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GOODMAN ON INDUCTION

Nelson Goodman's analysis of inductive inference is certainly one of his most important contributions to modern philosophy of science. This is true even if we have to consider the constructive part of it, viz., his theory of projectibility, less significant than his criticism, viz., his 'new riddle of induction'. For with the latter, Goodman has pointed out the central problem of induction as it presents itself today.

I shall begin by giving a brief sketch of Hume's old riddle of induction, which provides the starting point of all modern discussions, including that of Goodman. Then the new riddle of induction is outlined and Goodman's proposed solution, his theory of projectibility. I shall argue that this theory is unsatisfactory in some fundamental points even in its most recent version. Finally, I shall indicate in what form the problem of induction reappears in the subjective theory of probability, and show that even in this transformation Goodman's riddle remains the central problem of inductive reasoning.

1. Hume's Problem

An inductive inference, in its most elementary form, is an inference of the type

(I) $Fa_1, \ldots, Fa_n \rightarrow Fa_{n+1}$, or

(II) $Fa_1, \ldots, Fa_n \rightarrow \Lambda xFx$.

In (I), a_{n+1} is supposed to be an object different from the objects $a_1, \ldots, a_n$. In any application of principles (I) or (II) it is assumed that the $a_1, \ldots, a_n$ are the only objects of which we know that they are F's, and that no non-F's are known to us. For the arguments which follow it doesn't matter how the number n is fixed. Usually an inference of type (I) is called a *singular*, one of type (II) a *general predictive inference*.

Now Hume has argued with incontrovertible evidence that inferences of type (I) or (II) cannot be justified as inferences in the ordinary sense

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of the word, in which an inference is valid iff the truth of its conclusion follows from the truth of its premisses.

His argument is roughly this: These types of inferences are clearly not logically valid. Therefore we have to rely upon an additional premiss, a principle of uniformity, e.g., "that instances of which we have had no experience, must resemble those of which we have had experience, and that the course of nature continues always uniformly the same".[1]

Such a principle cannot be logically valid, for otherwise the inferences (I) and (II) themselves would be logically valid. Neither can it, as an essentially universal proposition, be deduced logically from finitely many observations concerning particular facts. Any attempt to justify it by observation would have to rely on some process of inductive inference, that has already been validated. But this would clearly be circular or lead to an infinite regress.

Therefore, according to Hume, induction cannot be justified deductively or inductively, and that implies that there is no rational justification of inductive inferences at all. All we can do is to explain inductive reasoning psychologically: The observation of past regularities induces an expectation and belief that they will continue to hold also in the future.

Goodman accepts Hume's arguments in (65)[2]. He also considers it impossible to justify inductive inferences in the sense of showing that, on the basis of the knowledge contained in their premisses, their conclusions as sentences about hitherto untested objects or future events must be true. There can be no rational principles for prophecy. Justifying an inductive argument, according to Goodman, cannot be anything more than showing that it agrees with the rules of inductive reasoning; and to justify such rules is merely to show that they conform to accepted inductive practice. Rules of induction then are just codifications of precedent common habits concerning the extrapolation of observations. Their validity is established by demonstrating their conformity to practice. Thus for Goodman as well as for Hume the basis of inductive reasoning is customs and conventions.

If inductive rules follow inductive practices which in turn are regulated by rules that does not involve circularity: "A rule", says Goodman, "is amended if it yields an inference we are unwilling to accept; an inference is rejected if it violates a rule we are unwilling to amend. The process of justification is the delicate one of making mutual adjustments between rules and accepted inferences; and in the agreement achieved lies the only justification needed for either".[3]

Goodman is quite well aware that he is exposing himself to the objection of having confused the logical question concerning the justification of inductive inferences with the empirical question concerning the description of accepted inductive practice, i.e. a question *quid iuris* with a question *quid facti*. But he points out first that a logical justification of induction is impossible, as Hume has shown, so that only the latter question is still open. And furthermore he refers to a parallel, as he sees it, in logic: Even here the rules of inference are derived from an antecedent deductive practice.

This parallel, however, is a rather weak argument, since logical principles can be justified by semantical considerations without referring to any ordinary deductive practice. If logic were derived from practice it would be quite inconceivable that it should have reached so far beyond this level and that logicians are so little concerned with the psychology or sociology of deductive behavior.

In logic as well as philosophy of science we are not interested in the problem as to whether deductive or inductive arguments are in conformity with how people usually argue. Our concern is rather whether they are valid, i.e. whether they prove what they purport to prove. If they do, we need not bother about customary usage, and if they don't, such arguments would be ineffective even if they would conform to ordinary practice.

Although inductive arguments are not valid inferences, we shall see that they can be so interpreted that a problem of validity arises in the logical sense of the word. Thus the question of justification has not been disposed of by Hume once and for all. In what follows, we shall accordingly not adopt Goodman's descriptive use of the terms 'justification' and 'valid', but rather employ them in their normal logical sense.

