Causality and Determination

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I

It is often declared or evidently assumed that causality is some kind of necessary connection, or alternatively, that being caused is – non-trivially – instancing some exceptionless generalization saying that such an event always follows such antecedents. Or the two conceptions are combined. Obviously there can be, and are, a lot of divergent views covered by this account. Any view that it covers nevertheless manifests one particular doctrine or assumption. Namely:

> If an effect occurs in one case and a similar effect does not occur in an apparently similar case, there must be a relevant further difference.

Any radically different account of causation, then, by contrast with which all those diverse views will be as one, will deny this assumption. Such a radically opposing view can grant that often – though it is difficult to say generally when – the assumption of relevant difference is a sound principle of investigation. It may grant that there are necessitating causes, but will refuse to identify causation as such with necessitation. It can grant that there are situations in which, given the initial conditions and no interference, only one result will accord with the laws of nature; but it will not see general reason, in advance of discovery, to suppose that any given course of things has been so determined. So it may grant that in many cases difference of issue can rightly convince us of a relevant difference of circumstances; but it will deny that, quite generally, this *must* be so.

The first view is common to many philosophers of the past. It is also, usually but not always in a neo-Humeian form, the prevailing received opinion throughout the currently busy and productive philosophical schools of the English-speaking world, and also in some of the European and Latin American schools where philosophy is pursued in at all the same sort of way; nor is it confined to these schools. So firmly rooted is it that for many even outside pure philosophy, it routinely determines the meaning of 'cause', when consciously used as a theoretical term: witness the terminology of the contrast between 'causal' and 'statistical' laws, which is drawn by writers on physics—writers, note, who would not conceive themselves to be addicts of any philosophic school when they use this language to express that contrast.

The truth of this conception is hardly debated. It is, indeed, a bit of *Weltanschauung:* it helps to form a cast of mind which is characteristic of our whole culture.

The association between causation and necessity is old; it occurs for example in Aristotle's *Metaphysics:* "When the agent and patient meet suitably to their powers, the one acts and the other is acted on OF NECESSITY." Only, with 'rational powers', an extra feature is needed to determine the result: "What has a rational power [e.g. medical knowledge, which can kill *or* cure] OF NECESSITY does what it has the power to do and as it has the power, when it has the desire" (Book IX, Chapter V).

Overleaping the centuries, we find it an axiom in Spinoza, "Given a determinate cause, the effect follows OF NECESSITY, and without its cause, no effect follows" *(Ethics*, Book I, Axiom III). And in the English philosopher Hobbes:

> A cause simply, or an entire cause, is the aggregate of all the accidents both of the agents how many soever they be, and of the patients, put together; which when they are supposed to be present, IT CANNOT BE UNDERSTOOD BUT THAT THE EFFECT IS PRODUCED at the same instant; and if any of them be wanting, IT CANNOT BE UNDERSTOOD BUT THAT THE EFFECT IS NOT PRODUCED. *(Elements of Philosophy Concerning Body,* Chapter IX)

It was this last view, where the connection between cause and effect is evidently seen as *logical* connection of some sort, that was overthrown by Hume, the most influential of all philosophers on this subject in the English-speaking and allied schools. For he made us see that, given any particular cause – or ‘total causal situation’ for that matter – and its effect, there is not in general any contradiction in supposing the one to occur and the other not to occur. That is to say, we’d know what was being described – what it would be like for it to be true – if it were reported for example that a kettle of water was put, and kept, directly on a hot fire, but the water did not heat up.

Were it not for the preceding philosophers who had made causality out as some species of logical connection, one would wonder at this being called a discovery on Hume’s part: for vulgar humanity has always been over-willing to believe in miracles and marvels and *lusus naturae.* Mankind at large saw no contradiction, where Hume worked so hard to show the philosophic world – the Republic of Letters - that there was none.

The discovery was thought to be great. But as touching the equation of causality with necessitation, Hume’s thinking did nothing against this but curiously reinforced it. For he himself assumed that NECESSARY CONNECTION is an essential part of the idea of the relation of cause and effect (A *Treatise of Human Nature*, Book I, Part III, Sections II and VI), and he sought for its nature. He thought this could not be found in the situations, objects or events called ‘causes’ and ‘effects’, but was to be found in the human mind’s being determined, by experience of CONSTANT

CONJUNCTION, to pass from the sensible impression or memory of one term of the relation to the convinced idea of the other. Thus to say that an event was caused was to say that its occurrence was an instance of some exceptionless generalization connecting such an event with such antecedents as it occurred in. The twist that Hume gave to the topic thus suggested a connection of the notion of causality with that of deterministic laws – i.e. laws such that always, given initial conditions and the laws, a unique result is determined.

