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Historical Science[1]

History, Science, and Historical Science

The simplest definition of history is that it is change through time. It is, however, at once clear that the definition fails to make distinctions which are necessary if history is to be studied in a meaningful way. A chemical reaction involves change through time, but obviously it is not historical in the same sense as the first performance by Lavoisier of a certain chemical experiment. The latter was a nonrecurrent event, dependent on or caused by antecedent events in the life of Lavoisier and the lives of his predecessors, and itself causal of later activities by Lavoisier and his successors. The chemical reaction involved has no such causal relationship and has undergone no change before or after Lavoisier's experiment. It always has occurred and always will recur under the appropriate historical circumstances, but as a reaction in itself it has no history.

A similar contrast between the historical and the nonhistorical exists in geology and other sciences. The processes of weathering and erosion are unchanging and nonhistorical. The Grand Canyon or any gully is unique at any one time but is constantly changing to other unique, nonrecurrent configurations as time passes. Such changing, individual geological phenomena are historical, whereas the properties and processes producing the changes are not.

The unchanging properties of matter and energy and the likewise unchanging processes and principles arising therefrom are *immanent* in the material universe. They are nonhistorical, even though they occur and act in the course of history. The actual state of the universe or of any part of it at a given time, its configuration, is not immanent and is constantly changing. It is *contingent* in

[1] Parts of this essay have been developed from a talk on the explanation of unique events given in the Seminar on Methods in Philosophy and Science at the New School for Social Research on May 20, 1962. On that occasion I also profited from discussion and other talks pertinent to the present topic, especially by Dobzhansky, Nagel, and Pittendrigh.

Bernal's (1951) term, or *configurational*, as I prefer to say (Simpson, 1960). History may be defined as configurational change through time, i.e., a sequence of real, individual but interrelated events. These distinctions between the immanent and the configurational and between the nonhistorical and the historical are essential to clear analysis and comprehension of history and of science. They will be maintained, amplified, and exemplified in what follows.

Definitions of science have been proposed and debated in innumerable articles and books. Brief definitions are inevitably inadequate, but I shall here state the one I prefer: Science is an exploration of the material universe that seeks natural, orderly relationships among observed phenomena and that is self-testing. (For explanation and amplification see Simpson, 1962, 1963.) Apart from the points that science is concerned only with the material or natural and that it rests on observation, the definition involves three scientific activities: the description of phenomena, the seeking of theoretical, explanatory relationships among them, and some means for the establishment of confidence regarding observations and theories. Among other things, later sections of this essay will consider these three aspects of historical science.

Historical science may thus be defined as the determination of configurational sequences, their explanation, and the testing of such sequences and explanations. (It is already obvious and will become more so that none of the three phases is simple or thus sufficiently described.)

Geology is probably the most diverse of all the sciences, and its status as in part a historical science is correspondingly complex. For one thing, it deals with the immanent properties and processes of the physical earth and its constituents. This aspect of geology is basically nonhistorical. It can be viewed simply as a branch of physics (including mechanics) and chemistry, applying those sciences to a single (but how complex!) object: the earth. Geology also deals with the present configuration of the earth and all its parts, from core to atmosphere. This aspect of geology might be considered nonhistorical insofar as it is purely descriptive, but then it also fails to fulfill the whole definition of a science. As soon as theoretical, explanatory relationships are brought in, so necessarily are changes and sequences of configurations, which are historical. The fully scientific study of geological configurations is thus historical science. This is the *only* aspect of geology that is peculiar to this science, that is simply geology and not also something else. (Of course I do not mean that it can be studied without reference to other aspects of geology and to other sciences, both historical and nonhistorical.)

Paleontology is primarily a historical science, and it is simultaneously biological and geological. Its role as a part of historical biology is obvious. In this role, like all other aspects of biology, it involves all the immanent properties and processes of the physical sciences, but differs from them not only in being historical but also in that its configurational systems are incomparably more complex and have feedback and information storage and transmittal mechanisms unlike any found in the inorganic realm. Its involvement in geology and inclusion in that science as well as in biology are primarily due to the fact that the history of organisms runs parallel with, is environmentally contained in, and continuously interacts with the physical history of the earth. It is of less philosophical interest but of major operational importance that paleontology, when applicable, has the highest resolving power of any method yet discovered for determining the sequence of strictly geological events. (That radiometric methods may give equal or greater resolution is at present a hope and not a fact.)

Description and Generalization

In principle, the observational basis of any science is a straight description of what is there and what occurs, what Lloyd Morgan (1891) used to call "plain story." In a physical example, plain story might be the specifications of a pendulum and observations of its period. A geological plain story might describe a bed of arkose, its thickness, its attitude, and its stratigraphic and geographic position. An example of paleontological plain story would be the occurrence of a specimen of a certain species at a particular point in the bed of arkose. In general, the more extended plain stories of historical science would describe configurations and place them in time.

In fact, plain story in the strictest, most literal sense plays little part in science. Some degree of abstraction, generalization, and theorization usually enters in, even at the first observational level. The physicist has already abstracted a class of configurational systems called "pendulums" and assumes that only the length and period need be observed, regardless of other differences in individuals of the class, unless an observation happens to disagree with the assumption. Similarly, the geologist by no means describes all the characteristics of the individual bed of arkose and its parts but has already generalized a class "arkose" and adds other details, if any, only in terms of such variations within the class as are considered pertinent to his always limited purpose. The paleontologist has departed still further from true, strict plain story, for in recording a specimen as of a certain species he has not only generalized a particularly complex kind of class but has also reached a conclusion as to membership in that class that is not a matter of direct observation at all.

Every object and every event is unique if its configurational aspects are described in full. Yet, and despite the schoolteachers, it may be said that some things are more unique than others. This depends in the first place on the complexity of what is being described, for certainly the more complex it is the more ways there are in which it may differ from others of its general class. A bed of arkose is more complex than a pendulum, and an organism is to still greater degree more complex than a bed of arkose. The hierarchy of complexity and individual uniqueness from physics to geology to biology is characteristic of those sciences and essential to philosophical understanding of them. It bears on the degree and kind of generalization characteristic of and appropriate to the various sciences even at the primary observational level. The number of pertinent classes of observations distinguished in physics is much smaller than in geology, and much smaller in geology than in biology. For instance, in terms of taxonomically distinguished discrete objects, compare the numbers of species of particles and atoms in physics, of minerals and rocks in geology, and of organisms in biology. Systems and processes in these sciences have the same sequence as to number and complexity.