2. Goodman's problem

Even if Goodman is addressing himself not to the old problem of justifying induction but to the new one of describing accepted inductive practice, he is still faced with a fundamental difficulty: The principles I and II do not correctly represent our inductive reasoning. They are much too strong in allowing the extrapolation of every possible observed regularity into the future. This permits the derivation of conflicting predictions, since finitely many observational data can always be taken as instances of different regularities.

There is, therefore, another problem of induction which Hume didn't see: to distinguish that sort of regularity compatible with the evidence which we actually single out for extrapolating from the data; i.e. to restrict the principles (I) and (II) in such a way that they represent accepted inductive practice correctly.

This problem is not just generated by Goodman's descriptive turn. It is very essential even if we still look for some sort of vindication of induction, since it indicates that principles (I) and (II) are much too strong to be suitable objects for attempts at justification.

Goodman's new riddle of induction is a consequence of the simple fact, that for every application of (I) and (II), i.e. for every predicate F and every set $A = \{a_1, \ldots, a_n\}$ of objects which have been tested for the property expressed by F, a predicate F^* can be defined such that we have $F^*(a) \equiv F(a)$ for all $a \in A$ and $F^*(a) \equiv \neg F(a)$ for all $a \in \bar{A}$. We can simply define F^* by

$$F^*x := x \in A \wedge Fx \vee \neg x \in A \wedge \neg Fx.$$

Together with the true premisses $Fa_1, \ldots, Fa_n$ of an intended application of (I) or (II), which gives us Fa_{n+1} or ΛxFx respectively, we then have also the true premisses $F^*a_1, \ldots, F^*a_n$ of another possible application, which gives F^*a_{n+1}, i.e. $\neg Fa_{n+1}$, or ΛxF^*x, i.e. $\Lambda x(x \in \bar{A} \supset \neg Fx)$ respectively. If F is defined on some non-empty subset of $\bar{A}$ we may thus derive a contradiction.

Goodman's standard example for such a 'pathological' predicate F^* is 'grue'. 'Grue' is defined as 'having already been tested for colour and being green or having not yet been tested for colour and being blue'. From the fact that all emeralds which have been tested so far have been green (and therefore grue), we can derive the hypothesis that all emeralds not tested so far also will be green, as well as the hypothesis that they will be grue, i.e. blue.

Goodman has shown convincingly that it is impossible to make a distinction based on logical or empirical criteria between *projectible* predicates F, for which inferences of type (I) or (II) are accepted inductive practice, and pathological, i.e. non-projectible predicates. His main points are these:

(1) It has been argued that Goodman's, like Hempel's paradox of confirmation,[4] derives from the fact that the requirement of total evidence is violated. In applying (I) or (II) we should take into account further relevant information; in Goodman's example, for instance, our assumption that all sorts of precious stones are uniform in colour – but this would not eliminate the problem, but only shift it to another place: Why do we accept the uniformity-hypothesis instead of its pathological counterpart?

(2) It has often been pointed out that pathological predicates F^* like 'grue', in contrast to their projectible counterparts F, like 'green', are defined with reference to certain points in space or time, or to specific objects.[5] The proposal has been to admit only *qualitative* or *non-positional* predicates in (I) and (II) – but Goodman has pointed out that the concept of qualitativeness is relative to a language S. We can only say: Relatively to S and its basic terms a predicate F is qualitative if no names for specific objects, times or places occur (essentially) in it (or in its definiens). Since we can define F from F^*, as we defined F^* from F, the distinction qualitative vs. non-qualitative depends upon our choice of S. So again the problem has not been eliminated but only shifted to the question: Which language should we choose so that for its qualitative predicates the principles (I) and (II) are acceptable? There are, furthermore, non-qualitative predicates (in English) like 'medieval', 'terrestrial', 'European', 'Indo-Germanic' etc. that we do in fact use in inductive inferences.

(3) Neither can we restrict the predicates in (I) and (II) to observational predicates.[6] First, the concept of an observational predicate expressing some directly observable property is quite vague and problematic in itself. Second, inductive reasoning would then be too restricted, since no hypotheses with theoretical predicates, like 'elastic', 'magnetic' etc., could then be justified inductively. Third, the applicability of some pathological predicates like 'grue' can be tested by direct observation

given a calendar and a watch, as B. Skyrms has pointed out in (65), so that they could also be termed 'observational'.

