The Science of History

Given at Philosopher's Holiday
Vassar College
Poughkeepsie, New York
April 13, 1973
by
Richard T. Hull, Ph.D.

Introduction

It seems customary these days for papers on the philosophy of history to be one of two sorts: either they are forays into the enormous complex of literature on the nature of historical explanation, or they consist in elaboration and application of some particular theory of history. For example, discussion of the nature of historical explanation is frequently couched in terms of a dispute between Hempelians (followers of Carl G. Hempel's account of explanation in the sciences0 and anti-Hempelians. Hempelians insist on a rationalist model for all scientific explanation, the deductive-nomological scheme, wherein descriptions of events to be explained are deductively subsumed under statements of general laws and descriptions of initial relevant conditions. Anti-Hempelians deny that the DN-model of explanation in the physical sciences is to be applied to all explanatory disciplines as an ideal which they must approximate, maintaining that all that is required in history (and other social sciences as well) for complete explanations is "coherent narrative," sometimes involving the statement of specific conditions that are necessary, or perhaps jointly sufficient, for the event being explained. The point is that no universally quantified law-statements are required.

Again, we find theories of history concerned with identifying the chief level of historical process, with disagreements revolving around the Great Man thesis, the thesis of National Character, the Economy theses, and various teleological, idealistic theses.

The general result of these approaches has been the emergence of a plurality of views, whose proponents either engage actively in the thrust and parry of philosophical combat or ignore one another in a sort of studied contempt. Few have essayed an overview; not many have attempted to step out of their particular context of concerns to gain a perspective on the sources of these pluralities and disagreements, especially with reference to the subject matter of history itself.

It seems to me that there are far more serious issues faced by the historian in his reflective moments, issues having to do with the identity of the discipline. Shall we say that the historian, insofar as his task is unique and unlike that of others, is merely one of discovering on the basis of available evidence what the past was like? If so, history is an explanatory science only insofar as it borrows its explanatory schemes from the true sciences of human behavior; in other words, it is reducible to psychology, sociology, and the like. On the other hand, if history is to take its place as a *bona fide* discipline with its own distinctive subject matter and explanatory patterns, it must discover justification for certain non-reductionist claims whereby it can show that its descriptions and explanations are not all applications of terms and law statements found in other sciences. In other words, the identity of history as a science in its own right, presupposed by many discussions over the nature of historical explanation, turns upon the demonstration of its non-reducibility to other sciences. But more of these matters presently.

What I propose to do is to locate some possible sources of these disputes by means of first describing in most general terms one commonly held, reasonably

intuitive view of the physical universe, its laws, and our ideal knowledge of it; then surveying the possibilities there seem to be in such a physical universe for emergence of new features and new laws that are not reducible to the features and laws picked out by physics; and finally indicating some likely restrictions on the results of the inquiry. Having done this, we will be in a position to gain some perspective on the divisive issues in both historical explanation and theories of history.

Process Knowledge in Newtonian Physics

In Newtonian physics, the ideal of scientific knowledge is process knowledge. Process knowledge of some subject matter consists in an ideal theory which, together with a description of the relevant features of the subject matter at a given time, permits deduction of a description of the relevant features of the subject matter at any other time. For there to be an ideal theory[1] about a system of entities, several conditions must be met. (1) The various law statements of the theory should be arranged in such a way that some (few, hopefully) of them stand as axioms to the rest, which are called theorems, and only true law statements and all true law statements for the system are theorems of those axioms. All of this requirement amounts to saying that the axioms should be independent, and must be consistent and complete in stating the governing laws of the system. (2) The axioms must contain at least one process law which permits computation of any state of a *basic system* of the entities at any moment from the state of that basic system at any other moment. A basic system is one such that the values of its variables at any time t_o completely determine the values of its variables at any time t_n (where n is other than o). (3) The axioms contain cross-sectional laws that state functional connections among the values of various variables at the same moment, irrespective of time. (4) The axioms also contain composition laws. Such laws have three parts. (a) The first specifies how the system is to be decomposed (in imagination, through thought experiments) into basic systems. (b) The second specifies the characteristics of the (imagined) basic systems that are to be computed by means of the process laws for those basic systems. © The third states how to obtain the process law for the entire system from the results of (b). Composition laws are thus process laws of a derivative sort.