Another aspect of generalization and degree of uniqueness arises in comparison of nonhistorical and historical science and in the contrast between immanence and configuration. In the previous examples, the physicist was concerned with a nonhistorical and immanent phenomenon: gravitation. It was necessary to his purpose and inherent in his method to eliminate as far as possible and then to ignore any historical element and any configurational uniqueness in the particular, individual pendulum used in the experiment. He sought a changeless law that would apply to all pendulums and ultimately to all matter, regardless of time and place. The geologist and paleontologist were also interested in generalization of common properties and relationships between one occurrence of arkose and another, between one specimen and another of a fossil species, but their generalizations were of the configurational and not the immanent properties and were, or at least involved, historical and not only nonhistorical science. The arkose or the fossil had its particular as well as its general configurational properties, its significant balance of difference and resemblance, not only because of immanent properties of its constituents and immanent processes that had acted on it, but also because of its history, the configurational sequence by which these individual things arose. The latter aspect, not pertinent to the old pendulum experiment or to almost anything in the more sophisticated physics of the present day, is what primarily concerns geology and paleontology as historical sciences, or historical science in general.

Scientific Law

It has been mentioned that the purpose of the pendulum experiment was to formulate a law. The concept of scientific law and its relationship with historical and nonhistorical science are disputed questions requiring clarification. The term "law" has been so variously and loosely used in science that it is no longer clear unless given an explicit and restrictive definition. The college dictionary that I happen to have at hand (Barnhart, 1948) defines "law" in philosophical or scientific use as "a statement of a relation or sequence of phenomena invariable under the same conditions." This is satisfactory if it is made clear that a law applies to phenomena that are themselves variable: it is the *relationship* (or sequence, also a relationship) that is invariable. "Under the same conditions" must be taken to mean that other variables, if present, are in addition to and not inextricably involved with those specified in the law. Further, it is perhaps implicit but should be explicit that the relationship must be manifested or repeatable in an indefinitely recurrent way. A relationship that could or did occur only once would indeed be invariable, but surely would not be a law in any meaningful scientific sense. With these considerations, the definition might be rephrased thus: a scientific law is a recurrent, repeatable relationship between variables that is itself invariable to the extent that the factors affecting the relationship are explicit in the law.

The definition implies that a valid law includes all the factors that *necessarily* act in conjunction. The fact that air friction also significantly affects the acceleration of a body falling in the atmosphere does not invalidate the law of gravitational acceleration, but only shows that the body is separately acted on by some factor defined by another law. Friction and the factors of gravitational acceleration are independent. Both laws are valid, and they can be combined into a valid compound law. But if some factor *necessarily* involved in either one, such as force of gravitation for acceleration or area for friction, were omitted, that law would be invalidated.

Laws, as thus defined, are generalizations, but they are generalizations of a very special kind. They are complete abstractions from the individual case. They are not even concerned with what individual cases have in common, in the form of descriptive generalizations or definitions, such as that all pendulums are bodies movably suspended from a fixed point, all arkoses are sedimentary rocks containing feldspar, or all vertebrates are animals with jointed backbones. These and similar generalizations are obviously not laws by any usage. When we say, for instance, that arkose is a feldspathic sedimentary rock, we mean merely that we have agreed that if a rock happens to be sedimentary and within a certain range of texture and of composition including a feldspar, we will call it "arkose." We do not mean that the nature of the universe is such that there is an inherent relationship among sedimentary rocks and feldspars reducible to a constant. Laws are inherent, that is *immanent*, in the nature of things as abstracted entirely from contingent configurations, although always acting on those configurations.

Until recently the theoretical structure of the nonhistorical physical sciences consisted largely of a body of laws or supposed laws of this kind. The prestige of these sciences and their success in discovering such laws were such that it was commonly believed that the proper scientific goal of the historical sciences was also to discover laws. Supposed laws were proposed in all the historical sciences. By way of example in my own field, paleontology, I may mention

"Dollo's law" that evolution is irreversible, "Cope's law" that animals become larger in the course of evolution, or "Williston's law" that repetitive serial structures in animals evolve so as to become less numerous but more differentiated. The majority of such supposed laws are no more than descriptive generalizations. For example, animals do not invariably become larger in time. Cope's law merely generalizes the observation that this is a frequent tendency, without establishing any fixed relationship among the variables possibly involved in this process.

Even when a relationship seems established, so-called "historical laws" are almost always open to exceptions. For example, Rensch (1960), an evolutionist convinced of the validity of historical laws, considers "Allen's rule" a law: that when mammals adapt to colder climates their feet become shorter. But of the actual mammals studied by him, 36% were exceptions to the "law." Rensch explains this by supposing that "many special laws act together or interfere with one another. Thus 'exceptions' to the laws result." This is a hypothetical possibility, but to rely upon it is an act of faith. The "interfering" laws are unknown in this or similar examples. A second possibility is that the "laws," as stated, are invalid as laws because they have omitted factors necessarily and inherently involved. I believe this is true, not in the sense that we have only to complete the analysis and derive a complete and valid law, but in the sense that the omissions are such as to invalidate the very concept of historical law.

The search for historical laws is, I maintain, mistaken in principle. Laws apply, in the dictionary definition "under the same conditions," or in my amendment "to the extent that factors affecting the relationship are explicit in the law," or in common parlance "other things being equal." But in history, which is a sequence of real, individual events, other things never are equal. Historical events, whether in the history of the earth, the history of life, or recorded human history, are determined by the immanent characteristics of the universe acting on and within particular configurations, and never by either the immanent or the configurational alone. It is a law that states the relationship between the length of a pendulum—*any* pendulum—and its period. Such a law does not include the contingent circumstances, the configuration, necessary for the occurrence of a real event, say Galileo's observing the period of a particular pendulum. If laws thus exclude factors inextricably and significantly involved in real events, they cannot belong to historical science.

It is further true that historical events are unique, usually to a high degree, and hence cannot embody laws defined as recurrent, repeatable relationships. Apparent repetition of simple events may seem to belie this. A certain person's repeatedly picking up and dropping a certain stone may seem to be a recurrent event in all essentials, but there really is no applicable *historical* law. Abstraction of a law from such repeated events leads to a nonhistorical law of immanent relationships, perhaps in this case of gravity and acceleration or perhaps of neurophysiology, and not to a historical law of which this particular person, picking up a certain stone, at a stated moment, and dropping it a definite number of times would be a determinate instance. In less trivial and more complex events, it is evident that the extremely intricate configurations involved in and necessary, for example, as antecedents for the erosion of the Grand Canyon or the origin of *Homo sapiens* simply cannot recur and that there can be no laws of such one-of-a-kind events. (Please bear in mind that the true, immanent laws are equally necessary and involved in such events but that they remain nonhistorical; the laws would have acted differently and the historical event, the change of configuration, would have been different if the configuration had been different; this historical element is not included in the operative laws.)

