Retrodiction in Geology

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1. Introduction

Our view of the first half of the 20th century has been influenced by what we suppose to have occurred in the middle of that century. It is by now part of the conventional wisdom of the geological community that during the 1950's and 1960's a revolution occurred. It is further supposed by many, that before the revolution there was among geologists an uneasiness resulting from the lack of an organizing principle in terms of which accumulating facts could be understood. It is difficult to see our own times in the kind of perspective that historians consider so important an ingredient of 'good' history. Geologists now only in their middle years began their careers before the 'new tectonics' came to pervade their discipline. They do not look back upon prerevolutionary days as a time waiting to be saved from crisis nor do they see themselves as having collected facts in anticipation of being able to weave them into some pattern on the basis of some as yet undiscovered truth.

But there is no question that something happened in the middle of this century which profoundly altered our account of the history of the earth. Underlying this revolutionary change was another distinctly evolutionary change. The view that geological principles are comprehended by physical laws dates from the very beginning of geology in the 18th century. In the 20th century the explicit and rigorous application of physical theory to geological inference became increasingly common. This provided a continuity that underlay the revolutionary changes which occurred. Whatever the revolution accomplished, it left the theoretical foundations of geology untouched.

There is among geologists a close association between the degree to which physical theory is invoked in some branch of geology and the extent to which that branch is accorded "scientific" status. Many geologists, and especially those who were inclined to talk about geological

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PSA 1978, Volume 2, pp. 215-226

methodology, came to see physics not only as a source of theories which could serve as a foundation for historical inferences, but also as a source of an ideal scientific methodology which could with advantage be employed by geology. A striking result of such a view is that prediction, which is supposed by most geologists to play a critical role in physics, is often supposed by them to play an equally critical role in geology. Ghiselin expresses a common view when he says, "Geological generalizations frequently seem not to predict in the manner usual for physics because the conditions with which we must deal are so intricate. The reasoning involved has more in common with our operations in driving an automobile; we predict, crudely, our future path, but since we cannot account for every contingency, we must repeatedly supply conditions and remake the calculations." ([3], p. 28).

Geologists do not suppose that their inability to predict with the precision of a physicist arises from any failing on their part but from the fact that they, unlike physicists, have chosen to deal with the world in all its unsimplified and unidealized richness. Nearly everyone has held that this predictive uncertainty rests upon geological generalizations which are statistical in form. But one may search for statistical hypotheses in geology almost in vain. Those who deny this base their view on the assumption that geological generalizations which express uncertainty are loosely formulated statistical hypotheses; that the 'usually' and 'probably' and 'tends to' of these statements are meant, in the course of further investigation, to be replaced by expressions of frequency. This may be true in some cases. In other cases it is clearly not. Neither in geology nor in everyday life are all expressions of uncertainty to be regarded as covert or badly formulated statistical statements.

Suppose a rifleman were to report that from a particular benchrest position a certain rifle <u>tends to</u> put a bullet within a five centimeter circle on a target located 100 meters from the rifle. In response to a suggestion that the uncertainty conveyed by the expression 'tends to' could be reduced in the course of further investigation, the rifleman might fire a series of bullets into the target, counting those which fell within the circle and those which did not. On the basis of this he might report that rifle <u>r</u> from benchrest position <u>p</u> puts bullets in a five centimeter circle at 100 meters with a frequency <u>f</u>. If the rifleman did respond in this way it would be plausible to suppose that he had intended his original report to be taken as an imprecisely formulated frequency hypothesis.

