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INDUCTION IN SCIENCE (1963)

Laws, the vertebrae of science, are sometimes believed to be established by induction (empiricist tradition, as represented by Bacon), and at other times to be the product of reason and free imagination (rationalist tradition, as exemplified by Einstein). The first belief is frequent among field and laboratory workers, the second among theoreticians. When the *establishment* of a law statement is mentioned, either of two entirely different inferential procedures may be meant: the *inception* or introduction of the statement, or its *test*. In either case it is accepted that inferences are involved, rather than direct and immediate apprehensions. The question is whether the inferences are inductive, deductive, or perhaps neither exclusively inductive nor exclusively deductive, but a combination of the two with the addition of analogy and of some kind of invention or creation.

In this paper we shall investigate the question of whether scientific inference is predominantly inductive, as claimed by inductivist metascience (e.g., Keynes 1921; Reichenbach 1949; Carnap 1950; Jeffreys

1957; von Wright 1957), or predominantly deductive, as maintained by deductivism (e.g., Duhem 1914; Wisdom 1952; Popper 1959)—or, finally, whether it actually goes along a third way of its own. The discussion will be confined to factual statements, usually called 'empirical sentences', without thereby denying the great heuristic value that case examination also has in mathematical invention and problem solving (Pólya 1954).

1. INDUCTION PROPER

Before approaching the problem, let us clear the ground. By *induction stricto sensu* I shall understand the type of nondemonstrative reasoning consisting in *obtaining or validating general propositions on the basis of the examination of cases*. Or, as Whewell put it long ago (1858), "by *Induction* is to be understood that process of collecting general truths from the examination of particular facts". This linguistic convention makes no appeal to epistemological categories such as "new knowledge", which are often used in the characterization of inductive inference, although the enlargement of knowledge is the purpose of both inductive and deductive inference.

The proposed equation of induction and generalization on the basis of case examination leaves the following kinds of inference *out* of the domain of inductive inference: (1) *analogy*, which is a certain reasoning from particular to particular, or from general to general, and which probably underlies inductive inference; (2) *generalization* involving the introduction of *new* concepts, i.e., of concepts absent in the evidential basis; (3) the so-called *induction by elimination*, which is nothing but the refutation of hypotheses found unfit because their observable consequences, derived by deduction, do not match with the empirical evidence at hand; (4) scientific *prediction*, which is clearly deductive, since it consists in the derivation of singular or existential propositions from the conjunction of law statements and specific information; (5) *interpolation* in the strict sense (not, however, curve fitting), which is deductive as well, since it amounts to specification; (6) *reduction*, or assertion of the antecedent of a conditional on the ground of the repeated verification of the consequent.

With the above definition of induction in mind, let us inquire into the role of induction in the formation and testing of the hypotheses that are dignified with the name of *laws of nature* or *of culture*.

2. INDUCTION IN THE FRAMING OF HYPOTHESES

The premises of induction may be singular or general. Let us distinguish the two cases by calling *first-degree induction* the inference leading from the examination of observed instances to general statements of the lowest level (e.g., "All humans are mortal"), and *second-degree induction* the inference consisting in the widening of such empirical generalizations (leading, e.g., from such statements as "All humans are mortal", "All lobsters are mortal", "All snakes are mortal" to "All metazoans are mortal"). First-degree induction starts from singular propositions, whereas second-degree induction is the generalization of generalizations.

Empirical generalizations of the type of "Owls eat mice" are often reached by first-degree induction. Necessary, though not sufficient, conditions for performing a first-degree induction are: (a) the facts referred to by the singular propositions that are to be generalized must have been observed, must be actual facts, never merely possible ones like the burning of this book or the establishment of a democratic government in Argentina; (b) (the referents of) the predicates contained in the generalization must be observable *stricto sensu*, such as predicates designating the color and size of perceptible bodies. Hence, the "observables" of atomic theory, such as the variables representing the instantaneous position or angular momentum of an electron, will not do for this purpose, since they are actually theoretical predicates (constructs).

Condition (a) excludes from the range of induction all inventions, and countless elementary generalizations, such as those involving dispositions or potential properties. Condition (b) excludes from the domain of induction all the more important scientific hypotheses: those which have been called *transcendent* (Kneale 1949) or *noninstantial* (Wisdom 1952), because they contain nonobservable, or theoretical, predicates, such as "attraction", "energy", "stable", "adaptation", or "mental". Transcendent hypotheses, i.e., assumptions going beyond experience, are most important in science because, far from merely enabling us to colligate or summarize empirical data, they enter into the explanation of data.