It might be maintained that my definition of law is old-fashioned and is no longer accepted in the nonhistorical sciences either. Many laws of physics, considered nonhistorical, are now conceived as statistical in nature, involving not an invariable relationship but an average one. The old gas laws or the new laws of radioactive decay are examples. The gas laws used to assume an ideal gas. Now they are recognized as assuming that directions of molecular motion tend to cancel out if added together, and that velocities tend to vary about a mean under given conditions. This cannot be precisely true of a real gas at a given moment, but when very large numbers of molecules are involved over an appreciable period of time, the statistical result is so close to the state described by the gas laws that the difference does not matter. In this and similar ways the descent from the ideal to the real in physical science has been coped with, not so much by facing it as by finding devices for ignoring it.

The historical scientist here notes that a real gas in a real experiment has *historical* attributes that are *additional* to the laws affecting it. Every molecule of a real gas has its individual history. Its position, direction of motion, and velocity at a given moment (all parts of the total configuration) are the outcome of that history. It is, however, quite impractical and, for the purposes of physics, unnecessary to make an historical study of the gas. The gas laws apply well enough "other things being equal," which means here that the simple histories of the molecules tend, as observation shows, to produce a statistical result so nearly uniform that the historical, lawless element can be ignored for practical purposes.

The laws immanent in the material universe are not statistical in essence. They act invariably in variable historical circumstances. The pertinence of statistics to such laws as those of gases is that they provide a generalized description of usual historical circumstances in which those laws act, and not that they are inherent in the laws themselves. Use of statistical expressions, not as laws but as generalized descriptions, is common and helpful in all science and especially in historical science. For example, the statistical specifications of land forms or of grain size in sediments clearly are not laws but descriptions of configurations involved in and arising from history.

To speak of "laws of history" is either to misunderstand the nature of history or to use "laws" in an unacceptable sense, usually for generalized descriptions rather than formulations of immanent relationships.

Uniformitarianism

Uniformitarianism has long been considered a basic principle of historical science and a major contribution of geology to science and philosophy. In one form or another it does permeate geological and historical thought to such a point as often to be taken for granted. Among those who have recently given conscious attention to it, great confusion has arisen from conflicts and obscurities as to just what the concept is. To some, uniformitarianism (variously defined) is a law of history. Others, maintaining that it is not a law, have tended to deny its significance. Indeed, in any reasonable or usual formulation, it is not a law, but that does not deprive it of importance. It is commonly defined as the principle that the present is the key to the past. That definition is, however, so loose as to be virtually meaningless in application. A new, sharper, and clearer definition in modern terms is needed.

Uniformitarianism arose around the turn of the 18th to 19th centuries, and its original significance can be understood only in that context. (The historical background is well covered in Gillispie, 1951.) It was a reaction against the then prevailing school of catastrophism, which had two main tenets: (1) the general belief that God has intervened in history, which therefore has included both natural and supernatural (miraculous) events; and (2) the particular proposition that earth history consists in the main of a sequence of major catastrophes, usually considered as of divine origin in accordance with the first tenet. (For a historical review anachronistically sympathetic with these beliefs see Hooykaas, 1959.) Uniformitarianism, as then expressed, had various different aspects and did not always face these issues separately and clearly. On the whole, however, it embodied two propositions contradictory to catastrophism: (1) earth history (if not history in general) can be explained in terms of natural forces still observable as acting today; and (2) earth history has not been a series of universal or quasi-universal catastrophes but has in the main been a long, gradual development—what we would now call an evolution. (The term "evolution" was not then customarily used in this sense.) A classic example of the conflicting application of these principles is the catastrophist belief that valleys are clefts suddenly opened by a supernally ordered revolution as against the uniformitarian belief that they have been gradually formed by rivers that are still eroding the valley bottoms.

Both of the major points originally at issue are still being argued on the fringes of science or outside it. To most geologists, however, they no longer merit attention from anyone but a student of human history. It is a necessary condition and indeed part of the definition of science in the modern sense that only natural explanations of material phenomena are to be sought or can be considered scientifically tenable. It is interesting and significant that general acceptance of this principle (or limitation, if you like) came much later in the historical than in the nonhistorical sciences. In historical geology it was the most important outcome of the uniformitarian-catastrophist controversy. In historical biology it was the still later outcome of the Darwinian controversy and was hardly settled until our own day. (It is still far from settled among nonscientists.)

As to the second major point originally involved in uniformitarianism, there is no *a priori* or philosophical reason for ruling out a series of *natural* worldwide catastrophes as dominating earth history. However, this assumption is simply in such flat disagreement with everything we now know of geological history as to be completely incredible. The only issues still valid involve the way in which natural processes still observable have acted in the past and the sense in which the present is a key to the past. Uniformitarianism, or neo-uniformitarianism, as applied to these issues has taken many forms, among them two extremes that are both demonstrably invalid. They happen to be rather amusingly illustrated in a recently published exchange of letters by Lippman (1962) and Farrand (1962).

Lippman, one of the neocatastrophists still vociferous on the fringes of geological science, attacks uniformitarianism on the assumption that its now "orthodox" form is absolute gradualism, i.e., the belief that geological processes have *always* acted gradually and that changes catastrophic in rate and extent have never occurred. Farrand, who would perhaps consent to being called an orthodox geologist, demonstrates that Lippman has set up a straw man. Catastrophes do now occur. Their occurrence in the past exemplifies rather than contradicts a principle of uniformity. It happens that there is no valid evidence that catastrophes of the kind and extent claimed by the original catastrophists and by Lippman have ever occurred or that they could provide explanations for some real phenomena, as claimed. This, however, is a different point. Farrand expresses a common, probably the usual modern understanding of uniformitarianism as follows: "The geologist's concept that processes that acted on the earth in the past are the *same processes* that are operating today, on the *same scale* and at approximately the *same rates*" (italics mine). But this principle also seems to be flatly contradicted by geological history. Some processes (those of vulcanism or glaciation, for example) have evidently acted in the past on scales and at rates that cannot by any stretch be called "the same" or even "approximately the same" as those of today. Some past processes (such as those of Alpine nappe formation) are apparently not acting today, at least not in the form in which they did act. There are innumerable exceptions that disprove the rule.