(4) Finally it has been proposed to admit only such predicates F in (I) and (II) with which lawlike sentences can be formulated. 'All emeralds are green', for instance, is lawlike, but 'All emeralds are grue' is not. However, as Hempel has pointed out in (60), this restriction is not sufficient. He refers to problems of determining by measurement how a physical quantity y depends on a parameter x. If we have data relating to values $x_1, \ldots, x_n$ values $y_1, \ldots, y_n$, then there are infinitely many functions f for which $y_i = f(x_i)$ $(i = 1, \ldots, n)$, but which assign different values to some other arguments x; and many of these functions can be used to formulate lawlike sentences $\Lambda xy(y = f(x))$. This is a very frequently occurring case of Goodman's paradox. Furthermore, there is no generally accepted definition of lawlikeness, and the most promising definition, according to Goodman, makes use of the inductive confirmability of such hypotheses. But then this concept cannot be used without circularity in defining projectibility.

3. Projectibility

Goodman, therefore, has concluded that satisfactory criteria for projectibility have to be pragmatical and must refer to the actual and accepted use of the predicates in inductive practice.

In discussing his theory of projectibility we shall rely on the version given in Goodman (70), since it contains the most recent formulation of his theory. Although the formulation in the 2nd edition of 'Fact, Fiction, Forecast' is better known and has been the object of most of the critical discussions, we shall not dwell upon it, since we are interested here neither in the development of Goodman's ideas nor in objections that have been invalidated by later modifications.

Goodman considers only hypotheses of the elementary form $\Lambda x(Fx \supset Gx)$. We shall also presuppose in what follows that the hypotheses are synthetical and essentially universal sentences.

Goodman defines:

(D1) A hypothesis $\Lambda x(Fx \supset Gx)$ is *supported*, or *violated*, in time t iff it has been ascertained by t that for some objects a it is true that

$Fa \wedge Ga$, or $Fa \wedge \neg Ga$ respectively. It is *exhausted* in t iff for all a with Fa it has been determined up to t whether Ga or $\neg Ga$ holds.

(D2) A hypothesis H is *admissible* in t iff H is supported, not violated and not exhausted in t.

(D3) A hypothesis H is *projected* in t iff H is admissible and is accepted in t.

(D4) A *predicate* G is *projected* in t iff a hypothesis of the form $\Lambda x(Fx \supset Gx)$ is projected in t.

(D5) A predicate G is *better entrenched* in t than G' iff predicates coextensive with G have been projected more often up to t than those coextensive with G'.

(D6) A hypothesis $\Lambda x(Fx \supset Gx)$ is *entrenched at least as well* as $\Lambda x(F'x \supset G'x)$ in t iff F is at least as well entrenched as F' and G at least as well as G' in t.

(D7) An admissible hypothesis H *overrides an admissible* hypothesis H' in t iff H conflicts with H' and H is better entrenched in t than H', and H does not conflict with another hypothesis that is better entrenched in t than H.[7]

(D8) An admissible hypothesis H is *projectible* in t iff all conflicting hypotheses are overridden in t. It is *unprojectible* in t, iff it is overridden in t. And it is *non-projectible* in t, iff there is an admissible hypothesis H' conflicting with H and neither H nor H' is overridden in t.

The basic ideas in these definitions are first, that for inductive confirmation only supported, unviolated and unexhausted hypotheses are eligible (D3), and second, that the choice among concurring hypotheses consistent with the observational data refers to past inductive behavior in such a way that those hypotheses are preferred, whose predicates have so far been used more frequently in inductive inferences (D7). The projectible hypotheses are to be those for which principle (II) holds.

The hypothesis 'All emeralds are grue', for example, is unprojectible since it is overridden by 'All emeralds are green'. Since 'green' has been used more often in inductive arguments than 'grue', the latter hypothesis is better entrenched than the former. The hypothesis 'All emerubies are green' is unprojectible, since it is overridden by 'All rubies are red'. The latter contains the predicate 'ruby' which is better entrenched than 'emeruby' (a predicate defined as 'tested already for colour and being an emerald or not yet tested and being a ruby') while 'green' and 'red' are equally well entrenched.

In (65) Goodman also defines a comparative concept of projectibility by introducing an *initial degree* of projectibility of projectible hypotheses – H has a higher initial degree of projectibility in t than H' if H is better entrenched than H' in t – and indicating how this initial degree of projectibility can be changed by positive or negative overhypotheses. $\Lambda x(Fx \supset Gx)$ is a positive, or negative overhypothesis of $\Lambda x(F'x \supset G'x)$ iff the extension of F' is an F and the extension of G' a G, or a non-G respectively. So 'All sorts of precious stones are uniform in color' is a postive overhypothesis of 'All emeralds are green'. But since only such overhypotheses are eligible that are themselves projectible in t, the decisive point of Goodman's approach is the determination of the initial degree of projectibility.