The hypothesis "Copper is a good conductor" is a second-degree inductive generalization. It contains the class terms 'copper' and 'conductor' (a dispositional term). Its generalization "All metals are good conductors" is, a fortiori, another second-degree induction: it refers not only to the class of metals known at the moment it was framed, but to the conceptually open class of metals known and knowable. We do not accept the latter generalization just because of its inductive support, weighty as it is, but also—perhaps mainly—because the theoretical study of the crystal structure of metals and the electron gas inside them

shows us that the property denoted by the predicate "metal" or, if preferred, "solid" is functionally associated with the property of being a conductor. This association, which transcends the Humean juxtaposition of properties, is expressed in law statements belonging to the theory of solid state. We accept the generalization with some confidence, because we have succeeded in understanding it by subsuming it under a theory. Similarly, we know since Harvey that "There are no heartless vertebrates" is true, not because this statement has been found and verified inductively, but because we understand the function of the heart in the maintenance of life.

Compare the above examples with the low-level generalization "All ravens are black", the stock-in-trade example of inductivists. Ornithology has not yet accounted for the constant conjunction of the two properties occurring in this first-degree induction. The day animal physiology hits upon an explanation of it, we shall presumably be told something like this: "All birds having the biological properties *P, Q, R*, . . . are black". And then some ornithologist may inquire whether ravens do possess the properties *P, Q, R*, . . . , in order to ascertain whether the old generalization fits in the new systematic body of knowledge.

In sum, enumerative induction does play a role in the framing of general hypotheses, though certainly not as big a role as the one imagined by inductivism. Induction, important as it is in daily life and in the more backward stages of empirical science, has not led to finding a single important scientific law, incapable as it is of creating new and transempirical (transcendent) concepts, which are typical of theoretical science. In other words: induction may lead to framing *low-level, pretheoretical, ad hoc*, and *ex post facto* general hypotheses; the introduction of comprehensive and deep hypotheses requires a leap beyond induction.

3. INDUCTION IN THE TEST OF HYPOTHESES

Scientific hypotheses are empirically tested by seeking *both* positive instances (according to the inductivist injunction) and unfavorable ones (deductivist rule). In other words, the empirical test of hypotheses includes both confirmations and unsuccessful attempts at refutation. But only first-degree inductive generalizations *have* instances; hence they are the only ones that can be directly checked against empirical evidence. Statements expressing empirical evidence, i.e., basic statements, do not contain theoretical predicates such as "mass", "recessive character", or "population pressure". Hence, case examination by itself is irrelevant both to the framing and to the testing of transcendent hypotheses.

However, we do perform inductive inferences when stating plausible "conclusions", i.e., guesses, from the examination of observed consequences of our theories. Granted, we cannot examine instances of transcendent hypotheses such as "The intensity of the electric current is proportional to the potential difference", because they are noninstantial. But hypotheses of this kind, which are the most numerous in the advanced chapters of science, do have observable consequents when conjoined with lower-level hypotheses containing both unobservable and observable predicates, such as "Electric currents deflect the magnetic needle". (The deflections can literally be observed, even though electricity and magnetism are unobservable.) And, if we wish to validate transcendent hypotheses, we must examine instances of such end points of the piece of theory to which they belong.

To sum up, in the factual sciences the following rule of method seems to be accepted, at least tacitly: "All hypotheses, even the epistemologically most complex ones, must entail through inferential chains as long and twisted as is necessary instantial hypotheses, so that they can be inductively confirmed". This rule assigns to induction a place in scientific method, the overall pattern of which is admittedly hypothetico-deductive.

Inductivism rejects the deductivist thesis that what is put to the test is always some (often remote) observable consequence of theories, and that we never test isolated hypotheses but always some potpourri of fragments of various theories—eventually including those involved in the building and reading of instruments and in the performing of computations. Inductivism maintains that this description of scientific procedure might square only with very high-level hypotheses, such as the postulates of quantum mechanics. However, an analysis of elementary scientific hypotheses, even of existential ones such as "There is an air layer around the Earth" confirms the deductivist description, with the sole, though important, exception of the contact line between the lowest-level theorems and the empirical evidence.