By way of illustration, consider a schematic of Newtonian mechanics for a system of three bodies. (1) One process permits us to compute the position and velocity of any basic system of two bodies, considered as point-masses (i.e., where their spatial dimensions bay be ignored), at some time n, given their positions and velocities at 0. This is the theory's basic process law. (2) The cross-sectional laws of such a theory will contain, among others, the laws of an interpreted geometry — in newton's case, Euclidean geometry. Such laws permit, for example, the determination of values of the position of a point mass in terms of all three Cartesian coordinates, given knowledge of two of the coordinates and the angle subtended by the line of one coordinate and the line through the intersection point of the two known coordinates and the point-mass. (3) The composition law for the 3-bodied system (a) first indicates that the initial state of that system is to be decomposed (in imagination) into the initial states of the three 2-bodied basic systems contained in it; (b) directs that for each of the imagined basic systems, the acceleration of each body is to be calculated by the basic process law and expressed in terms of its positive and negative vector components; © states that the process law for the 3-bodied system is that the results of these calculations applying to any

1. Much of what I say in the following paragraphs is based on work done by Gustav Bergmann, in his *Philosophy of Science* (1967), Chapters 2 & 3 *passim.*

given point-mass are to be added algebraically to obtain the net acceleration of that point-mass in the system of 3 members.

While a composition law may hold for any of a number of systems of increasing complexity, it is possible that it may break down at some point at a new level of complexity. An examination of the ways in which such breakdowns may occur serves to illustrate the notion of emergence.

Suppose a particular composition law applies successfully to systems of less than 1,000 bodies, but, when applied to a system of 1,000 bodies or more, it is found that the old composition law yields predictions about the motions of some body that deviate from a description of its actual movements, to a degree not dismissible as being due to experimental or observational error. That is, the predictions for one or more bodies' movements at time n in a system of 1,000 bodies, yielded by its composition laws together with descriptions of the positions and velocities of those bodies at time o, are false. At least four possibilities could follow upon such a situation.

(A) We may search for and find no process law for 1,000-bodied systems; our conclusion is that there is none. Hence, the behavior of bodies in the system obeys laws of some form other than process ones.

(B) We may find a process law for such systems, but it involves variables that were not relevant for less complex systems. Such newly relevant variables may or may not also be newly occurrent; that is, they involve characteristics that may or may not be found in less complex systems.

(C)We may find a process law for such systems that involves only those variables relevant tot he theory's successful application to less complex systems, but this process law cannot be made by the composition law out of the basic process laws.

(D) We may find a process law for the system that can be made out of the basic process laws but not in the way prescribed by the composition law for systems of fewer bodies. (For example, part © of the composition law may involve a functional relation other than algebraic addition.)

For the purposes of illustration, I have talked about breakdowns in composition laws when applied to systems of the same sorts of entities that differ only in numerical complexity. But composition laws may also break down when applied to systems of different kinds of entities. Consider what happens when Newtonian dynamics are applied to quanta.

We are now prepared for a discussion of the concept of emergence. I can think of at least four sorts of emergence involving the breakdown of a composition law.

(1) The first sort of emergence consists in a particular level or kind of complexity being such that one (or more) aspect(s) of the behavior of its elements no longer is described by the old composition law statements. We may not conclude that there is no functional interrelation any longer among its variables, either from the failure of the old composition law or from our failure to find any new process law. It may seem that there is no process law which governs the behavior of the elements of a system of the proper sort or degree of complexity, but we are not free to invoke the belief that the behavior is now strictly random, or unconditioned. (This is so because of the fact that for *any* sequence of events, there is a generating function which will predict that sequence together with a description of any other event that might occur next in that sequence, as a function of some variable (such as position in the sequence). As one author[2] has put it, "To say that nature is uniform in *some* respects is trivial." What we may well discover is that only-necessary-

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2. Brian Skyrms, *Choice and Chance*, 1966, p. 67.

condition or only-sufficient-condition relationships now hold between those states of the system between which necessary-and-sufficient-condition relationships held before.