Goodman has emphasized that his proposal is only a program for a solution of the new riddle of induction, and not a fully elaborated theory. This is true already in view of the fact that he considers only hypotheses of a very elementary form. But even so, his basic ideas have been severely criticised. The main critical points are these:

(1) In (D7) a 'conflict' between hypotheses is mentioned. For a long time it has been unclear how this term is to be understood. Only after repeated attacks by H. Kahane and others[8] has Goodman stated in (72), that two hypotheses $\Lambda x(Fx \supset Gx)$ and $\Lambda x(F'x \supset G'x)$ are in conflict with each other iff $Vx(Fx \wedge F'x \wedge \neg(Gx \wedge G'x))$, i.e. iff there are objects for which at most one of the two hypotheses is true.[9] Whether two hypotheses are in conflict, is then an *empirical* question, not a logical one. It can be objected, therefore, that two hypotheses are known to be in conflict only if one of them is already violated, i.e. non-admissible, while in (D7) the term 'conflict' refers to two admissible hypotheses. Goodman, however, thinks that even if neither of the two hypotheses has as yet been violated, we generally have an opinion as to whether they are in conflict or not, and that our choice of the overriding hypotheses according to (D7) is based on such opinions. But if, in justifying inductive inferences, we always have to rely upon opinions about conflicts obtaining or not, i.e. on hypotheses of the form $\Lambda x(Fx \wedge F'x \supset Gx \wedge G'x)$ being

false or true, we in fact have to rely upon some inductive inferences being valid, since these hypotheses are not logical consequences of our observational data and thus can be derived from them only by induction. This is flatly circular: The criteria for valid inductive inferences refer to hypotheses which in turn can be justified only by showing that they are conclusions of valid inductive inferences.

Neither is this the only point of circularity. In (D5) reference is made to the fact that some predicates have the same extension. But $\Lambda x(Fx \equiv F'x)$ again is a general empirical hypothesis that can only be justified by a valid inductive inference.

Moreover, the following example shows that, aside from the issue of circularity, Goodman's concept of conflict is simply inadequate. Suppose that a new species F of animals is discovered and only black specimens have so far been tested for some physiological property G – all with positive result. If we do not believe that all F's are black then the two hypotheses 'All F's are black' and 'All F's are G's' are in conflict with each other, and the former overrides the latter if 'black' is better entrenched than 'G' (D6, D7). This means that an admissible, well entrenched hypothesis, which we consider wrong, overrides an admissible, less well entrenched hypothesis which we consider right, and therefore makes it unprojectible.

On the other hand, a narrower concept of conflict, according to which two hypotheses $\Lambda x(Fx \supset Gx)$ and $\Lambda x(F'x \supset G'x)$ are in conflict only if they are analytically incompatible, seems difficult to define in a manner suitable for Goodman's purposes. First, $Vx(Fx \wedge F'x \wedge \neg(Gx \wedge G'x))$ is not analytically true, since $Vx(Fx \wedge F'x)$ is not. In the example of the two hypotheses (1) 'All emeralds are green' and (2) 'All emeralds are grue' it is not an analytical truth that there are emeralds. Second, the proposal to define the conflict by $Vx(Fx \wedge F'x) \supset Vx(Fx \wedge F'x \wedge \neg(Gx \wedge G'x))$ being analytically true is not feasible either, since from the fact that there are emeralds it does not follow that there are emeralds not tested for colour so far. And third, defining a conflict to obtain iff there is a set A of objects such that (a) $Vx(x \in A \wedge Fx \wedge F'x)$ is not analytically false, and (b) $\Lambda x(x \in A \wedge Fx \wedge F'x \supset \neg(Gx \wedge G'x))$ is analytically true (where A in our example is the set of objects not yet tested for colour), yields much too strong a notion of conflict. This way there would be a conflict between the (presumably) equally well entrenched hypotheses (1) and (3) 'All emeralds are precious stones' which would make them both nonprojectible. If we define A as the set of non-green objects, then (a) holds, since (1) is not analytically true, and (b) is a truth of logic. In general, if $\Lambda x(Fx \wedge F'x \supset Gx \wedge G'x)$ is not analytically true, the choice of A as the set of all X such that $\neg(Gx \wedge G'x)$ demonstrates a conflict between the two hypotheses.

Thus there seems to be no way out of this difficulty about conflicts in sight.

(2) It was again Kahane who pointed out in (65) and (71) that hypotheses with predicates which have been newly introduced into the scientific vocabulary and therefore are just as badly entrenched as their pathological counterparts, are nonprojectible according to (D8). All hypotheses involving new terms are therefore eliminated, contrary to Goodman's intentions. Goodman says in his rejoinder in (72) that a definiendum may also inherit entrenchment from the defining predicates. But neither is this in accordance with (D5), nor would it save new theoretical predicates, which are not explicitly but only implicitly defined, sometimes by a whole theory; where do they derive some positive initial entrenchement?

(3) Finally, it has to be asked, how we measure entrenchment, i.e. the number of projections of hypotheses according to (D4, D5). How often has Hooke's law been accepted? Does 'accept a hypothesis' mean 'proclaim that it is true', or does it mean 'act as if one knew it to be true'? But in the latter case: how do we count acceptances? If n individuals or groups accept the same hypothesis at the same time, or if one person or group accepts it at n successive times, has it then been accepted n times?