Then what uniformity principle, if any, is valid and important? The distinction between immanence and configuration (or contingency) clearly points to one: the postulate that immanent characteristics of the material universe have not changed in the course of time. By this postulate all the immanent characteristics exist today and so can, in principle, be observed or, more precisely, inferred as generalizations and laws from observations. It is in this sense that the present is the key to the past. Present immanent properties and relationships permit the interpretation and explanation of history precisely because they are *not* historical. They have remained unchanged, and it is the configurations that have changed. Past configurations were never quite the same as they are now and were often quite different. Within those different configurations, the immanent characteristics have worked at different scales and rates at different times, sometimes combining into complex processes different from those in action today. The uniformity of the immanent characteristics helps to explain the fact that history is not uniform. (It could even be said that uniformitarianism entails catastrophes, but the paradox would be misleading if taken out of context.) Only to the extent that past configurations resembled the present in essential features can past processes have worked in a similar way.

That immanent characteristics are unchanging may seem at first sight either a matter of definition or an obvious conclusion, but it is neither. Gravity would be immanent (an inherent characteristic of matter *now*) even if the law of gravity had changed, and it is impossible to prove that it has not changed. Uniformity, in this sense, is an unprovable postulate justified, or indeed required, on two grounds. First, nothing in our incomplete but extensive knowledge of history disagrees with it. Second, only with this postulate is a rational interpretation of history possible, and we are justified in seeking—as scientists we *must* seek—such a rational interpretation. It is on this basis that I have assumed on previous pages that the immanent is unchanging.

Explanation

Explanation is an answer to the question "Why?" But as Nagel (1961) has shown at length, this is an ambiguous question calling for fundamentally different *kinds* of answers in various contexts. One kind of answer specifies the inherent necessity of a proposition, and those are the answers embodied in laws. Some philosophers insist that this is the only legitimate form of explanation. Some (e.g. Hobson, 1923) even go so far as to maintain that since inherent necessity cannot be *proved*, there is no such thing as scientific explanation.

Nagel demonstrates that all this is in part a mere question of linguistic usage and to that extent neither important nor interesting. The only substantial question involved is whether explanation must be universal or may be contingent. Nagel further shows, with examples (ten of them in his Chapter 2), that contingent explanations are valid in any usual and proper sense of the word "explanation." Nagel does not put the matter in just this way and he makes other distinctions not pertinent in the present context, but in essence this distinction between universal and contingent explanation parallels that between, on one hand, immanence and nonhistorical science, which involves laws, and, on the other, configuration and historical science, which does not involve laws but which does also have explanations.

The question "Why?" can be broken down into three others, each evoking a different kind of explanation, as Pittendrigh (1958) and Mayr (1961), among others, have discussed. "How?" is the typical question of the nonhistorical sciences. It asks how things work: how streams erode valleys, how mountains are formed, how animals digest food—all in terms of the physical and chemical processes involved. The first step toward explanation of this kind is usually a generalized description, but answers that can be considered complete within this category are ultimately expressed in the form of laws embodying invariable relationships among variables. It is at this level that nonhistorical scientists not only start but usually also stop.

The historical scientists nevertheless go on to a second kind of explanation that is equally scientific and ask a second question, in the vernacular, "How come?" How does it happen that the Colorado River formed the Grand Canyon, that cordilleras arose along the edge of a continent, or that lions live on zebras? Again the usual approach is descriptive, the plain-story history of changes in configurations, whether individual, as for the Grand Canyon, or generalized to some degree, as for the concurrent evolution of lions and zebras. This is already a form of explanation, but full explanation at this more complex level is reached only by combination of the configurational changes with the immanent properties and processes present within them and involved in those changes. One does not adequately explain the Grand Canyon either by describing the structure of that area and its changes during the Cenozoic or by enumerating the physical and chemical laws involved in erosion, but by a combination of the two.

There are two other kinds of scientific explanation to be mentioned here for completeness, although they enter into geology only to a limited extent through paleontology and are more directly biological and psychological. Both are kinds of answers to the question "What for?" This question is inappropriate in the physical sciences or the physical ("How?") aspects of other sciences, historical or nonhistorical. "What does a stone fall for?" or "Why was the Grand Canyon formed?" (in the sense of "What is it now for?") are questions that make no sense to a modern scientist. Such questions were nevertheless asked by primitive scientists (notably Aristotle) and are still asked by some nonscientists and pseudoscientists. The rise of modern physical science required the rejection of this form of explanation, and physical scientists insisted that such questions simply *must not* be asked. In their own sphere they were right, but the questions are legitimate and necessary in the life sciences.

One kind of "What for?" question, for example, "What are birds' wings for?" calls for a teleonomic answer. That they are an adaptation to flying is a proper answer and partial explanation near the descriptive level. Fuller explanation is historical: through a sequence of configurations of animals and their environments wings became possible, had an advantageous function, and so evolved through natural selection. Such a history is possible only in systems with the elaborate feedback and information-storage mechanisms characteristic of organisms, and this kind of explanation is inapplicable to wholly inorganic systems (or other configurations). "What for?" may also be answered teleologically in terms of purpose, explaining a sequence of events as means to reach a goal. Despite Aristotle and the Neo-Thomists, this form of explanation is scientifically legitimate only if the goal is foreseen. It therefore is applicable only to the behavior of humans and, with increasing uncertainty, to some other animals.

The question "How come?" is peculiar to historical science and necessary in all its aspects. Answers to this question are *the* historical explanations. Nevertheless, the full explanation of history requires *also* the reductionist explanations (nonhistorical in themselves) elicited by "How?" Teleonomic explanations are also peculiar to historical science, but only to that part of it which deals with the history of organisms.

Predictive Testing and Predictability

All of science rests on postulates that are not provable in the strictest sense. The uniformity of the immanent, previously discussed, is only one such postulate, although perhaps the most important one for historical science. Indeed it may be said that not only the postulates but also the conclusions of science, including its laws and other theories, are not strictly provable. Proof in an absolute sense occurs only in mathematics or logic when a conclusion is demonstrated to be tautologically contained in axioms or premises. Since these disciplines are not directly concerned with the truth or probability of axioms or premises, and hence of conclusions drawn from them, their proofs are trivial for the philosophy of the natural sciences. In these sciences, the essential point is determination of the probability of the premises themselves, and mathematics and logic only provide methods for correctly arriving at the implications contained in those premises. Despite the vulgar conception of "proving a theory," which does sometimes creep into the scientific literature, careful usage never speaks of proof in this connection but only of establishment of degrees of confidence.

