24 January 1958, Volume 127, Number 3291 SCIENCE

Inductive Inference: A New Approach

Israel Scheffler

On what grounds do we choose the theories by which we anticipate the future? How do we decide what to predict about cases never before observed? These questions concerning what is traditionally called "induction" are among the most fundamental and most difficult which can be asked about the logic of science. Much reflection has been devoted to these questions in recent years, but no contribution has proved more incisive and challenging than that of Nelson Goodman of the University of Pennsylvania, whose papers on induction and allied problems have activated lively philosophic controversy over the past twelve years.

In 1955, Goodman published *Fact, Fiction, and Forecast* (*1*), in which he presented the outlines of a new approach to the understanding of induction. This recent work has also aroused considerable comment by philosophers, both in print and out, and it is safe to say that the discussion is still in its early stages. The scientific public is, however, largely unaware of this new development, just as it was largely unacquainted with the controversies that preceded it. If there is no real boundary between science and the philosophy of science, the consideration of fundamental research in the logic of science ought not to be confined, even at the early stages, to circles of philosophers. The aim of this article is thus to acquaint the scientific reader with the background and the direction of Goodman's investigations, as they bear on the interpretation of induction.

The author is lecturer on education in the Graduate School of Education, Harvard University, Cambridge, Mass.

Hume's Challenge and the Generalization Formula

The starting point for all modern thinking about induction is David Hume's denial of necessary connections of matters of fact: between observed cases recorded in the evidence and predicted cases based on the evidence there is a fundamental logical gap, which cannot be bridged by deductive inference. If, then, the truth of our predictions is not guaranteed by logical deduction from available evidence, what can be their rational justification? This challenge, arising out of Hume's analysis, has evoked a variety of replies. Leaving aside the reply of the skeptics, who are willing to admit that all induction is indeed without rational foundation, and that of the deductivists, who strive vainly to show Hume wrong, we find two replies which have gained wide popularity, the first primarily among philosophers, the second among scientists as well.

The first reply criticizes the assumption that rational justification can be only a matter of deduction from the evidence, pointing out that the normal use of expressions such as "rational," "reasonable," "based on good reasons," and so forth sanctions their application to statements referring to unexamined cases, and hence not deducible from accumulated evidence. This reply, although true, is, however, woefully inadequate. For not every statement which outstrips available evidence is reasonable, though some are. Outstripping the evidence is, to be sure, no bar to rationality, but neither does it guarantee rationality. If we are to meet the challenge posed, we must go on to formulate the specific criteria by which some inductions are justified as reasonable while others are rejected as unreasonable, though both groups outstrip the available evidence. Now it is likely that at least part of the reason why this further task has been slighted is that the adequacy of the second reply has largely been taken for granted.

This second reply, stated in one form by Hume himself, is that reasonable inductions are those which conform to past regularities. In modern dress, it appears as the popular assertion that predictions are made in accordance with general theories which have worked in the past. What leads us to make one particular prediction rather than its opposite is not its deducibility from evidence but rather its congruence with a generalization thoroughly in accord with all such evidence, and the correlative disconfirmation of the contrary generalization by the same evidence. (I shall refer to this hereafter as the "generalization formula"). Of course, if no relevant evidence is available to decide between a given generalization and its contrary, or if the available evidence is mixed, neither generalization will support a particular inductive conclusion. But it is only to be expected that every limited body of evidence will fail to decide between *some* generalization and its contrary, and hence that we will generally not be able to choose between *every* particular prediction and its opposite. It is sufficient, therefore, for a formulation of the criteria of induction to show how certain bodies of evidence enable us to decide between certain conflicting inductions. This the generalization formula seems to accomplish. For if there is evidence which consistently supports a given generalization, then the contrary generalization is *ipso facto* disconfirmed, and our particular inductive conclusions seem automatically selected for us. There are, of course, details to be taken care of, relating to such matters as the calculation of degrees of support which generalizations derive from past evidence, but, in principle, we have our answer to the challenge of induction.

