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Morrison: Do Social Events Defy Scientific Prediction?

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DO SOCIAL EVENTS DEFY SCIENTIFIC PREDICTION?

by

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In a recent essay, Professor Alasdair Macintyre adduces the following syllogism to show that the particulars studied by social scientists are not available for scientific explanation: “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.” In defending his thesis, its author explicated the intended type of predictability as that exemplified by states of the solar system as interpreted through Kepler’s laws, and by states of a body of gas as dealt with in Boyle’s law. In short, he appears to have adopted as the touchstone of scientific explanation and prediction the ideal of the computability of one quantifiable natural state from another in accordance with what today are called causal laws—an ideal defended in another vocabulary by the astronomer Laplace in the heyday of Newtonian science.

Now I readily agree with the author’s feeling that social scientists to date have not produced an impressive number of causal laws correlating individual states of selected social systems in the Laplacian way in which Kepler’s and Boyle’s laws correlate states of selected physical systems. I further agree with the presumption that scientific explanation and prediction are intimately related, and that computation involving the application of general formulae to quantifiable reports of observed particulars forms an indispensable part of the more responsible predictions made by any scientist.

Having said this much, however, I must add that I cannot accept the conclusion of his syllogism. For if I have understood the context in which he introduces it, he is equating the particulars characteristically studied by social scientists with the only particulars which it will ever make sense for them to study—social events, or events with social relevance. But then, his conclusion, considered in the full context of his essay seems to suggest the following: There is not, and cannot be, any such thing as a scientific explanation or prediction of anything social; and hence, there can never be, strictly speaking, any social science or sciences. At best, the serious social investigator can only arrive at a body of common sense generalities about man as a social being—generalities whose numerous exceptions in application render them perennially valueless as instruments for scientifically respectable explanation or prediction.

As I see it, however, investigators in the natural and social sciences alike have for some time been using rigorous calculative techniques of prediction other than those fashionable in the age of Newton.

If I have exaggerated the primary thrust of Professor MacIntyre’s presentation, I nevertheless hope to collapse the distinction which he would have us make between natural and human or social particulars. For, on one hand, I
believe that I can show his reverence for the laws of Kepler and Boyle to rest on putative relationships between particulars which those laws do not express. And on the other, while I shall readily concede that the statistical laws generated by more recent scientific techniques do not yield predictive success in each instance of their application, I shall maintain that these statistical laws bear little further resemblance to our common sense generalizations about man, society, and nature. It may be, of course, that mundane verbal generalities of the latter sort help to direct and to perpetuate a laudable and even indispensable social cohesion, both because of their almost automatic production and propagation and because of their specious aura of universal applicability. In contrast with methodologically-controlled statistical laws, however, they understandably do little to promote that scientific coherence thanks to which men may often reliably predict, and sometimes successfully manage, otherwise intractable social or natural particulars of an unusually foreboding or inviting mien.

My chief aim in this paper will be to make a critical analysis of the minor premise of the valid syllogism cited in my first paragraph. I shall endeavor to show that if there are any reasons why social events are not scientifically predictable, Professor MacIntyre has not uncovered them in the essay in which he presents that syllogism. Moreover, in making my critique, I shall defend the view that scientific prediction is as endemic in the social sciences as it is in any of the other natural sciences.

In support of his premise to the effect that the particulars studied by social scientists are not predictable, that is, not computable with the help of causal laws, Professor MacIntyre claims three kinds of unpredictability with respect to human behavior. They are: (1) the unpredictability of creative innovation in a rational tradition; (2) the impossibility of predicting, on the basis of advance knowledge, one’s own future predictions; and (3) the inability of any participant in a sufficiently complex competition to foresee at the outset who will win it.

A consideration of each of these points in turn may help both to bring out the claimant’s underlying view of the method of natural science and to clarify his conviction that serious social investigators can never attain to genuinely scientific predictions.

(1) Let us first consider the claim that our inability to predict any creative innovation in a rational tradition somehow tells against the possibility of genuine prediction by social scientists. When we reflect that the particulars which it is the business of any scientist to predict are precisely those which exhibit repeatable regularities of simultaneous or successive connection, it is easy to see that the fact that we cannot readily predict a creative innovation has little probative value for or against the hypothesis that a predictive social science is impossible.