(2) The emergence of a new process law (as in (C), (D), and some versions of (B), above) which is not derivable from the axioms of the theory, signals for revision of the theory by the addition of one or more axioms. This may well require no more than adding the new process law to the old axioms; however, this need not be the case, as when one encounters in a complex theory the breakdown of a number of the theory's composition laws. In this case (or even in the case of the breakdown of one composition law), we may find that a radical revision of the theory, amounting to its replacement with a new theory, results in a set of axioms from which those of the old theory may be derived as special cases or as only approximate truths. (I think it is fair to say that this is what happened when Einstein's general theory of relativity superceded Newtonian mechanics; however, some[3] would argue that the old theory isn't derivable even as a special case, since each theory defines all its own terms by placing them in the law statements of that theory — i.e., contextually. Since those laws differ in the two theories, the terms no longer mean the same, and a law statement embodying a term from one theory is thus not derivable from law statements of the other theory. This argument involves some important confusions between the descriptive and explanatory significance of terms, which confusions I have detailed elsewhere.[4]

(3) One may find that a characteristic occurs at the new level of complexity which does not occur in less complex systems, which interacts with other variables of the system, but which is definable in terms of the descriptive language required for full description of those less complex systems. For example, being a cube is a characteristic of certain systems of rigid rods, nuts and bolts which is not a property of any elements in the system, nor of any numerically less complex systems. Even so, it can be fully analyzed (the term 'cube' fully defined) in terms of the variables already appearing in the descriptive vocabulary adequate for simpler systems.

(4) Finally, we have the possibility that a characteristic or variable occurs in the new system which does not occur in simpler systems, which interacts with other relevant variables of the system, and which is indefinable in the vocabulary adequate to full description of simpler systems. The traditional view known as interactionism fits this characterization: the emergent mental characters (or, in some versions, substances) interact with the physical systems that they are associated with in such a way that the future states of those systems are other than would be predicted by the composition law that was successful for less complex systems. The breakdown of that composition law, then, is due to the interaction of the emergent character with the system's other characteristics.

On the other hand, epiphenomenalism maintains that there does emerge such an unanalyzable characteristic at certain levels of complexity which does *not* interact with the variables of those systems. In such a situation we do not have a breakdown of the composition law at the level of complexity of the emergence of the new characteristic. The characteristic is a byproduct of the system, but the original theory's power to predict that system's future physical states is not affected by such emergence. It will of course be true that the theory is unable to explain the emergent. More specifically, one would not be able to predict either at what level of complexity a new character would emerge, or what the new character would be,

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3. For example, Paul K. Feyerabend, "Explanation, Reduction, and Empiricism," in *Minnesota Studies in the philosophy of Science*, vol. 3, ed. by H. Feigl *et al.*, 1962.

4. I have argued at length against this confusion in my "Feyerabend's Attack on Observation Sentences," *Synthese* 23, 1972.

given only the process, cross-sectional, and composition laws for the original theory together with the description of the new system. So, if one's aim is a theory which does yield such predictions, the axioms of the original theory must be enriched by one or more new cross-sectional laws which express the functional dependence of the emergent character(s) upon the numerical complexity of the physical system and the relevant configurations and variable values of its constituent members.

The inverse correlate of the situations we have been describing is that of the reduction of one theory to another. Suppose we have two theories, T_1 and T_2, each of whose applications is conducted independently of the other. We discover that the undefined descriptive terms of T_1 apply just when certain terms of T_2 apply (the latter may be defined or undefined. Let us call the statement of such a correspondence of two such terms a *coordination rule.* We also discover that wen we so coordinate the terms of the two theories, every law statement of T_1 is in correspondence with a theorem of T_2, such that the truth-functional relation of material equivalence holds between all law statements of T_1 and the corresponding theorems of T_2. Finally, we discover that some undefined non-logical terms of T_2 are not coordinated with any terms of T_1, and that some theorems of T_2 cannot be put into correspondence with law statements of T_1 (i.e., that some theorems of T_2 are not materially equivalent to theorems of T_1).