Goodman's Refutation of the Generalization Formula

It is this sanguine estimate which has been thoroughly upset by Goodman's researches. Published in 1946 and 1947, his early papers in the philosophical journals dealt with a variety of interrelated questions: the nature of scientific law, of dispositional properties, of potentiality, of relevant conditions, of counterfactual judgments, of confirmation or induction (*2*). They immediately aroused a storm of controversy. What made the papers so disturbing to the philosophic community was the fact that, while all these questions were shown to be intimately connected, Goodman's logically rigorous attempts to answer them without going around in circles ended in a big question mark. Appearing at a time when logicians had been making considerable progress in analyzing other aspects of scientific method, these results came as a shock. Goodman's investigations, it seemed, had sufficed to undermine all the usual formulas concerning the most basic concepts of the logic of science, but his repeated and ingenious efforts to supply a positive alternative had all turned out fruitless. In the philosophic discussions that followed, every attempt was made to skirt Goodman's disheartening results. They were declared unimportant for the practicing scientist. The initial questions were asserted to be insoluble, hence worthless. Many papers, on the other hand, proposed what seemed perfectly obvious solutions that turned out to be question-begging. Only a very few authors fully recognized the seriousness of the situation for the philosophy of science and tried to cope with it directly (*3*).

In 1953, with the whole matter still very much unsettled, Goodman delivered a series of three lectures at the University of London, in which he again addressed himself to the problem. These lectures, together with his major 1946 paper, were then published together in his book *Fact, Fiction and Forecast,* which appeared in 1955 (*1*). Here Goodman essayed a new and positive approach to some of the major questions he had faced earlier. He did not offer his book as a final solution to all the original problems. He did, however, present a fresh approach, worked out with sufficient rigor to put discussion of it on a fruitful basis. But we are getting ahead of our story and must now return to see how Goodman's early work affected the theory of induction.

How did Goodman's early papers upset complacency with respect to the generalization formula (according to which we make these predictions congruent with generalizations thoroughly in accord with past evidence)? We may profitably approach this matter in the light of a passage from J. S. Mill's *Logic*. Although it does seem true that, for every particular induction we make, there is some generalization related to it in the manner described, Mill argues that generalizations which are equally well supported by available evidence vary in the sanction they provide for their respective particular inductions: "Again, there are cases in which we reckon with the most unfailing confidence upon uniformity, and other cases in which we do not count upon it at all. In some we feel complete assurance that the future will resemble the past, the unknown be precisely similar to the known. In others, however invariable may be the result obtained from the instances which have been observed, we draw from them no more than a very feeble presumption that the like result will hold in all other cases. . . . When a chemist announces the existence and properties of a newly discovered substance, if we confide in his accuracy, we feel assured that the conclusions he has arrived at will hold universally, though the induction be founded but on a single instance. . . . Now mark another case, and contrast it with this. Not all the instances which have been observed since the beginning of the world in support of the general proposition that all crows are black would be deemed a sufficient presumption of the truth of the proposition, to outweigh the testimony of one unexceptionable witness who should affirm that in some region of the earth not fully explored he had caught and examined a crow, and had found it to be grey. Why is a single instance, in some cases, sufficient for a complete induction, while in others myriads of concurring instances, without a single exception known or presumed, go such a very little way towards establishing an universal proposition?" (*4*).

And Goodman gives an analogous example when he writes: "That a given piece of copper conducts electricity increases the credibility of statements asserting that other pieces of copper conduct electricity, and thus confirms the hypothesis that all copper conducts electricity. But the fact that a given man now in this room is a third son does not increase the credibility of statements asserting that other men now in this room are third sons, and so does not confirm the hypothesis that all men now in this room are third sons. Yet in both cases our hypothesis is a generalization of the evidence statement. The difference is that in the former case the hypothesis is a *lawlike* statement; while in the latter case, the hypothesis is a merely contingent or accidental generality. Only a statement that is *lawlike*—regardless of its truth or falsity or its scientific importance—is capable of receiving confirmation from an instance of it; accidental statements are not." (*1,* p. 73)