In the nonhistorical sciences the testing of a proposition, that is, the attempt to modify the degree of confidence in it, usually has one general form. A possible relationship between phenomena is formulated on the basis of prior observations. With that formulation as a premise, implications as to phenomena not yet observed are arrived at by logical deduction. In other words, a prediction is made from an hypothesis. An experiment is then devised in order to determine whether the predicted phenomena do in fact occur. The premise as to relationships, the hypothesis, often has characteristics of a law, although it may be expressed in other terms. As confidence increases (nothing contrary to prediction is observed) it becomes a theory, which is taken as simultaneously explaining past phenomena and predicting future ones.

Physical scientists (e.g. Conant, 1947) have often maintained or assumed that this is the paradigm of testing ("verification" or increase of confidence) for science in general. On this basis, some philosophers and logicians of science (notably Hempel and Oppenheim, 1953) have concluded that scientific explanation and prediction are inseparable. Explanation (in this sense) is a correlation of past and present; prediction is a correlation of present and future. The tense does not matter, and it is maintained that the logical characteristics of the two are the same. They are merely two statements of the same relationship. This conclusion is probably valid as applied to scientific laws, strictly defined, in nonhistorical aspects of science. In previous terms, it has broad—perhaps not completely general—validity for "How"? explanations. But we have seen that there are other kinds of scientific explanations and that some of them are more directly pertinent to historical science. It cannot be assumed and indeed will be found untrue that parity of explanation and prediction is valid in historical science.

Scriven (1959 and personal communication) has discussed this matter at length. One of his points (put in different words) is that explanation and prediction are not necessarily symmetrical, that in some instances a parity principle is clearly inapplicable to them. Part of the argument may be paraphrased as follows. If *X* is always preceded by *A*, *A* is a cause, hence at least a partial explanation, of *X*. But *A* may not always be followed by *X*. Therefore, although *A* explains *X* when *X* does occur, it is not possible to predict the occurrence of *X* from that of *A*. A simple geological example (not from Scriven) is that erosion causes valleys, but one cannot predict from the occurrence of erosion that a valley will be formed. In fact, quite the contrary may occur; erosion can also obliterate valleys.

The example also illustrates another point by Scriven (again in different terms). The failure of prediction is due to the fact that erosion (*A*) is only a partial cause of valleys (X). It is a (complex) immanent cause, and we have omitted the configurational cause. Erosion is always followed by a valley formation, A is followed by X, *if* it affects certain configurations. The total cause, as in all historical events, comprises both immanent and configurational elements. It further appears that prediction is possible in historical science, but only to a limited extent and under certain conditions. If the immanent causation is known and if the necessary similarities of configurational circumstances are known and are recurrent, prediction is possible.

The possibility of predicting the future from the past is nevertheless extremely limited in practice and incomplete even in principle. There seem to be four main reasons for these limitations. Mayr (1961) has discussed them in connection with historical aspects of biology, and with some modification his analysis can be extended to historical science in general.

(1) A necessary but insufficient cause may not be positively correlated with the usual outcome or event. This is related to the asymmetry of explanation and prediction already discussed, and it is also discussed in other words by Scriven (1959). Scriven's example is that paresis is caused by syphilis, but that most syphilitics do not develop paresis. A modification of Mayr's example is that mutation is a necessary cause for evolutionary change, but that such change rarely takes the direction of the most frequent mutations. A geological example might be that vulcanism is essential for the formation of basalt plateaus, but that such plateaus are not the usual result of vulcanism.

(2) The philosophical interest of the foregoing reason for historical unpredictability is reduced by the fact that the outcome might become predictable in principle if *all* the necessary causes were known. But as soon as we bring in configuration as one of the necessary causes, which must always be done in historical science, the situation may become extremely, often quite impossibly, complicated. Prediction is possible only to the extent that correlation can be established with pertinent, abstracted and generalized, recurrent elements in configurations. Considerations as to base level, slope, precipitation, and other configurational features may be generalized so as to permit prediction that *a* valley will be formed. It would be impossibly difficult to specify all the far more complex factors of configuration required to predict the exact form of a particular valley, an actual historical event. In such cases it may still be possible, as Scriven has pointed out in a different context, to recognize *a posteriori* the configurational details responsible for particular characteristics of the actual valley, even though these characteristics were not practically predictable. This reason for unpredictability of course becomes more important the more complex the system involved. As both Scriven and Mayr emphasize, it may become practically insurmountable in the extremely complex organic systems involved in evolution, and yet this does not make evolution inexplicable. Even in the comparatively extremely simple physical example of the gas laws, it is obviously

impossible in practice and probably also in principle (because of the limitations of simultaneous observation of position and motion) to determine the historical configurations of all the individual molecules, so that the *precise* outcome of a *particular* experiment is in fact unpredictable.

In this example the complications may be virtually eliminated and in historical science they may often be at least alleviated by putting specification of configurational causes on a statistical basis. This may, however, still further increase the asymmetry of explanation and prediction. For instance, in Scriven's previously cited example, as he points out, the only valid *statistical* prediction is that syphilis will not produce paresis; in other words, that a necessary cause of a particular result will *not* have that result. If, as a historical fact, a syphilitic does become paretic, the event was not predictable even in principle. The point is pertinent here because it demonstrates that a statistical approach does not eliminate the effect of configurational complication in making historical events unpredictable.

(3) As configurational systems become more complex they acquire characteristics absent in the simpler components of these systems and not evidently predictable from the latter. This is the often discussed phenomenon of emergence. The classical physical example is that the properties of water may be explicable but are not predictable by those of hydrogen and oxygen. Again the unpredictability increases with configurational complications. It is difficult to conceive prediction from component atoms to a mountain range, and to me, at least, prediction from atoms to, say, the fall of Rome, is completely inconceivable. It could be claimed that prediction of emergent phenomena would be possible if we really knew *all* about the atoms. This might just possibly, and only in principle, be true in nonhistorical science, as in the example of $2H+O \rightarrow H_2O$. It would, however, be true in historical science only if we knew all the immanent properties and *also* all the configurational histories of all the atoms, which is certainly impossible in practice and probably in principle. Whether or not the predictability of emergent phenomena is a philosophical possibility (and I am inclined to think it is not), that possibility would seem to have little heuristic and no pragmatic value.

(4) Scientific prediction depends on recurrence or repeatability. Prediction of unique events is impossible either in practice or in principle. Historical events are always unique in some degree, and they are therefore never precisely predictable. However, as previously noted, there are different degrees of uniqueness. Historical events may therefore be considered predictable in principle to the extent that their causes are similar. (This is a significant limitation only for configurational causes, since by the postulate of uniformity the immanent causes are not merely similar but identical.)