For, first of all, the phrase ‘creative innovation’ refers in each of its typical utterances to a particular context in which a thing of a certain kind, K, happens for the first time. And regardless of how many subsequent contexts may include happenings of the same kind, none of these further contexts will have the requi-
site spatiotemporal structure. For each of them will be contexts involving preceding occurrences of a no-longer-novel kind. Only when we disregard the element of temporal primacy, and with it, the element of creative innovation, does the kind, K, come into its own as a repeatable regularity instances of which science may aspire to predict.

Where K is a kind of (social or individual) human behavior, then, it is not repeated instances of K that are unpredictable because of creative innovation. Rather, it is the property Lone-First-Instance-of-K, which if it belongs to any particular belongs to at most one,\(^8\) which is synonymous with the property Creative-Innovation-of-K. In other words, while the one and only particular, if any, which has the property Creative-Innovation-of-K is also one of the particular contexts each of which has the property K, the overlapping properties in question which this particular manifests are not the same. On the other hand, the plausibility of the argument from our inability to predict the creative innovation of K to our alleged inability to predict random repeatable particulars which exhibit K would appear to depend, in one sense, on an unnoticed equivocation between the concept K and the concept Lone-First-Instance-of-K.

It may be urged in rebuttal that the foregoing argument, far from removing an impediment to social scientific prediction, actually only reinforces it. For in conceding the unpredictability of unique social particulars of undeniable importance to man, those with the property Creative Innovation, one is simply adding to the evidence against the scientific predictability of social particulars. My retort, at this point, is to concede that it is most unlikely that creative innovations as a class will ever be discovered to manifest a causal or statistical regularity of connection with some independently identifiable trait. Consequently, I would further admit that we will scarcely be able to predict, with any substantial degree of success, the occurrence of specific innovations. Still, I do not see that our inability to discover rational techniques for predicting social contexts of this undeniable complexity detracts from the possibility of social prediction in general, any more than our inability to discover methodologically sound procedures for predicting physical particulars in contexts of comparable complexity totally rules out prediction in the natural sciences of geophysics, astrophysics, or meteorology.

In the latter connection, Professor MacIntyre would argue, I suspect, that the two situations are not comparable. For on his view, the unpredictability of an earthquake, or of any other particular in nature (excluding "human nature"), resides not so much in the complexity of the context in which accurate information about a future occurrence is sought, as it does in the momentary technical unavailability of information about sufficiently simple aspects of relevant starting particulars from which, in conjunction with suitable causal laws, an accurate computation of the character of a future particular could regularly be made.\(^9\) For reasons which will emerge shortly, however, this is not what is holding up successful scientific prediction of those complex social particulars, which, allegedly, we can never hope to achieve.

Next, let us turn to the view that the impossibility of successfully predicting, on the basis of one's own advance knowledge, what one will subse-
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quently predict contributes, at least indirectly, to the undermining of the possibility of scientifically predicting any social particulars whatever. As in the first case, there is a strong presumption that any context within which a person might want to predict what he would subsequently predict will be almost impossibly complex.

In the present case, however, a new notion has been added: that of "advance knowledge." The suggestion, here, is that the natural scientist, but not the social investigator, can arrive by an unimpeachable computation, applying causal laws to an observed state of a system, at actual foreknowledge of a particular future state of that system.

Moreover, in the light of Professor MacIntyre's characterization of the remediable unpredictability of nature, presumably in contrast to the irremediable unpredictability of human nature, the further suggestion is that no natural particular lies forever, or in principle, outside the scope of possible scientific prediction in a Laplacian framework. And this means, I suspect, that any future natural particular, given the prior discovery of relevant causal laws, is a possible object of "advance knowledge." In the case of future human or social particulars, however, this would presumably never be the case. But that would be tantamount to holding that while natural science in the narrow classical sense, science in the style of Kepler and Boyle, virtually gives us certainties about future matters of fact, the social sciences, because in principle they can never do this, can never achieve the status of genuine predictive sciences.