But it is Goodman's further formulation of the problem that is crucial. For what has so far been shown is that, in addition to all credible particular inductions, generalization from the evidence also would select certain incredible ones. Now Goodman shows that among these incredible ones lie the very negations of our credible predictions concerning new cases. To apply his previous example, it is not merely that by generalization we selectively establish, in addition to the credible prediction that the next specimen of copper will conduct electricity, also the incredible one that the next present occupant of this room to be examined is a third son. Rather, we do not even establish that the next specimen of copper conducts electricity, for we can produce a generalization equally supported by the evidence and yielding the prediction that it does not. Or, putting this point in the form of a specific example, while the available evidence clearly supports:

(S_1) All specimens of copper conduct electricity.

and clearly disconfirms its contrary:

(S_2) All specimens of copper do not conduct electricity.

this is not sufficient to yield the particular induction concerning a new copper specimen c, to be examined:

(S_3) c conducts electricity.

since the same evidence also and equally supports:

(S_4) All specimens of copper are either such that they have been examined prior to t and conduct electricity or have not been examined prior to t and do not conduct electricity.

while clearly disconfirming *its* contrary:

(S_5) All specimens of copper are either such that they have been examined prior to t and do not conduct electricity or have not been examined prior to t and do conduct electricity.

thus giving rise to the negate of S_3:

(S_6) c does not conduct electricity.

if it is assumed true that:

(S_7) c has not been examined prior to t.

For cases assumed new, then, the generalization formula selects no particular inductions at all. Merely to be told to choose our inductions by reference to theories which work relative to past evidence is hence to be given worthless advice. Nor does this situation improve with the accumulation of relevant data over time. For even if we later find S_6 false and add S_3 to our evidence, leading to a rejection of S_4, we do not thereby eliminate other hypotheses which are exactly like S_4 but which specify times later than t. Accordingly, no matter how much empirical data we have accumulated and no matter how many hypotheses like S_4 we have disconfirmed up to a given point in time, we still have (by the generalization formula) contradictory predictions for every case not yet included in our data. No matter how fast and how long we run, we find we are standing still at the starting line.

This predicament holds, of course, only for cases assumed to be new. Using our previous example, if neither S_7 nor its negate is assumed, then S_4 yields neither S_3 nor S_6, while if S_7 is assumed false, then S_4 coincides with S_1, implying S_3 rather than S_6. This is not surprising, however, since, if S_7 is false, c is identical with one of our original evidence cases, all of which are described by the evidence itself as conducting electricity; S_3 is thus implied deductively by the evidence at hand, given the general understanding that no cases have been omitted.

As soon as we leave the safe territory of examined cases, however, and try to deal with a new one, generalization yields contradictory inductions, deciding for neither. And, further, since the adoption of a generalization constitutes wholesale endorsement of appropriate particular inductions yet to be made, then even if we do not know about some specific case that it is a new one, our unrestricted adoption of generalizations gets us into trouble if we can make the assumption of novelty for at least one case within the appropriate range. Since, moreover, we patently do choose between contradictory inductions covering new cases, as well as between competing generalizations, the generalization formula must be wrong as a definition of our inductive choices. In our previous example, we obviously in practice would *not* hold S_4 equally supported by uniformly positive evidence supporting S_1, nor would we under such conditions have any hesitation in rejecting S_6 in favor of S_3. This clearly indicates that the generalization formula is not adequate to characterize our inductive behavior. We apparently employ additional, nonsyntactic criteria governing the extension of characteristics of our evidence-cases to other cases in induction.

These criteria of what Goodman calls "projectibility" select just those generalizations *capable* of receiving support from their positive instances and in turn sanctioning particular inductions. Projectible hypotheses may, in individual cases, fail to sanction any particular inductions (for example, in cases where we have two such hypotheses which conflict), but no non-projectible hypothesis sanctions any induction, no matter how much positive support it has in the sense of the generalization formula. Goodman's problem is then to define projectibility, which is, in turn, needed to define induction. Since counterfactual judgments (for example, "If this salt, which has not in fact been put in water, had been put in water, it would have dissolved.") are, moreover, construable as resting upon just such generalizations as are projectible, that is, legitimately used for induction (in this case, "Every sample of salt, when put into water, dissolves."), and, furthermore, are themselves used to explain dispositional predicates, such as "is soluble," the definition of projectibility would throw light on these additional issues as well.