In practice, further severe limitations are imposed by the difficulties of determining what similarities of cause are pertinent to the events and of observing these causal factors. It must also again be emphasized that such prediction can only be general and not particular. In other words, prediction does not include any unique aspect of the event, and in historical science it is often the unique aspects that most require explanation. One might, for instance, be able to provide a predictive explanation of mountain formation (although in fact geologists have not yet achieved this) and also explanations of the particular features of say, the Alps (achieved in small part), but the latter explanations would not be predictive. (This is also an example of the fact that unpredictive *ad hoc* explanation may be easier to achieve than predictive general explanation.)

At this point, one might wish to raise the question of what is interesting or significant in a scientific investigation. In the physical study of gases or of sand grains, the individuality (uniqueness) of single molecules or grains, slight in any case, is generally beside the point. In dealing with historical events, such as the formation of a particular sandstone or mountain range, individuality often is just the point at issue. Here, more or less parenthetically, another aspect and another use of the statistical approach are pertinent. A statistical description of variation in sand grains or of elevations, slopes, etc. in a mountain range is a practical means of taking into account their individual contributions to the over-all individuality of the sandstone or the mountain range.

Two other aspects of explanation and prediction in historical science may be more briefly considered: the use of models, and prediction from trends and cycles. Past geological events cannot be repeated at will, and furthermore, prediction loses practical significance if, as is often the case in geology, its fulfillment would require some thousands or millions of years. This is the rationale for the experimental approach, using physical models to study the historical aspects of geology and, when possible, other historical sciences. The models abstract what are believed to be the essential general configurational similarities of historical events (folding and faulting, valley erosion, and the like) and scale these in space and time in such a way as to make them repeatable at will and at rates that permit observation. With such models predictive explanations can be made and tested. (The further problems of projecting from model to geological space and time need not be considered here.)

Finally, the most common form taken by attempts at actual historical prediction is the extrapolation of trends. In fact, this approach has no philosophical and little pragmatic validity. Its philosophical justification would require that contingent causes be unchanging or change always in the same ways, which observation shows to be certainly false. Its degree of pragmatic justification depends on the fact that trends and cycles do exist and (by definition) continue over considerable periods of time. Therefore, at randomly distributed times, established trends and cycles are more likely to continue than not. Predictions through the extrapolation of trends are useful mostly for short ranges of time; for larger ranges their likelihood decreases until the appropriate statistical prediction becomes not continuation but termination or change of trend or cycle within some specified time. The period of likely continuation or justifiable extrapolation is, furthermore, greatly reduced by the fact that a trend or cycle must *already* have gone on for a considerable time in order to be recognized as actually existent. Present knowledge of geological and biological (evolutionary) history suggests that all known trends and cycles have in fact ended or changed except those which are now still within the span of likelihood that is statistically indicated by the trends and cycles of the past. Moreover, many supposed examples, such as regular cycles of mountain building or trends for increase in size of machairodont sabers, now seem to have been mistaken. Many real trends and cycles also turn out to be neither so uniform nor so long continued as was formerly supposed, often under the influence of invalid historical "laws" such as that of orthogenesis or of the pulse of the earth. It is improbable that prediction about a total historical situation on this basis alone is ever justified, even when prediction from causal properties and configurations is possible within limits.

Strategy in Historical Science

The sequence hypothesis-prediction-experiment is not the only strategy of explanation and testing in nonhistorical physical science. It is, however, so often appropriate and useful there that philosophers who base their concepts of science on physical science, as most of them do, tend to consider it ideal if not obligatory. (On this point of view see, as a single example among many, Braithwaite, 1953.) This is an example of the existing hegemony of the physical sciences, which is not logically justifiable but has been fostered by human historical and pragmatic factors. It has been shown that this strategy is also possible in historical science, but that it here plays a smaller and less exclusive role. It must be supplemented and frequently supplanted by other strategies. These are in part implicit in what has already been said, but further notice of some of the more important ones remains as the final aim of this essay. One purpose is to demonstrate more fully and distinctly that nonpredictive explanation and testing are in fact possible in historical geology and other historical sciences.

The primary data of the historical scientist consist of partial descriptions of configurations near the level of plain story. If the configurations are sequential and connected, that is, if the later historically arose from the earlier, the antecedent can be taken as including, at least in part, the configurational requirements and causes for the consequent. Even in such simple circumstances, a direct causal connection can often be assumed on the basis of principles already developed or on the basis of known parallels. For instance, partial configurational causation is clearly involved in the sequence *Hyracotherium* (Eohippus)–*Orohippus* or sand-sandstone. The latter example adds an important point: the earlier configuration of a stratum now sandstone is not actually observed but is inferred from the latter. The examples illustrate two kinds of explanatory sequences available to the historical scientist. In one we have dated documents contemporaneous with the events and so directly historical in nature and sequence. In the other we have a pseudohistorical sequence such as that of presently existing sands and sandstones. Their resemblances and differences are such that we can be confident that they share some elements of historical change, but that one has undergone more change than the other. In this case it is easy to see that the sandstone belongs to a later period in the pseudohistorical sequence. One therefore infers for it a historically antecedent sand and can proceed to determine what characteristics are inherited from that sand and the nature of the subsequent changes.

The use of pseudohistorical sequences is another way in which the present is a key to the past, but it does not involve another principle of uniformity. The addition to the element of uniformity of immanent characteristics is simply a descriptive resemblance or generalization of configuration applicable to the particular case as a matter of observation. In practice, an historical interpretation commonly involves both historical and pseudohistorical sequences. For example, study of the stratigraphy of a given region simultaneously concerns the directly historical sequence of strata and the history of each stratum from deposition (or before) to its present condition as inferred on the basis of appropriate pseudohistorical sequences.

A second form of strategy has a certain analogy with the use of multiple experiments with controlled variables. The method is to compare different sequences, either historical or pseudohistorical, that resemble each other in some pertinent way. Resemblances in the antecedent configurations may be taken to include causes of the consequent resemblances. It is not, however, legitimate to assume that they are all necessary causes or that they include sufficient causes. Even more important at times is the converse principle that factors that differ among the antecedents are not causes of resemblances among the consequents. By elimination when many sequences are compared, this may warrant the conclusion that residual antecedent resemblances are necessary causes. There is here applicable a principle of scientific testing in general: absolute proof of a hypothesis or other form of inference is impossible, but disproof is possible. Confidence increases with the number of opportunities for disproof that have not in fact revealed discrepancies. In this application, confidence that residual resemblances are causal increases with the number of different sequences involved in the comparison. This form of strategy is applicable to most geological sequences, few of which are unique in *all* respects. Obvious and important examples include the formation of geosynclines and their subsequent folding, or many such recurrent phenomena as the stratigraphic consequences of advancing and retreating seas.

