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PREDICTABILITY AND EXPLANATION IN THE SOCIAL SCIENCES

by
Alasdair MacIntyre

The aim of this paper is to elicit, if possible, a refutation of its argument. The argument which it embodies is so simple and crude and rough hewn that one is inclined to say either that it must rest on a misunderstanding or that perhaps, while the premises are true and the logic impeccable, the outcome is in fact trivial. But of course, if so, I want to know why. This paper is therefore an appeal from the unsophisticated to the suotive. It may be supposed that I am being ironic; but the only irony is self-directed. If what I am now saying is true, surely it is obviously true; but if it is obviously true, why have I taken so long to see it? Those who can tell hawks from hangschaws better than I can may do me some service.

Kepler's account of the regularities in the movements of the planets is sometimes treated by historians of science as simply a step along the way that runs from Copernicus towards Galileo and Newton. But I take its importance to be rather more than this. It is only when the facts have been observed to be of a certain kind that science can aspire to explain them. In a world without regularity of course there would be no foothold for explanation at all. But even in a world with regularities it is only those of a certain kind that set the characteristically scientific task. The daily sequence of light following darkness following light is not of the requisite kind; the regularities in the planetary movements, specified as Kepler specified them, and the regularities in the relationship of gas pressures, temperatures, and volumes, specified as Boyle and Dalton specified them, are. What is the difference?

If you conjoin Kepler's statement of the laws of planetary motion with a statement of the relative positions of the planets and the sun at a given time, you can specify both forwards and backwards the condition of the planetary system. If you conjoin the gas equations with a statement of the volume and temperature of a particular sample of a particular gas, you will be able to specify what given changes in pressure will produce or have produced in the way of changes in volume and temperature. It is this specification of identifiable particulars through time that provides science with what it has to explain.

It is of course true that we can often only carry through such specifications by means of theoretical concepts and laws. But were it not for the fact that there are such pretheoretical examples as the two I have just cited, we should have no starting-point for theory at all. This is not of course to say that the relevant observations could have been made by the light of nature; Kepler's laws and the gas laws are works of art which organise our observations of the planets or of gases so as to make them available for science.

Philosophers of science have argued over the question of whether explanation...
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and prediction are tied together logically or not. In this paper I do not intend to join in this argument, at least directly. For I am concerned not with the character of explanation but with the character of what has to be explained and what can be explained; and my account so far of what can be explained already entails one important consequence. The regularities which are available for scientific explanation, so I have asserted, involve the specification of the character of identifiable particulars under certain conditions, backwards and forwards in time. If we cannot do this, we have not isolated a phenomenon for enquiry successfully. So Kepler's laws and the gas equations involve being able to specify what is behaviour of a particular planetary system of a particular gas as distinct from an alien intrusion (such as the exertion of gravitational attraction by a body outside the system; pre-Keplerian astronomy could not have enabled us to do this adequately). But if this is so, then isolating the phenomenon for explanation already involves the discovery of a certain predictability. Were such predictability lacking, there would be no phenomenon eligible for explanation and hence no warranted scientific explorations.

It is only a matter of contingent fact that nature is so constructed that such predictability is not lacking. Had the dispersion of matter in the universe, the speed of light or the laws of geometrical optics varied in some obvious ways, then Kepler could never have made the observations necessary to frame his laws. Nature could have been as law-governed as it is now, but we could not have known the fact. Suppose that this were indeed the case and that somehow or other in such a universe philosophers could exist. Then one can imagine a debate over the possibility of natural science in which one side argues that the failure to discover the laws of nature arises because in principle the formulation of such laws is impossible or at least inappropriate for the explanation of natural events, while their opponents argue that the contingent failure to supply such laws may be remedied at any time, that there is no logical or conceptual barrier to the formulation of such laws.

Taking such a God's eye view of this imaginary debate as I have done, it is easy to see how these two views are not quite as sharply opposed as their proponents would take them to be. The denial of the possibility of formulating laws of nature is perfectly compatible with allowing that nature is in fact law-governed. This suggests that in the far from imaginary philosophical debate over the possibility of genuine social science, there is a third position waiting to be stated, parallel to that of the God's eye view spectator in the imaginary debate over natural science. Perhaps it is the case that even if social phenomena are law-governed, we could not know it. But how could this be so?