Attempts to Repair the Generalization Formula

It may be thought that the characterization of projectibility can be accomplished rather easily, simply by ruling out generalizations making reference to time. Recall that, in our above example, the trouble arose because the available evidence equally supported S_1 and S_4. But whereas the predicate "conducts electricity" makes no reference to time, the predicate "has been examined prior to t and conducts electricity or has not been examined prior to t and does not conduct electricity" makes reference to time of examination, and moreover can be explained, given such reference, in terms of the former predicate. It may further be pointed out that, without assumption S_7 (making reference to time of examination), no contradiction arises. It is only when we add S_7 to S_4 that S_6, which contradicts S_3, is derived. Why not use this, then, as a rule for eliminating S_4—namely, its requiring an additional assumption about time of examination to produce one of our contradictory inductions?

The answer is that the situation is easily reversed. Symbolize the predicate "conducts electricity" by C and the other, more complicated one, of S_4, by K; symbolize "has been examined before t" by E. It is true that, as the present argument maintains, K is then definable as

$$(E \text{ and } C) \text{ or } (\text{not-}E \text{ and not-}C)$$

("has been examined before t and conducts electricity or has not been examined before t and does not conduct electricity"). However, it is also true that, taking K as our primitive idea, C is definable as

$$(E \text{ and } K) \text{ or } (\text{not-}E \text{ and not-}K)$$

Furthermore, in the latter mode of description, S_1 would become:

(S_1') All specimens of copper are either such that they have been examined prior to t and have the property K or have not been examined before t and do not have the property K.

while S_4 would become:

(S_4') All specimens of copper have the property K.

To derive a parallel to S_3, we need to show that a new case c does not have the property K. This we can do if we now supplement S_1' with S_7 getting:

(S_3') c does not have the property K.

And we derive our contradictory particular induction, parallel to S_6, from S_4', without using S_7:

(S_6') c has the property K.

Thus, neither the employment by a hypothesis of a predicate referring to time nor its need of supplementation by S_7 in order to produce contradiction is a reliable clue with which to try to repair the generalization formula. Neither is, strictly speaking, any clue at all.

But perhaps the generalization formula

is being applied too narrowly. We have, after all, been considering isolated statements in abstraction from other, relevant and well-established, hypotheses. In the above illustration we have, for instance, so far ignored the fact that available evidence also supports (by the generalization formula) a number of hypotheses of the following kind:

(S_8) All specimens of iron conduct electricity.

(S_9) All specimens of wood fail to conduct electricity.

and that these in turn lend credence to the following larger generalization:

(S_{10}) All classes of specimens of the same material are uniform with respect to electrical conductivity.

This larger generalization, having independent warrant and conflicting with S_4, serves thereby to discredit it, thus eliminating the troublesome induction S_6. In this way, it may be argued, the generalization formula can be rendered viable simply by taking account of a wider context of relevant hypotheses.

It takes but a moment of reflection, however, to see the weakness of such an argument. For, by reasoning analogous to that initially employed in introducing S_4, it will be seen that the very same evidence which supports S_8, S_9, and S_{10} also and equally (by the generalization formula itself) supports:

(S_8') All specimens of iron have the property K.

(S_9') All specimens of wood fail to have the property K.

(S_{10}') All classes of specimens of the same material are uniform with respect to possession of the property K.

This latter large generalization, it will be noted, produces just the opposite effect from that of S_{10}. It conflicts with S_1, thereby, by analogous argument, discrediting it and eliminating the induction S_3 rather than S_6. Which of these conflicting large generalizations shall we now choose to take account of, S_{10} or S_{10}'? It is evident that we are again face to face with the very problem with which we started and that the proposal to repair the generalization formula by referring to other relevant hypotheses selected by it serves merely to postpone our perplexity. For these other hypotheses, in conflict themselves, are of no help unless we have some way of deciding which of them are projectible. In the face of difficulties such as these, *it becomes impossible to explain our choice of predictions by reference to whether or not they accord with generalizations which work,* no matter how widely the scope of this principle is construed.

