Induction," *Philosophical Review* LXIX, 4 (October 1960), 511–522; B. Skyrms, *Choice and Chance* (Belmont, Calif: Dickenson, 1966). The tendency (e.g., by Barker and Kyburg) to slip between D_1 and D_2 may be due to the fact that 'x is $grue_1 \equiv \forall t(x$ is $grue_2$ at $t)$' is true.

8. See, e.g., Kelley, *op. cit.*, §III.
9. As Goodman points out in *Problems and Projects* (Indianapolis: Bobbs-Merrill, 1972); see p. 359.
10. As, in effect, R. Carnap points out in "On the Application of Inductive Logic," *Philosophy and Phenomenological Research* VIII, 1 (September 1947), 133–147; see §3.
11. I take this to be essentially the argument in H. Leblanc, "That Positive Instances Are No Help," *J. Philosophy* LX, 16 (Aug. 1, 1963), 452–462.
12. For just one, typical example, see Skyrms, *op. cit.,* 61, 62.
13. As in W. V. Quine, "Natural Kinds," in *Ontological Relativity* (New York: Columbia, 1969).
14. And we could bring in the time factor by, for instance, supposing the method of examining changes at *T.*
15. R. C. Jeffrey, "Goodman's Query," *J. Philosophy* LXIII, 11 (May 26, 1966), 281–288; see p. 288. He actually has 'bleen' for 'grue' in the relevant passage.
16. Cf. Kelley, *op. cit.,* 196.

Concepts of Projectibility and the Problems of Induction

6

John Earman

Projectibility is most often discussed in connection with the distinction between "genuine" and "Goodmanized" predicates. But questions about projectibility arise for the most mundane of hypotheses and predicates where not the slightest hint of Goodmanian trickery is present. And there are a number of different concepts of projectibility, each corresponding to a different problem of induction. Some of these problems are not only solvable but have actually been solved, solved in the sense that interesting sets of sufficient and/or necessary conditions for projectibility have been found. In some cases the conditions are so mild that a coherent inductive skepticism is hard to maintain, whereas in other cases the conditions are so demanding that skepticism seems to be the only attractive alternative. Again, in some of the cases Goodmanian considerations are the key; in others they are irrelevant.

The purpose of this note is to provide a classification scheme for the various senses of projectibility that will reveal what is at stake in the corresponding problems of induction. A useful beginning can be made by recalling the twofold classification Russell offered in *Human Knowledge:*

> Induction by simple enumeration is the following principle: "given any number of α's which have been found to be β's, and no α which has been found to be not a β, then the two statements: (a) 'the next α will be a β,' (b) 'all α's are β's,' both have a probability which increases as n increases and approaches certainty as a limit as n approaches infinity." I shall call (a) "particular induction" and (b) "general induction."[1]

Each of Russell's categories needs to be refined. Under particular or instance induction I will recommend a fourfold partition, first distinguishing weak

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and strong senses according as the induction is on the next instance or the next m instances, and second distinguishing two ways of taking the limit as the number n of instances increases toward infinity according as we march into the future with the accumulating instances or stand pat in the present and reach further and further back into the past for more instances. Under general or hypothesis induction I will recommend a twofold partition depending on whether the hypothesis is a simple generalization on observed instances or a theoretical hypothesis that outruns the data. The upshot is a collection of six problems of induction with six rather different solutions.

1. Instance Induction: Marching into the Future

Only nonstatistical hypotheses will be considered. Further, it is assumed that the "instances" E_i, i = 1, 2, 3, . . . , of the hypothesis H are deductive consequences of H and the "background evidence" B (i.e., $H, B \vdash E_i$). If you want H to be a universal conditional, e.g., $(\forall x)(Px \supset Qx)$, take instances to be $(Pa \supset Qa)$ and the like; or else let B state that all the objects examined are P's and take instances to be $(Pa \,\&\, Qa)$ and the like.

DEFINITION 1. Relative to B, H is *weakly projectible in the future-moving-instance sense* for the instances $E_1, E_2, \ldots$ *iff* $\lim_{n\to\infty} Pr(E_{n+1}/E_1 \,\&\ldots\&\, E_n \,\&\, B) = 1$.

DEFINITION 2. Relative to B, H is *strongly projectible in the future-moving instance sense* for the instances $E_1, E_2, \ldots$ *iff*

$$\lim_{m,\, n\to\infty} Pr(E_{n+1} \,\&\ldots E_{n+m}/E_1\&\ldots\&E_n\&B) = 1.$$

Claim: A sufficient condition for both weak and strong future-moving-instance projectibility is that $Pr(H/B) > 0$.