The well-known philosophers who have lived after Hume may have aimed at following him and developing at least some of his ideas, or they may have put up a resistance; but in no case, so far as I know, 1 has the resistance called in question the equation of causality with necessitation.

Kant, roused by learning of Hume's discovery, laboured to establish causality as an *a priori* conception and argued that the objective time order consists "in that order of the manifold of appearance according to which, IN CONFORMITY WITH A RULE, the apprehension of that which happens follows upon the apprehension of that which precedes .... In conformity with such a rule there must be in that which precedes an event the condition of a rule according to which this event INVARIABLY and NECESSARILY follows" *(Critique of Pure Reason*, Book II, Chapter II, Section III, Second Analogy). Thus Kant tried to give back to causality the character of a *justified* concept which Hume's considerations had taken away from it. Once again the connection between causation and necessity was reinforced. And this has been the general characteristic of those who have sought to oppose Hume's conception of causality. They have always tried to establish the necessitation that they saw in causality: either *a priori*, or somehow out of experience.

Since Mill it has been fairly common to explain causation one way or another in terms of 'necessary' and 'sufficient' conditions. Now 'sufficient condition' is a term of art whose users may therefore lay down its meaning as they please. So they are in their rights to rule out the query: “May not the sufficient conditions of an event be present, and the event yet not take place?” For ‘sufficient condition’ is so used that if the sufficient conditions for *X* are there, *X* occurs. But at the same time, the phrase cozens the understanding into not noticing an assumption. For ‘sufficient condition’ sounds like: ‘enough’. And one certainly *can* ask: “May there not be *enough* to have made something happen—and yet it not have happened?”

Russell wrote of the notion of cause, or at any rate of the ‘law of causation’ (and he seemed to feel the same way about ‘cause’ itself), that, like the British monarchy, it had been allowed to survive because it had been erroneously thought to do no harm. In a destructive essay of great brilliance he cast doubt on the notion of necessity involved, unless it is explained in terms of universality, and he argued that upon examination the concepts of determination and of invariable succession of like objects upon like turn out to be empty: they do not differentiate between any conceivable course of things and any other. Thus Russell too assumes that necessity or universality is what is in question, and it never occurs to him that there may be any other conception of causality (‘The Notion of Cause’, in *Mysticism and Logic).*

Now it’s not difficult to show it prima facie wrong to associate the notion of cause with necessity or universality in this way. For, it being much easier to trace effects back to causes with certainty than to predict effects from causes, we often know a cause without knowing whether there is an exceptionless generalization of the kind envisaged, or whether there is a necessity. For example, we have found certain diseases to be contagious. If, then, I have had one and only one contact with someone suffering from such a disease, and I get it myself, we suppose I got it from him. But what if, having had the contact, I ask a doctor whether I will get the disease? He will usually only be able to say, “I don’t know—maybe you will, maybe not.”

But, it is said, knowledge of causes here is partial; doctors seldom even know any of the conditions under which one invariably gets a disease, let alone all the sets of conditions. This comment betrays the assumption that there is such a thing to know. Suppose there is: still, the question whether there is does not have to be settled before we can know what we mean by speaking of the contact as cause of my getting the disease.

All the same, might it not be like this: knowledge of causes is possible without any satisfactory grasp of what is involved in causation? Compare the possibility of wanting clarification of ‘valency’ or ‘long-run frequency’, which yet have been handled by chemists and statisticians without such clarification; and valencies and long-run frequencies, whatever the right way of explaining them, have been known. Thus one of the familiar philosophic analyses of causality, or a new one in the same line, may be correct, though knowledge of it is not necessary for knowledge of causes.

There is something to observe here, that lies under our noses. It is little attended to, and yet still so obvious as to seem trite. It is this: causality consists in the derivativeness of an effect from its causes. This is the core, the common feature, of causality in its various kinds. Effects derive from, arise out of, come of, their causes. For example, everyone will grant that physical parenthood is a causal relation. Here the derivation is material, by fission. Now analysis in terms of necessity or universality does not tell us of this derivedness of the effect; rather it forgets about that. For the necessity will be that of laws of nature; through it *we* shall be able to derive knowledge of the effect from knowledge of the cause, or vice versa, but that does not show us the cause as source of the effect. Causation, then, is not to be identified with necessitation.

If *A* comes from *B*, this does not imply that every *A*-like thing comes from some *B*-like thing or set-up or that every *B*-like thing or set-up has an *A*-like thing coming from it; or that given *B, A* had to come from it, or that given *A,* there had to be *B* for it to come from. Any of these may be true, but if any is, that will be an additional fact, not comprised in *A*'s coming from *B.* If we take 'coming from' in the sense of travel, this is perfectly evident.