Goodman's New Approach

Goodman's new idea is to utilize pragmatic or historical information that may fairly be assumed available at the time of induction, and to define projectibility in terms of such extrasyntactic information. The generalization formula, it will be recalled, rests on the notion of an *accordance* between a predictive generalization and the evidence by which it is supported, an accordance which can be determined solely by an examination of the generalization and its evidence-statements. In this sense, the relation of accordance is formal or syntactic (as the relation of deduction is), making use of no material or historical information. Goodman now suggests that, in order to specify the predictive generalizations we choose on the basis of given evidence, we need not restrict ourselves merely to the syntactic features of the statements before us. Rather, he makes the radical proposal that we use also the historical record of past predictions, and in particular, the *biographies* of the specific terms or predicates employed in previous inductions. Our theories, he suggests, are chosen not merely by virtue of the way they encompass the evidence, but also by virtue of the way the language in which they are couched accords with past linguistic practice.

His basic concept is "entrenchment," applicable to terms or predicates in the degree to which they (or their extensional equivalents, that is, words picking out the same class of elements, like "triangle" and "trilateral") have actually been previously employed in projection: in formulating inductions on the basis of positive, though incomplete evidence. To illustrate with our previous example, the predicate "has been examined prior to t and conducts electricity or has not been examined prior to t and does not conduct electricity" is less well entrenched than the predicate "conducts electricity," *because the class it singles out has been less often mentioned in formuating inductions.* The factor of actual historical employment of constituent predicates or their equivalents can thus be used to distinguish between hypotheses such as S_1 and S_4, which are equal in point of available positive instances. Goodman appeals, then, to "recurrences in the explicit use of terms as well as to recurrent features of what is observed," suggesting that the features which we fasten on in induction are those "for which we have adopted predicates that we have habitually projected" (*1*, pp. 96, 97). With this idea as a guide, Goodman first defines presumptively projectible hypotheses. Next, he defines an initial projectibility index for these hypotheses. Finally, he defines degree of projectibility by means of the initial projectibility index as modified by indirect information embodied in what he calls "overhypotheses," *which must themselves qualify as presumptively projectible.* The latter use made of indirect evidence is worked out with great care and detail and is of independent theoretical interest.

Roughly, degree of projectibility is to represent what Goodman earlier called "lawlikeness," (that is, that property which, together with truth, defines scientific laws) and constitutes therefore not only an explanation but also a refinement of the latter. With the explanation of lawlikeness, Goodman suggests that the general problem of dispositions is solved. For this general problem is to define the *relationship* between "manifest" or observable predicates (for example, "dissolves") and their dispositional counterparts (for example, "is soluble") and manifest predicates may now be construed as related by true lawlike or projectible hypotheses to their dispositional mates. Other problems, such as the nature of "empirical possibility" are also illuminated by this approach, and some light is thrown on the difficult question of counterfactual judgments which, however, still resists full interpretation.

The most natural objection to Goodman's new approach is that it provides no explanation of entrenchment itself. In using this notion to explain induction, however, Goodman does not at all rule out a further explanation of why certain predicates as a matter of fact become entrenched while others do not. His purpose is to formulate clear criteria, in terms of available information, that will single out those generalizations in accordance with which we make predictions. The strong point of his treatment is that his criteria do indeed seem effective in dealing with the numerous cases he considers.

A possible misconception concerning the use of "entrenchment" as a basic idea is that it may lead to the ruling out of unfamiliar predicates, thus stultifying the growth of scientific language. Unfamiliar predicates may, however, be well entrenched if some of their extensionally equivalent mates have been often projected, and they may acquire entrenchment indirectly through "inheritance" from "parent predicates"—that is, other predicates related to them in a special way outlined in detail in Goodman's discussion (*1*, p. 105). Furthermore, Goodman's criteria provide methods for evaluating *hypotheses*, not predicates, so that wholesale elimination of new scientific terms is never sanctioned in his treatment.

As remarked previously, the critical discussion of Goodman's new approach is still in its early stages (*5*). His formulations will undoubtedly undergo further refinement and revision with continuing study, but even in their present form they will have contributed much toward putting important questions in the philosophy of science on a scientific basis.

