INDUCTION AND THE EMPIRICIST MODEL OF KNOWLEDGE

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1. Empiricism can be described as that philosophical position which holds that experience is the only source and the only justification for all synthetic knowledge. All descriptive concepts are taken from experience, and all statements about the world are derived from it.

Referring to empiricism in its modern form, these two principles can be formulated thus:

1. *The language of the natural sciences contains only observation terms as undefined descriptive terms.*[1]
2. *All (true) synthetic statements can be and can only be justified by observation sentences.*

That the first of these two theses cannot be upheld in this form has been shown by the discussions of the introduction and function of theoretical terms.

In other discussions in the philosophy of science it has emerged that the second thesis of empiricism is untenable also. But it seems to me that this consequence has been less widely accepted, and I therefore propose in this paper to state the case against this thesis as succinctly as the well-known facts permit.

2. For a discussion of the second thesis of empiricism we have first to take a look at the different ways of justification of empirical sentences by observation sentences.

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[1] This is only a first approximation of the empiricist thesis that all descriptive concepts are 'abstracted from experience', but here it may suffice, since we are dealing only with the second thesis.

A (true) sentence A can be validated by (true) observation sentences $B_1, \ldots, B_n$ either deductively or inductively. A *deductive* validation can be first a *verification* of A by $B_1, \ldots, B_n$, i.e., a logical derivation of A from $B_1, \ldots, B_n$. But verification is much too restricted, for neither (essentially) universal sentences nor sentences about future events can be derived from singular sentences treating past or present observations.

Therefore the postulate of verifiability has been dropped in connection with the empiricist thesis, and today deductive validation is understood mainly in the sense of a *corroboration* of a hypothesis A, which consists in deriving observation sentences B from A and testing them by observations: If B is thereby revealed as true, A has survived the test and is (temporarily) corroborated—if B is false, A has been refuted. This kind of deductive validation of A is therefore a failure of an attempt to falsify A. This failure, of course, does not show A to be true: a validation by failure in trying to refute is no validation that would establish the validated sentence as true. Since from many hypotheses nonanalytical observation sentences can be derived (if necessary by making use of other theories, e.g., on the functioning of the instruments that are employed for measuring the relevant quantities, that are supposed to be true in this context), they are refutable by observation, and in that sense capable of deductive validation. Other empirical hypotheses, however, like statistical ones, do not imply observation sentences and the concept of corroboration is therefore not applicable to them.[2]

It is often maintained that the progress of the empirical sciences consists simply in designing theories, which are then tested by attempts to refute them, and if necessary discarded and replaced by new ones. The corroboration of a theory would then be the only validation for it that we have and that we need.

But this conception of scientific procedure is not adequate even for deterministic hypotheses, simply for the reason that science has not only a theoretical, but also an eminently practical side: The laws of the empirical sciences, which tell us what is always the case, tell us what will be the case tomorrow, and thereby permit us to design our actions according to our aims. For these empirical laws we need a concept of validation that allows us to proceed from information on past and present events to conjectures about future ones. On the basis of such a concept it must, for

[2] The empirical test of statistical hypotheses is essentially an inductive procedure.

instance, be possible—if not always, at least in many cases—to select one of two competing hypotheses H_1 and H_2 that have been equally corroborated, but conflict in their predictions for the future, as the better confirmed and probably right one. We cannot be content that tomorrow, when the problematic future has become the present, it will become evident, which of the two hypotheses is the right one, for we must act today, and today it does not help us that we will be wiser tomorrow.

But a conception of the purely theoretical side of science that divides its statements into verified singular facts of natural history and equally unsubstantiated conjectures about the future would be inadequate also. Only the very smallest part of the accepted scientific facts consists of sentences that have been verified by observation. Even the evaluations of the simplest measurements and experiments rely on many hypotheses so that most observation sentences are hypothetical in character also on closer scrutiny and can therefore only be justified inductively. Inductive reasoning is just as common and indispensable in science as it is in everyday life.[3]

If, finally, the term 'justification' in the empiricist thesis were replaced by 'corroboration' there would be nothing left of the empiricist idea of experience as a sufficient criterion for the validity of empirical statements—experience would then be nothing but a necessary criterion for them.

Thus besides the deductive procedure of justification as verification or corroboration, there must be an inductive procedure of validation, and the empiricist thesis can only be upheld if a theory of induction can be constructed, which explains in a satisfactory way how we arrive (by a process of inductive reasoning from observations) at those of our empirical hypotheses that have not been verified and are not favored against their competitors by their corroboration.