It would be so if we could not isolate the relevant phenomena in the relevant way; and we could not do this if there was too much unpredictability in the behaviour of the phenomena. Explicability requires predictability. This is a major premise of my argument.

The particulars which as a matter of fact are the objects of enquiry by social scientists are not isolated for study in the way that planetary systems or gases
are; social classes and political parties, for example. The identification of these particulars by the social scientist is made via the identification made by the agents who engage in the relevant activities. The social scientists' concepts attempt to capture phenomena which are partially constituted by and in terms of the concepts of social and political agents. But in the activities which embody their concepts agents extend, reinterpret and reshape those concepts. The arguments about the rise of the middle classes or those as to whether the British parliamentary groupings of the eighteenth century were parties rest on a genuine problematic in the application of the key concepts which is embodied in the concepts themselves. Thus at any given point in the history of such particulars as classes and parties there is an openness towards the future which involves an openness toward the future application of the concept.

This openness requires as a necessary condition for its occurrence, but is not to be identified with, the openness of texture of the concepts involved, remarked on by Waismann and others. To say that the concept of gold has an open texture is to say that we cannot determine completely in advance any set of necessary and/or sufficient conditions for its application in particular circumstances. To say that the concept of party is open is to say that debate as to the character and application of the concept of party is inherent in the activities of those to whom in their organised political life the concept applies. The openness has to do with the undetermined and unpredictable outcomes of such debates. Whence this unpredictability? It has at least three sources.

The first is the unpredictability of creative innovation within a rational tradition. The point is a familiar one in a variety of formulations, of which Sir Karl Popper's has been as influential as any. For my present purposes it is most usefully formulated as follows. If we had available to us a method for predicting what new theorems in a particular area of mathematics would be discovered next year, what we would actually possess would be a method for discovering those theorems now. We should in fact possess just that mechanical heuristic device which we have very good reason to believe to be impossible. But its impossibility is not the paramount consideration. It is rather that in trying to specify a method of prediction about the future we find that we have specified a method of discovery in the present, thus putting any prediction out of court.

What holds of theorems holds also of theories, of theatrical works and of theologies. In the case of natural science we may note that it is a consequence of the present point that although such a scientist may be able to predict the course of nature, we cannot predict what he will predict in the future, for we cannot predict what theories will inform and warrant his predictions; and indeed he himself for the same reason cannot predict what he will predict. Natural science is not just one among those social practices which possess the openness to the future of which I have spoken, but it is indeed a paradigm case of such a practice. But if we cannot at any point predict the future content of science, then we cannot predict its applications either.

As with science and technology, so obviously with literature and with art.
The notion of having been able to predict the writing of *King Lear* or the painting of Rembrandt's self-portraits has indeed an oddity all its own. But it is not of course only particular works of which this is the case. Innovations in genre are even more striking. The transition from the choral singing and miming of the major Attic festival in honour of the god Dionysus in the late sixth century B.C. to the first authentic tragedies of the early fifth century B.C. is the kind of transition which we do not know even how to begin to subsume under any generalisation that would yield predictions.

This then is the first type of unpredictability in human affairs which I want to consider. Let me in connection with it raise a difficulty which may also be raised in connection with the types of unpredictability that I shall consider later, and which therefore it would be well to deal with early in the argument. It may be suggested that the considerateness which I have been adducing curiously and unfortunately resemble some of those which provided premises for eighteenth century versions of the Argument from Natural Design to the Existence of God. For there it was suggested that certain structures, those of the eye or the honeycomb for example, or of certain crystalline formations had a kind as well as a degree of complexity that forbade their subsumption under any generalisations that would have connected them with antecedent natural states. There was indeed in the eighteenth century a contingent absence of observed regularities of the requisite kind; but this was not because such regularities were unobservable, but only because they were as yet unobservable. It required the theoretical focus and the power of observation of a Darwin, but no more than these, to remove the difficulty. The structures of Greek tragedy or of theoretical innovations await explanation now, so this argument concludes, just as the structures of the eye or the honeycomb once did.