References and Notes

1. N. Goodman, *Fact, Fiction and Forecast* (Harvard Univ. Press, Cambridge, Mass., 1955).
2. — -, "A Query on Confirmation," *J. Philosophy* 43, 383 (194 "The Problem of Counterfactual Conditionals, *J. Philosophy* 44, 113 (1947).
3. See in particular R. Carnap, "On the Application of Inductive Logic," *Philosophy and Phenomenological Research* 8, 133 (1947); N. Goodman, "On Infirmities of Confirmation Theory," *Philosophy and Phenomenological Research* 8, 149 (1947); R. Carnap, "Reply to Nelson Goodman," *Philosophy and Phenomenological Research* 8, 461 (1947).
4. J. S. Mill, *A System of Logic*. (Longmans, London, 1843; new impression, 1947), book III, chap. III, sect. 3, p. 205.
5. See, in this connection. the long study of *Fact, Fiction and Forecast* J. C. Cooley [*J. Philosophy* 54, 293 (1957)] Goodman's reply [*J. Philosophy* 54, 531 (1957)].

Robert H. Lowie, Anthropologist

With the death of Robert Lowie, on 21 September 1957, American anthropology lost an ethnographer and ethnologist who had earned international esteem during the richly productive course of his 74 years.

Ethnographically his interests were world-wide, and his command of the literature on primitive peoples was unmatched. He belonged to a generation whose research among tribal peoples was still opening up unsuspected ranges of social organization and human values. Testifying both to his self-discipline and to his enthusiasm, monograph after monograph on the Indians of the American plains appeared under his name. He wrote steadily, with a fine pen and in a flowing hand, manuscripts that required little revision. His name is permanently associated with scholarly records of the North American Indians: the Plains, the Shoshone, the Hopi. His early and enduring devotion was given primarily to the Crow Indians, whose language and people he prized. His early enthusiasm was given to his adventures among the Lake Athabasca Chippewa. Later, his interests turned to South America. Here he had no opportunity for field research, but his characteristic generosity and his interest in the work of others did much to advance knowledge of the primitive peoples of Brazil.

His concern with European ethnography was the last phase of his area interests and represented a return to his youth. Born in Vienna in 1883, he was brought to New York at about the age of ten, where he grew up as a bilingual in a middle-class Jewish intellectual milieu. Thus, his interest in languages and in bilinguality, as well as his bent for scholarship, were early established and deep. Similarly, he retained throughout his life a certain old-world courtliness.

During the second world war he began teaching European ethnography, and from this his interest in Germanic culture and society, with certain autobiographical implications, developed steadily. Yet he delighted, despite its anti-Germanic overtones, in Rebecca West's *Black Lamb and Grey Falcon.* On the other hand, his unremitting capacity for detail made him suspect facile theorizing in the "national character" school of anthropology that blossomed during and after the war. He set himself against such overgeneralizations in *The German People* (1945).

However, these successive periods of ethnographic interest—study of the North American Indians, the tribes of South America, and the German peoples—were evenly balanced by a desire to formulate broader ethnologic syntheses. *Primitive Society* appeared in 1920. Reissued and widely translated, it remains today a fundamental statement. *History and Theory of Anthropology* (1937), *Primitive Religion* (1924), and *Origin of the State* (1927) are only indices of his indefatigable, informed, and systematic intellect. He dealt with theories as if they were artifacts—dryly, carefully, factually. This approach was, and is, a salutary corrective to half-cocked enthusiasts.

Although Lowie was never personally as close to Boas as some of his contemporaries were, he was formed (in the French sense of the word) by that seminal genius. Although he had studied chemistry in his undergraduate days and had been an enthusiastic admirer of Karl Pearson, it was in anthropology that Columbia awarded him a Ph.D. degree in 1908. From that time his devotion to anthropology was unflagging. Nevertheless he found opportunities to express a youthful militancy in support of the feminist movement, encouraged by his lifelong friend Elsie Clews Parsons. And he participated in what must have seemed, to so nonpolitical a character, the innocuous ferment created by "the Masses" and John Reed. He always retained a warm memory of his liberalism of those early days.

From 1921 to 1950 he taught—and, more important, he trained, in critical objectivity—generation after generation of students at the University of California in Berkeley. He served for nine years (1924 to 1933) in the thankless post of editor of the *American Anthro*