N_1, in which P does not occur, such that one of the possibly relevant factors, say A, is present in I_1 but absent from N_1, but each of the other possibly relevant factors is either present in both I_1 and N_1 or absent both from I_1 and from N_1.

An example of such an observation can be set out as follows, with 'a' and 'p' standing for 'absent' and 'present':

	P	A	B	C	D	E	
I_1	p	p	p	a	a	p	etc.
N_1	a	a	p	a	a	p	

Given the above-stated assumption, we can reason as follows about any such observation:

Since P is absent from N_1, every sufficient condition of P is absent from N_1, and therefore every disjunct in Z is absent from N_1. Every disjunct in Z which does not contain A must either be present in both I_1 and N_1 or absent from both, since each of its constituents is either present in both or absent from both; so every disjunct in Z which does not contain A is absent from I_1 as well as from N_1. But since P is present in I_1, and Z is a necessary condition of P, Z is present in I_1. Therefore at least one disjunct in Z is present in I_1. Therefore at least one disjunct in Z contains A. And it must contain A un-negated, if it is to be present in I_1 where A is present.

What this shows is that Z, the supposed necessary and sufficient condition for P in F, must have one of these forms: (A), (AX), $(A$ or $Y)$, $(AX$ or $Y)$. That is, A is either an inus condition for P in F, if the necessary and sufficient condition is of the form $(AX$ or $Y)$—that is, if there are both other factors conjoined with A and other disjuncts as well as the one in which A figures—or, as we may say, better than an inus condition, if the necessary and sufficient condition has one of the other three forms. Our assumption and the difference observation together entail a regularity of the form

For some X and for some Y (which may, however, be null), all F (AX or Y) are P, and all FP are (AX or Y).

This analysis is so far merely formal. To justify my suggestion that we can obtain fairly direct evidence for regularities of this form from modest ranges of observation I must show that it is sometimes reasonable to make the required assumptions and that we can sometimes make the corresponding observations. A number of points can be stated in support of this claim.

First, the actual listing of possibly relevant factors is not needed in practice: this was only a device for formal exposition. All that matters is that any features other than the one, A, that is eventually established as an inus condition (or better) should be matched as between I_1 and N_1, that there should be no other likely-to-be-causally-relevant difference between these two cases: *what* features they agree in having or in lacking is irrelevant. In a causal inquiry in a field in which we already have some knowledge we may, of course, already know what sorts of item are likely to be relevant, and so can check that these are matched between the two cases; but if an inquiry is starting from cold in a field in which little is known, the only available criterion of possible relevance is spatio-temporal neighbourhood: we simply have to see that things are, as far as possible, alike in the space-time regions where an instance of A is followed by an instance of P (our I_1) and where there is neither an instance of A nor one of P (our N_1).

Secondly, there are at least two well-known ways in which some approximation to the classical difference observation is often achieved. One of these is the before-and-after observation. Some change, A, is introduced, either naturally or by deliberate human action, into an otherwise apparently static situation. The state of affairs just after this introduction is our I_1, the state of affairs before it is N_1. If this introduction is followed, without any further intervention, by some other change P, then we can and almost instinctively do reason as the

Method of Difference suggests, concluding both that this instance of A helped to produce this instance of P, and that A generally is at least, in Mill's terms, an indispensable part of the cause of P. The singular causal judgement in such a case could, as I said in Chapter 2, arise in a primitive way from imaginative analogizing; but the corresponding general judgement requires something like the pattern of reasoning that we have just been trying to formalize; and of course once we have this general causal judgement, it could in turn sustain the counterfactual conditionals implicit in the singular judgement. The second well-known approximation to the difference observation is the standard controlled experiment, where what happens in the 'experimental case'—our I_1—is compared with what happens, or fails to happen, in a deliberately constructed 'control case' which is made to match the experimental case in all ways thought likely to be relevant other than the feature, A, whose effects are under investigation.