An interesting special case arises when there is more resemblance among consequent than among antecedent configurations: the phenomenon of convergence. This has received much more attention in the study of organic evolution than elsewhere, but nonorganic examples also occur. If I am correctly informed, the origin of various granites from quite different antecedents is a striking example. Another fair example might be the formation of more or less similar land forms by different processes: for example, the formation of mountains by folding, faulting, or by erosion of a plateau; or the development of plains and terraces by erosion or deposition. Doubtless most geologists can find still other examples in their specialities. The special strategic interest of convergence is that its elimination of noncausal factors often gives confidence in identification of causes and increases knowledge of them. In organic evolution it has greatly increased understanding of the nature and limits of adaptation by natural selection. In the example of the granites it shows that an essential antecedent is not some one kind of lithology but atomic composition, and it pinpoints the search for the processes bringing about this particular kind of configuration of the atoms.

It has been previously pointed out that the explanation of an historical event involves both configuration and immanence, even though the latter is not historical in nature. Historical science therefore requires knowledge of the pertinent immanent factors, and its strategy includes distinguishing the two and studying their interactions. Nonhistorical science, by its primary concern with the immanent, is the principal source of the historian's necessary knowledge of immanent factors and his principal means of distinguishing these from configurational relationships. A typical approach is to vary configurations in experiments and to determine what relationships are constant throughout the configurational variations. To a historical geologist, the function of a physical geologist is to isolate and characterize the immanent properties of the earth and its parts in that and other ways. The historical geologist is then interested not in what holds true regardless of configuration, but in how configuration modifies the action of the identified immanent properties and forces. In this respect, the nonhistorical scientist is more interested in similarities and the historical scientist in differences.

Here the historical scientist has two main strategies, both already mentioned. They may be used separately or together. One proceeds by controlled experimentation, in geology usually with scaled-down models although to a limited extent experimentation with natural geological phenomena is also possible. (The opportunities for experimentation are greater in some other historical sciences.) The other might be viewed as complementary to the previously discussed study of similarities in multiple sequences. In this strategy, attention is focused on consequent differences, the causes of which are sought among the observable or inferable differences of antecedent configurations. Although the explanation is rarely so simple or so easy to identify, a sufficiently illustrative example would be the presence in one valley and absence in another of a waterfall caused by a fault, of a ledge of hard rock above shale, or of some other readily observable local configuration.

Points always at issue in historical science are the consistency of proposed immanent laws and properties with known historical events and the sufficiency and necessity of such causation acting within known configurations. Probably the strongest argument of the catastrophists was that known features of the earth were inconsistent with their formation by known natural forces within the earth's span of existence, which many of them took to be about 6000 years. The fault of course was not with their logic but with one of their premises. The same argument, with the same fallacy, was brought up against Darwin when it was claimed that his theory was inadequate to account for the origin of present organic diversity in the earth's span, then estimated by the most eminent physicists as a few million years at most. Darwin stuck to his guns and insisted, correctly, that the calculation of the age of the earth must be wrong. Historical science has an essential role, both philosophical and practical, in providing such cross checks (mostly nonpredictive and nonexperimental), both with its own theories and with those of other sciences as part of the self-testing of science in general. A current geological example, perhaps all the more instructive because it has not yet reached a conclusion, is the controversy over continental drift and the adequacy of physical forces to bring it about if indeed it did occur. (Incidentally the original motive for writing Simpson, 1944, was to test the consistency and explanatory power of various neontological theories of evolution by comparison with the historical record.)

The testing of hypothetical generalizations or proposed explanations against a historical record has some of the aspects of predictive testing. Here, however, one does not say, "If so and so holds good, such and such will occur," but, "If so and so has held good, such and such must have occurred." (Again I think that the difference in tense is logically significant and that a parity principle is not applicable.) In my own field one of the most conspicuous examples has been the theory of orthogenesis, which in the most common of its many forms maintains that once an evolutionary trend begins it is inherently forced to continue to the physically possible limit regardless of other circumstances. This view plainly has consequences that should be reflected in the fossil record. As a matter of observation, the theory is inconsistent with that record. A more strictly geological example is the "pulse of the earth" theory, that worldwide mountain-making has occurred at regularly cyclic intervals, which also turns out to be inconsistent with the available historical data (Gilluly, 1949, among others).

The study of human history is potentially included in historical science by our definition. One of its differences from other branches of historical science is that it deals with configurational sequences and causal complexes so exceedingly intricate that their scientific analysis has not yet been conspicuously successful. (Toynbee's (1946) correlation of similar sequences would seem to be a promising application of a general historical strategy, but I understand that the results have not been universally acclaimed by his colleagues.) A second important difference is that so much of this brief history has been directly observed, although with varying degrees of accuracy and acuity and only in its very latest parts by anyone whose approach can reasonably be called scientific. Direct observation of historical events is also possible in geology and other historical sciences, and it is another of their important strategies. Meticulous observation of the history of a volcano (Parícutin) from birth to maturity is an outstanding example. More modestly, anyone who watches a flash flood in a southwestern arroyo or, for that matter, sees a stone roll down a hillside is observing an historical event.

In geology, however, and in all historical science except that of human history, the strategic value of observing actual events is more indirect than direct. The processes observed are, as a rule, only those that act rapidly. The time involved is infinitesimal in comparison with the time span of nonhuman history, which is on the order of $n10^9$ years for both historical geology and historical biology. Currently practicable resolution within that span varies enormously but is commonly no better, and in some instances far worse, than $n10^6$. The observed events are also both local and trivial in the great majority of instances. They are in fact insignificant in themselves, but they are extremely significant as samples or paradigms, being sequences seen in action and with all their elements and surrounding circumstances observable. They thus serve in a special and particularly valuable way both as historical (and not pseudohistorical) data for the strategies of comparison of multiple sequences and as natural experiments for the strategies of experimentation, including on some but not all occasions that of prediction. (This, incidentally, is still a third way in which the present is a key to the past, but again it involves no additional uniformity principle.)

Direct observation of historical events is also involved in a different way in still another of the historical strategies, that of testing explanatory theories against a record. For example, such observations are one of the best means of estimating rates of processes under natural conditions and so of judging whether they could in fact have caused changes indicated by the record in the time involved. Or the historical importance of observed short-range processes can be tested against the long-range record for necessity, or sufficiency, or both. An interesting paleontological example concerns the claims of some Neo-Lamarckians who agree that although the inheritance of acquired characters is too slow to be directly observed, it has been an (or *the*) effective long-range process of evolution. The fossil record in itself cannot offer clear disproof, but it strips the argument of all conviction by showing that actually observed short-range processes excluded by this hypothesis are both necessary and sufficient to account for known history.