"But that's because we can observe travel!" The influential Humeian argument at this point is that we can't similarly observe causality in the individual case (A *Treatise of Human Nature,* Book I, Part III, Section II). So the reason why we connect what we call the cause and what we call the effect as we do must lie elsewhere. It must lie in the fact that the succession of the latter upon the former is of a kind regularly observed.

There are two things for me to say about this. First, as to the statement that we can never observe causality in the individual case. Someone who says this is just not going to count anything as 'observation of causality'. This often happens in philosophy; it is argued that 'all we find' is such-and-such, and it turns out that the arguer has excluded from his idea of 'finding' the sort of thing he says we don't 'find'. And when we consider what we are allowed to say we do 'find', we have the right to turn the tables on Hume, and say that neither do we perceive bodies, such as billiard balls, approaching one another. When we 'consider the matter with the utmost attention', we find only an impression of travel made by the successive positions of a round white patch in our visual fields ... etc.

Now a 'Humeian' account of causality has to be given in terms of constant conjunction of physical things, events, etc., not of experiences of them. If, then, it must be allowed that we 'find' bodies in motion, for example, then what theory of perception can justly disallow the perception of a lot of causality? The truthful—though unhelpful—answer to the question: 'How did we come by our primary knowledge of causality?' is that in learning to speak we learned the linguistic representation and application of a host of causal concepts. Very many of them were represented by transitive and other verbs of action used in reporting what is observed.

Others - a good example is ‘infect’ - form, not observation statements, but rather expressions of causal hypotheses. The word ‘cause’ itself is highly general.

How does someone show that he has the concept *cause?* We may wish to say: only by having such a word in his vocabulary. If so, then the manifest possession of the concept presupposes the mastery of much else in language. I mean: the word ‘cause’ can be *added* to a language in which are already represented many causal concepts. A small selection: *scrape, push, wet, carry, eat, burn, knock over, keep off, squash, make* (e.g. noises, paper boats), *hurt.* But if we care to imagine languages in which no special causal concepts are represented, then no description of the use of a word in such languages will be able to present it as meaning *cause.* Nor will it even contain words for natural kinds of stuff, nor yet words equivalent to ‘body’, ‘wind’, or ‘fire’. For learning to use special causal verbs is part and parcel of learning to apply the concepts answering to these and many other substantives. As surely as we learned to call people by name or to report from seeing it that the cat was on the table, we also learned to report from having observed it that someone drank up the milk or that the dog made a funny noise or that things were cut or broken by whatever we saw cut or break them.

(I will mention, only to set on one side, one of the roots of Hume’s argument, the implicit appeal to Cartesian scepticism. He confidently challenges us to ‘produce some instance, wherein the efficacy is plainly discoverable to the mind, and its operations obvious to our consciousness or sensation’ *(A Treatise of Human Nature,* Book I, Part III, Section XIV). Nothing easier: is cutting, is drinking, is purring not ‘efficacy’? But it is true that the apparent perception of such things may be only apparent: we may be deceived by false appearances. Hume presumably wants us to ‘produce an instance’ in which *efficacy* is related to sensation as *red* is. It is true that we can’t do that; it is not *so* related to sensation. He is also helped, in making his argument that we don’t perceive ‘efficacy’, by his curious belief that ‘efficacy’ means much the same thing as ‘necessary connection’! But as to the Cartesian-sceptical root of the argument, I will not delay upon it, as my present topic is not the philosophy of perception.)

Secondly, as to that instancing of a universal generalization, which was supposed to supply what could not be observed in the individual case, the causal relation, the needed examples are none too common. ‘Motion in one body in all past instances that have fallen under our observation, is follow’d upon impulse by motion in another’: so Hume *(A Treatise of Human Nature,* Book II, Part III, Section I). But, as is always a danger in making large generalizations, he was thinking only of the cases where we do observe this—billiard balls against freestanding billiard balls in an ordinary situation; not billiard balls against stone walls. Neo-Humeians are more cautious. They realize that if you take a case of cause and effect, and relevantly describe the cause *A* and the effect *B,* and then construct a universal proposition, ‘Always, given an *A,* a *B* follows’, you usually won’t get anything true. You have got to describe the absence of circumstances in which an *A* would not cause a *B.* But the task of excluding all such circumstances can’t be carried out. There is, I suppose, a vague association in people’s minds between the universal propositions which would be examples of the required type of generalizations, and scientific laws. But there is no similarity.