Thirdly, it may seem to be in general difficult to satisfy the requirement of the Method of Difference, that there should be only *one* point of difference between I_1 and N_1. But fortunately very little really turns upon this. Suppose that two possibly relevant factors, say A and B, had been present in I_1 but absent from N_1. Then reasoning parallel to that stated above would show that at least one of the disjuncts in Z either contains A un-negated, or contains B un-negated or contains both. This observation still serves to show that the cluster of factors (A, B) contains something that is an inus condition (or better) of P in F, whether in the end this turns out to be A alone, or B alone, or both these, or the conjunction AB. Similar considerations apply if there are more than two points of difference. However many there are, an observation of the suggested form, coupled with our assumption, shows that a cause—an inus condition or better—lies somewhere within that cluster of features in which I_1 differs from N_1. It does not, of course, show that the other features, those shared by I_1 and N_1, are irrelevant; our reasoning does not, as some misleading formulations of the method suggest, exclude factors as irrelevant, but positively locates *some at least* of the relevant factors within the differentiating cluster. This point rebuts a criticism sometimes levelled against the eliminative methods generally, that they presuppose and require a finally satisfactory analysis of causal factors into their simple components, which we never actually achieve. On the contrary, any distinction of factors, however rough, enables us to start using such a method. We can proceed, and there is no doubt that discovery has often proceeded, by what we may call *the progressive localization of a cause.* Using the Method of Difference in a very rough way, men discovered first that the drinking of wine causes intoxication. The cluster of factors crudely indicated by the phrase 'the drinking of wine' contains somewhere within it an inus condition of intoxication. Later, by distinguishing various possibly relevant factors within this cluster, and making further observations and experiments of the same sort, they located a cause of intoxication more precisely—the consumption of the alcohol contained in the wine. In a context in which the cluster of factors is put in or left out as a whole it is correct to say, of any particular case, 'He would not have become intoxicated if he had not drunk that wine'. But in a context in which alcohol and certain other constituents were put in or left out separately, it would be correct to say rather 'He would not have become intoxicated if he had not consumed that alcohol'. Did the wine make him drunk? At one level of analysis, of course it did; but in relation to a finer analysis of factors it was not strictly speaking the wine but the alcohol it contained that made him drunk. In different contexts, different specifications of a cause or inus condition may be equally correct.

Fourthly, it is instructive to contrast the Method of Difference as a logical ideal with any concrete application of it. If the assumption and the observation were known to be true, then the conclusion, asserting a typical causal regularity, would be established. No doubt the assumption and the observation are never known with certainty, but it may still be reasonable to accept them provisionally, and, if so, our formal analysis shows that it is equally reasonable to accept the regularity conclusion. In particular, once the assumption is accepted, we may well be in a position to say that we cannot see any other difference that might be relevant between I_1 and N_1, and consequently that we cannot see any escape from the causal conclusion.

Fifthly, we need not and in practice do not rely so heavily on a single observation (with just one I_1 and one N_1) as our formal account might suggest. Of course there might, in such a single pair of cases, be an unnoticed but relevant further difference which undermined our reasoning. It might be that our control case did not match our experimental case closely enough, or, in a before-and-after observation, that some other relevant change occurred at the critical time. But repeating the experiment or the observation reduces this danger. If we can add an I_2 and an N_2, and an I_3 and an N_3, and so on, and each time the presence of A is the only noticed difference between the two sets of antecedent conditions, it becomes progressively less likely that any other relevant change occurred just at the right moment on each occasion—unless, of course, this other change is itself connected with A by what Mill calls some fact of causation. But it is important to note that it is not the mere repeated co-occurrence of A and P that supports the causal conclusion; we are not moving over from the Method of Difference to the Method of Agreement; what we are relying on is the repetition of a sequence which on each single occasion was already, because of the Method of Difference pattern it appeared to exhibit, prima facie a causal one. The repetition merely confirms this by greatly reducing the likelihood that some unnoticed factor might be undermining this prima facie interpretation.