The concept of entrenchment therefore is much too unprecise as a basis for judging the validity of inductive inferences with some assurance.

These objections show, in my opinion, that Goodman's approach towards solving his riddle of induction is just as hopeless as the other attempts to which we referred in section 2, and which have been so acutely criticised by Goodman himself. Furthermore we should remind ourselves that, even if his theory of projectibility would have been successful, it would just have solved a problem only few philosophers of science have been interested in, namely how to describe actual inductive practice, while the original problem of whether and in what sense inductive arguments are *sound*, i.e. the problem of justifying induction in the usual sense, would still remain unsolved.

4. Exchangeability

As we have seen, Hume has definitely shown that inferences of type (I) or (II) are not valid inferences, in the sense that, in every application, their conclusion is true if their premisses are. Hume even indicates that no such principle as

$$\text{(III)} \qquad F_n^r \rightarrow p(F) = \frac{r}{n} \pm \varepsilon(n)$$

can be justified, which would allow us to conclude that the objective probability of F-events is close to r/n given that of n observed cases r have been F-events.[10] Since the objective probability of F is something like the limit of the relative frequencies of F's in an infinite series of trials, any probability $p(F)$ is compatible with any relative frequency of F's in a finite segment of the series.

So can there still be a question of justifying induction?[11] The deductivists, first and foremost K. Popper, think not. All we can do, according to them, is stick to deductive inference and turn the arguments of type (II) upside down, remarking that, while ΛxFx does not follow from $Fa_1, \ldots, Fa_n$, $\neg\Lambda xFx$ follows from $\neg Fa$. Instead of inferring general hypotheses from observations we can only falsify them by observations. But apart from the fact that such falsification does not work for all general hypotheses and especially not for statistical hypotheses, this is no substitute for induction. Nor is it intended to be one, since induction is supposed to be a means of justifying hypotheses, and 'justify' ordinarily means 'show to be true' or at least 'show to be probably true'.

But Hume himself in his analysis of induction has shown another way out. He argues that, even though the premisses of (I), (II) and (III) do not imply the conclusion, they nevertheless make it subjectively *probable*. So if we cannot answer the question 'What will be?' without some gift of prophecy, there still remains the question 'What is to be expected?', and to this question there is a positive answer. It is given by the theory of subjective probability, as developed mainly by B. de Finetti.

While the objective probability $p(F)$ of some type of event F – e.g. to get 'heads' when throwing a coin – is a physical property (of the coin) related to other physical properties (the form, the density distribution, etc.), the subjective probability $w_X(A)$ of some event A is the degree of certainty with which the person X expects A to happen. Although statements about subjective probabilities have quite a different meaning from those about objective probabilities they have the same formal properties, as expressed by Kolmogoroff's axioms. In the subjective case they can be derived from certain minimal requirements concerning the rationality of X.[12] These axioms then are analytical postulates for a rational concept of subjective probability. Now if $w(A, B)$ is the (conditional) probability of A (for some person) given that B, it is a theorem of subjective probability theory that

$$\text{(IV)} \qquad \lim_{n \to \infty} \left(w(Fa_{n+1}, F_n^r) - \frac{r}{n} \right) = 0, \quad \text{if}$$

(a) w is regular with respect to F, and
(b) the events Fa_i are exchangeable.

Roughly speaking, a probability measure w is *regular* with respect to F iff it does not assign zero probability to any limit of the relative frequencies of F. (a) then in many cases is also a requirement of rationality: 'Do not exclude any statistical hypotheses *a priori*', and in so far it is unproblematic.

The events $Fa_1, Fa_2, \ldots$ are called *exchangeable* iff $w(Fa_{i_1} \wedge \cdots \wedge Fa_{i_n})$ depends only on the number n, but not on the choice of the objects $a_{i_1}, \ldots, a_{i_n}$. If the Fa_i are exchangeable, we shall also say that F is exchangeable.

If the F-events are taken to be physically independent events then F will be exchangeable. Consider the example of the coin. Assuming that the outcomes of throwing it are not mutually dependent in some way, heads on the i_1-th and ... and i_n-th throw will be just as probable for us as heads on the j_1-th and ... and j_n-th throw.