3. The main problem of the theories of induction today is the paradox of induction by GOODMAN (cf. 1946, 1965). It shows that for every predicate F and every finite set M of objects there can be defined a predicate F^*, that is logically equivalent with F on M and logically equivalent with $\neg F$ on the complement of M. Thus if M is the set of all objects that have been tested until today for the property expressed by F, the hypotheses $/\backslash xFx$ and $/\backslash xF^*x$ say the same about the past and present, but make conflicting predictions about the future.

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[3] Cf., e.g., the statements of RUSSELL (1950) against those of FEYERABEND (1968) and POPPER (1959).

Goodman's paradox goes therefore right to the heart of the problem of induction by showing that two hypotheses, equivalent for past and present and conflicting for the future, cannot be distinguished inductively anymore than deductively. But that was just the aim of the concept of inductive as opposed to that of deductive justification.

It is an advantage of Goodman's paradox that it can be stated without reference to some specific theory of induction. In all such theories there is the following very weak principle of singular predictive inference:

(*P*) If the predicate F applies to all objects $a_1, \ldots, a_n$ tested so far then for sufficiently great n we have to count more on F applying to the next object a_{n+1} than on F not applying to it.

The paradox shows that the principle (*P*) is not universally valid, for if applied to F and the correlated F^* it would lead to contradiction. The paradox does not, of course, jeopardize the whole of inductive logic, but it refutes those theories of induction in which (*P*) holds for all predicates F. Since (*P*) must hold at least for some predicates F in all serviceable theories of induction—(*P*) expressing a fundamental principle of inductive reasoning—the problem arises for all the other theories of how to distinguish those predicates for which (*P*) holds.

With any amount of preciseness this problem can only be treated on the basis of specific theories of induction. Since I believe that a satisfactory theory of induction can only be constructed in the context of a theory of probability, and that only the theory of subjective probability is satisfactory from an epistemological point of view (cf. KUTSCHERA, 1972, Ch. 2), I will at once turn to the form this problem takes in the theory of subjective probability, i.e., to the question:

How can the predicates F be singled out whose instances $F(a_i)$ *represent exchangeable events*?

According to de Finetti, events are called *exchangeable* (relatively to a subjective probability measure) if all conjunctions of n of these events have the same probability. For exchangeable events $F(a_1), F(a_2), \ldots$ the principle (*P*) as well as the other fundamental principles of induction hold in the theory of subjective probability. Such predicates are called *projectible* in Goodman's terminology.

The discussions of Goodman's paradox have shown that there are no *logical* criteria for distinguishing the projectible and the nonprojectible predicates. In other words: There are no logical criteria for exchangeability. Many attempts to formulate such criteria, and thereby to overcome the

paradox, have been criticized and disposed of by Goodman (as others, especially H. Kahane, have criticized and disposed of Goodman's own solution).[4]

Now even logical principles cannot be logically established, but derive from conventions on the logical operators. Is it not possible to say in a similar sense that the assumption of a predicate F being projectible has the character of a meaning postulate for F and thus to maintain that inductive principles are analytic in character as well? There are three arguments against that: First, we can attribute a degree of probability to an event that is expressed by a sentence $F(a)$ only when we know what this sentence means, i.e., what event we are talking about. We can therefore assume F to be projectible only when F already has a well-defined meaning. Second, sentences like $p(F(a_i)) = p(F(a_k))$ are no meaning postulates in the usual sense, since they say nothing about the events $F(a_i)$ and $F(a_k)$, but only something about our expectations concerning these events. Third, there are many cases in which we revise our exchangeability assumptions without changing the meaning of the employed terms. If, for instance, we learn that in some die there is a mechanism moving the center of gravity in a regular manner after each throw, we will not take the results '6 points at the n-th throw' to be exchangeable anymore, but we still understand the expressions for these results in the same way.

The discussions have also shown that we cannot draw an *empirical* distinction between the projectibles and the nonprojectibles. Nothing corresponds to the predicates of these two groups that could be termed an observable difference of the properties they stand for. Among the nonprojectibles there are for instance also observation predicates. And we can neither make a difference between the two kinds of predicates inductively, for an inductive foundation of assumptions of exchangeability, e.g., by what GOODMAN (1965, p. 104 ff.) calls "parent hypotheses", is only possible by referring to hypotheses, which are well confirmed on observational evidence, taking other exchangeability assumptions for granted. Thus an inductive justification of such assumptions involves a regress, which cannot be repeated ad infinitum.