Sixthly, it is worth noting that the assumption required, while it is of course a deterministic one, is much weaker than the usual formulations of the uniformity of nature. We need not assume that *every* event has a cause, but merely that for events of the kind in question, P, in the field in question, F, there is some, possibly very complex, necessary and sufficient condition. It is true that we also need to assume that this condition is made up from a range of possibly relevant factors that is restricted in some way: if we have no previous causal knowledge in the relevant sphere, we have to take spatio-temporal nearness as a criterion of possible relevance. But this is not a final or absolute assumption: if, using it as a working assumption, we reach some conclusion, assert some causal regularity, but then this is disconfirmed by further observations, we can always relax this working assumption and look a bit further afield for possibly relevant differences. There is, no doubt, still a philosophical problem about what justifies *any* such deterministic assumption, however local and however weak. But at least this analysis makes it clearer what precise form of assumption is needed to back up our ordinary detecting and establishing of causal regularities. In particular, I hope to have shown that while we can agree with von Wright that 'in normal scientific practice we have to reckon with plurality rather than singularity, and with complexity rather than simplicity of conditions', this does *not* mean, as he says, that 'the weaker form of the Deterministic Postulate . . . is practically useless as a supplementary premiss or "presupposition" of induction'.[9]

Towards the end of Chapter 2 I said that although necessity$_1$, the distinguishing feature of causal sequences, is not something that can be observed, we can explain how a certain sort of observation can set off a psychological process of imaginative analogizing that can yield a singular causal judgement. We can now add to this by explaining how a not implausibly strict assumption, coupled with that same sort of observation, can entail a causal regularity statement of the form we ordinarily use, and that this statement in turn will sustain the corresponding singular causal judgement.

It is a further merit of such an account of complex but incompletely known regularities that it disposes altogether of a type of objection that is sometimes brought against regularity (or 'Humean') theories of causation in general. Geach, for example, says that

> . . . the laws that scientists aim at establishing are not *de facto* uniformities, either necessary or contingent. For any alleged uniformity is defeasible by something's interfering and preventing the effect. . . . Scientists do not try to describe natural events in terms of what always happens. Rather, certain natural agents . . . are brought into the description, and we are told what behaviour is proper to this set of bodies in these circumstances. If such behaviour is not realized, the scientist looks for a new, interfering agent. . . .

And he goes on to argue that 'interference just cannot be logically brought into a uniformity doctrine of causality'; criticizing some of Mill's statements, he says that Mill 'retreats into saying that physical laws do not state what *does* happen, but what *would failing interference* happen; but this is to abandon the Humian position'.[10]

[9] *A Treatise on Induction and Probability*, p. 135.
[10] *Three Philosophers*, by G. E. M. Anscombe and P. T. Geach, pp. 102–3

It will be clear from what has been said above that though interference could not be brought into a doctrine of simple uniformities, it is easily accommodated in a doctrine of complex uniformities. Interference is the presence of a counteracting cause, a factor whose negation is a conjunct in a minimal sufficient condition (some of) whose other conjuncts are present. The fact that scientists rightly hesitate to assert that something always happens is explained by the point that the complex uniformities they try to discover are nearly always incompletely known. It would be quite consistent with an essentially Humean position—though an advance on what Hume himself says—to distinguish between a full complex physical law, which would state what always does happen, and the law as so far known, which tells us only what would, failing interference, happen; such a subjunctive conditional will be sustained by an incompletely known law. Moreover, the rival doctrine can be understood only with the help of this one. What it would be for certain behaviour to be 'proper to this set of bodies in these circumstances', what Aquinas's tendencies or *appetitus* are, remains utterly obscure in Geach's account; but using the notion of complex regularity we can explain that *A* has a tendency to produce *P* if there is some minimally sufficient condition of *P* in which *A* is a non-redundant element. (This is, indeed, not the only sense of the terms 'tend' and 'tendency'. We could say that *A* tends to produce *P* not only where *A* conjoined with some set of other factors is always followed by *P*, but also where there is an indeterministic, statistical, law to the effect that most, or some, instances of *A*, or some definite percentage of such instances, are followed by *P*, or perhaps where an *A* has a certain objective chance of being followed by a *P*.[11] These statistical tendencies are not reducible even to complex regularities: if they occur, as contemporary science asserts, then they constitute something different from, though related to, strict deterministic causation. But they have little to do with Geach's problem of interference.)