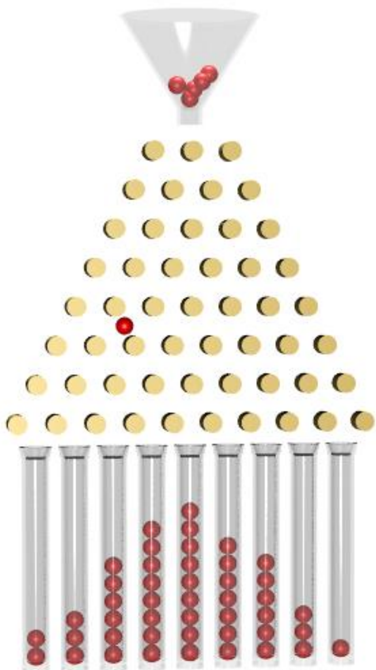
Suppose we were to call propositions giving the properties of substances ‘laws of nature’. Then there will be a law of nature running ‘The flash-point of such a substance is ... ‘, and this will be important in explaining why striking matches usually causes them to light. This law of nature has not the form of a generalization running ‘Always, if a sample of such a substance is raised to such a temperature, it ignites’; nor is it equivalent to such a generalization, but rather to: ‘If a sample of such a substance is raised to such a temperature and doesn’t ignite, there must be a cause of its not doing so.’ Leaving aside questions connected with the idea of a pure sample, the point here is that ‘normal conditions’ is quite properly a vague notion. That fact makes generalizations running ‘Always ... ‘ merely fraudulent in such cases; it will always be necessary for them to be hedged about with clauses referring to normal conditions; and we may not know in advance whether conditions are normal or not, or what to count as an abnormal condition. In exemplar analytical practice, I suspect, it will simply be a relevant condition in which the generalization, ‘Always if such and such, such and such happens ... ‘, supplemented with a few obvious conditions that have occurred to the author, turns out to be untrue. Thus the conditional ‘If it doesn’t ignite then there must be some cause’ is the better gloss upon the original proposition, for it does not pretend to say specifically, or even disjunctively specifically, what *always* happens. It is probably these facts which make one hesitate to call propositions about the action of substances ‘laws of nature’. The law of inertia, for example, would hardly be glossed: ‘If a body accelerates without any force acting on it, there must be some cause of its doing so.’ (Though I wonder what the author of *Principia* himself would have thought of that.) On the other hand just such ‘laws’ as that about a substance’s flash-point are connected with the match’s igniting because struck.

Returning to the medical example, medicine is of course not interested in the hopeless task of constructing lists of all the sets of conditions under each of which people always get a certain disease. It is interested in finding what that is special, if anything, is always the case when people get a particular disease; and, given such a cause or condition (or in any case), in finding circumstances in which people don’t get the disease, or tend not to. This is connected with medicine’s concern first, and last, with things as they happen in the messy and mixed up conditions of life: only between its first and its last concern can it look for what happens unaffected by uncontrolled and inconstant conditions.

II

Yet my argument lies always open to the charge of appealing to ignorance. I must therefore take a different sort of example. Here is a ball lying on top of some others in a transparent vertical pipe. I know how it got there: it was forcibly ejected with many others out of a certain aperture into the enclosed space above a row of adjacent pipes. The point of the whole construction is to show how a totality of balls so ejected always build up in rough conformity to the same curve.

[Anscombe is talking about a Galton board, like this]



But I am interested in this one ball. Between its ejection and its getting into this pipe, it kept hitting sides, edges, other balls. If I made a film of it I could run it off in slow motion and tell the impact which produced each stage of the journey. Now was the result necessary? We would probably all have said it was in the time when Newton's mechanics was undisputed for truth. It was the impression made on Hume and later philosophers by that mechanics, that gave them so strong a conviction of the iron necessity with which everything happens, the 'absolute fate' by which "Every object is determin'd to a certain degree and direction of its motion" (A *Treatise of Human Nature*, Book II, Part III, Section I).

Yet no one could have deduced the resting place of the ball – because of the indeterminateness that you get even in the Newtonian mechanics, arising from the finite accuracy of measurements. From exact figures for positions, velocities, directions, spins and masses you might be able to calculate the result as accurately as you chose. But the minutest inexactitudes will multiply up factor by factor, so that in a short time your information is gone. Assuming a given margin of error in your initial figure, you could assign an associated probability to that ball's falling into each of the pipes. If you want the highest probability you assign to be really high, so that you can take it as practical certainty, it will be a problem to reckon how tiny the permitted margins of inaccuracy must be – analogous to the problem: how small a fraction of a grain of millet must I demand is put on the first square of the chess board, if after doubling up at every square I end up having to pay out only a pound of millet? It would be a figure of such smallness as to have no meaning as a figure for a margin of error.