But another way to an empirical justification of exchangeability seems to be open still: Though learning from experience, it could be argued, begins only on the basis of assumptions of exchangeability, we can make such assumptions blindly at first and formulate hypotheses which are then

[4] Cf. for instance KAHANE (1965), TELLER (1969) and WALLACE (1966).

borne out by subsequent experience—then they are warranted by this experience—or which are not borne out—then they are invalidated by experience. We thus assume exchangeability in the way of a trial-and-error procedure: These assumptions are not empirically well founded when we make them, but in the long run only the empirically adequate assumptions survive.

This argument, however, is not sound: A trial-and-error procedure decides, for instance, whether a method of solving a task is successful, but it does not decide whether the trial-and-error procedure itself is successful. Similarly in the case of inductive assumptions: If we hold the results of throwing a coin to be exchangeable events, then the (conditional) probability of the result 'heads' converges to its observed relative frequency.[5] But the difference of the a posteriori probability and the initial or a priori probability of those events shows only whether the latter was adequate, but says nothing about the exchangeability of the events: that is the prerequisite for induction, for learning from experience, and cannot therefore be refuted by it. Exchangeability holds for the a posteriori probabilities as well as for the a priori ones, and remains the condition under which the observed frequencies are at all relevant for our probability assessments. So there is a trial-and-error procedure only *on the basis* of our assumptions of exchangeability, but not *for* them.

Claiming that there are no empirical criteria for exchangeability does not mean, of course, that exchangeability assumptions are incorrigible: we can change them, but there are no empirical criteria that tell us if or how we should change them.

4. The second thesis of empiricism was:

All (true) synthetic statements can be justified, and can only be justified by observation sentences.

We have seen that for the general hypotheses and theories of the empirical sciences, but also for very many singular sentences, a deductive justification by observation sentences is impossible, so that they can only be justified inductively. But the discussion of the Goodman paradox has shown that the inductive justification relies on assumptions of ex-

[5] This holds, to be exact, only under an additional assumption on the rationality of the probability measure employed. Cf. DE FINETTI (1937, Chap. 5) or VON KUTSCHERA (1972, Section 2.1.3).

changeability that can themselves be established by observation sentences neither deductively nor inductively.

Therefore the statements of science cannot all be justified only by observation sentences, but depend on our inductive a priori hypotheses. That refutes the second thesis of empiricism.

To this argument it could be replied: The empiricist thesis does not say anything about the nature and foundations of the procedures of justification; it is only presumed that they are generally accepted scientific methods, as the inductive inferences doubtlessly are. The inductive a priori assumptions are not on a level with the observation sentences and other premises in establishing general hypotheses, but are part of the inductive procedures themselves. Their existence therefore is no argument against the empiricist thesis.

But if we were to understand the thesis in this way, it would be just as trivial as it would be nonempiristic. If the procedures of justification may rely on material, for instance on synthetical a priori principles, the fact that a statement can be justified by observation does not imply that it is empirical in character; and the premises of an argument can then be made as weak as one wishes to.

It is correct, however, that the exchangeability assumptions are not synthetic, and therefore not synthetic a priori sentences in the usual sense of this term, for they say nothing about the world but only something about the expectations of a subject.[6] Therefore we cannot say that the inductive procedures rely on synthetic, but empirically not justifiable premises, but only that these procedures cannot be empirically validated.

This fact was clearly understood by Hume, and it is Hume's old riddle of induction, if, and if so, how we can justify the inductive inferences at all. In the framework of subjective probability theory we can take the axioms of probability to be analytic meaning postulates for this concept. But the principles of induction follow from these axioms only by additional assumptions of exchangeability. Therefore Hume's old riddle of induction is re-

[6] Even so they would be synthetic sentences, if we had the belief structures of an empirical subject in mind. Subjective probability theory, however, is not an empirical discipline treating the beliefs of specific human beings—just as logic is no empirical discipline inquiring how human beings in fact think and argue—and therefore exchangeability assumptions are not empirical. It is to be kept in mind that the term 'a priori' is here taken more in the sense it has in probability theory than in traditional epistemology. In the latter sense the term applies only to sentences *known*, and not to those just *assumed* to be true.

duced today to Goodman's new riddle, if, and if so, how these exchangeability assumptions can be justified.

The arguments that have been presented so far imply only that observation sentences are not *sufficient* for the validation of all synthetic sentences of the empirical sciences. But they do not affect the weaker version of empiricism according to which observation sentences are *necessary* for the justification of all synthetic sentences, so that there are no such sentences that could be validated purely on a priori grounds.

Regarding this version two cases must be distinguished: Either it is additionally claimed that there are synthetic sentences, which can be justified only by observation sentences, or there is no such claim.