Does this improved and corrected account of causal regularities, with all its merits, constitute a defence of a regularity theory of causation? Can we identify causation with the holding

[11] See Chapter 9 below. Objective chance is also discussed in *Truth, Probability, and Paradox*, Chapter 5.

of regularities of this sort? No progress can be made with this problem unless we keep separate the three sorts of question, what do causal statements mean, what do we know about causation and how do we know it, and what constitutes causation as it is in the objects themselves.

It seems very clear that singular causal statements do not mean that the sequences about which they are made are instances of regularities of any sort. In the Humean tradition there is a doctrine that they *ought* to mean this, but Hume himself, as we have seen, did not claim that they do so. As I have argued in Chapter 2, the main part of our concept of the distinguishing feature of a causal sequence is expressible by a counterfactual conditional, or, what comes to the same thing, by the assertion that the cause was necessary in the circumstances for the effect, and the meaning of a singular causal statement will be analysable into the conjunction of this with, probably, some further claims or suggestions.

It is, however, sometimes said that causal statements are *implicitly* general. It is easy to refute the claim that a singular causal statement normally implies a simple regularity statement, of the form that instances of a certain kind of event are always, or even often, followed by instances of another kind of event: the taking of a contraceptive pill may cause one woman's death although millions of women have taken large numbers of exactly similar pills and survived. Nor can a singular causal statement imply a complex but complete regularity statement, since we commonly assert statements of the former kind when we are quite unable to formulate any corresponding complete generalizations. But perhaps singular causal statements imply our elliptical generalizations, or the equivalent forms with second-order existential quantifiers. It is not so easy conclusively to refute this suggestion, but since it is only in recent years that a number of philosophers have approached a correct formulation of the generalizations in question, we must say at least that this would be an implication of which most users of singular causal statements can be only very vaguely aware. In fact, I would go further and say, referring to what I called (at the end of Chapter 2) a primitive and unsophisticated way of arriving at counterfactuals and the associated causal judgements, that a singular causal statement need not imply even the vaguest generalization. This is true even of physical and mechanical examples. One can judge that this (very hot) stone was cracked by water being poured over it without being committed to any generalization, meaning only that the stone would not in the circumstances have cracked had the water not been poured on, and that this pouring was causally prior to the cracking in the sense explained in Chapter 2. It is, of course, even more obviously true of mental examples; I can judge that Bill's warning shout made me stop short of the precipice without generalizations of any sort being involved.