However, so long as you believed the classical mechanics you might also think there could be no such thing as a figure for a difference that had no meaning. Then you would think that though it was not feasible for us to find the necessary path of the ball because our margins of error are too great, yet there *was* a necessary path, which could be assigned a sufficient probability for firm acceptance of it, by anyone (not one of us) capable of reducing his limits of accuracy in measurement to a sufficiently small compass. Admittedly, so small a compass that he'd be down among the submicroscopic particles and no longer concerned with the measurements, say, of the ball. And now we can say: with certain degrees of smallness we get to a region for which Newton's mechanics is no longer believed.

If the classical mechanics can be used to calculate a certain real result, we may give a sense to, and grant, the 'necessity' of the result, given the antecedents. Here, however, you can't use the mechanics to calculate the result, but at most to give yourself a belief in its necessity. For this to be reasonable the system has got to be acknowledged as true. Not, indeed, that that would be enough; but if so much were secured, then it would be worthwhile to discuss the metaphysics of absolute measures of continuous quantities.

The point needs some labouring precisely because 'the system does apply to such bodies' – that is, to moderately massive balls. After all, it's Newton we use to calculate Sputniks! "The system applies to these bodies" is true only in the sense and to the extent that it yields sufficient results of calculations about these bodies. It does not mean: in respect of these bodies the system is the truth, so that it just doesn't matter that we can't use it to calculate such a result in such a case. I am not saying that a deterministic system involves individual predictability: it evidently does not. But in default of predictability the determinedness declared by the deterministic system has got to be believed because the system itself is believed.

I conclude that we have no ground for calling the path of the ball determined – at least, until it has taken its path – but, it may be objected, is not each stage of its path determined, even though we cannot determine it? My argument has partly relied on loss of information through multiplicity of impacts. But from one impact to the next the path is surely determined, and so the whole path is so after all.

It sounds plausible to say: each stage is determined and so the whole is. But what does ‘determined’ mean? The word is a curious one (with a curious history); in this sort of context it is often used as if it *meant* ‘caused’. Or perhaps ‘caused’ is used as if it meant ‘determined’. But there is at any rate one important difference – a thing hasn’t been caused until it has happened; but it may be determined before it happens.

(It is important here to distinguish between being *determined* and being *determinate.* In indeterministic physics there is an apparent failure of both. I am concerned only with the former.)

When we call a result determined we are implicitly relating it to an antecedent range of possibilities and saying that all but one of these is disallowed. What disallows them is not the result itself but something antecedent to the result. The antecedences may be logical or temporal or in the order of knowledge. Of the many – antecedent – possibilities, *now* only one is – antecedently – possible.

Mathematical formulae and human decisions are limiting cases; the former because of the obscurity of the notion of antecedent possibilities, and the latter because decisions can be retrieved.

In a chess-game, the antecedent possibilities are, say, the powers of the pieces. By the rules, a certain position excludes all but one of the various moves that were in that sense antecedently possible. This is logical antecedence. The next move is determined.

In the zygote, sex and eye-colour are already determined. Here the antecedent possibilities are the possibilities for sex and eye-colour for a child; or more narrowly: for a child of these parents. *Now*, given the combination of this ovum and this spermatozoon, all but one of these antecedent possibilities is excluded.

It might be said that anything was determined once it had happened. There is now no possibility open: it *has* taken place! It was in this sense that Aristotle said that past and present were necessary. But this does not concern us: what interests us is *pre*-determination.

Then “each stage of the ball’s path is determined” must mean “Upon any impact, there is only one path possible for the ball up to the next impact (and assuming no air currents, etc.).” But what ground could one have for believing this, if one does not believe in some system of which it is a consequence? Consider a steel ball dropping between two pins on a Galton board to hit the pin centred under the gap between them. That it should balance on this pin is not to be expected. It has two possibilities; to go to the right or to the left. If you have a system which forces this on you, you can say: “There has to be a determining factor; otherwise, like Buridan’s ass, the ball must balance.” But if you have not, then you should say that the ball may be undetermined until it does move to the right or the left. Here the ball had only two significant possibilities and was perhaps unpredetermined between them. This was because it cannot be called determined – no reasonable account can be given of insisting that it is so – within a small range of possibility, actualization within which will lead on to its falling either to the right or to the left. With our flying ball there will also be such a small range of possibility. The further consequences of the path it may take are not tied down to just two significant possibilities, as with one step down the Galton board: the range of further possibility gets wider as we consider the paths it may take. Otherwise, the two cases are similar.