Given that these relationships hold between the two theories, we may dispense with T_1 for purposes of prediction and explanation. For, in the situation that has been described, we can make predictions and explanations of the states of T_1's domain of application using only the laws of T_2 and the coordination rules (supplemented, of course, by the relevant statements of initial conditions). To put it differently, we have explained the laws of T_1's domain of application by means of T_2 supplemented with the coordination rules. Hence, the theory obtained by adding those coordination rules to the axioms of T_2 (call it T3) is adequate for explaining all events in the union of the domains of T_1 and T_2.

All that the foregoing sort of reduction shows is that the statement of the composition and basic process laws of the systems of entities which is the domain of T_1 can be obtained from those of T_2 *when the latter is supplemented by the proper coordination rules*. What it does not show is that the two domains are identical. For example, suppose T_1 is some psychological theory of perception and T_2 is an ideal neurophysiology. Suppose further that the terms of the psychological theory are such that they apply just when certain defined terms of the neurophysiological theory apply. Suppose further that, using the appropriate coordination rules, we are able to dispense with the law statements of the psychological theory. All that is shown by such reduction is that there is a correspondence between mind and body, between the psychological facts of perception and certain neurophysical facts of brain activity. Nothing in the situation shows that there is no mind, no distinctively psychological fact or domain.[5]

For the latter sort of reduction to take place, the coordination rules must either be definitions, or they must coordinate terms that have the same criteria for application and otherwise unspecifiable meanings. A decision under any other conditions, in the face of such a successful reduction, to dispense with references to the domain of the first theory is justifiable only on pragmatic grounds, for limited purposes. Any simplifying ontological conclusion drawn on the basis of such a reduction is done so invalidly.

Let us summarize the results of our enquiry so far. There are two fundamental sorts of emergence of which we must take note: the emergence of one or more

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5. This claim, that such causal criteria as mind/body correlation are compatible with and irrelevant to various ontological positions is defended in my "on Taking Causal Criteria to be Ontologically Significant," *Behaviorism*, Spring, 1973.

characteristics not found in simpler systems, and the emergence of one or more new laws not holding for simpler systems. Each of these, in turn, has variants. The emergent characteristic may be complex — i.e., analyzable in terms of the characteristics of simpler systems. Or, it may be simple — i.e., not so analyzable. And, it would seem that the emergent law may be a new process law; or it may be a law of some other form. (I'll express some reservations about this shortly.) When one considers the possible combinations of the variants, we get the following spectrum of possible emergents:

1) Emergent complex characters with no new laws for the rest of the system.
2) Emergent simple characters with no new laws for the rest of the system.
3) Emergent complex characters with new process laws for the rest of the system.
4) Emergent simple characters with new process laws for the rest of the system.
5) Emergent complex characters with new non-process laws for the rest of the system.
6) Emergent simple characters with new non-process laws for the rest of the system.

It should be understood that I am using 'complex character' in the sense suggested earlier — that of a complex of simple characters, where what is newly occurrent is a particular arrangement of simple characters already themselves comprehended in the ideal theory for pre-emergent levels of complexity of the system of entities.

Having elaborated this spectrum of views, I now want to indicate the relative unimportance of 1) and 2), and to attack the viability of 5) and 6).

The first type of emergence is insignificant because the emergence and interaction of a complex character within the system covered by process knowledge is predictable without the addition of any new knowledge. That is, if the values of a system's various variables at any time t_n are computable from the ideal theory and the system's description at time t_o, then the particular configuration which is the complex character in question is derivable from that theory and that description.

With 2) I have in mind the emergence of characters which do not interact with the other variables of the system. (Recall that epiphenomenalism was characterized above as an instance of this sort of emergence.) Since this is so, the old process laws will determine the values of the other variables of the system; no new law statements are required for the description of their determinations.