On the other hand, I have argued that there is a sophisticated way of arriving at causal and counterfactual statements which does involve elliptical generalizations: one can use something like the Method of Difference to establish such a generalization, which will then sustain the counterfactuals involved in the singular causal judgement. But even here it is the generalization that supports the causal statement, rather than the causal statement that implies the generalization. Also, it is worth stressing that the generalization here is of the elliptical universal form, it does not say what always or normally or generally or even often happens—for example, careful checks might make it probable that a certain pill had caused someone's death even if this was the very first time such a pill had had any ill effect (though further cases of the same sort would confirm this conclusion), but the doctor who reached this conclusion would say that the victim must have been unusually susceptible, that she had some as yet unknown combination of characteristics which, in conjunction with the consumption of the pill, would regularly lead to death. Again, it is worth stressing that the generalization need not be known in advance: it may be discovered and (tentatively) established by the observation of the very sequence of events about which the causal statement is made. Not even the vaguest foreknowledge about what often happens is needed to smooth the way even for a physical causal discovery. The doctor (Sir Norman Gregg) who discovered that German measles in pregnancy had caused eye defects in a number of children had no previous reason for regarding this as anything but the mildest of ailments, with no lasting effects. It is true that previously known generalizations may contribute to a causal conclusion, but they do so by supporting the belief that the *other* features present on this occasion would be unlikely to have produced the observed result. If someone eats of a certain dish, and then becomes ill, what helps to show that (something in) this dish made him ill is the fact that everything else that he ate and drank on the same day, and everything else that he then did, were of kinds and in quantities that he regularly ate and drank and did without becoming ill. Such previously known generalizations work, indeed, in the way explained by what Mill called the Method of Residues: in effect, they are used to *construct* a negative instance, corresponding to our N_1, instead of *observing* one, as in the classical form of the Method of Difference.[12] Even these previously known generalizations, then, are only useful, not essential: an observed negative instance that resembles sufficiently closely the positive one makes them superfluous. No specific generalization, however vague, then, needs to be known in advance in order to support the interpretation of an observed sequence as causal: even for the sophisticated way of handling this observation all that is required is the assumption that what has happened is an instance of *some*, probably complex, regularity, that some perhaps as yet quite unknown and unsuspected uniformity is instantiated here. The singular causal statement says that without A, on this occasion (our I_1), P *would* not have occurred; this is very often supported by the observation that without A, on some other similar occasion (our N_1), P *did* not occur. In the sophisticated procedure, this 'did not' supports the 'would not' because it is assumed that there is *some* underlying regularity of behaviour; it is this assumption that justifies the transfer of the non-occurrence of P from N_1 to the suppositional reconstruction of I_1. In the unsophisticated procedure, the transfer is made imaginatively, by analogy; but one could say that to be prone to make such imaginative moves is somewhat like having an unconscious belief that there is some underlying regularity in the world. Even my judgement that I would not have stopped if I had not heard Bill's shout involves a similar transfer from what I know of my own thoughts and movements before he shouted to the hypothetical situation in which, a moment later, I did not hear his shout. But it is only in these very tenuous senses that singular causal statements, sophisticated or primitive,

[12] See Appendix.

physical or mental, are implicitly general, that they necessarily assert or presuppose regularities of any sort.

But what about general causal statements? The statements that heating a gas causes it to expand, that hammering brass makes it brittle, and such related dispositional statements as that strychnine is poisonous and that lead is malleable, can indeed be interpreted as assertions that the cause mentioned or indicated is an inus condition of the effect. But even here it would be more appropriate to take the general statements as quantified variants of the corresponding singular ones, for example, as saying that heating a gas always or often or sometimes causes it to expand, where this 'causes' has the meaning that 'caused' would have in a singular causal statement. However, the essential point is that singular causal statements are prior to general ones, whereas a regularity theory of the meaning of causal statements would reverse this priority.

The question whether regularities enter into what we know about causation and into our ways of learning about it has been answered incidentally in this discussion. The crucial and outstanding question is to what extent such complex regularities as we have described constitute causation as it is in the objects.

It is undeniable that we ordinarily suppose both that there are some such regularities underlying many of the sequences that we take to be causal and that in scientific inquiries, at least in the physical and the biological sciences, we make progress towards fuller formulations of them. Whether these suppositions are correct can still be questioned. The methods so far examined for establishing them rest upon assumptions of uniformity which those methods cannot themselves establish: to ask whether such success as they seem to have achieved can be taken to have confirmed their assumptions, or whether those assumptions can be justified or vindicated in some other way, is to raise the fundamental philosophical problem of induction which I cannot pursue further here; though I noted in Chapter 1 (following Stove) that the reasons given by many of Hume's successors for supposing it to be insoluble are poor ones.* But assuming that this problem can be somehow solved or dissolved, that we are justified in placing some reliance upon our ordinary methods of induction or of the confirmation of hypotheses, it seems very likely that there are in fact regularities of this complex sort. The only plausible alternative view is that the physical world works merely by statistical laws (which we shall consider in Chapter 9), and that these generate only approximations to regularities of the form discussed here.

