A Critique of Induction

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PARTS I and II of this article are a review of Karl Popper's **Logic of Scientific Discovery,**[1] but Part III, the application of his ideas to the social sciences, goes well beyond anything contained in the book. After some brief comments on the author and the book I will present some of his more important ideas and then I will compare them with the development of the social sciences.

I.

Although it was published in 1934 when Popper was still under the rather strong influence of the Vienna Circle, the **Logik der Forschung** nevertheless maintained an independent viewpoint. Popper's criticisms of the Vienna Circle are even more pronounced today and in becoming a founding editor of the new journal **Ratio,** he has placed himself squarely in the tradition of rationalistic philosophy. At the same time he is opposed to the many recent attempts to provide philosophy with a restrictive definition and assign to it the one true method. The rational attitude, he declares, may be equated with the critical attitude (p. 16). But before showing how, with the aid of the seemingly trite phrase, "critical attitude," he arrives at some fresh ideas regarding scientific discoveries, I will refer briefly to his criticism of contemporary language analysis.

Two groups primarily concerned with language analysis are criticized; they are the ordinary language analysts and those attempting to construct formal linguistic models. Although with respect to method both groups are, he believes, properly within the mainstream of philosophical development, Popper defines his own position with regard to their efforts by denying the fruitfulness of their basic programs when considered as the whole of philosophy. Thus, while the analysis of ordinary language is not wholly beside the point in philosophy, by beginning there, the advocates of this method attack the most difficult tasks first. They err in assuming that when compared to scientific language ordinary language is the easier to analyse. Popper not only argues that it is the other way around but refuses to limit philosophy to linguistic analysis. His study of the logic of scientific discovery is not regarded as a fringe activity on a problem which should be postponed until later, but philosophy proper. Furthermore it is not limited merely to a linguistic study but is a considered appraisal of the sciences as such. This latter is, of course, a radical break with both groups. The most significant problem in epistemology, he claims, is the study of the growth of our knowledge and the most significant phase of that growth is the scientific growth.

It will be helpful to clarify a point concerning Popper's use of 'logic' in the phrase, 'the logic of scientific discovery.' Ordinary language philosophers sometimes speak of the logic of ordinary language and mean by that a logic which is not applied to, but is implicit in, ordinary language. The logic of which Popper speaks is, to be certain, a logic in use and it is used in the sciences, but with one possible exception (p. 62 and n.) it is not a peculiar or novel logic that he finds

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[1] London: Hutchinson and Co., Ltd., 1959, 479 pp. Included are Popper's translation of his *Logik der Forschung* published in Vienna, 1934 (with the imprint, 1935), additional footnotes, twelve new appendices and two indices. No changes were made in the original text but the additional footnotes contain rebuttals of criticisms of the earlier volume plus some modificaions of the original argument.

there. It is classical, deductive logic. The sciences and physics in particular, he claims, have advanced with the use of deductive, as opposed to inductive, logic.

The second group, those who are concerned with the construction of artificial models of scientific language (Carnap, **et al.**), he attacks on the ground that such languages do not meet the requirements of contemporary science, to say nothing of providing for their growth. What they have succeeded in constructing are languages suitable for formulating observation statements. Now it is the characterization of the scientific endeavor as an attempt to compile great masses of observation statements which the author opposes. This view he labels pejoratively, "Bacon's myth," and commits his book to the task of dispelling it.

II.

Here I will attempt to describe the most significant aspects of Popper's proposed alternative to "Bacon's myth," or rather, inductive logic. The approach is constructive, i.e. he shows how the most significant advances can be accounted for with deductive logic alone and then where relevant indicates certain defects in the inductive techniques.

Scientific laws and some of their best advertised products have the same function; they are labor saving devices. The laws are the bridges, the big steps in our knowledge, the means whereby we eliminate all, or most, of the intermediate steps. Analogously, and more so for Popper than for the advocates of induction, there is an interesting sense in which they are also the gaps in our knowledge. We will show in what sense this is true as the argument develops.

Both Popper and his opponents would agree to this, I believe, but they disagree as to the method for obtaining the laws and as to the criteria for acceptance. Therefore, in order to compare the views we will see how they answer these questions: (1) How do we arrive at the law in the first place, and (2) on what grounds is it accepted as true? The inductive techniques are somewhat vague concerning an answer to the first, but it goes something like this. The investigator begins by observing some set of entities and describing them as thoroughly as possible until he decides that some one of the descriptive propositions is of sufficient scientific interest to verify further. He may, of course, simply begin with some hypothesis and seek to verify it, but even then he makes every effort to see that the hypothesis is based upon the description of particular entities. Concerning the second question he is not so vague. The hypothesis is secured only when there is a relatively large (though indeterminate) number of confirming instances.