The last two types of emergence, 5) and 6), are to be understood as variations of situation (A), described above. In such cases, it is supposed, our inability to find a new process law for the system reflects the emergence of behavior of the system not governed by any process laws. Recall that we indicated why the conclusion that no regularity is embodied in such a system was unwarranted. The point may now be strengthened by observing that, for any set of events, states, or values of variables, there is a function which will generate a description of that set together with a description of any additional event, state, or value one may wish to specify. The great difficulty (and one way to indicate the nature of the problem of induction) lies in the fact that, for any finite segment of some such set, there is an infinite number of functions that will generate its description but which will diverge in their description of some other part of the set.

To take a simple example by way of illustration, the sequence

1, 2, 3, 4, 5, . . .

is described by both of the functions '2k-1' and '(k-1)(k-2)(k-3)(k-4)(k-5) + k." That is, let k = the place in the series of some number. Then each function will give '1' as the solution for that number found in the first place in the series, '2' as the solution for the second, '3' for the third, '4' for the fourth, and '5' for the fifth. But, while

'2k-1' gives '6' for the sixth place n the series, the other function predicts that '126' is the number that will occur there. Since our observations of the states of systems over periods of time are finite, we face precisely the same problem as one w\does who looks at the series '1, 2, 3, 4, 5' ans is asked to find the generating function of this series which correctly predicts the rest of the members of the series, whatever they may be: the answer is not uniquely determined by the known members of the series; we must choose from a theoretically infinite class of generating functions one function which describes the whole series correctly. That there is such a function is indisputable. It follows that for any sequence of events, states, or values of variables occurring over time, there exists a "governing" process law.[6] This eliminates types 5) and 6) of emergence.

We are thus left with types 3) and 4) as the only significant sorts of emergence whose possibility one may wish to entertain for some system. Given a system in which there occurs a modification of its complexity accompanied by the failure of one or more of the old process laws for that system, that modification may consist in the occurrence of a new complex of characters that interacts with the simpler elements of the system in ways not predictable sis of the ideal theory for simpler systems, or it may consist in the occurrence of a wholly new character that so interacts.

We are now in a position to begin an assessment of the various disputes over the nature of historical explanation, and of history as a science, to which I alluded in the opening remarks of the paper.

Historical Explanation

In the disputes over the nature of historical explanation, the divisive points do not lie with the questions of what sorts of explanations historians actually give, or of what kinds of explanations are commonly accepted as satisfactory. Rather, they lie with normative claims about what should be required of an historical explanation in order that it be ideal — the most we could ever hope fur. To be sure, those who maintain that historical explanations generally are as they should be, often cite examples of commonly accepted historical explanations; but I think that underlying this view is a conviction that process knowledge of the variables in history is unattainable, either because of some alleged epistemic difficulty in access to the relevant variables, or because of a conviction that the relevant variables do not stand in process relationships with one another (a position sometimes thought to be a consequent of agent theories of human action, for example). But if I am correct in my general claim that there exists a generating function for any series of events, states, or variable values, then this latter view (that in human historical processes there exist events that do not stand in process relationships) is *a priori* false since it denies a truism. And while it is indeed true that we are not in practice presently able to reconstruct or otherwise recapture most segments of the past, had we "the" correct process law in hand, the inaccessibility of the past would be no more a problem than is the retrodiction of planetary conjunctions at the time of the Christian nativity.

But, if the thesis that human behavior is governed by process laws is undeniable once clearly understood, what are we to make of these hot disputes? For, it seems equally undeniable that the ideal of explanation for my system governed by process laws will be in terms of statements of those laws; anything short of that will

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6. For defenses of and reflections on this claim, see Bertrand Russell, "On the Notion of Cause, with Applications to the Free Will Problem," in H. Feigl and M. Brodbeck, eds., *Readings in the Philosophy of Science*, 1953; John Earman, "Laplacian Determinism, or Is This Any Way to Run a Universe?", *The Journal of Philosophy* 68, 1971.

be short of the fullest possible knowledge for that system. If there are to be norms whereby we judge explanations as more or less satisfactory, surely they will embody reference to the extent to which knowledge of what is to be explained is theoretically possible. And since any system is governed by process laws, what must be shown in order to justify withholding allegiance to the ideal of process knowledge in the field of historical explanation is that historical explanations are different from but compatible with explanations in terms of process laws, or that it is in principle impossible to attain process knowledge for some historical processes. Let us take up each of these possibilities in turn.