Even if we grant, provisionally, that such regularities are involved in at least some cases of causation, we must still question whether all cases of causation involve them, and whether causation ever consists only in the holding of such regularities.

The stock argument for a negative answer to this last question is that we can point to regularities of succession that are not causal: day regularly follows night, but night does not cause day, and is not even a partial cause of day;[13] the sounding of factory hooters in Manchester may be regularly followed by, but does not cause, London workers leaving their work.[14] Mill, being aware of this problem, tried to solve it by saying that causal sequences are not merely invariable but unconditional: night is not unconditionally followed by day, because we can describe changed conditions in which there would be night but day would not follow. But this suggestion is in the end fatal to the theory which it was designed to save. In the first place, Mill himself has stressed that no ordinary causal sequences are unconditional: what we commonly accept as causes are only members of assemblages of conditions, positive and negative, and only such a complete assemblage is unconditionally followed by the effect. But this is a fairly superficial criticism. There is a problem, to which we shall return, why some conditional regularities should be not accepted as cases of the earlier item causing the later, while other regularities, no less conditional, are accepted as cases of causation. But there are more fundamental objections to Mill's way out. We must distinguish *de facto* unconditional regularity from counterfactually unconditional regularity. If night in, say, Glasgow is always in fact followed by day in Glasgow (it is evening in Glasgow when the earth eventually blows up), then this regularity is *de facto* unconditional; Mill's protest that we know of conditions in

[13] T. Reid, *Essays on the Active Powers of Man*; Mill, *System of Logic*, Book III, Ch. 5, Sect. 6.

[14] C. D. Broad, *The Mind and its Place in Nature*, pp. 455–6. Broad's actual example is of workers going *to* work, but the reverse is in some ways more convenient.

which night *would not* be followed by day (for example, those in which the earth blows up just before daybreak in Glasgow) means only that this regularity is not counterfactually unconditional. Mill is claiming, then, that to be causal a regularity must be counterfactually unconditional. But while this may throw some light upon our concept of causation, it cannot apply directly to our present question whether regularity constitutes causation *in the objects*. For the holding of a counterfactual conditional is not a fully objective matter: we must go back from the conditional to whatever grounds make it reasonable to assert it or to suppose it to hold. Now if we find some 'causal mechanism', some continuous process connecting the antecedent in an observed conditional regularity with the consequent, we may be able to sort out some counterfactually unconditional regularities which underlie the conditional one. (This will be discussed and illustrated in Chapter 8.) On the other hand, even if we can find no mechanism, no continuous connecting process, and even if we believe that there is none to be found, we may still assert, and have reason for asserting, that there is some counterfactually unconditional regularity. Russell at one time postulated, though he later rejected, 'mnemic' causation, in which an earlier event (an experience) is the *proximate* cause of a later one (a memory).[15] This would not mean that experiences are unconditionally followed by rememberings, but it presumably would mean that in an appropriate field (which would have to include the survival, and the consciousness, of the subject) some assemblage of conditions, including an experience, was unconditionally followed by the corresponding memory, but without any specific linking mechanism over and above the mere survival of the intermittently conscious subject. This hypothesis may well be false and is indeed, as Russell admitted, extravagant, but it is not incoherent, and we could fairly easily describe experiments and observations which might confirm it by disconfirming alternative explanations of remembering. Such a regularity, then, could be reasonably asserted to be counterfactually unconditional; its instances would then fall under our concept of genuine causal sequences, but all that was present in the objects would be that pattern of regularities and

[15] *The Analysis of Mind*, pp. 78–9; *An Inquiry into Meaning and Truth*, p. 297; 'Reply to Criticisms' in *The Philosophy of Bertrand Russell*, ed. P. A. Schilpp, p. 700.

irregularities, 'agreements' and 'differences', which we reasonably took as confirming this hypothesis and as disconfirming its rivals. Of course, this and all similar hypotheses may well be false; it may be that causation in the objects always does involve continuous processes, but we cannot say that it must do so, that such continuity must be added, even to those regularities which we reasonably take as counterfactually unconditional, in order to make them causal.