But now what has happened? Instead of the large step, he has taken many small steps. Furthermore, the motive which has sustained him in this laborious and perhaps expensive task, i.e. the desire for a dependable law, is frustrated by the fact that although innumerable observations may confirm it, only one instance is required for its falsification. For example, suppose we wish to verify or confirm the hypothesis, 'all water is wet.' According to the theory of indirection we begin by setting down in lexical order observation statements such as, 'this water is wet,' 'here is wet water,' etc. until we have a very large number of confirming instances. But we know that we could never hope to account for all water empirically and the number, no matter how large it becomes in fact, will be pitifully small compared with all the water there is. Certainly here is a case where we believe the law to be approximately true but where the actual number of confirming instances compared with the water we know to exist could in no way be said to correspond to the degree of certainty we possess. In fact induction seems hardly able to account for our belief at all inasmuch as on the

basis of our observations we have almost no reason for believing that all water is wet.

The example may be pushed one step further to anticipate one of Popper's conclusions. Perhaps the reader already has some reservations about the law as it is stated above. Such reservations may turn about the conditions under which the water is observed, i.e. as to the quantities involved and the temperature. But note here that the law as stated could be confirmed with an unlimited number of observations and that the suggested modificatioins of the law regarding the amount and the temperature need never arise. Induction provides no guidance here.

Inductive techniques not only seem to be incapable of accounting for such beliefs but thy are also confronted with certain formal difficulties. That is, no law may be deduced from a series of singular statements, or stated formally, a universal (all-statement) may not be deduced from particular statements (some - statements). The attempt to show how this is possible, the so-called justification of induction, is the problem of induction. And induction comes perilously close to being all problem.

Now if this is true what is the force of a given claim to the effect that a certain law has been verified inductively? Why have we accepted it as a reason for believing any law whatsoever? There are at least two reasons for this according to Popper; one is a logical error and the other is based on a confusion between a logical and a phychological question.

1. Strict and Numerical Universality.

The first is explicated when we make clear the distinction between "strict" and "numerical" universality (pp. 62-64). Strict universality contains no reference to place or time while numerical universality does. Numerical universality asserts something about all members of a finite class. The entities constituting the class may all be enumerated and the truth of the universal statement may be determined categorically. Now here it should be clear that there is no problem; each member is observed in precisely the same way we would verify a law concerning an unlimited class and as we approach the limit we feel more and more certain of the result. But, needless to say, in the finite class there is no inductive leap. The willingness to depend on the inductive techniques may be based on the treatment of a strictly universal statement as if it were only numerically universal. The latter is treated by Popper as a singular statement while all natural laws are, of course, strictly universal. If they were not they would be mere summary statements of our observations and of no scientific use whatsoever.

2. The Confusion Between the Logical And the Psychological Problem.

The second reason why we have been willing to accept as reliable the inductive techniques rests upon an attempt to make objective (logical), a purely psychological process. We have done this in spite of the fact that inductive logic is formally invalid. It lends force (psychological?) to our beliefs when we are able to say that they have been inductively verified even though this is merely a way of describing a psychological process about which we feel uneasy.

This leads us to a consideration of Popper's answers to the two questions above. His answer to the first (i.e. How do we arrive at our laws?) is straight forward. It is a psychological problem and has nothing to do with logic. Laws are given; we simply universilize. In fact, we universilize and theorize even when we describe a particular object inasmuch as the predicate always refers to a quality or a universal class. There is no more reason to require a logical account of this process than there is to require a logical account of how we come to describe a particular object. The fact is we do both; the only

relevant logical question becomes, given the fact of universal statements, how do we distiguish between them? This is a logical question for it has to do with relations between statements.

At least some advocates of induction would perhaps give a similar answer to the manner in which we first formulate the law, but where a clear distinction may be made between Popper and his opponents is in Popper's answer to the second question. We accept laws, in the sciences at least, not because they have been inductively verified but because they are well corroborated.

3. Corroboration.

I will illustrate this with an example which is usually employed to illustrate inductive verification. The reader may recall J. S. Mill's example of the shipload of wheat. In order to determine its quality, portions were taken from various widely separated points, the bow, the stern, the bottom, the top, etc., and from these portions the quality of the whole load was determined. But there is nothing in the inductive process which requires this. Why not begin anywhere? Take first one grain of wheat, determine its quality; a second, a third, etc., until we have determined enough grains compared with the estimated total to satisfy the inductivist's demand—whatever percentage that may be. Why were a few grains selected from widely separated points and certainly not enough to verify an inductive generalization concerning the quality of the shipload. It is as if some of the grains, simply because they were widely scattered, were of more value in determining its quality than others. There is nothing in inductive logic to justify this estimate. Induction was not employed here but rather a kind of testing which Popper refers to as the "falsification of the generalization." Tests were conducted at those points where the most variation would be revealed. Involved in their selection were other well tested theories which in turn were no more based upon induction than the one being tested here.