Consider, for instance, the process that led to the establishment of the existence of the atmosphere. An analysis of this process (Bunge 1959b) will show that Torricelli's basic hypotheses, "We live at the bottom of a sea of elemental air" and "Air is a fluid obeying the laws of hydrostatics", were framed by analogy, not by induction, and that the remaining process of reasoning was almost entirely deductive. Induction occurred neither in the formulation nor in the elaboration of the hypotheses: it was equally absent in the design of the experiments that put them to the test. Nobody felt the need of repeating the simple experiments imagined by Torricelli and Pascal, nor of increasing their poor precision. Rather on the contrary, Torricelli's hypotheses were employed to explain further known facts and were instrumental in suggesting a number of new spectacular experiments, such as Guericke's and Boyle's. Induction did appear in the process, but only in the final estimate of the whole set of hypotheses and experimental results, namely when it was concluded that the former had been confirmed by a large number and, particularly, by a great variety of experiments, whereas the rival peripatetic hypothesis of the abhorrence of void had been conclusively refuted.

To sum up, enumerative induction plays a role in the test of scientific hypotheses, but only in their *empirical* checking, which is not the sole test to which they are subjected.

4. INDUCTIVE CONFIRMATION AND DEDUCTIVE REFUTATION

Deductivists may object to the above concessions to induction, by stating that confirming instances have no value as compared with negative ones, since the rule of *modus tollens* ("If p, then q; now, not-q; hence, not-p") shows that a single definitely unfavorable case is conclusive, whereas no theorem of inductive logic could warrant a hypothesis through the mere accumulation of favorable instances. But this objection does not render the examination of cases worthless and does not invalidate our "concluding" something about them; hence, it does not dispose of induction by enumeration.

Consider, in fact, a frequent laboratory situation, such as the one described by the following sentence: 'The results of n measurements of the property P of system S by means of the experimental set-up E agree, to within the experimental error ε, with the values x_i predicted by the theory T'. Certainly, ninety favorable instances will have little value in the face of ten definitely unfavorable measured values, at least if high precision is sought. (On the other hand, a single unfavorable case against ninety-nine favorable ones would pose the question of the reliability of the anomalous measurement value itself rather than rendering the theory suspect.) But how do we know that an instance is definitely unfavorable to the central hypothesis of the theory we are examining, and not to some of the background hypotheses, among which the usual assumption may occur, that no external perturbations are acting upon our system? Moreover, do we not call 'negative' or 'unfavorable' precisely those instances which, if relevant at all, *fail to confirm* the theory under examination?

Confirmation and refutation are asymmetrical to each other, and the latter is weightier than the former; moreover, a theory that can only be confirmed, because no conceivable counterexample would ruin it, is not a scientific theory. But confirmation and refutation cannot be separated, because the very concept of negative instance is meaningful only in connection with the notion of favorable case, just as "abnormality" is meaningless apart from "normality". To say that hypotheses such as natural laws (or, rather, the corresponding statements) are only refutable, but not confirmable by experiment (Russell 1948; Popper 1959), is as misleading as to maintain that all humans are abnormal.

How do we know that a skilled and sincere attempt to refute a hypothesis has failed, if not because the attempt has *confirmed* some of the lowest-level consequences of the theory to which the given hypothesis belongs? How do we know that an attempt has succeeded—thereby forcing us to abandon the hypothesis concerned, provided we are able to isolate it from the piece of theory to which it belongs, and provided better ones are in sight—if not because we have obtained no positive instances of its low-level consequences, or even because the percentage of positive instances is too poor?

The falsifiabilist rule enables us to discard certain hypotheses even *before* testing them; in fact, it commands us to reject as nonscientific all those conjectures that admit of no possible refutation, as is the case with "All dreams are wish fulfillments, even though in some cases the wishes are repressed and consequently do not show up". But refutability, a necessary condition for a hypothesis to be *scientific*, is not a criterion of truth: to establish a proposition as at least partially true, we must confirm it. Confirmation is insufficient, but it is necessary.

The falsifiabilist rule *supplements* the characterization of the difficult notion of positive instance, or favorable case, but provides no substitute for it. Refutation enables us to (provisionally) eliminate the less fitted assumptions, which are those that fit the data less adequately, but it does not enable us to justify alternative hypotheses. And, if we wish to resist irrationalism, if we believe that science and scientific philosophy constitute bulwarks against obscurantism, we cannot admit that scientific hypotheses are altogether unfounded but lucky guesses, as deductivism claims. Law statements do not hang in the air: they are both *grounded* on previous knowledge and successfully *tested* by fresh evidence, both empirical and theoretical.

The attitude of attempting to refute a theory by subjecting it to severe empirical tests belongs to the pragmatic and methodological level, and pertains even to the ethical code of the modern scientist. The problem of confirmation and, consequently, the problem of the degree of validation and, hence, of acceptability of factual theories belong both to the methodological and the epistemological levels. There is no conflict between the procedure that aims at refuting a theory, and the assignment to it of a degree of validation or corroboration on the basis of an examination of positive instances: they are complementary, not incompatible operations. Yet none of them is sufficient: pure experience has never been the supreme court of science.