Conclusion

The most frequent operations in historical science are not based on the observation of causal sequences—events—but on the observation of results. From those results an attempt is made to infer previous causes. This is true even when a historical sequence, for example one of strata, is observed. Such a sequence is directly historical only in the sense that the strata were deposited in a time sequence that is directly available to us. The actual events, deposition of each stratum, are not observable. In such situations, and in this sense, the present is not merely a key to the past—it is all we have in the way of data. Prediction is the inference of results from causes. Historical science largely involves the opposite: inferring causes (of course including causal configurations) from results.

The reverse of prediction has been called, perhaps sometimes facetiously, postdiction. In momentary return to the parity of explanation and prediction, it may be noted that if A is the necessary and sufficient cause of X and X is the necessary and sole result of A, then the prediction of X from A and the postdiction of A from X are merely different statements of the same relationship. They are logically identical. It has already been demonstrated and sufficiently emphasized that the conditions for this identity frequently do not hold in practice and sometimes not even in principle for historical science. Here, then, postdiction takes on a broader and more distinct meaning and is not merely a restatement of a predictive relationship. With considerable oversimplification it might be said that historical science is mainly postdictive, and nonhistorical science mainly predictive.

Postdiction also involves the self-testing essential to a true science, as has also been exemplified—although not, by far, fully expounded. Perhaps its simplest and yet most conclusive test is the confrontation of theoretical explanation with historical evidence. A crucial historical fact or event may be deduced from a theory, and search may subsequently produce evidence for or against its actual prior occurrence. This has been called "prediction," for example, by Rensch (1960), sometimes with the implication that historical science is true science because its philosophical basis does not really differ from that of nonhistorical physics. The premise that the philosophy of science is necessarily nonhistorical is of course wrong, but the argument is fallacious in any case. What is actually predicted is not the antecedent occurrence but the subsequent discovery; the antecedent is postdicted. Beyond this, perhaps quibbling, point, the antecedent occurrence is not always a *necessary* consequence of any fact, principle, hypothesis, theory, law, or postulate advanced before the postdiction was made. The point is sufficiently illustrated on the pragmatic level by the sometimes spectacular failure to predict discoveries even when there is a sound basis for such prediction. An evolutionary example is the failure to predict discovery of a "missing link," now known (*Australopithecus*), that was upright and tool-making but had the physiognomy and cranial capacity of an ape. Fortunately such examples do not invalidate the effectiveness of postdiction in the sense of inferring the past from the present with accompanying testing by historical methods. In fact the discovery of *Australopithecus* was an example of such testing, for without any predictive element it confirmed (i.e. strengthened confidence in) certain prior theories as to human origins and relationships and permitted their refinement.

Another oversimplified and yet generally significant distinction is that historical science is primarily concerned with configuration, and nonhistorical science with immanence. Parallel, not identical, with this is a certain tendency for the former to concentrate on the real and the individual, for the latter to focus on the ideal and the generalized, or for both to operate with different degrees of abstraction. We have seen, however, that interpretation and explanation in historical science *include* immanence and, along with it, *all* the facts, principles, laws, and so on, of nonhistorical science. To these, historical science adds its own configurational and other aspects. When it is most characteristically itself, it is compositionist rather than reductionist, examining the involvement of primary materials and forces in systems of increasing complexity and integration.

Historical science, thus characterized, cuts across the traditional lines between the various sciences: physics, chemistry, astronomy, geology, biology, anthropology, psychology, sociology, and the rest. Each of these has both historical and nonhistorical aspects, although the proportions of the two differ greatly. Among the sciences named, the historical element plays the smallest role in physics, where it is frequently ignored, and the greatest in sociology, where the existence of nonhistorical aspects is sometimes denied—one of the reasons that sociology has not always been ranked as a science. It is not a coincidence that there is a correlation with complexity and levels of integration, physics being the simplest and sociology the most complex science in this partial list. Unfortunately philosophers of science have tended to concentrate on one end of this spectrum, and that the simplest, so much as to give a distorted, and in some instances quite false, idea of the philosophy of science as a whole.

Geology exhibits as even a balance of historical and nonhistorical elements as any of the sciences, and here the relationships of the two may be particularly clear. It is in a strategic position to illuminate scientific philosophy—an opportunity not yet sufficiently exploited.

Addendum

The preceding essay was read in manuscript and constructively criticized by a number of geologists, mostly authors of other chapters in this work. The following additional comments bear on points brought up by them.

A few critics objected to the term "immanent" on the grounds that it is unfamiliar and is liable to confusion with "imminent," a word different in origin and meaning. The most nearly acceptable substitute proposed was "inherent," which does not seem to me equally precise or strictly appropriate in the intended sense. Since "immanent" is here clearly defined and consistently used, I cannot believe that it will prove misleading. It did not, in fact, mislead the readers who nevertheless criticized it.

Another critical suggestion was that under some special circumstances extrapolation from historical trends may make unique events predictable. This is, I think, possible to a limited extent and for relatively short-range prediction on a strictly probabilistic basis, as is, indeed, pointed out in the preceding essay. The example given by the commentator also illustrates the limitations: that the exhaustion of the preponderant part of the fossil fuels within the next few centuries is predictable. In fact, contingent circumstances have changed so radically and unpredictably over recent years that the term of this prediction has had to be greatly changed and has evidently become looser and less reliable than was earlier believed. Even though some confidence may yet be felt in the eventual outcome, such a historical prediction is on a different level from one based on causal analysis apart from or in addition to trends.

Along similar lines, it was also remarked that prediction from cycles may be extremely reliable when the phenomena are definitely known to be cyclical, with planetary motions as one example. That is, of course, true for the given example and for others in which such current configurational changes as occur have come to be almost entirely governed by cyclical immanent processes. The prediction is then based on the latter, alone, and a truly historical element is limited. If a nonrecurrent historical change should occur, for example if the mass of any planet were significantly altered, the predictions would prove false. To the extent that prediction is possible in such examples, it depends on knowledge of immanent causes and on strictly recurrent configurations, as specified above. Moreover, as this critic agrees in the main, similarly predictable cycles may be discounted so far as geology is concerned.

Finally, radioactive decay of an isotope is cited as an example of a precisely predictable noncyclical phenomenon. I consider radioactive decay to be analogous to the gas laws or to a chemical reaction; in each case the prediction of actual historical events is not precise in principle, but the historical circumstances may be statistically so uniform that changes in them can often be ignored in practice. Then the laws or the generalized descriptions expressed in the appropriate equations hold good just to the extent that the historical element can safely be ignored, and their predictions are not historical in principle.

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