For $r = n$ F_n^r is $Fa_1 \wedge \cdots \wedge Fa_n$. Then we have for sufficiently large n

$$w(Fa_{n+1}, Fa_1 \wedge \cdots \wedge Fa_n) \geqslant 1 - \varepsilon \quad \text{for any} \quad \varepsilon > 0.$$

How large 'sufficiently large' is, depends on ε and on w and can be calculated in every instance. This, then, is the probabilistic analogon to (I). And from the theorem

$$\text{(V)} \qquad \lim_{n \to \infty} w\left(p(F) = \frac{r}{n} \pm \varepsilon, F_n^r\right) = 1 \quad \text{for all} \quad \varepsilon > 0,$$

for all exchangeable F and w regular with respect to F we get the following analogon to (III): For sufficiently large n

$$w\left(p(F) = \frac{r}{n} \pm \varepsilon, F_n^r\right) \geqslant 1 - \delta \quad \text{for all} \quad \varepsilon, \delta > 0.$$

Now for the inductive principles (IV) and (V) we can answer the question of justification positively: They are mathematical theorems derived from analytical postulates for the concept of rational subjective probability.[13] And Goodman's problem, as to which predicates are projectible, can be answered thus: All and only predicates expressing exchangeable properties are projectible.[14] Pathological predicates like 'grue' are not projectible on the class of emeralds, then, since we expect the colour of emeralds to be independent of whether it is tested before some specific time or not.

If inductive principles construed as statements about conditional belief were purely descriptive, i.e. statements about what people who start with certain *a priori* probability assignments in fact believe after making some observations, then they would not tell us anything about the legitimacy of these beliefs, and therefore principles (IV) and (V) would not be of any use for justifying induction. But the concept of subjective probability is not purely descriptive, as we have seen, but a concept of rational belief.[15] The statements therefore tell us what we should believe given some *a priori* probability and some observations. In this sense the principles justify conditional beliefs on the basis of some *a priori* assumption. But how rational are the *a priori* probabilities, especially the exchangeability-assumptions?

Two questions are important in this connection:

(1) Can we justify probability distributions that mark out some predicates rather than others, for instance 'green' rather than 'grue', as exchangeable in some objective manner? The answer is no: The only objective measures for the legitimacy of belief are coherence (or rationality) and truth. If we do not know what is true, any coherent guess is as good

as another. We certainly can learn from experience: (IV) and (V) are, in fact, basic principles for doing so. But they also show that learning from experience depends on *a priori* probabilities, in particular on which events are exchangeable.

So exchangeability is a prerequisite for, but not a product of learning from experience. Our *a priori* assumptions determine in part how we evaluate experience, and are therefore not directly dependent on it.

Carnap's attempts to mark out rational *a priori* assignments by introducing further axioms besides Kolmogoroff's, have failed, since they involved him in Goodman's paradox. And Goodman's discussions of proposed rational criteria for projectibility have made it very likely that there are no such criteria for exchangeability. Since exchangeabilities determine how and what we learn from experience, and since the correct manner of extrapolating from observations depends on matters of fact we do not yet know, there cannot be purely rational criteria for correctly learning from experience. All we can do is proceed by trial and error.[16]

(2) How can the fact be accounted for that our probability distributions agree to a large extent with regard to what events are exchangeable? This can be explained in general, I think, by pointing out that our more fundamental beliefs are not privately conceived but publicly imparted. What we believe is not only derived from our own experience but also from what is commonly believed, what is transmitted by education, cultural tradition etc. *A priori* assumptions also seem to be connected with the language we speak: Since we learn to employ the words by induction from observed instances of their use by others, the appropriation of our language is accompanied by an adjustment of our inductive procedures to those of our fellows.

Now it has been objected, for instance by W. C. Salmon and also by Goodman,[17] that by interpreting induction probabilistically we substitute a question concerning beliefs for a question of fact. In passing from the question: What can we *infer* from past observations about future events?' to the problem 'What have we to *expect*, given past observations, about future events?' it is claimed, we miss the original problem of induction.

Since even $w(A) = 1$ is compatible with $\neg A$, what use are probable predictions, or in what sense are they better justified than improbable ones? According to Salmon, inductive inferences have to be 'ampliative', i.e. the content of the conclusion has to be greater than that of the premisses; only thereby can they become the 'great guide of life', in Hume's words. Since a proposition $w(Fa_{n+1}, Fa_1 \wedge \cdots \wedge Fa_n) = 1$ is not 'ampliative' in this sense and does not guarantee that we shall be successful if we act on the assumption Fa_{n+1}, given $Fa_1, \ldots, Fa_n$, it is considered of no use for an inductive argument.

But this objection is based on a fundamental misunderstanding of what inductive arguments can do: Hume has shown once and for all that there is no rational basis for prophecy. Therefore, inductive arguments cannot be ampliative any more than deductive ones. But prophecy is not the only 'guide of life', for it is actually of great help to us if we know what we may reasonably expect on the basis of past observations. This gives us a criterion for rational decisions. That these rational decisions shall be successful is not certain, but to follow probabilities where certainties are not available is surely an indispensable postulate of rationality. And that we *expect* to be successful if we follow our expectations is as evident as any tautology.