Now this, then, is Popper's answer to the second question. In the sciences we accept those universal statements, whether of the wetness of water, the quality of wheat, etc., which have been most vigorously tested, not those which have been confirmed most frequently. When two or more theories explain the same or almost the same events we perform only those experiments which decide between them.

4. Prohibited and Permitted Observation Statements and the Criterion of Demarcation.

The technique of corroboration requires that we view a law, a strictly universal or all-statement, as drawing a sharply defined line between all relevant, logically possible observation statements which are permitted and those which are prohibited. (We will have something more to say about the character of the observation statements later.) For example, the law "All water is wet," draws a line between all statements of the form, "This water is wet," and those of the form, "That water is not wet." The latter are relevant and logically possible, but if true the law is falsfiied. They are prohibited, for any one of them forms the contradictory of the law. The law is empirical if there are possible falsifying statements, and it is false if any one of them is true. The requirement that the theory must be falsifiable is labelled by Popper his "criterion of demarcation." With the criterion of demarcation empirical laws are distinguished from metaphysical laws.

It should be added here that the distinction is not intended, as it is for the positivists, to be pejorative. Metaphysical laws have a definite place—for example in the original inspiration for the law and certainly as the justification for such criteria as that of demarcation—but they have no place in the actual formulation of the law. Seeing

to it that the law is so constructed that it is in principle falsifiable is something which must be accomplished before even the first test is performed. The requirement that a law be verifiable does not distinguish empirical from metaphysical laws for metaphysical laws are verifiable.

On Popper's view the law says nothing about those statements which are permitted; in particular, it does not say that they are true. "In fact," he claims, "many of the 'permitted' basic statements will, in the presence of the theory, contradict each other" (p. 97, n.). This is simply another way of saying that whereas universal statements may not be fully confirmed by particular statements, they may be falsified. Instead of inductive logic which always carries with it the problem of induction, the author employs the much more powerful tool, deduction. Not only is the logical problem avoided, but it is, he believes, a well corroborated account of the way in which the most significant theories, especially in physics, have evolved and why they have been accepted. Our laboratories are not fed by conveyor belts in which large numbers of entities are examined, but they are places where only a few relevant experiments are performed and seldom do any two have the same value for the law in question.

5. Basic Statements.

Now we turn to the observation statements so crucially involved in the falsification of theories. Popper labels them "basic statements" and circumscribes them with two formal and one material condition (Sec. 28). The formal conditions are: "(a) from a universal statement without initial conditions, no basic statement can be deduced. On the other hand, (b) a universal statement and a basic statement can contradict each other" (p. 100-101). We will see how this works out with one of the author's favorite examples. Our strictly universal theoretical statement is: "All swans are white." This may be "instantiated" as, "If there is a swan at k, then that swan is white." Rule (a) is satisfied when we supply the initial condition, i.e., the antecedent for our instantiated law is "There is a swan at k." The consequent then follows necessarily, "The swan at k is white." This is one of our permitted basic statements and as with all others whether permitted or prohibited, it has the character of a singular, existential ("There is . . ."—"There is not . . .") statement. Furthermore, it meets the material condition that it is observable.

But what is not permitted? Basic statements and their negations do not have the same logical form; a basic statement is a singular "there—is" or existential statement; its negation is a singular non-existence statement or a "there—is—not" statement. The negation of "That swan (at k) is white" is "That swan (at k) is not white" (a prohibited basic statement) and by dropping the reference to a place and time we have "There is at least one swan that is not white," an existential, particular statement. This is an "O" statement in Aristotelian logic and it is the contradictory of the law, "All swans are white." The "I" and "O" statements of Aristotelian logic, the "some . . ." or "there is at least one . . ." statements are, according to the criterion of demarcation, metaphysical statements for they may not be falsified. Nevertheless they serve a useful function in falsifying laws for only by virtue of such statements is condition (b) satisfied.

So much for the character of basic statements, but I wish to quarrel with Popper's use of the phrase. Intuitively, "basic statement" suggests that the laws and theories in question are somehow grounded on such statements. Yet this, to me, connotes inductive verification rather than corroboration. If the law says nothing about the permitted statements and if they in turn are unable to fully confirm the law then it is difficult to see how the law could be based

upon them. Furthermore, if we know of no true basic statement which would falsify the law (although there are any number of logically possible statements which if true could falsify it), I do not see how these could be properly labelled basic statements. The phrase "test statement" would seem to be intuitively more acceptable. Nothing, of course, hinges upon this because the issue is merely terminological provided the reader does not understand the phrase to be a modification of Popper's central argument.