Consider now that statement, "Wind-driven sand, like snow, tends to settle in the wind-shadow of topographic obstructions." ([2], p. 19). A geologist would not respond to a suggestion that he undertake an investigation to reduce the uncertainty expressed by the 'tends to' of this statement by counting instances of deposition of sand in wind-shadows. The reason that he would not is clear. Frequency hypotheses are considered significant only when initial and boundary conditions are controlled or known to some minimum degree. The acceptable minimum degree of control over boundary conditions is determined by the investigator's knowledge, according to some theory, of what the relevant boundary conditions are, and how well he can determine them in a given case. The 'tends to' in the statement of the rifleman presumably refers to an uncertainty that remains after initial and boundary conditions have been controlled as much as they can be or as much as it is appropriate to do. In the statement of Dunbar and Rodgers the 'tends to' presumably refers not only to an uncertainty that would remain after the boundary conditions had been controlled as much as possible, but also to an uncertainty that stems from the values of a large unspecified variety of boundary conditions. The scope of the statement is the virtually infinite number of combinations of variables within the limits defined by 'wind', 'sand', 'topographic obstruction', and 'wind shadow'. When all of its instances are considered, the statement seems to imply, the frequency of deposition is greater than one half.

But a geologist reads much more into the statement than just this. He understands, among other things, that the 'tends to' holds <u>only</u> over the entire scope of the statement and may not apply to some designated part of the scope. If a geologist were asked, "If fine grained sand driven by a wind of velocity 45 meters per second and concentrated one grain to the cubic meter were to encounter a hemispheric topographic obstruction 2 centimeters in diameter, would some of it settle in the windshadow of that obstruction?", the answer to the question would be, "No, never." Yet, on the other hand, if the geologist were asked, "If medium grained sand driven by a wind of velocity 15 meters per second and concentrated ten grains to the cubic centimeter were to encounter a cuboid topographic obstruction one meter in height, would some of it settle in the windshadow of that obstruction?", the answer to the question would probably be, "Yes, always." The geologist takes the statement to imply that under some combinations of initial and boundary conditions sand <u>never</u> settles in the windshadow of a topographic obstruction, and that under other combinations of initial and boundary conditions some of it <u>always</u> does. The statement does not explicitly delimit the two classes. The geologist is able to do this to some extent on the basis of his previous knowledge. This does not remove all uncertainty. In the case under consideration, and in most other geological generalizations, an area of uncertainty remains in the form of a class of combinations of initial and boundary to which 'tends to' will still apply. But the degree of uncertainty about the deposition of wind driven sand is much lower than the statement taken by itself would indicate. The statement as it stands may not enter directly into explanatory inferences, but may serve rather as a summary and reminder of other statements that do.

Geological generalizations are, by and large, much more like Scriven's (see [10]) normic statements than they are like frequency hypotheses. The normic statement asserts that under 'normal' or 'standard' conditions an event of a particular kind will always be followed by an event of another particular kind. Where these conditions are not specified there is presumably some understanding as to what they are. As Hempel points out, however, "But to the extent that those conditions remain indeterminate, a general statement of causal connection amounts at best to the vague claim that there are certain further unspecified background conditions whose explicit mention in the given statement would yield a truly general law connecting the 'cause' and the 'effect' in question." ([5], p. 348). In many cases the conditions under which a geological generalization would hold are not specified because they cannot be. In other cases, however, they could be. The specification would be very complex, but it might permit the formulation of a universal statement. But such a statement would be of no use whatever under circumstances where the relevant initial and boundary conditions could not be determined, just as the gravitational equation is of no use under circumstances where mass, distance and force cannot be determined. It is very rare indeed that values for all of the relevant initial and boundary conditions can be obtained within the context of a geologic inference, and for this reason generalizations are, I believe, intentionally left in a loosely formulated state. It is in this state that they find their widest applicability.

This does not preclude the possibility that relevant specific conditions can be taken into account under favorable circumstances. It is clear, however, that the normic generalization with its implicit reference to initial and boundary conditions identified by theories which are, by universal agreement, applicable to geologic events, often gets at the sort of uncertainty encountered in geology far better than does the frequency hypothesis.