In view of the tenet, quite pervasive in the Philosophy of Science literature, that inductive logic fails in the confirmation of general laws since $w(\Lambda xFx, F_n^n) = 0$ (for $w(\Lambda xFx) < 1$) even if n is very large, it is of interest to see that there is also an inductive analogon to principle (II).[18] If F is exchangeable and w is regular with respect to F there are also *a priori* assignments such that $\lim_{n\to\infty} w(\Lambda xFx, F_n^n) = 1$ even if $w(\Lambda xFx) < 1$. In Carnap and Jeffrey (71), pp. 205 seq. R. Jeffrey gives the following example: A coin is tossed and Fa_i is the event that it shows heads on the ith toss. Then if we assign probability 1/3 to the hypothesis that it will always show heads (or that it has two heads), 1/3 to the hypothesis that it will always show tails (or has two tails) and 1/3 that the relative frequency of heads will be somewhere between 0 and 1 (with equal probability for each such statistical hypothesis) then $w(Fa_i) = 1/2$, F is exchangeable and w is regular with respect to F. In this case $w(\Lambda xFx, F_n^n)$ and $w(\Lambda x\neg Fx, F_n^0)$ converge quickly to 1.

Generalizing we obtain the principle

(VI) $$\lim_{n\to\infty} w(\Lambda xFx, F_n^n) = 1,$$
if $w(\Lambda xFx) > 0$ and conditions (a) and (b) of (IV) hold.

So if we choose the *a priori* probability w such that the hypothesis ΛxFx does not have zero-probability to begin with, we may confirm this hypothesis on the basis of finitely many observations.

This is in conformity with standard scientific practice. Consider as an example our newly discovered animal species F and some physiological property G (e.g. of having a heart with two chambers). A biologist, after testing some F-specimen for G with positive result will often pronounce that all F's are G's. This confidence on the basis of a relatively small sample may be explained by (VI) in pointing out that he attributed a not too small probability to the hypothesis to begin with. In fact the biologist may even start out with the assumption, that all F's have the same basic physiological properties, i.e. with a uniformity assumption $w(\Lambda xy(Fx \equiv Fy)) = 1$ (such a probability w would not be regular, of course). And this would not just be a bold conjecture either. The natural classification of animals is so designed that every species comprises only individuals with the same fundamental biological properties. So if our scientist should find, as after all he very well might, that some other F's have hearts with only one chamber, there would be a change in terminology. The F's would be subdivided into F_1's and F_2's. The aim of a taxonomical system of concepts for a set of objects is to be able to substitute general statements for the objects in the classes for particular statements about individuals without loss of important information. So in using the concepts of such a system we rely on their doing duty in this way, i.e. we make the corresponding uniformity assumptions.

Generally speaking, the *a priori* assignments we use in confirming lawlike statements on the basis of observations are not any more out of the way than the assumptions we use in making singular predictive inferences in accordance with (V).

We can conclude, then, by saying that, according to the probabilistic interpretation of induction, inductive arguments can be justified, but only relative to *a priori* assumptions. There are no purely rational or empirical criteria for the correctness of such assumptions, as Goodman has shown for the case of projectibility, or exchangeability.

If we interpret beliefs as dispositions to act on certain observations in a specific way, an organism with built-in dispositions of this kind cannot adapt itself to a changing environment in which the observed events are no longer accompanied by circumstances which guarantee the success of his reaction to them. This is possible only if the organism can learn from experience, i.e. if he has a built-in propensity to form and change such dispositions on the basis of certain observations. Human beings do not even possess such built-in propensities, which correspond to *a priori* probabilities, but can choose the way they learn from experience. On the one hand this makes for still greater adaptability, on the other hand our reactions to observations by way of dispositions become still more uncertain. We may thus view the theoretical problem of learning from experience as the price we have to pay for our unbounded adaptability.

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NOTES

[1] 'Treatise of Human Nature', 1, III, §6 (p. 91 in A. D. Lindsay's, ed., London, 1968).
[2] Cf. pp. 59 seq.
[3] Goodman (65), p. 64.
[4] Cf. Hempel (45).
[5] Cf. Carnap (47), pp. 139 seq.
[6] Cf. Salmon (63).
[7] In (70), p. 606, Goodman defines (for admissible hypotheses): "H overrides H' if the two conflict and if H is the better entrenched and conflicts with no still better entrenched hypothesis". And he adds in a footnote: "So stated, this covers only hierarchies of at most three supported, unviolated, unexhausted, and successively better entrenched and conflicting hypotheses. Hierarchies of more such hypotheses can be covered if necessary by making the definition more general so that a hypothesis is overridden if it is the bottom member of a hierarchy that cannot be extended upwards and has an even number of members". This footnote may, perhaps, be interpreted thus: If we assume that for any hypothesis H there is only a finite set $\mathfrak{H}(H)$ of hypotheses that are at least as well entrenched as H, this set can be ordered in a sequence $H = H_1 \leqslant \cdots \leqslant H_n$, where $\leqslant$ is the relation of being at most as well entrenched as. Then H_n is not overriden; H_{n-1} is only overriden if $H_{n-1} < H_n$ and H_{n-1} cf. H_n (H_{n-1} conflicts with H_n), etc. In this way we can eliminate all overridden hypotheses in the sequence. If $\mathfrak{H}^0(H)$ is the reduced sequence, H is overridden iff $\neg H \in \mathfrak{H}^0(H)$.