If the claim that all human behavior is governed by process laws is true, then one who argues that historical explanation need not have process knowledge as its ideal may be maintaining that historical explanation does not seek either to replicate explanation by reference to process laws, nor to contradict it. And this is a familiar enough view. Many who have engaged in historical explanations seek in them to assign responsibility for events and processes in history to individuals. Often such historical explanations are narratives of the lives of one or more individuals; great care is exercised in locating " turning points" in those person's lives in which some decision or choice they have made can be seen to have affected the course of history.

The idea that underlies the significance attached to such momentous decisions is that, at that point, the individual in question might have done otherwise than he or she did and thereby affect the course of history. We recognize that our own lives have been greatly shaped and influenced by the events of the past; we believe that we make decisions and choices among genuine alternatives; and the instructive character of such historical narratives, as well as our involvement in the outcomes of the turning points of history, justifies our interest in such narratives. But the acceptance of this view of historical explanation is reasonable only if historical explanations are not scientific, int eh sense of not competing with explanations of the same events given in terms of process laws and being compatible with them. If this is the case, then the historian need not worry about his discipline lacking an identity. For, his explanations aren't intended to be scientific in the sense of being of the sort that proceeds by listing laws and descriptions.

This view of historical explanation has at its core the position sometimes called "soft determinism," i.e., the view that determinism is true but compatible with free will, i.e., compatible with saying of an individual that he could have done otherwise than perform some act which he did perform. Hence, in order to know whether historical explanations of the sort described do constitute a challenge to the hard sciences' view of the world, we must inquire into the acceptability of the view known as soft determinism.

Are free will and determinism compatible? Let us define 'x does y of his own free will' as 'when s did y, he could have done other than y,.' and define the view known as determinism as 'the state of this world at any time t is strictly a function of its state at any other time, as embodied in the process laws of the world.' (I favor this over the more usual 'every event has a cause' because of the difficulties that Russell and others have pointed out in the notion of a cause.[7])

7. Russell, op cit.

The following argument, or something very much like it, has been proposed[8] to show that free will and determinism are compatible. First, assume that they are incompatible: that is, that if free will is true, determinism is false. It follows from some fairly basic principles of the probability calculus that anything rendering the one probable will render the other improbable. But indeterminism is the denial of determinism. Hence, anything that renders free will probable renders indeterminism probable, if free will and determinism are incompatible.

Second, suppose Jones is a soldier, and suppose that one morning he stays in bed and doesn't report for roll call. In order to determine whether he is to be disciplined, the commanding officer endeavors to determine whether he could have reported. Jones maintains that he couldn't report — that he was literally unable to bet out of bed. However, he is unable to provide any evidence of this inability except his own assertion (and, of course, the fact that he didn't report). All the evidence that seems relevant is constant over a long period of time — his health, physical condition, mental and emotional stability show no significant changes since previous days when he reported without fail. Furthermore, Jones can offer no plausible explanation of his claim that he couldn't report. He grows increasingly uneasy about the whole inquiry as he is confronted with the evidence against his claim, and he finally admits concocting the excuse in order to grab some extra sleep because he was up late the night before playing poker in the latrine.

All of this evidence certainly seems to render it highly probable that Jones could have reported for roll call, even though he didn't That is, the evidence renders highly probable that he could have done otherwise than he did. But it is equally apparent that nothing in it is evidence for there having been some uncaused event, or for the breakdown of the laws of the universe. That is, it provides no support for the claim that indeterminism is true. Hence, it contains no evidence that determinism is improbable. But if this is so, then it is false that anything that renders free will probable renders determinism improbable. Hence, free will and determinism are not incompatible. hence, no view is rendered incompatible with determinism, and thus in competition with the view that scientific explanation proceeds (ideally) in terms of composition laws, by basing its explanations on the supposition that the doctrine of free will is true. To the extent that history engages in such explanations, it is both compatible with deterministic science and different from it in its forms of explanation.