2. Retrodiction

Geologists, in their discussions of methodology, are preoccupied with uncertainty. But if one catches them in the course of their everyday inferential business, rather than in a moment of methodological self examination, a very different picture emerges. Geologists have a high degree of confidence in their assertions about the earth. In many cases, in fact, they are very nearly certain about what conditions obtained at certain times and places. Geologists, for example, know that 20,000 years ago a thick sheet of ice covered the place where Amherst, Massachusetts is now located. The mystery, I shall maintain, is not that geologists are so confident that certain events have occurred, but that in talking about their method they should be so insistent that they have no valid means of achieving such confidence. The uncertainty that the methodologists insist upon pointing to is predictive uncertainty. Yet geologists, in the actual practice of their discipline *almost never predict. The body of knowledge that geologists have pre*sented to us, before the revolution and since, is not only historical in the broad sense that it consists of statements about particular events, but in the narrow sense that those statements refer to events which are, almost without exception, located in the past. It is retrodiction, not prediction, that is the foundation of geological knowledge. The recognition of this obvious fact clears the way for the solution of some methodological problems in geology, particularly those relating to uncertainty.

The discussions of historical explanation which occupied so much of our attention during the 1950's and 1960's usually began with the events of the past as given and proceeded immediately to a discussion of how they might be explained. Considerations of how we come to have knowledge of historical events in the first place may have been largely ignored because the inferences that lead to them are likely to invoke truisms about human behavior rather than laws and theories. It may be a trivial undertaking to pursue the question of how we come to infer an event from a document which purports to describe that event. But it is important to be reminded from time to time that the statements with which historical inferences begin are not themselves historical but are instead assertions about the present. Statements about the geologic past are generated within an immensely complicated inferential context which is theoretical either in employing principles directly from the theoretical sciences, or in employing geological generalizations which have significant logical properties in common with, and important conceptual relationships with formal theories.

Our willingness to accept a geologist's account of the past results in part from the fact that he can often present more than a single inference to support his contention that certain events have occurred. He thus avoids circularity by meeting the condition that in adequate explanatory inferences the premises be supported by empirical evidence independent of the evidence presented in support of the event to be explained. But this is not always a critical factor. Our confidence in an inference is sometimes so high that the lack of independent support for its conclusions is of little consequence. Scriven's paresis case may illuminate this fact.

Scriven's paresis case is not an example of an explanation in Hempel's definition of explanation. But the failure to provide a Hempel explanation does not prevent the valid inference of a necessary antecedent condition. Given the generalization about the connection between syphilis and paresis, and given someone with paresis, it follows that the person has a history of syphilis. The significant factor here is not our ability to provide independent empirical support for the antecedent conditions, but our confidence in the generalization. "Oxidation of ore deposits may occur without attendant sulphide enrichment, but enrichment cannot take place without accompanying oxidation" ([1], p. 274) is a statement of the same form as the generalization in Scriven's paresis example, and like it will support neither a Hempel explanation nor a prediction but will support a retrodiction.

Let us take another generalization from geology which I take to be normic. "If these fissures remain open while the surface is being buried and persist after the surrounding sediment has hardened to a rock, they constitute a trustworthy indicator of the top surface of the layer they penetrate." ([11], p. 189). We can easily imagine observing some newly formed mudcracks with this knowledge in mind and thinking, "If this surface is buried, and if the fissures remain open, and if they persist while the surrounding sediment hardens, and if the whole mass is not then removed by erosion, and if it is not altered beyond recognition by heat and pressure, and if the overburden is removed, then some geologist might encounter it and be able to recognize the top surface of the layer." Clearly this does not constitute a prediction. But let us suppose we find some structures that we recognize as fossil mudcracks. By invoking these same generalizations we may retrodict that some mudcracks did form, and that they did remain open, and that they were buried, and that they did persist while the surrounding sediment hardened, and that the whole mass was not removed by erosion or altered beyond recognition by heat and pressure, and that finally the overburden had been removed to reveal the structures.