6. The Probability of Hypotheses and the Probability of Events.

What has been said so far is concerned exclusively with universal laws of a categorical character. But what of this situation? Someone asserts that "all swans are white" but when questioned further qualifies or appraises it with, "Well, it is at least probably true that all swans are white." Probability appraisals constitute the most severe hurdles (tests?) for Popper's theory, for, if permitted in the above form, they are not falsifiable—consequently they are metaphysical statements. On the other hand it must be admitted that probability is employed effectively in physics.

Recognizing fully the difficulties for his theory presented by any kind of probability statement, the author devotes some three chapters (VIII, IX and X) plus about one-half of his appendices to the problem. Because of the rather widespread confusion concerning the nature of probability in the first place, he is forced to construct a calculus of probability which will admit of the various known interpretations (New App. IV). Although this project grew out of his concern with the character of probability statements and their relation to his own argument, it may now be considered an independent contribution to the history of probability theory. However, in this discussion I will only be concerned with the bearing the notion of probability has on his argument.

Popper quotes Reichenbach as asserting that, "Whether we ascribe probability to statements or to events is only a matter of terminology." (p. 256). Then he is represented as arguing further that the probability of a hypothesis is merely a special case of the probability of statements (p. 257). It is the latter, when the probability of a hypothseis is concieved as being dependent on the events described, which Popper denies. It is, of course, a matter of terminology whether we speak of the probability of events or the **singular** statements describing them. But the probability of a hypothesis cannot be shown to be a special case of the probability of singular statements. Consequently, the probability of a hypothesis cannot be established according to the calculus of probability. When a hypothesis is said to be neither true nor false but merely probable and then this statement in turn is appraised as being probable, etc., we are confronted with an infinite regress which leaves the probability of any event described in the original hypothesis completely indeterminate. Thus a sharp distinction is enforced between the probability of a hypothesis and the probability of events (Sec. 80) and it is this distinction which saves his thesis of falsifiability.

7. Empirical Content.

At the same time, of course, comparable hypotheses (e.g., "All swans are white." "All swans are yellow.") may be thought of as events so that a probability may be assigned to one hypothesis among others. What then is the probability of a given universal statement when arrayed beside all other logically possible universal statements explaining the same event? In an infinite universe it is zero, and even when it is not infnite but very large, the probability is little more than zero. In fact, one of Popper's criteria for a test-

able law demands that it be as improbable as possible in order to make it easier to falsify. This is the criterion requiring the greatest possible empirical content which means that the larger the class of possible, prohibited basic statements, the more informative the law becomes. The empirical content increases when the subject of the law is elevated to a higher level of universality and the predicate is made more precise. It decreases as the level of universality is lowered and the predicate becomes vague, i.e., achieves a higher degree of probability.

Now we can see why the empirical content of any basic statement is small and why it has scientific value only in the presence of existing theories. It has this value only in the testing of theories however; it is negligible when employed as confirming instances, even when vast collections are so used. Although neither practice is grounded in the theory of induction, even the advocate of induction, in practice, loses interest as the number of confirming instances increases and prefers some over others.

It should also be clear that a probability statement assigning a certain probability to events is metaphysical; since it may not be falsified it has no empirical content. But probability statements without methodological rules which do make them falsifiable are unknown in physics. Thus with the distinction between the probability of a hypothesis and the probability of events on the one hand and the implicit requirement that probability statements be accompanied by rules rendering them falsifiable on the other, Popper's argument is saved from the dilemma.

By explaining the theoretical development of the sciences in a way which escapes the necessity for contradicting the rules of the syllogism for **all** inference, perhaps Popper, as a rationalist, receives more satisfaction than would be the case for a non-rationalist. He freely admits this. But even so, if his account has led to a fresh and productive insight, it deserves to be judged on its own merit. For this is not all that can be said for it. Returning to our introductory remarks concerning the nature of science, there is little question but what corroboration by attempted falsification — when compared to inductive verification—makes the most of scientific laws as labor saving decises. The advocate of induction, in order to obtain certainty, does much of the work (i.e., takes the small steps) which it is the sole function of a law to do. A law, no matter how precise, is of no value if it has no **further** application. There are a number of purely formal operations which we can perform on any proposed law before we arrange even the first experiment. We would have far fewer fruitless guesses if we did just that, for almost any universal statement has countless confirming instances in an extremely large universe, such as ours. If we had the time and means for inductive procedures we would have little or no need for scientific laws.