This argument against my position may be understood in both a strong version and a weak version. The strong version I shall repudiate, the weak I shall accept. The strong version I repudiate because the problem created by human unpredictability is not one of accounting either for complexity as such or for novelty as such. The impossibility of predicting the elaboration of the Darwinian theory that enables us to subsume the creation of the structure of the honeycomb under a set of generalisations is not of the same order as the impossibility of predicting the structure of the honeycomb. In the case of the honeycomb, and of the eye, the problem is that of explaining the structure of the members of a class of particulars which have already been identified as possessing a law-governed structure, just as a planetary system or a particular gas do. In the case of Greek tragedies and scientific theories we have been able to identify no such class of particulars, and it is as a barrier to such identification that the unpredictability associated with intellectual and aesthetic creativity has been cited. Hence I am in a position to reject outright the counter-argument to my own in the strong version in which I formulated it.

If however the argument were to amount to no more than this: that the history of science is a history of conceptual innovation and that because I cannot foresee future conceptual innovations I cannot hope to show that any task of explanation is impossible in principle, then I not only accept the argument,
but it is indeed one entailed by my own position. This position entails a refusal to accept the dichotomy: either merely contingently impossible or impossible in principle (logically impossible, as is sometimes said). Arguments in philosophy are not like arguments in logic, where statements of the form p u q.—q bring certain chains of formulae to a crucial end, although sometimes they just are arguments in logic. I therefore do not regard the weaker version of the objection to my position as an objection at all, but rather as an elucidation.

The second type of unpredictability in human affairs with which I am concerned is that which springs from the making of decisions. Just as in characterising the first type I was elaborating a point made by others and especially by Sir Karl Popper, so in this second point I am elaborating on a view advanced by H.L.A. Hart and Stuart Hampshire. Hart and Hampshire had suggested that when an agent believes that he will make some future decision he cannot predict that decision on inductive grounds. David Pears has however argued cogently that this is not true unrestrictedly and in order to meet his arguments I wish to exclude from consideration at least two kinds of example. The first is that in which the agent can predict the outcome of his own decision under some description other than that which characterises one of the alternative courses of action which he is posing to himself. If in a chess game I am trying to decide which of two alternative moves to make and I am told that yesterday my brother faced precisely the same choice in a precisely similar situation the agent, on grounds of past experience, may be able to predict confidently that he will make the same move that his brother made (Pears' example). I therefore restrict my argument to the class of cases where no description of the outcome of the agent's choice is available to the agent which allows for such contingent predictions, that is to say where no description of the outcome is available to the agent other than that which specifies one of his alternatives. Secondly Pears considers the possibility of a case where there is complete knowledge available to the agent of his own beliefs and desires insofar as they are relevant to the choice. Again I do not want to enter into argument over this possibility. So I shall restrict myself further to that class of cases in which the agent has conflicting desires and no means of attaching relative weights to them. There certainly are such cases, where the agent may well feel that he will only know what he wanted most when he has chosen. Combining these two restrictions yields a set of decisions of which the agent cannot predict the outcome. It may at once be said that this is not unpredictability at all; for from the fact that an agent cannot predict the outcome of a certain class of his own decisions, it does not follow that others cannot predict such outcomes. This is of course true, but it is also misleading. For consider what follows simply from the fact that an agent cannot predict the outcome and therefore the consequences of one subset of his own decisions. Let us suppose that up to a certain point in time A observes and predicts with complete success the actions of agent B, although there is a subset of agent A's own decision which agent A cannot predict. For this state of affairs to continue agent A must know that he, agent A, will not at any point have an impact on agent B which changes B's behaviour as a result of the unpredictable outcome of a causal chain which includes at least one of agent A's unpredictable-by-A
decisions. But agent A cannot know what future decisions of this same kind he will have to take and therefore he cannot predict whether or not his decisions and their outcomes will affect B's behaviour. But it follows from this not only that whole segments of A's behaviour are unpredictable by A, but that in consequence B's future behaviour is unpredictable by A also. This result is clearly of general application. For all agents there is a set of decisions which render at the very least not only large parts of their own future behaviour, but also large parts of the behaviour of others, unpredictable. This is the second type of unpredictability.