5. THEORIFICATION

Neither unsuccessful attempts to refute a hypothesis nor heaps of positive instances of its observable consequents are enough to establish the hypothesis for the time being. We usually do not accept a conjecture as a full member of the body of scientific knowledge unless it has passed a further test which is as exacting as the empirical one or perhaps even more so: to wit, the rational test of *theorification*, an ugly neologism that is supposed to suggest the transformation of an isolated proposition into a statement belonging to a hypothetico-deductive system. We make this requirement, among other reasons, because the hypothesis to be validated acquires in this way the support of allied hypotheses in the same or in contiguous fields.

Consider the hypothesis "All humans live less than two hundred years". In order to test it, a confirmationist would accumulate positive instances, whereas a refutationist would presumably establish an enrolling office for bicentenaries, the simplest and cheapest but not the most enlightening procedure. Old age medicine does not seem to pay much attention to either procedure, but tends in contrast to explain or deduce the given statement from higher-level propositions, such as "The arteries of all humans harden in time", "All cells accumulate noxious residues", "Neurons decrease in number after a certain age", and so on.

The day physiology, histology, and cytology succeed in explaining the empirical generalization "All humans live less than two hundred years" in terms of higher-level laws, we shall judge it as established in a much better way than by the addition of another billion deaths fitting the low-level law. At the same time, the hypothesis will, after theorification, offer a larger target to refutation—which is, after all, a desideratum of geriatry—since it will become connected with a host of basic laws and may consequently contact with a number of new contiguous domains of experience.

The degree of support or sustenance of scientific hypotheses—which is not a quantitative but a comparative concept (among other reasons because hypotheses have philosophical supports besides empirical ones)—increases enormously upon their insertion into nomological systems, i.e., upon their inclusion in a theory or development into a theory.

No inference can even provisionally be justified outside the context of some theory, including of course one or more chapters of formal logic. Factual hypotheses can be justified up to a certain point if they are grounded on deep (nonphenomenological) laws that, far from being just summaries of phenomenal regularities, enable us to explain them by some "mechanism" (often nonmechanical). Thus, the age-long recorded succession of days and nights does not warrant the inference that the sun will "rise" tomorrow, as Hume rightly saw. But a study of the dynamic stability of the solar system and of the thermonuclear stability of the sun, as well as a knowledge of the present positions and velocities of other neighboring celestial bodies, renders our expectation highly plausible. Theory affords the validation refused by plain experience: not *any* theory but a theory including deep laws transcending first-degree inductive generalizations. In this way inductivism is inverted: we may *trust inductions to the extent that they are justified by noninductive theories*.

In sum, empirical confirmation is but one phase, though an indispensable one, of the complex and unending process of inventing, checking, mending, and replacing scientific hypotheses.

6. INDUCTIVIST METHODOLOGY AND THE PROBLEM OF INDUCTION

According to inductivism, empirical knowledge (a) is obtained by inductive inference alone, (b) is tested only by enumerative induction, (c) is more reliable, as it is closer to experience (epistemologically simpler), (d) is more acceptable, as it is more probable, and consequently (e) its logic, inductive logic, is an application or an interpretation of the calculus of probability. Deductivists, especially Popper (1957a, 1959, 1960), have shown that these claims are untenable, particularly in connection with theoretical laws, which are neither obtained nor directly tested by induction, and which have exactly zero probability in any universe that is infinite in some respect. They and a few others (e.g., Kneale 1949) have also conclusively shown that the theory of probability does not solve the riddles of induction and does not provide a warrant for inductive leaps.

All this, however, does not prove the vanity of the cluster of problems concerning induction, conceived as the set of questions connected with both the inductive inception and, particularly, the inductive confirmation of hypotheses; hence, those arguments do not establish the impossibility of *every* logic of induction, even though they considerably deflate the claims of available systems of inductive logic. It is, indeed, a fact that induction is employed in the formulation of some hypotheses both in formal and in factual science, even though it is true that such hypotheses are rarely impressive and deep. And it is a fact too (or rather a metascientific induction!) that induction is employed in the validation of all factual

theories. The mere mention of statistical inference should suffice. Now, if a subject exists, scientific philosophy suggests that the corresponding scientific (or metascientific) approach should be attempted. And why should induction be left in the hands of inductivists?