Thus a concept O of being overridden may be defined, for which we have $O(H) \equiv \vee H'(H' \text{cf. } H \wedge \neg O(H') \wedge H < H')$.
[8] Cf. Kahane (71), e.g.
[9] He says: "Two hypotheses conflict only if neither follows from the other and they ascribe to something different predicates such that only one actually applies" ((72), p. 84). The latter condition can be translated into (a) $\vee x(Fx \wedge F'x \wedge \neg(Gx \wedge G'x))$ or (b) $\vee x(Fx \wedge F'x \wedge (Gx \wedge \neg G'x))$ – depending on whether Goodman, speaking

of 'only one', means 'at most one' or 'exactly one' – for two hypotheses $\Lambda x(Fx \supset Gx)$ and $\Lambda x(F'x \supset G'x)$. From (a) as well as (b) follows $\Lambda x(Fx \supset Gx) \supset \neg \Lambda x(F'x \supset G'x)$ and $\Lambda x(F'x \supset G'x) \supset \neg \Lambda x(Fx \supset Gx)$. But then the former condition, that neither entail the other, could be violated only, if one of them were known to be false, i.e. inadmissible. Therefore the first condition is superfluous. The following arguments against (a) may also be directed at (b).

[10] This, however, is a controversial interpretation of the text. Cf. Stove (65).

[11] There have been attempts to justify induction inductively, e.g. by Black in (49), pp. 86 seq, or deductively, e.g. by Reichenbach in (38), §39, Braithwaite in (53) or Salmon in (63), or to show that induction is not in need of any justification in the first place, cf. Ayer (56), pp, 71–75 and Strawson (52), pp. 248 seq. All these attempts have been shown to be unsuccessful, so that we need not go into them here. Cf. Kutschera (72), 2.5.

[12] Cf. de Finetti (37), or Kutschera (72), 2.1, e.g.

[13] Principles (IV) and (V) are not inference-schemes but statements about conditional probability. Something like an inductive argument can be derived from them, however, in the way that has been discussed for objective or logical probabilities under the title of 'statistical syllogism' especially by R. Carnap and C. G. Hempel. For a discussion see Kutschera (72), 2.5.4.

[14] We have to take the expression 'projectible' in a modified sense here against its use in Goodman's theory, since we refer to the use of predicates in (V) instead of (I) or (II). Exchangeability is the probabilistic counterpart of Hume's uniformity thesis. Exchangeability is only the simplest case for studying inductive reasoning. Cf. de Finetti's notion of 'partial exchangeability' in (72), pp. 217 seq. and 229 seq. But since the following philosophical remarks hold essentially also for more liberal notions of exchangeability, we confine the discussion to this simple case.

[15] This does not imply that the theory of subjective probability is normative, just as deductive logic is neither descriptive nor normative. In both disciplines no statements about how or what we should think or believe are formulated, but only theorems about what is correctly believed if we believe something else. And from that, of course, norms of correct thinking or believing may be derived. Hume was interested only in the psychological mechanics of believing and therefore did not use his probabilistic analysis to justify induction.

[16] The evolution of exchangeabilities by trial and error, however, is very indirect. First, they are compatible with all observations. Second, it is only when the conditional probabilities do not converge with the number of observations in the way we would expect them to if there were an objective probability of independent *F*-events that we may come, after some time, to the decision of changing our *a priori* assignments so as to give up the exchangeability of *F*. Carnap's axioms as proposed in (59) imply the principle of positive instantial relevance which implies $w(Fa_{n+1}, F_n^n) > w(Fa_{n+1}, F_n^{n-1})$ for all predicates *F*, i.e. for the pathological along with the normal predicates (cf. the articles of J. Humburg and H. Gaifman in Carnap and Jeffrey (71)). Therefore Carnap's system is saved from inconsistency only by being based on a language of predicate logic without identity in which the definition given above of the pathological predicates by their normal counterparts is not generally possible. In his later version of inductive logic in (71), however, Carnap gave up all his special axioms save that of regularity, so that his system is essentially that of the theory of subjective probability.

[17] Cf. Salmon (67), pp. 75 seq., 82, (68), and Goodman (65), p. 62.
[18] There is no such analogon, however, in Carnap's original version of inductive logic, since probabilities of the type discussed below are not in the λ-continuum.

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