However, even if Mill could so far defend the claim that counterfactually unconditional regularity is sufficient for causation, we can refute this claim by using, in a slightly different way, the stock counter-examples to the regularity theory. Typically, these are cases of branched causal patterns.[16] A common cause, the rotation of the earth relative to the sun, is responsible for night in Glasgow, that is, for a period of, say, twelve hours of darkness there, and also for the ensuing day, that is, for a period of, say, twelve hours of light. A similar, though more complicated, account can be given of the Manchester hooters and the London workers. Generalizing, we have the sort of causal pattern that is roughly indicated by this diagram:

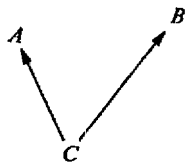

Diagram (iv)

Here C is the common cause, A one effect, and B the other; the pattern is repeated over and over again, but each instance of A occurs just before the associated instance of B. But of course this linear diagram is only rough: in fact other conditions will be conjoined with C to produce A, and others again to produce B: C is presumably only an inus condition of A and of B. We can concede to Mill that the A–B sequence is not unconditional. But it is not this that prevents A from being the (or a) cause of B; for we can find, underlying this, an unconditional sequence which is still not causal. Suppose that the full cause

[16] But these are not the only counter-examples. More thoroughly accidental regularities will be discussed in Chapter 8.

of A is (CX or Y), and the full cause of B is (CZ or W), X and Z being present whenever this whole pattern is instantiated. Then clearly $A\bar{Y}$ is *unconditionally* preceded by C, while CZ is *unconditionally* followed (after a longer time interval) by B; hence $A\bar{Y}Z$ is *unconditionally* followed by B, though $\bar{Y}Z$ presumably is not. There is an unconditional sequence in which the antecedent is an assemblage of conditions of which A is a non-redundant member, and the consequent is B. In more concrete terms, the sounding of the Manchester factory hooters, plus the absence of whatever conditions would make them sound when it wasn't five o'clock, plus the presence of whatever conditions are, along with its being five o'clock, jointly sufficient for the Londoners to stop work a moment later—including, say, automatic devices for setting off the London hooters at five o'clock, is a conjunction of features which is unconditionally followed by the Londoners stopping work. In this conjunction the sounding of the Manchester hooters is an essential element, for it alone, in this conjunction, ensures that it should *be* five o'clock. Yet it would be most implausible to say that this conjunction causes the stopping of work in London. So the antecedent in even an unconditional sequence may fail to cause the consequent. (And this is not because the sequence is logically necessary, though our description may have suggested this. Though I have spoken of whatever conditions are sufficient for this or that, this is only a way of indicating the concrete conditions Y and Z, whatever they may be; Y and Z are not themselves logically related to A, B, and C, though our descriptions of them are; the sequence in which $A\bar{Y}Z$ is followed by B is logically contingent though unconditional.)

Nor can this sort of counter-example be undermined by saying that to be causal a sequence must be such as to be reasonably taken to be counterfactually unconditional. For if the (CX or Y)–A and (CZ or W)–B sequences are counterfactually unconditional, so is the $A\bar{Y}Z$–B one. This sort of counter-example shows, too, that adding a causal mechanism, a continuity of process, is not enough; for if there are such mechanisms or continuities between C and A and between C and B, there will inevitably be a set of mechanisms, a resultant continuous process, linking A with B.