The third type of unpredictability is again one in which the inability of agents to predict each others' behaviour results in a corresponding inability of the observer, but in this case possible future interaction between the observer and the agents is not part of the matter. Many characteristic human situations have the following three characteristics: first, A's decision as to what he will do will be affected by his predictions as to what B and C will do, while B's and C's decisions will be affected in corresponding ways; secondly, all three agents know this to be the case; and thirdly, all three agents are therefore involved in a process of revising their decisions in accordance not only with their predictions of what the other agents will decide, but, prior to that, their predictions of what the other agents will predict that they will predict.

Politicians, trade union leaders and generals are often in this type of situation; diplomats, poker players and those who have just fallen in love always are. There are of course occasions where a standard game-theoretical decision, say a maximin decision, is open to all the participants and known by all to be available to all. But there are many occasions when this is not the case. When it is not the case, either because of imperfections in the information possessed by some or all of the participants or because there is no one uniquely rational solution for some or all of the participants, then the agents cannot hope to predict each others' decisions and consequent actions.

Suppose that it is suggested that an external observer might be in a superior position to the participant in this respect. For this to be so, it would have to be the case that if the observer were to become a participant he would be in, so to speak, a winning position, or at the very least the highly advantageous one of being able to predict in a way superior to the other participants. (A great deal of political science, just because it supposes a vantage point for the observer superior to that of the participants seems to entail that, other things being equal, the political scientist would be more successful at political action than those when he is studying. To doubt this therefore commits one to doubting the claims of such political science.) But in the type of case that I have specified, it follows from the specifications that no participant can be in a winning position at the outset, for no participant can overcome the barrier of unpredictability. The observer's predictions would thus be subject to all those contingencies of the other participants' predictions which derive from their lack of knowledge of each other's predictions, whatever other advantages in superior information he might possess. Thus there is here again a type of unpredictability which affects the observer as well as the agent.
These three types of unpredictability are ineliminable from the behaviour of agents in such collectivities as social classes, political parties, cities or armies and in such institutions and practices as those of art, science, medicine, witchcraft and philosophy. Because this is so, we cannot specify in advance what belongs to any such particular collectivity or any such particular institution, practice, or tradition and what is external to it, in the way in which we can specify in advance what movements or changes belong to a particular planetary system or a particular gas and what movements or changes will be the affect of some external agency. But from this it follows that we cannot treat counter-examples to our proposed specification of a particular as we do in natural science. When a \textit{prima facie} counter-example to any scientific hypothesis is produced, it is necessary to be able to allot the \textit{prima facie} counter-example to one of four classes: the class of those in the light of which the specification ought to be substantially rejected; that of those in the light of which it ought to be modified in respect of its content; that of those in the light of which it ought to be modified in respect of its scope; and that of those in the light of which by invoking an auxiliary hypothesis, in support of which in turn independent evidence can be produced, the original specification can be left in tact.

Now to allocate a \textit{prima facie} counter-example to one of these four classes we must have framed our original specification by means of generalisations the scope of which is clear; and if the generalisations that we use in the social sciences do not permit us to distinguish between changes which are part of the behaviour of the particular we aspire to specify from changes which are the effect of some external agency we are obviously precluded from framing such generalisations. It is, I believe, in part a failure to perceive this point that has led many social scientists who aspire to the condition of natural scientists to be so cavalier with \textit{prima facie} counter-examples. This last part of my argument amounts to the contention that the type of unpredictability involved in the life of those particulars characteristically studied by the social sciences entails a failure to meet the standards of refutability required in natural science. Note that I am once again speaking not of the canons for framing theories or explanations, but of the canons for specifying the particulars the behaviour of which theories are propounded in order to explain. To note this is to note also that both the major premise and the minor premise of my argument have now been formulated. The major premise, it will be recalled, was that to be a subject for scientific explanation a particular must behave with a certain type of predictability; the minor premise is that the particulars characteristically studied by social scientists do not behave with the relevant type of predictability. I pause for a moment however before drawing the conclusion.