Events whose occurrence is covered by statistical hypotheses may sometimes be retrodicted although they could not have been predicted. Given, for example, the encounter of a Drosophila egg with a group of sperm we cannot predict whether the egg will be fertilzied by a sperm carrying a Y chromosome or by a sperm carrying an X chromosome. We can only say that the probability of each is about equal. If, however, the fertilized egg develops into a male we can say that it was certainly fertilized by a Y chromosome bearing sperm, and if the egg develops into a female we can say that it was certainly fertilized by an X chromosome bearing sperm. Similarly the laws of physics do not permit us to predict which U_{238} atoms will disintegrate, but they permit us to retrodict which ones did disintegrate. To take an example of geological interest, consider the following statement: "Imagine a broad hill slope of uniform material and constant slope subjected to the same conditions of rainfall, an ideal case not realized in nature. Assume that the slope, material and precipitation were such that a large number of rills existed on the surface in the form of a shallow drainage net. 'Would it be supposed that rills comparable in size and position were absolutely identical? The postulate of indeterminacy would suggest that they would be very similar but not identical. A statistical variation would exist, with a small standard deviation to be sure, but the lack of identity would reflect the chance variation among various examples, even under uniform conditions."([7], p. 190).

It would not be possible to predict the path that a drop of water falling on the slope would take. I cannot imagine a geologist wanting to do so. There are a number of possibilities among which we could not choose with certainty. After the rill had developed, however, we might be able to determine the path that the water had in fact taken; an item of information that might be of considerable geological interest.

3. Retrodictive Uncertainty

Hypotheses to the effect that the same kinds of initial conditions can result in different kinds of outcomes are the foundation of predictive uncertainty. After the occurrence of an event designated as a possible outcome by such a hypothesis, we may be able to determine directly or inferentially what the outcome was. This determination does not depend upon the frequency hypothesis that covers the occurrence of such events. If it were to be said of a Drosophila embryo that it probably resulted from an egg fertilized by a Y chromosome bearing sperm, the 'probably' might not refer to the frequency with which eggs are fertilized with different kinds of sperm but rather to our confidence in our ability to determine the sex of an embryo at some early stage of its development.

To pursue this point, consider the following example. We bet on the outcome of the roll of a die on the basis of a hypothesis about the frequency of outcomes in the long run. The hypothesis conditions our expectation about how the die will come to lie. After the die has been thrown we determine the outcome by looking at it. This determination is in no way conditioned by the hypothesis concerning the frequency of outcomes in a long run of throws. Now suppose we roll the die under a shelf that prevents us from seeing how it comes to lie. We are permitted to bet after the die has been rolled but before the shelf has been removed to reveal its face. We would make the same bets, supported by the same hypothesis, that we would make in a conventional game of dice. But suppose that after a roll the player was permitted to reach under the shelf and feel the uppermost surface of the die. Those with sensitive fingers might be able to determine how the die had come to lie. Others with less sensitive fingers might be almost, but not quite, sure and be willing to express their degree of certainty with a numerical value. That numerical value would not express the frequency with which certain faces come to lie uppermost when dice are thrown. If it expressed any frequency at all, it would be the frequency with which a given person can determine by the sense of touch which face of a die is uppermost <u>no matter how</u> the die came to lie that way. At this point the frequency of outcomes in a long series of trials is irrelevant. Thus two questions might arise in the course of a dice game. The first is, "What is the probability of rolling die so that some particular face comes to lie uppermost?" and the second is, "How is it determined which face has come to lie uppermost?" The second question seems trivial because the answer is likely to be, "By looking at it." But the determination might be based upon a retrodictive inference that did not depend upon the statistical generalization that covers the frequency of possible outcomes of dice rolling. A frequency hypothesis permits us to say of an event that has already occurred that it was one of a series in which events like it occurred with a certain frequency. But this leaves the question, "What events have actually occurred?" which is the historical question, unanswered. When someone says "He probably threw an ace," it may be supposed that he is expressing his degree of confidence, or rational credibility, in the hypothesis, "an ace was thrown." The numerical expression of the degree of confidence in the hypothesis would correspond to the numerical value for the frequency with which aces are thrown in fair games <u>only</u> if the framer of the hypothesis had no independent evidence bearing on the outcome, which is to say, only if he were unable to make a retrodiction.