However, as attractive as the foregoing argument is, there are difficulties with it that, upon exposure, seem insurmountable. Let us examine the presuppositions of the claim that Jones could have done otherwise than he did. One way to view this claim is that Jones had access to a possible world which was like ours in having the same physical laws, and like ours in having at least one common state (actually, in Jones' case, the entire histories of this world and the probable world prior to Jones' failure to report for roll call, or rather his decision not to report for roll call, are the same). For, it seems indisputable that any possible world which Jones has access to at time t must have at least one state in common with this world (namely, the state of this world at time t); and it seems equally undeniable that such a possible world must have the same physical laws as this one, since we are explicitly wanting to maintain that Jones' actions are compatible with the deterministic view of things. But these two conditions of, or restrictions on, Jones' access to some possible world are sufficient to show that any possible world that Jones has access to is no different than our own. For, from the definition of determinism, any possible world that has

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8. See Keith Lehrer, "An Empirical Disproof of Determinism?", in his *Freedom and Determinism*, 1966. I am indebted to Peter Van Inwagen for drawing my attention to this argument, and for the counter argument which follows. See his"The Incompatibility of Free Will and Determinism," *Philosophical Studies* 27 (1975): 185-199.

both the same laws as ours and at least one state in common will have every other state in common, since the state of the world at any time is strictly a function of its state at any other time, as expressed by the laws of nature.

Hence, contrary to what we supposed earlier, anything that renders it probable that Jones could have done otherwise than he did *is* something that renders it probable that Jones had access to a possible world not identical with our own. But if determinism is true, then there is no possible world other than our own to which anyone ever has access. Hence, any evidence rendering it probable that Jones could have done other than he did, since it is evidence that Jones had access to a possible world not identical with our own, is evidence which renders it probable that determinism is false. Hence, free will and determinism are not compatible, and views that imply or presuppose soft determinism are false.

Notice that whether one regards certain crucial elements in these historical explanations, such as individuals' intentions, choices, deliberations, decisions, and evaluations, as emergent simple characteristics or not makes no difference to the compatibility issue. Nothing was said in the argument against compatibility that rests on the distinction between 3) and 4) above — that is, between emergent complex characters, definable in terms of the component features of the rest of the system, and emergent simple characters not definable in terms of the component features of the rest of the system. Given the emergence of variables that we refer to by various psychological terms, which interact with other, physical variables and thereby make the governing process laws more complex than it would otherwise appear, it makes no difference to the result whether those emergents are merely new complexes of old variables or are composed of new, simple characters.

I think we can now see why there is such resistance to acknowledging the various alternative forms of explanation found in history as legitimately viewed as complete. Any explanation that implies, or presupposes, that someone at some point could have done other than he or she did, stands in direct conflict with determinism. This is not to deny, of course, that methodologically and as a result of the incompleteness of our knowledge of both the state of the world at any time or the process laws that govern it, we must be satisfied with all sorts of incomplete explanations, including those in historical contexts as well as elsewhere. We should be equally tolerant and encouraging of alternate theories of history, which (as I view them) seek to capture descriptions of operative factors that isolate significant emergent features of human activity and indicate where they interact with the physical world so as to make a difference in what would otherwise be its process laws. After all, we are still faced with the tasks of discovering the process laws and fully describing the world's categories.

But if what I've argued about process laws is correct, the conflict between those who hold that only process knowledge is complete and those who maintain alternative views, isn't genuine. Historical processes, like any other kind, are deterministic when viewed in their total context. History, insofar as it produces explanations, is a science; insofar as its explanations don't contain statements of process laws, it is an imperfect science. If its descriptive content is definable in terms of, say , psychological concepts, and its laws are reducible to psychological laws, then history is not a distinct science but is defined at best only by its selective interest in certain individuals of the past. If either descriptive or explanatory reduction doesn't hold for history and any other science, then history is a science in its own right. These it seems to me, are the genuine issues to which those who engage in the philosophy of history should attend. But the supposed uniqueness of historical explanation is, I submit, simply a confusion.