In the latter case a priori assumptions as well as observation sentences are necessary for the justification of all synthetic sentences, i.e., no such sentence can be validated by a priori or by a posteriori reasons alone. But then there is a complete symmetry between a priori and empirical reasons, so that it makes no sense to speak of an *empirical* position.

In the first case, however, empirical reasons are preferred, since there are sentences that can be justified only with empirical reasons, but none that can be justified in a purely a priori way.[7]

There are, however, arguments even against this weak version of empiricism: It has often been remarked that even the simplest observation sentences cannot be directly verified or falsified by observations. From every observation, every experiment of measurement an observation sentence follows only with the help of other singular and general sentences that are presumed to be true and unproblematic in this context; therefore an observation sentence cannot directly, but only hypothetically be verified or falsified (Duhem's argument). And every observation sentence A is closely connected with other sentences by laws that we have accepted as true—and among them those that (partially) determine the meaning of terms of A—so that we cannot say that A has been definitely verified or falsified before these other sentences have also been decided. Even so simple a sentence as *This surface is red* has to be tested by checking the spectral distribution of the reflected light, etc., to be scientifically acceptable, and in this test we use quite a number of optical laws, hypotheses on the functioning of the spectrometer, etc.

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[7] This position is taken up in RUSSELL (1950, Cf. pp. 372, 381), where it is emphasized that questions of fact can only be decided by observations, but that not all such questions can be decided by observations alone.

This last argument implies that by using general hypotheses in testing even the simplest observation sentences the inductive a priori assumptions find their way into the justification of all synthetic sentences. But then even the weak form of empiricism is untenable which asserts that, if not all, at least some synthetic sentences can be justified only by experience.

We arrive, therefore, at the following result:

Our beliefs about the world are founded on a priori assumptions that cannot be established by experience. Every statement that cannot be directly decided by simple observations—in other words, the vast majority of all synthetic sentences, which we hold to be true, perhaps even all of them—rely on inductive inferences and therefore on a priori assumptions.

Our beliefs about the world would be quite different if we took instead of predicates like 'green' their Goodman correlates like 'grue' to be projectible. So there can be no doubt as to the relevance and importance of these a priori assumptions. Our scientific as well as our prescientific beliefs about the world are not derived only from experience, as the empiricist would have it, but are based partly on empirically not justifiable presuppositions.

5. In concluding this paper I would like to make a remark on the connections of these a priori assumptions with the choice of an underlying language system.

If we accept nonanalytic a priori elements of empirical knowledge it does not follow that we have to take up a rationalistic position or one of transcendental idealism, and say that the a priori assumptions derive from general nonanalytical principles evident in themselves, or from necessary conditions of all empirical knowledge. That would be, indeed, rather unplausible in the face of the vast number of these widely divergent assumptions.

It is much more plausible to think that these a priori assumptions are correlated with the language we use in describing the world.

A hint in this direction lies already in the observation that the different exchangeability assumptions are not independent of one another. So we could not for instance simply give up the projectibility assumption for 'green' and 'blue' in favor of 'grue' and 'bleen' without getting in conflict with several generally accepted physical laws. The projectibility assumptions for predicates are closely connected by the laws in which they occur, among them laws that have analytical character for these predicates. That has been pointed out especially in HESSE (1969). Thus projectibility assumptions

must refer to systems of predicates which we use to describe the phenomena of a certain domain of experience, and are therefore correlated with a system of language.

The second argument is this: When we learn to use a new basic predicate F, we use inductive inferences. We start from finitely many examples and counterexamples for the use of F and try to arrive at a system of general rules for its use. Every system of rules is a hypothesis which can only be inductively established on the basis of such examples of observed uses of F. This induction works only if we take the F-events to be exchangeable, and only then we can learn something about the general use (the rules of use) of F out of the observed instances. If, for instance, F has been applied in the examples only to those objects that another predicate applies to also, the expectation must increase that this will be so in all the other cases too.

This argument shows that for the basic predicates of our language exchangeability has to be assumed as a prerequisite for our ability to learn how to use them. These exchangeability assumptions are a priori in the sense of constituting necessary conditions of our understanding of language, for the intelligibility of the general use of its basic terms from examples of their application.

The choice of a language implies in this way making certain projectibility assumptions for its primitive predicates, and these assumptions can then be the basis for further empirically supported assumptions of exchangeability. For on the basis of a system of such assumptions, inductive inference from observation leads to hypotheses with which we can confirm further hypotheses and assumptions.[8]

The basic assumptions are not invariant against a change of language: While in our language 'green' as a basic observation predicate is projectible, so in another language it could be 'grue'. The a priori character of these assumptions is therefore a pragmatic a priori; it derives from our choice of this language instead of another.[9]

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[8] Parallels between Goodman's problem and Wittgenstein's problem of the right continuation of a predicate learned from examples have also been pointed out in STEGMÜLLER (1965, p. 637 ff).