But it is not too difficult to begin to see what the key additional feature is that marks off genuine cause–effect sequences, and that is lacking in this *A–B* counter-example. It is what we spoke of in Chapter 2 as causal priority. In the branched pattern, each instance of *A*, or of $A\bar{Y}Z$, is not causally prior (though it is temporally prior) to the associated instance of *B*. Each *A* is indeed related to its *B* by 'some fact of causation', by what is roughly indicated by the arrows in the diagram; but the *C–A* arrow is pointing the wrong way. The $A\bar{Y}Z$–*B* sequence is causally maintained, but $A\bar{Y}Z$ does not cause *B*. Admittedly this is only a schematic answer, since we have not yet discovered in what causal priority in the objects consists: the account sketched at the end of Chapter 2, in terms of conditionals and possible worlds, may help to identify *our notion* of causal priority, but it falls far short of anything that could be an objective description. But though it is elusive, causal priority can hardly be non-existent. The regularity theorist could rebut this last criticism only if he were prepared to say that there is no difference in the objects between the causal pattern represented, however crudely, by the above diagram and that which would be represented thus:

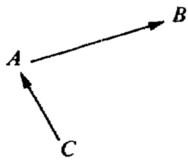

Diagram (v)

—in other words, that there is no difference between a means (or intermediate link in a causal chain) and a side-effect. But this would be most implausible.

We can now return to our first criticism of Mill's suggestion, to the point that some *conditional* regularities are accepted as cases of the earlier item causing the later, while others are not. While a number of considerations (of which some were mentioned in Chapter 2, and others will be touched upon in Chapter 5) restrict the application of the term 'cause' to some inus conditions in preference to others, what is most important in the present context is again the absence of causal priority. The sounding of a hooter in Manchester is not causally prior to Londoners stopping work, while the sounding of a hooter in London is, although both these hooter–stopping regularities are equally conditional, both *de facto* and counterfactually.

In conclusion, then, regularity of the sort we have elucidated, even if it is of a kind that can be called counterfactually unconditional, is not the whole of what constitutes causation in the objects. Some causal mechanism or continuity of process *may be* an additional and distinguishing feature of sequences in which the earlier item causes the later, but whether it is or not it seems certain that something which we can call causal priority is such an additional distinguishing feature. The regularity theory, even in its improved form, is not a complete account of causation in the objects, and as we saw earlier it is not adequate either as an account of what causal statements mean or of what we know about causation. But to say this is by no means to deny that causal regularities, complex and only partly known, contribute something to the answers to all three of these questions.

Those who find regularity theories inadequate commonly insist on some intrinsic necessity in causal relations, or suggest a 'genetic' view of causation, according to which causes produce or generate their effects and are not merely followed by them, or both.[17] But it is not enough to reiterate such words as 'necessity' and 'production' and 'generation'; we need some clear account of what this necessity is, or of what producing or generating can be. Moreover, this account must be able to resist Hume's criticisms, to take up his challenges, to explain how these key relations escaped his notice. Logical necessitation between distinct occurrences or their features is ruled out, and Hume's challenge to his opponents to point out anything like what we have called necessity$_2$ is not easy to meet. Nor, I think, is it at all helpful to say that things have causal *powers*: the concept of powers needs to be elucidated in terms of causation rather than causation in terms of powers.[18] Since what was called, in Chapter 1, Hume's third exclusion or third negative point was poorly supported, there is no serious obstacle to the

[17] For example, A. C. Ewing, *Idealism*, pp. 151–87; W. Kneale, *Probability and Induction*, pp. 70–103; R. Harré, *The Principles of Scientific Thinking*, Chapters 4, 10, and 11.

[18] I have discussed powers and dispositional properties in Chapter 4 of *Truth, Probability, and Paradox*.

description of some empirical relations that might be called producing and generating; but the description needs to be given and, if possible, related in some way to the counterfactual conditionals that are at the heart of the ordinary notion of causing. In Chapters 5 and 8 I shall try to resolve these problems, by both borrowing from and criticizing the work, in particular, of Ducasse and Kneale, while in Chapter 7 I shall investigate that other essential supplement to regularity, the direction of causation.