We see that to give content to the idea of something’s being determined, we have to have a set of possibilities, which something narrows down to one – before the event. This accords well with our understanding of part of the dissatisfaction of some physicists with the quantum theory. They did not like the undeterminedness of individual quantum phenomena. Such a physicist might express himself by saying “I believe in causality!” He means: “I believe that the real physical laws and the initial conditions must entail uniqueness of result.” Of course, within a range of co-ordinate and mutually exclusive identifiable possible results, only one happens: he means that the result that happens ought to be understood as the only one that was possible before it happened.

Must such a physicist be a 'determinist'? That is, must he believe that the whole universe is a system such that, if its total states at *t* and *t*' are thus and so, the laws of nature are such as then to allow only one possibility for its total state at any other time? No. He may not think that the idea of a total state of the universe at a time is one he can do anything with. He may even have no views on the uniqueness of possible results for whatever may be going on in any arbitrary volume of space. For "Our theory should be such that only the actual result was possible for that experiment" doesn't mean "Our theory should be such that only this result was possible as *the result of the experiment.*" He hates a theory, even if he has to put up with it for the time being, that essentially assigns only probability to a result, essentially allows of a range of possible results, never narrowed down to one until the event itself.

It must be admitted that such dissatisfied physicists very often have been determinists. Witness Schrodinger's account of the 'principle of causality': "The exact physical situation at *any* point P at a given moment is unambiguously determined by the exact physical situation within a certain surrounding of *P* at any previous time, say $t - \tau$. If τ is large, that is, if that previous time lies far back, it may be necessary to know the previous situation for a wide domain around *P*." Or Einstein's more modest version of a notorious earlier claim: if you knew all about the contents of a sphere of radius 186,000 miles, and knew the laws, you would be able to know for sure what would happen at the centre for the next second. Schrodinger says: *any* point *P;* and *a* means *any* sphere of that radius. So their view of causality was not that of my hypothetical physicist, who I said may not have views on the uniqueness of possible results for whatever may be going on in any arbitrary volume of space. My physicist restricts his demand for uniqueness of result to situations in which he has got certain processes going in isolation from inconstant external influences, or where they do not matter, as the weather on a planet does not matter for predicting its course round the sun.

The high success of Newton’s astronomy was in one way an intellectual disaster: it produced an illusion from which we tend still to suffer. This illusion was created by the circumstance that Newton’s mechanics *had a good model in the solar system.* For this gave the impression that we had here an ideal of scientific explanation; whereas the truth was, it was mere obligingness on the part of the solar system, by having had so peaceful a history in recorded time, to provide such a model. For suppose that some planet had at some time erupted with such violence that its shell was propelled rocket-like out of the solar system. Such an event would not have violated Newton’s laws; on the contrary, it would have illustrated them. But also it would not have been calculable as the past and future motions of the planets are presently calculated on the assumption that they can be treated as the simple ‘bodies’ of his mechanics, with no relevant properties but mass, position and velocity and no forces mattering except gravity.

Let us pretend that Newton’s laws were still to be accepted without qualification: no reserve in applying them in electrodynamics; no restriction to bodies travelling a good deal slower than light; and no quantum phenomena. Newton’s mechanics is a deterministic system; but this does not mean that believing them commits us to determinism. We could say: of course nothing violates those axioms or the laws of the force of gravity. But animals, for example, run about the world in all sorts of paths and no path is dictated for them by those laws, as it is for planets. Thus in relation to the solar system (apart from questions like whether in the past some planet has blown up), the laws are like the rules of an infantile card game: once the cards are dealt we turn them up in turn, and make two piles each, one red, one black; the winner has the biggest pile of red ones. So once the cards are dealt the game is determined, and from any position in it you can derive all others back to the deal and forward to win or draw. But in relation to what happens on and inside a planet the laws are, rather, like the rules of chess; the play is seldom determined, though nobody breaks the rules.

Why this difference? A natural answer is: the mechanics does not give the special laws of all the forces. Not, for example, for thermal, nuclear, electrical, chemical, muscular forces. And now the Newtonian model suggests the picture: given the laws of all the forces, then there is total coverage of what happens and then the whole game of motion is determined; for, by the first law, any acceleration implies a force of some kind, and must not forces have laws? My hypothetical physicist at least would think so; and would demand that they be deterministic. Nevertheless he still does not have to be a 'determinist'; for many forces, unlike gravity, can be switched on and off, are generated, and also shields can be put up against them. It is one thing to hold that in a clear-cut situation – an astronomical or a well-contrived experimental one designed to discover laws – 'the result' should be determined: and quite another to say that in the hurly-burly of many crossing contingencies whatever happens next must be determined; or to say that the generation of forces (by human experimental procedures, among other things) is always determined in advance of the generating procedure; or to say that there is always a law of composition, of such a kind that the combined effect of a set of forces is determined in every situation.