The flaw in this position, however, is that neither Kepler's laws, nor Boyle's law, nor any other causal generalization in natural science ever serves as a conceptual springboard to incontestable pronouncements about future particulars. For while it is true that careful applications of well-confirmed causal laws yield predictive success more often than do careful applications of well-confirmed statistical laws, neither sort of law asserts more than a contingent connection of the independently observable attributes of the particulars of which it speaks.

Consider for a moment what a Laplacian prediction of a future state of the solar system involves. We begin with a suitable collection of singular observation statements which hopefully specify a contemporary instantaneous state of that system in a way that makes the laws of planetary motion applicable to them. Next, we compute from our observationally based state description, together with those laws, what we justifiably believe will be the observable momentary state of the same system a year hence. Now since the sun is currently a class-G star, there is a good probability that it may explode at any time in the "astronomical present"—a period of about four billion years. The probability that it will do so within the next year is, of course, minimal. But still, it is not ruled out completely. Should the sun become a supernova in less than twelve months from the time we make our justifiable prediction, then, our pronouncement would have conveyed anything but foreknowledge. For most of what had been the solar system, including the sun itself, would then be only a body of superheated gas. There would be no solar system at the moment specified by the prediction, even though our computation from Kepler's laws and observed particulars would still have been flawless. But after the cataclysm, it would turn out to have been a
false prediction all along. For it would have predicted a never-to-be particular.

Since any causal law of natural science may be applied legitimately in analogous situations to yield predictions which later turn out to be false, it seems likely that "advance knowledge" will not be forthcoming automatically from that quarter. Nor does the fact that causal laws, almost all of which belong to the physical and biological sciences, in each case "isolate" a kind of particular for inquiry account for the preponderance of their successful predictive yield over that of statistical laws. For whether it be social, biological, or physical, a statistical law isolates the independently-identifiable traits to which it refers just as much as a causal law does. To sum up, an utterance conveying advance scientific knowledge of particulars can only be "known" to have done so after those particulars have occurred. Advance certainties about matters of fact of any sort are still as unattainable now, after the advent of natural science, as they were before it. And false predictions either about man or about other aspects of nature can still be scientific predictions. For whether true or false, a scientific prediction about particulars is only a singular assertion derived by suitably applying our best methodologically-controlled generalizations, causal or statistical, to current state descriptions of relevant social or other natural systems of particulars.

(3) What confirmation does the alleged scientific unpredictability of social particulars receive from the fact that, in a sufficiently elaborate contest, none of the competitors can initially single out the winner? Of course, if we accept the nineteenth century Laplacian model of prediction as an undisputed paradigm for predictive technique in the twentieth century, this third item of testimony against the predictability of social particulars gains a certain air of credibility. But in actual fact, what appears as a handicap to social investigators on the nineteenth century view shows up as an advantage to them in contemporary methodology. For an initial uncertainty as to the outcome of a development in process—an uncertainty often attributable to insufficient starting data—typically spurs a researcher or a competitor on to uncover data of independent kinds which, in conjunction with the original data, radically diminishes the uncertainty.

Moreover, epistemic processes of this sort need not be viewed as eccentric. Indeed, they provide only one small illustration of a trait of stochastic processes of every kind. For it is a well-established generalization that however uncertain the initial tendency of any such process in a social or other natural system, that process will almost always subside, after a sufficient number of random changes of state, into a so-called "basin state"—a state of equilibrium in which the system tends to persist monotonously from then on.11

In fact, the keynote of the new statistical viewpoint in the empirical sciences is that order and regularity of the sort so admired in the nineteenth century is, from a chronological standpoint, the typical result of a long succession of random changes of state, whose tendency emerges ever more clearly as the Markov chain to which they belong gets longer and longer.12 And individual developmental or biographic sequences are obviously as important in the natural sciences as they are in the social ones.

Whether or not the empirical scientists of today customarily make explicit
use of the methods and viewpoint which lie at the heart of systems theory, however, they all employ statistical techniques of prediction of one sort or another. It is no coincidence that academic majors in the physical, biological, and social sciences alike are required to study statistics, frequently in conjunction with experimental design, early in their scientific training. For the foregoing reasons, among others, it is difficult for me to accept the premise that the only techniques of prognostication which qualify as scientific are those Laplacian ones which make the least use of statistical laws.