Granted, there is no inductive *method*, either in the context of invention or in the context of validation; at least, there is no inductive method in the sense of a set of secure rules or recipes guaranteeing once and for ever the jump to true general conclusions out of case examination. Nor is there an intuitive method or a hypnotic method. Yet induction, intuition, and hypnosis do exist and deserve to be studied scientifically. An analysis of scientific research shows the current employment of various patterns of plausible inference, such as analogy, reduction, weakened reduction, and weakened *modus tollens* (Keynes 1921; Pólya 1954; von Wright 1957; Czerwinski 1958); it also shows the operation of inductive policies, such as those connected with sampling, and which are after all designed to provide the best possible inductions. Why should we disregard these various kinds of nondemonstrative inference, especially knowing as we do that successful patterns tend to be accepted as rules admitted uncritically unless they are critically examined?

The rules of deductive inference, to which we all pay at least lip service, were not arbitrarily posited by some inspired genius in the late Neolithic: they were first *recognized* in sound discourse and then explicitly adopted because they lead from accepted statements to accepted statements—and statements are accepted, in turn, if they are deemed to be at least partially true. Conversely, statements that are not postulated by convention are regarded as true if they are obtained by procedures respecting accepted rules of inference. Such a *mutual and progressive adjustment* of statements and rules is apparently the sole ultimate justification of either (Bôcher 1905; Goodman 1955). Analogously, the belief in the possibility of a logic of plausible (nondemonstrative) reasoning rests not only on a false theory of knowledge which minimizes the role of constructs, and on a history of science biased against the theoretical, but also on the plain observation that some nondemonstrative inferences are crowned with success. (Usually, this is the case with recorded inferences, because humans, as Bacon pointed out, mark when they hit.) This is what entitles us to adopt as (fallible) rules of inference, and as inductive policies, those patterns that in good research lead from accepted propositions to accepted propositions.

Of course, the theory of plausible inference should not restrict itself to a *description* of the types of argument found in everyday life and in science: it should also refine them, devising *ideal* (least dirty) patterns of inference (Barker 1957). However, such a rational reconstruction should be preceded by a realistically oriented investigation into patterns of *actual* scientific inference, rather than by another study of the opinions of distinguished philosophers concerning the nature and role of induction.

Furthermore, ideal patterns of plausible reasoning should be regarded neither as binding rules nor as inference tickets, but rather as more or less successful, hence advisable, patterns. This, at least in the constructive stage, when the greatest freedom to imagine is needed, since creative imagination alone is able to bridge the gap separating precepts from concepts (Einstein 1944; Bunge 1962), first-degree inductions from transcendent hypotheses, and isolated generalizations from theoretical systems. Logic, whether formal or informal, deductive or inductive, is not supposed to concoct recipes for jumping to lucky conclusions—jumps without which there is as little science as there is without careful test—but it may show which are the best patterns that can be discerned in the test of hypotheses framed in whatever way.

7. CONCLUSION

As must have been suspected by many, scientific research seems to follow a *via media* between the extremes of inductivism and deductivism. In this middle course, induction is instrumental both heuristically and methodologically, by taking part in the framing of some hypotheses and in the empirical validation of all sorts of hypotheses. Induction is certainly powerless without the invention of audacious transcendent hypotheses, which could not possibly be suggested by the mere examination of experiential data. But the deepest hypotheses are idle speculation unless their lower-level consequents receive instantial confirmation. Induction plays scarcely a role in the design of experiments, which involves theories and demands creative imagination; but experiment is useless unless its results are interpreted in terms of theories that are partly validated by the inductive processing of their empirically testable consequences.

To sum up, induction—which is but one of the kinds of plausible reasoning—contributes modestly to the framing of scientific hypotheses, but is indispensable for their test, or rather at the empirical stage of their test. Hence, a noninductivist logic of induction should be welcome.

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THE GST CHALLENGE TO THE CLASSICAL PHILOSOPHIES OF SCIENCE

(1977)

The great majority of philosophers of science have ignored general systems theories (henceforth GSTs). And those few who have taken notice of GSTs have too often drawn on popularizations and on careless philosophical formulations, and as a result have come to the conclusion that GSTs constitute a new version of the old holistic metaphysics and the old antianalytic epistemology associated with that metaphysics.

This neglect, on the part of philosophers, of the technical literature in the various GSTs, is deplorable for a number of reasons. The main one, however, is that GSTs present a serious challenge to the two most popular philosophies of science, namely empiricism (or inductivism or confirmationism), as represented by Rudolf Carnap, and rationalism (or deductivism or refutationism), as championed by Karl Popper (1959). Indeed, none of these philosophers ever had GSTs in mind and, as a consequence, there is no room for GSTs in their philosophies. Worse, according to either of these philosophies, GSTs are nonscientific, for