Someone who is inclined to say those things, or implicitly to assume them, has almost certainly been affected by the impressive relation between Newton's mechanics and the solar system.

> We remember how it was in mechanics. By knowing the position and velocity of a particle at one single instant, by knowing the acting forces, the whole future path of the particle could be foreseen. In Maxwell's theory, if we know the field at one instant only, we can deduce from the equations of the theory how the whole field will change in space and time. Maxwell's equations enable us to follow the history of the field, just as the mechanical equations enabled us to follow the history of material particles ....

> With the help of Newton's laws we can deduce the motion of the earth from the force acting between the sun and the earth.

'By knowing the acting forces'- that must of course include the *future* acting forces, not merely the present ones. And similarly for the equations which enable us to follow the history of the field; a change may be produced by an external influence. In reading both Newton and later writers one is often led to ponder that word 'external'. Of course, to be given 'the acting forces' is to be given the external forces too and any new forces that may later be introduced into the situation. Thus those first sentences are true, if true, without the special favour of fate, being general truths of mechanics and physics, but the last one is true by favour, by the brute fact that only the force acting between earth and sun matters for the desired deductions.

The concept of necessity, as it is connected with causation, can be explained as follows: a cause *C* is a necessitating cause of an effect *E when* (I mean: on the occasions when) if *C* occurs it is certain to cause *E* unless something prevents it. *C* and *E* are to be understood as general expressions, not singular terms. If 'certainty' should seem too epistemological a notion: a necessitating cause *C* of a given kind of effect *E* is such that it *is not* possible (on the occasion) that *C* should occur and should not cause an *E*, given that there is nothing that prevents an *E* from occurring. A non-necessitating cause is then one that can fail of its effect without the intervention of anything to frustrate it. We may discover *types* of necessitating and non-necessitating cause; e.g. rabies is a necessitating cause of death, because it is not possible for one who has rabies to survive without treatment. We don't have to tie it to the occasion. An example of a non-necessitating cause is mentioned by Feynman: a bomb is connected with a Geiger counter, so that it will go off if the Geiger counter registers a certain reading; whether it will or not is not determined, for it is so placed near some radioactive material that it may or may not register that reading.

There would be no doubt of the cause of the reading or of the explosion if the bomb did go off. Max Born is one of the people who has been willing to dissociate causality from determinism: he explicates cause and effect in terms of dependence of the effect on the cause. It is not quite clear what ‘dependence’ is supposed to be, but at least it seems to imply that you would not get the effect without the cause. The trouble about this is that you might – from some other cause. That this effect was produced by this cause does not at all show that it could not, or would not, have been produced by something else in the absence of this cause.

Indeterminism is not a possibility unconsidered by philosophers. C. D. Broad, in his inaugural lecture, given in 1934, described it as a possibility; but added that whatever happened without being determined was accidental. He did not explain what he meant by being accidental; he must have meant more than not being necessary. He may have meant being uncaused; but, if I am right, not being determined does not imply not being caused. Indeed, I should explain indeterminism as the thesis that not all physical effects are necessitated by their causes. But if we think of Feynman’s bomb, we get some idea of what is meant by ‘accidental’. It was random: it ‘merely happened’ that the radioactive material emitted particles in such a way as to activate the Geiger counter enough to set off the bomb. Certainly the motion of the Geiger counter’s needle is caused; and the actual emission is caused too; it occurs because there is this mass of radioactive material here. (I have already indicated that, contrary to the opinion of Hume, there are many different sorts of causality.) But all the same the *causation* itself is, one could say, *mere hap.* It is difficult to explain this idea any further.

Broad used the idea to argue that indeterminism, if applied to human action, meant that human actions are ‘accidental’. Now he had a picture of choices as being determining causes, analogous to determining physical causes, and of choices in their turn being either determined or accidental. To regard a choice as such – i.e. any case of choice – as a predetermining causal event, now appears as a naïve mistake in the philosophy of mind, though that is a story I cannot tell here. It was natural that when physics went indeterministic, some thinkers should have seized on this indeterminism as being just what was wanted for defending the freedom of the will. They received severe criticism on two counts: one, that this ‘mere hap’ is the very last thing to be invoked as the physical correlate of ‘man’s ethical behaviour’; the other, that quantum laws predict statistics of events when situations are repeated; interference with these, by the *will’s* determining individual events which the laws of nature leave undetermined, would be as much a violation of natural law as would have been interference which falsified a deterministic mechanical law.