III

I have yet to deal with Professor MacIntyre's paramount indictment of the methodology of social prediction. For what he refers to in one place as three types of human unpredictability, he refers to earlier in his essay as three sources of a single type of unpredictability, a type which allegedly results from an “openness” peculiar to human or social concepts in distinction to natural ones. As he initially describes it, this openness is primarily a matter of the indeterminacy, at any particular moment, of the future application of any concept with a human or social referent.

But this aspect of openness will obviously not suffice to distinguish open social concepts from open physical or biological ones. For in the case of Boyle's law, itself, the concept of the kind of gas to which it applies has displayed a marked variability with respect to future application over its three-hundred year history. The formula

\[ \frac{P \cdot V}{T} = K \]

where 'P' stands for pressure, 'V' for volume, 'T' for absolute temperature, and 'K' for a specific numerical constant, has changed its meaning time and again during the last three centuries. The only thing that has retained its fixity during that period is the formula itself, together with its continued use in an open succession of “changing” contexts. There is no indication, even yet, that the current interpretation of that formula will not change again. As one philosopher of science remarks, Boyle's law was originally “said to apply to the air, later to gases, and since then our notion of what is to count as a gas has been continually modified. At very low pressures the law applies to all gases; at pressures nearer normal it applies, at any given temperature, to some gases and not to others; in some cases it applies exactly, in others approximately, and in others not at all.” If openness to future application is not at work even in the case of this causal law about natural particulars, I am at a loss to grasp what classical law of nature would succeed in avoiding such openness.

Professor MacIntyre appears to avoid my conflation of open natural concepts with open social ones, however, by stating that the openness of all social concepts is distinctive in being perpetuated by endless debates among social investigators who, themselves, are unavoidably designated by the very concepts over
whose scope they perpetually disagree.

My answer to the charge of ineliminable partial self-reference in such debates is that the natural scientist fares no better in this regard than does the social scientist. For as Moritz Schlick once remarked, one must breathe while discussing breathing. An obvious consequence is that two biologists who disagree over the most suitable technical meaning for the expression ‘pulmonary respiration’ in physiological generalizations must breathe while debating that issue. But the suggestion was that the inescapable self-reference of a descriptive expression subjected to a semantic dispute of this kind disqualifies it for use in a scientific prediction. If so, practitioners of the natural science of biology are forever interdicted from making scientific predictions about this important pulmonary process just as clearly as social scientists are allegedly barred from ever making scientific predictions about societal particulars. But surely, there is no more merit in the one charge than there is in the other.

Let us turn, now, to the other part of the alleged criterion for discriminating the presumably incurable openness of social concepts from the less lethal openness of “natural” ones. I refer, of course, to the suggestion that every human or societal concept is the subject of a semantic debate, and that all such debates, where societal concepts are at issue, are interminable. It would follow from this that serious students of human affairs could never arrive at any predictive laws, since they could never arrive at any fixed concepts which might be employed in such laws. For the undetermined and unpredictable outcome of debates over the extension of these concepts would perpetually block scientific prediction about social events.14 Is there no escape, then, from this attribution of perpetual ambiguity in concepts of human or social behavior? I personally think that there is, for two different reasons.

In the first place, I will freely concede that a small number of very general societal concepts, the most prominent of which, perhaps, are the words ‘society’ and ‘culture’, suffer from an incurable vagueness. For if they are not the subjects of overt, incessant semantic disputes among social scientists, they are tacitly regarded by the members of any human community as referring to something that endures, and will endure, through any structural changes, however drastic, short of total biological annihilation of the entire population.15 On the other hand, it strikes me that a large number of human or social predicates of more practical scope (for example, ‘insult’, ‘challenge’, ‘obedience’, and the like) are open neither to the sort of semantic contention, nor to the endless chameleon-like changes in scope, to which, on Professor MacIntyre’s account, all of our societal concepts are subject. But if not, then there is no automatic bar to their occurrence in possible laws—causal or statistical—about such matters.