For the geologist the critical question concerns uncertainty that arises within the context of a retrodictive inference. Consider the case of a body in free fall. If all we knew was the instantaneous velocity with which the body struck the ground, we would have no basis for choosing any particular set of values for the initial conditions from among the infinite number of values permitted by the equation for uniformly accelerated motion. Thus no retrodiction of initial conditions is possible. What is the difference between this case in which a retrodiction is not possible and the case of the fossil mudcracks in which a retrodiction is possible? The answer is that in the first case the initial conditions leave no trace, while in the second case they do. Perhaps this answer is too simple. A more general treatment which considers the further question of why some initial conditions leave traces and others do not may be called for. Grünbaum ([4]) has treated this further question in terms of the entropy statistics of branch systems. This principle, or something close to it, can be formulated in everyday terms. Past events may leave traces; future events do not, at least not in the same sense or to the same degree. The reason the initial conditions cannot be retrodicted in the case of the body striking the ground lies not in the logical form of the equation that covers the event, but in the fact that the initial conditions have left no trace. Generalizations, which are intended to be invoked in retrodictions, will point to strong interactions between systems. If an interaction leaves no trace, or if the trace of an interaction is destroyed we cannot retrodict the interaction. Uncertainty about interactions comes out of uncertainty about traces.

Predictive and retrodictive uncertainty arise out of different and to some extent unrelated circumstances. Predictive uncertainty results when a prediction is based upon a general hypothesis to the effect that identical kinds of initial conditions can lead to different kinds of outcomes. Retrodictive uncertainty, on the other hand, results when a retrodiction is based upon a general hypothesis to the effect that a single kind of outcome can result from different kinds of initial conditions. The smoldering ruin of a house is a trace consistent with a great number of possible initial conditions, including faulty wiring, the leakage of natural gas, and the deliberate setting of a fire by an arsonist. But for a fire marshal, 'smoldering house' is a term that includes different conditions each one of which is consistent with only one set of antecedents. By detailed specification of the outcome he may be able to adduce a hypothesis connecting this outcome with some particular antecedents.

How might uncertainty about fossil mudcracks arise? Suppose a geologist were to encounter some structures and say, "These are probably fossil mudcracks." This expression of uncertainty would not be about how mudcracks are formed or about the frequency with which, once having formed, traces of mudcracks are preserved. The geologist is uncertain about whether mudcracks have formed. If he knew that he was dealing with preserved mudcracks, then he could retrodict with certainty the necessary antecedents for the preservation of mudcracks. Retrodictive uncertainty arises in this case from the fact that as far as the geologist can tell the structure before him is consistent with more than one set of antecedents. Similarly a physician might be uncertain whether his diagnostic techniques were sensitive enough to identify paresis. As far as he can tell, the condition before him is consistent with a history of syphilis and consistent with a history of something else.

It may seem contrived to count these as cases of retrodictive uncertainty arising from the presumption that different antecedents are consistent with a single consequence. What are the covert hypotheses that might serve to justify such a presumption? The answer is that there are no such hypotheses, at least not in the sense that there are identifiable hypotheses that serve to justify the uncertainty of predictions about radioactive disintegration and dice rolling. Geologists are apparently more inclined to suppose that the same antecedents have different consequences than to suppose the contrary. A geologist will maintain that it is not really true that different initial conditions may result in indistinguishable subsequent states; it only seems to be true. The inability to distinguish among possible consequences is not the sort of uncertainty geologists are prepared to perpetuate in explicitly formulated hypotheses. It is not the sort of uncertainty to be enshrined metaphysically or even theoretically. It is an ephemeral uncertainty to be removed in the course of continuing investigation. The methodological consequence of this conviction among geologists is that they make no effort to formulate statistical hypotheses that might serve as grounds for assigning a numerical value to the degree of rational credibility, or probability, of a retrodictive inference, even though there is no reason in principle why they should not do so.