However, there is another reason which is, perhaps, even more important. It is this: inductive procedures tend to be circular. Since the laws grow in strength and character with additional observations, they seldom clash with any observed fact. They tend to be reworded, warped and finally transformed into innocuous and trivial generalizations which would be impossible to falsify. If, on the other hand, we have already decided just which observation statements will **not** be permitted there is much more likelihood of a genuine clash. Induction seeks to support the law; corroboration seeks to bring it crashing down as quickly as possible.

III.

Assuming Popper's account to be a true description of the development within the physical sciences we will turn to the social sciences. For here, it seems to me, is its most difficult test.

Is it then a true description of the techniques employed in the social sciences? I believe not. It not only fails utterly but its complete failure is indicative of its inner consistency. This, after all, is not surprising since it is from the social sciences that the advocates of induction have received their staunchest support. Not only is the whole endeavor based on induction, but induction is regarded as being synonymous with the scientific method proper. The method is frequently broken down into steps such as the following: (1) observation, (2) classification, (3) formulation of a hypothesis, and (4) verification of the hypothesis. A comparison of some of these techniques with Popper's recommendations should not only assist us in understanding Popper's argument but it should reveal in sharp outline the extent of their differences.

There is not, in the literature of the social sciences, one significant, falsifiable law. By a significant law I mean a law which more than a very few believe to be of scientific value or use, while by "falsifiable" all of the "tendency" or "dispositional" laws as well as the unrestricted probability statements are excluded. Most social scientists would readily agree; indeed, I am only repeating their own observations and estimates. But of course they do not stop here. They hastily add that they are conducting experiments which will culminate in the formulation of scientific laws, i.e., they are following the steps listed in the preceding paragraph. The laws are a kind of fifth step, the well cerified hypothesis. They are the products rather than the tools and will evolve in due course as the investigations progress.

Here then is the point of disagreement. On Popper's view, since the number of descriptive predicates we may attach to any object is infinite, scientific observations are futile apart from some testable law. A similar problem arises with regard to classification, or rather, it is the same problem in other terms inasmuch as predicates are the names of classes. As to hypotheses, every falsifiable law is hypothetical in the sense that the discovery of a true, prohibited basic statement will falsify it. At the same time no law is hypothetical in the sense that it is only probable. And this latter meaning has sometimes been employed in connection with the term hypothesis. Verification has already been discussed at length.

Laws, for Popper, are not discovered but invented. Unless nature is approached with falsifiable laws it is incapable of making a discriminating response, but says "yes" to any question inspired merely by itself. This is not to say anything about nature for Popper's program is neutral here. Lawfulness is a way of approaching nature, not what we find. If we do not begin with laws we certainly will not conclude with them.

In the absence of falsifiable laws there is nothing any experiment can tell us. Or, to put it differently, every describable characteristic is equally significant. Since there is nothing at issue, one or even countless experiments are incapable of solving anything whatsoever. The fact that the inductive processes are circular is nowhere better illustrated than in the social sciences. With each new confirming instance the law is altered to suit the new findings until it becomes either trivial and empty of scientific interest or metaphysical (e.g., unrestricted "tendency" or probability statements). This is partially traceable to a dogma in the social sciences—and among advocates of induction in general—to the effect that the results of every study, investigation or experiment are a contribution to science.

There are at least two interpretations of this claim which are true but only one of which has scientific interest. I will conclude by drawing out these interpretations. First, there is a sense in which all studies and investigations are contributions to knowledge. Every

observation is at least chronologically distinct from every other and adds to the observer's fund of impressions. But mere observations or even large numbers of observations do not constitute a science. If they did the scientific revolution would not have been so tardy.

Secondly, experiments, as Popper uses the term, are performed only when a serious attempt is being made to falsify a law. Where possible, they should be performed so that regardless of the outcome one of two laws will be falsified. Only if this condition were fulfilled would **every** experiment be a contribution to science. The power and utility of theoretical constructions are such that the question of whether or not a given experiment would be so decisive could be determined beforehand. Using "experiment" in this more restricted sense we may conclude that few, if any, have ever been conducted in the social sciences, nor is there the slightest indication that this condition will be altered.

Consequently, the view that every experiment must produce a scientific harvest and that none are in vain is in direct contrast to Popper's view. For him an experiment is a distinct achievement requiring not only the physical means for its effectiveness but requiring also considerable theoretical ground work of a very rigorous and precise kind. We can only conclude that either the social sciences have never availed themselves of the scientific method or that Popper has committed a gross error.