III

Something, it may be suggested, must have gone wrong with the argument. For nature is not in fact predictable in the way that I might have been taken to be suggesting and social behaviour often is in fact predictable. We regularly fail to predict storms and earthquakes; we do predict successfully from time to time the falls of regimes and the outcomes of elections. Consider each of these cases in turn.

When we successfully predict natural events on the basis of science we not
only need adequate information, but information of a specific kind. Earthquakes are unpredictable, for example, not only because our explanations are so far incomplete, but because our knowledge of the condition of the particular gases (I mean the particular gases, not the particular types of gas) and the particular geological strata cannot be organised in time series, so that the caused effectiveness of the gas acting on the stratum can be computed. But notice that this inability is characterisable only in terms of an ability to identify the relevant particulars as law-governed and predictable. Hence the present actual unpredictability of earthquakes not only is not incompatible with the kind of predictability I have ascribed to natural events, but in fact requires it. So it is with the unpredictability of nature in general.

The predictability of social behaviour is of course a familiar and central aspect of human life. Could not behaviour be rendered predictable in very large measure, social life as we know it would not exist. It is the drive towards regularity which enables us at a common-sense level to frame generalisations which in turn enable us to predict adequately enough for social purposes, that is to say, by and large and for the most part. But these generalisations do not resemble those of the natural sciences in crucial respects; we cannot specify their scope precisely, and we cannot say with any degree of precision what counterfactuals they do or do not entail. When we discover exceptions we not only cannot make the kind of allocation of prima facie counter-examples which natural scientists make, but to try to do so would be to suppose that we are able to characterise the exceptions in a way that we cannot. Moreover these socially necessary, socially useful and far from wholly reliable generalisations create a characteristic form of human behaviour to which there is no analogue in nature. We may use these generalisations not only to predict the behaviour of others, but in order to defeat their predictions of our behaviour. We may intentionally and purposely, once we have recognised the generalisations on which another is relying, use them to surprise and delight or disappoint him. That is to say, the type of predictability that we possess in social life embodies just the type of unpredictability that I specified earlier. Moreover my arguments about refutability at the close of the last section, as well as my arguments about the relation of the agent to the observer earlier in that section, show that the social scientist cannot in this respect transcend the situation of the agent. It thus turns out that the type of predictability which social behaviour does possess not only is not incompatible with, but is inseparable from the type of unpredictability relevant to my argument.

I can therefore proceed to draw the conclusion that follows from my major premise and my minor premise. Explicability entails a certain type of predictability; the particulars characteristically studied by social scientists do not possess that type of predictability; it follows that they are not available for scientific explanation. Consequently the aspiration to construct theories of a scientific or quasi-scientific kind in this area must fail. But is this conclusion important? It might indeed be accepted, but treated as unimportant for either of the following two reasons. First, it might be conceded that social scientists do make social classes, parties and the like the particular objects of their enquiry.
All that my argument shows is that they ought not to; social behaviour must be grasped in terms of some other way of organising our observations. But this negative remark has force only insofar as some alternative is formulated which is free from the kind of objection that I have set out. Certainly no present part of social science seems to be thus free; role theory, enquiries into the behaviour of small groups, the theory of organisations all fail in the same way.

A second response might be to challenge not my account of social science but my restricted notion of explanation. Of course, so it might be agreed, the natural sciences provide no model for the explanation of human behaviour. But all that we need is an alternative schematism. Once again the challenge must be: provide it. My own view is that when once we have understood the true grounds for rejecting the view that social behaviour is explicable in the way that natural events are, there turns out to be no way of characterising the relevant particulars which is not ineliminably historical and that the continuities through space and time of social life have to be understood in terms of specifically historical categories. Sociology, political science and—more contentiously—perhaps both economics and psychology will turn out to be less fundamental disciplines than history. To establish this thesis goes far beyond the scope of the present paper; but if the thesis of the present paper turned out to be an essential preliminary for establishing the latter it could not be denied a certain importance.