An example of this attitude may be found in the problem of frosted sand grains. Pettijohn noted,

> Rarely do quartz grains show a high polish. Some sand grains, on the other hand, have a striking surface character variously described as "mat," "frosted," or "groundglass." This surface character is most commonly seen on the grains of highly quartzose and well-rounded sands of which the St. Peter (Ordovician) is the best example in the United states. Frosting has been commonly attributed to aeolian action and has been mapped in the European Pleistocene deposits by Callieux (1942), who considered the feature a criterion of periglacial wind action. The similarity of the surface to that produced on glass by sandblast gives credence to this theory, though there is little or no field evidence to support this concept. Glass, subject to the action of hydroflouric acid, however, also acquires a frosted surface, and perhaps therefore this type of surface is a product of prolonged action by natural solvents ([8], p. 70).

In isolating the problem of retrodictive inference I have not done justice to the intricacy of geological practice. Historical inferences in geology are immensely complex. They do not consist of isolated retrodictive inferences, each one invoking a single generalization. Consider the relatively simple and straightforward example, "The first orogeny affecting the Cedar Hills is inferred from the coarse conglomerates of the Indianola, which indicates large active streams with high gradients and suggests that erosion had been accelerated by the folding or uplifting of mountains." ([9], p. 641). We are presented with a chain of events, including at least the uplifting of mountains, the steepening of gradients, the acceleration of erosion, the transportation of gravel, the deposition of gravel, the preservation of the sediment, and the formation of the conglomerate. Each step in this genetic series must, upon request for justification, be supported by appropriate generalizations.

Another complication arises from the fact that independent support for the antecedents cited in each retrodictive inference is sought. Whether or not this support is found will bear upon the credibility of the inference. A geologist is much more likely to account for some problematic scratches on a boulder as having resulted from glacial action if he has independent evidence for glaciation in the region where the boulder was found, just as a physician is more inclined to label some problematic symptoms as paresis if he knows on independent grounds that this patient has a history of syphilis.

Finally it should be mentioned that geologists are likely to regard geologic knowledge as incomplete unless their historical account can be understood, or explained, within the context of some comprehensive physical theory. A retrodiction does not always result in an explanation, although on occasion it may. Our inability to predict the path that a drop of water will take down a newly exposed surface is irrelevant to the practice of geology, but being able to treat the development of a drainage system as a stochastic process contributes to our understanding of the history of the earth.

4. Conclusion

Geological knowledge presents us with a paradox. Geologists have a level of confidence in the assertions they make about the past that often approaches certainty, and yet when we examine the principles that might serve to justify these historical statements, we find that they almost always express a degree of uncertainty. The paradox is resolved by a recognition of the asymmetry of retrodictability and predictability. Geologists are not much interested in the future. They are preoccupied with what has happened, and they can infer, without much difficulty, some of the antecedent conditions necessary for the occurrence of events in the present. To judge geological generalizations by their ability to support predictions is absurd. Geological generalizations are commonly used in support of retrodictive inferences, and when judged by their ability to do this they measure up very well. I have expressed in rather formal terms the truism that the future is more uncertain than the past. Geologists know this very well, and so do ordinary men. It would not be necessary to belabor the point were it not for the fact that a few geologists in their discussions of uncertainty have wholly ignored retrodiction and have thereby been led to overestimate the prevalence of uncertainty in geologic knowledge. But geology is not a faulty predictive historical science. It is the most highly developed retrodictive historical science.

Despite the asymmetry of recordability, retrodictive uncertainty arises in geology. Its precise character has been overlooked because the general hypotheses that entail retrodictive uncertainty are hardly ever explicitly formulated. They are not so formulated, because geologists hold that the uncertainty they would express can in principle, and often in fact, be eliminated.

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