[9] Here we have a certain affinity with Goodman's theory of entrenchment, which gives a pragmatic justification for projectibility too. But while in Goodman's theory projectibility depends on the projections of a predicate, i.e., on how often it is employed in formulating hypotheses, we see a pragmatic element already in choosing the language system, which in itself implies certain projectibility assumptions.

The question of why we choose this language rather than another goes, of course, much deeper than the problem of exchangeability, but it too could be answered only pragmatically by going back on the correlation of language and culture.

I do not intend to say that there are no empirical reasons for choosing one language rather than another. It may be that the organization of our senses and brains makes certain distinctions fundamental and thereby prejudices our choice of a system of corresponding primitive predicates. And it may be that from vital needs we choose systems of predicates leading to strong and simple laws to guide our actions. (From the projectibility of F it does not, of course, follow that the hypotheses $/\backslash xFx$, or $/\backslash x(Gx \supset Fx)$, etc., will be well confirmed by our observations.) Thus we can see quite a strong empirical justification of a system of language, particularly of the language of physics, in that it proves to be successful in this sense in practice.

But I believe that the simplest observation predicates expressing such fundamental distinctions constitute but a small part of those predicates we use in interpreting our experience; and I also believe that it is quite possible for several language systems to be equally successful but to lead to different theories about the world, among which we could not decide with these empirical reasons.

These empirical reasons, furthermore, are reasons in a metatheoretical sense: We cannot, by using this language and these corresponding inductive hypotheses, in describing and generalizing from our observations, prove this language and these hypotheses to be adequate or inadequate.

The linguistic thesis of relativity as formulated by Humboldt, Sapir, and Whorf says that there is a close correlation between our language and our 'world view', in which language is in many instances the determining factor. This thesis does not only state the trivial fact that our descriptions of the world depend on the linguistic means of these descriptions, as the equation of a curve depends on the coordinate system it refers to, but maintains that typically different languages lead to typically different 'world views' which are not 'equivalent in any sense however loose'.

In the same sense the discussions of the inductive a priori assumptions show that the choice of a language is connected with synthetic presuppositions and thus supports the linguistic thesis of relativity by demonstrating that in accepting different systems of primitive predicates we make different inductive assumptions which will in turn lead to different empirically confirmed hypotheses about the world.

References

FEYERABEND, P., 1968, *A note on two 'problems' of induction*, The British Journal for the Philosophy of Science, vol. 19, pp. 251–253

DE FINETTI, B., 1937, *La prévision: Ses lois logiques, ses sources subjectives*, Annales de l'Institut Henri Poincaré, vol. 7, pp. 1–68. English translation in: *Studies in Subjective Probability*, eds. H. E. Kyburg Jr. and H. E. Smokler (Wiley, New York), pp. 93–158

GOODMAN, N., 1946, *A query on confirmation*, Journal of Philosophy, vol. 43, pp. 383–385

GOODMAN, N., 1965, *Fact, fiction, forecast*, 2nd edition (Bobbs-Merrill, Indianapolis)

HESSE, M., 1969, *Ramifactions of "grue"*, The British Journal for the Philosophy of Science, vol. 20, pp. 13–15

KAHANE, H., 1965, *Nelson Goodman's entrenchment theory*, Philosophy of Science, vol. 32, pp. 377–383

VON KUTSCHERA, F., 1972, *Wissenschaftstheorie. Grundzüge der allgemeinen Methodologie der empirischen Wissenschaften* (München)

POPPER, K., 1959, *The logic of scientific discovery* (Hutchinson, London)

RUSSELL, B., 1950, *Logical positivism*, Revue Internationale de Philosophie, vol. 4, pp. 3–19; reprinted in *Logic and Knowledge*, ed. R. C. Marsh (Allen & Unwin, London, 1956), pp. 365–382

STEGMÜLLER, W., 1965, *Hauptströmungen der Gegenwartsphilosophie*, 3rd edition (Kröner, Stuttgart)

TELLER, P., 1969, *Goodman's theory of projection*, The British Journal for the Philosophy of Science, vol. 20, pp. 219–238

WALLACE, J. R., 1966, *Lawlikeness = truth?*, The Journal of Philosophy, vol. 63, pp. 780–781