In the second place, let us consider the claim that any particular societal concept is constantly shifting its reference, or that it is constantly ambiguous, thanks to the ongoing debate about its “proper” coverage. If it is, what of it? Students of nature have managed to develop natural laws in spite of similar disputes, and despite comparable multiple interpretations of their key predicates. Consider, for example, the controversy between Hobbes and Boyle over whether any “air” remains in the evacuated chamber of a vacuum pump. It is obvious
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that that dispute, which centered on the meaning of the word ‘air’, did not stop Boyle from formulating his famous law, at first, about air, rather than about gas. (The term ‘gas’ was subsequently coined by van Helmont.)

The point of the foregoing remarks is that talk of competitive or shifting meanings of a natural or societal concept, when properly adjusted, becomes talk about competing simultaneous or successive referents for a particular linguistic expression. Moreover, nothing in Professor MacIntyre’s argument requires that each of a set of competitive proposed meanings for the same expression, whether they be simultaneous or successive, be obscure simply because it belongs to such a congeries.

During the period when empirical science and the metaphysics of nature were unwittingly conflated by all parties under the label “natural philosophy,” scientists like Boyle achieved a methodological insight which philosophers like Hobbes could not. It had to do with working out a professional agreement, at least during a limited truce period, to entertain one relatively clear meaning of an expression suspected to have predictive merit, in preference to a host of other meanings, pending the outcome of relevant predictive tests.

The discovery of this scientifically profitable stratagem was, at first, not recognized, as it is today, to involve a distinction between two different functions of declarative assertions using predicates referring to space-time particulars: their use as unfalsifiable meaning conventions, and their use as falsifiable contingent assertions. The fact that a number of established social investigators today still do not appreciate this difference would have come as no surprise even to Auguste Comte and his disciples. For as the early positivists saw it, physical and biological investigators were to employ the scientific mode of explanation (in preference to the metaphysical one) long before students of societal events were to do so.

In my own opinion, it is the current inability of certain established investigators of social particulars to distinguish their descriptive meaning decisions from their contingent conjectures, rather than some kind of conceptual indeterminacy lying eternally beyond man’s remedial powers, which is hampering their personal attempts to discover social laws—whether causal or statistical—that may be used effectively in scientific prediction.
Alasdair MacIntyre, "Predictability and Explanation in the Social Sciences," in this volume.

2Ibid., Section III.

3Ibid., Section III, where the author speaks of the importance, for predicting an earthquake scientifically, of knowing enough about a particular gas to be able to compute its caused effectiveness in acting on a geological stratum.

4Philosophers of science today customarily distinguish between causal laws and statistical laws referring, in either case, to contingent correlations of space-time occurrences. Laws of both kinds are typically understood to assert hypothetically that if a specifiable state of affairs of one kind holds in a certain space-time region, an independently-identifiable state of affairs of some other kind is contingently correlated with it. (The independently-identifiable kinds of particulars mentioned in generalizations of either kind need not always be measurable magnitudes, although they have always been so in the more powerful and useful scientific laws.) A causal law further asserts such a hypothetical correlation to hold invariably and universally. On the other hand, while a statistical law does not assert that the correlation holds in all cases, it does state that the contingent connection characterizes a definite percentage, or a definite range of percentages (usually greater than fifty percent) of the cases denoted by the hypothesis of that generalization.

5On the other hand, I cannot agree that they have produced none at all. For the law of supply and demand (the law stating that in a perfectly elastic market, supply and demand determine the price of a commodity) seems to me to have a quantifiable structure quite comparable to that of Boyle's law relating the pressure, volume, and absolute temperature of "ideal" gases.

6Ibid., Section III.

7Ibid., Section II.

8The existential qualification is important. Because if two or more initial instances of K happen simultaneously, neither of them is "the" creative innovation of K. We would have to concede, in such limiting cases, in which novelty is possible without a unique creator, that creative novelty, since vacuous, would then be unpredictable on yet another count.

9Ibid., Section III, where the author characterizes "the unpredictability of nature in general."

10Ibid., Section II.

11Cf. W. Ross Ashby, Introduction to Cybernetics. New York: Wiley, 1957, p. 23. Of course, if the system in question is part of an initial, unsettled phase of a larger system, it may undergo further change. But once again, a monotonous uniformity will almost surely emerge after enough random changes of state have occurred in the larger system.

12Ibid., pp. 165-169.


14A. MacIntyre, op. cit., Section II.