Ever since Kant it has been a familiar claim among philosophers, that one can believe in both physical determinism and ‘ethical’ freedom. The reconciliations have always seemed to me either to be so much gobbledegook, or to make the alleged freedom of action quite unreal. My actions are mostly physical movements; if these physical movements are physically predetermined by processes which I do not control, then my freedom is perfectly illusory. The truth of physical indeterminism is thus indispensable if we are to make anything of the claim to freedom. But certainly it is insufficient. The physically undetermined is not thereby ‘free’. For freedom at least involves the power of acting according to an idea, and no such thing is ascribed to whatever is the subject (what would be the relevant subject?) of unpredetermination in indeterministic physics. Nevertheless, there is nothing unacceptable about the idea that that ‘physical haphazard’ should be the only physical correlate of human freedom of action; and perhaps also of the voluntariness and intentionalness in the conduct of other animals which we do not call ‘free’. The freedom, intentionalness and voluntariness are not to be analysed as the same thing as, or as produced by, the physical haphazard. Different sorts of pattern altogether are being spoken of when we mention them, from those involved in describing elementary processes of physical causality.

The other objection is, I think, more to the point. Certainly if we have a statistical law, but undetermined individual events, and then enough of these are supposed to be pushed by will in one direction to falsify the statistical law, we have again a supposition that puts will into conflict with natural laws. But it is not at all clear that the same train of minute physical events should have to be the regular correlate of the same action; in fact, that suggestion looks immensely implausible. It is, however, required by the objection.

Let me construct an analogy to illustrate this point. Suppose that we have a large glass box full of millions of extremely minute coloured particles, and the box is constantly shaken. Study of the box and particles leads to statistical laws, including laws for the random generation of small unit patches of uniform colour.
Now the box is remarkable for also presenting the following phenomenon: the word ‘Coca-Cola’ formed like a mosaic, can always be read when one looks at one of the sides. It is not always the same shape in the formation of its letters, not always the same size or in the same position, it varies in its colours; but there it always is. It is not at all clear that those statistical laws concerning the random motion of the particles and their formation of small unit patches of colour would have to be supposed violated by the operation of a cause for this phenomenon which did not derive it from the statistical laws.

It has taken the inventions of indeterministic physics to shake the rather common dogmatic conviction that determinism is a presupposition, or perhaps a conclusion, of scientific knowledge. Not that that conviction has been very much shaken even so. Of course, the belief that the laws of nature are deterministic has been shaken. But I believe it has often been supposed that this makes little difference to the assumption of macroscopic determinism: as if undeterminedness were always encapsulated in systems whose internal workings could be described only by statistical laws, but where the total upshot, and in particular the outward effect, was as near as makes no difference always the same. What difference does it make, after all, that the scintillations, whereby my watch dial is luminous, follow only a statistical law- so long as the gross manifest effect is sufficiently guaranteed by the statistical law? Feynman's example of the bomb and Geiger counter smashes this conception; but as far as I can judge it takes time for the lesson to be learned. I find deterministic assumptions more common now among people at large, and among philosophers, than when I was an undergraduate.

The lesson is welcome, but indeterministic physics (if it succeeds in giving the lesson) is only culturally, not logically, required to make the deterministic picture doubtful. For it was always a mere extravagant fancy, encouraged in the 'age of science' by the happy relation of Newtonian mechanics to the solar system. It ought not to have mattered whether the laws of nature were or were not deterministic. For them to be deterministic is for them, together with the description of the situation, to entail unique results in situations defined by certain relevant objects and measures, and where no part is played by inconstant factors external to such definition. If that is right, the laws' being deterministic does not tell us whether 'determinism' is true. It is the total coverage of every motion that happens, that is a fanciful claim. But I do not mean that any motions lie outside the scope of physical laws, or that one cannot say, in any given context, that certain motions would be violations of physical law. Remember the contrast between chess and the infantile card game.

Meanwhile in non-experimental philosophy it is clear enough what are the dogmatic slumbers of the day. It is over and over again assumed that any singular causal proposition implies a universal statement running 'Always when this, then that'; often assumed that true singular causal statements are derived from such 'inductively believed' universalities. Examples indeed are recalcitrant, but that does not seem to disturb. Even a philosopher acute enough to be conscious of this, such as Davidson, will say, without offering any reason at all for saying it, that a singular causal statement implies *that there is* such a true universal proposition – though perhaps we can never have knowledge of it.

Such a thesis needs some reason for believing it! ‘Regularities in nature’: that is not a reason. The most neglected of the key topics in this subject are: interference and prevention.