Proof: (a) Weak projectibility (Jeffreys).[2] By Bayes's theorem and the assumption that $H, B \vdash E_i$,

$$(1)\quad Pr(H/E_1 \,\&\ldots\&\, E_{n+1} \,\&\, B) = \frac{Pr(H/B)}{Pr(E_1/B)xPr(E_2/E_1 \,\&\, B)x\ldots xPr(E_{n+1}/E_1 \,\&\ldots\&\, E_n\&B)}$$

If $Pr(H/B) > 0$, the denominator on the right-hand side of (1) will eventually become smaller than the numerator, contradicting an axiom of probability, unless $Pr(E_{n+1}/E_1\&\ldots\&E_n\&B) \to 1$ as $n \to \infty$. (b) Strong projectibility (Huzurbazar).[3] Rearrange Bayes's theorem to read

$$(2)\quad Pr(E_1\&\ldots\&E_n/B) = \frac{Pr(H/B)}{Pr(H/E_1\&\ldots\&E_n\&B)}$$

Setting $u_n \equiv Pr(E_1\&\ldots\&E_n/B)$, (2) shows that $u_n \geq Pr(H/B) > 0$. Since $u_{n+1} = u_n Pr(E_{n+1}/E_1\&\ldots\&E_n\&B)$, $u_1, u_2, \ldots$, is a monotone decreasing sequence that tends to limit $L \geq Pr(H/B) > 0$. So

$$(3)\quad \lim_{m,\, n\to\infty} \frac{u_{n+m}}{u_n} = \frac{L}{L} = 1$$

There is an immediate application to the projectibility of predicates.[4]

DEFINITION 3. Relative to B, the predicate "P" is *weakly projectible in the future-moving sense* over the sequence of individuals $a_1, a_2, \ldots$ *iff* $\lim_{n\to\infty} Pr(Pa_{n+1}/Pa_1\&\ldots\&Pa_n\&B) = 1$.

DEFINITION 4. Relative to B, the predicate "P" is *strongly projectible in the future-moving sense* over the sequence of individuals $a_1, a_2, \ldots$ *iff* $\lim_{m,\, n\to\infty} Pr(Pa_{n+1}\&\ldots\&Pa_{n+m}/Pa_1\&\ldots\&Pa_n\&B) = 1$.

From the previous results we know that a sufficient condition for both weak and strong projectibility of "P" in the future-moving sense is that

$$(C)\quad Pr((\forall i)Pa_i/B) > 0.$$

Thus, contrary to what is sometimes suggested, definitions 3 and 4 do not serve to separate "grue" from "green,"[5] except on what I take to be the wholly implausible assumption that the universal generalization of the one but not the other receives a zero prior.

When is (C) necessary as well as sufficient for future-moving instance induction? The limit of $Pr(Pa_1\&\ldots\&Pa_n/B)$ as n goes to infinity exists and is independent of the order in which the instances are taken. Further, we know that

$$(4)\quad \lim_{n\to\infty} Pr(Pa_1\&\ldots\&Pa_n/B) \geq Pr((\forall i)Pa_i/B)$$

But to assure that

$$(A)\quad \lim_{n\to\infty} Pr(Pa_1\&\ldots\&Pa_n/B) = Pr((\forall i)Pa_i/B)$$

we need to assume what Kolmogorov calls an axiom of continuity.[6] Then (C) is a necessary condition for strong projectibility of "P" in the future-moving sense; for

$$(5)\quad \lim_{m,\, n\to\infty} Pr(Pa_{n+1}\&\ldots\&Pa_{n+m}/Pa_1\&\ldots\&Pa_n\&B) = \lim_{m,\, n\to\infty} [Pr(Pa_1\&\ldots\&Pa_{n+m}/B)/Pr(Pa_1\&\ldots\&Pa_n/B)],$$

and if (A) but ¬(C), this limit is not 1 independently of how m and n go to infinity; e.g., first taking the limit as $m \to \infty$ gives 0.

(C) is not a necessary condition for weak future-moving projectibility of "P." Carnap's systems of inductive logic provide examples where (C) and strong future-moving projectibility fail but weak future-moving projectibility holds. However, the point can be illustrated in a more general way, independently of Carnap's *c*-function apparatus.[7] Suppose that *Pr* is *exchangeable* for "P" over the a_is, i.e., for every m

(E) $Pr(\pm Pa_1 \& \ldots \& \pm Pa_m/B) = Pr(\pm Pa_{1'} \& \ldots \& \pm Pa_{m'}/B)$

where $\pm P$ indicates that either P or $\neg P$ may be chosen and $\{a_{i'}\}$ is any permutation of the a_is in which all but a finite number are left fixed. If (E) holds, De Finetti's representation theorem gives

(D) $Pr(Pa_1 \& \ldots \& Pa_n/B) = \int_0^1 \Theta^n \, d\mu(\Theta)$

where μ is a normed probability measure on the unit interval $0 \leq \Theta \leq 1$.[8] Choosing μ to be the uniform measure gives

(6) $Pr(Pa_1 \& \ldots \& Pa_n/B) = 1/n + 1.$

Thus, (C) fails. But

(7) $Pr(Pa_{n+1}/Pa_1 \& \ldots \& Pa_n \& B) = (n + 1)/(n + 2),$

which is Laplace's rule of succession, so that "P" is weakly projectible in the future-moving sense. Under (E) the necessary and sufficient condition for the failure of weak future-moving projectibility is that

(CM) $$\lim_{n \to \infty} \frac{\int_0^1 \Theta^{n+1} \, d\mu(\Theta)}{\int_0^1 \Theta^n \, d\mu(\Theta)} \neq 1.$$

The label (CM) is supposed to indicate a closed-minded attitude, for (CM) is equivalent to the condition that $\mu([0, \Theta^*]) = 1$ for some $\Theta^* < 1$, ruling out the possibility that an instance of "P" can have a probability greater than Θ^*. The extreme case of closed-mindedness is represented by a μ concentrated on a point; for example, if $\mu(\{1/2\}) = 1$, then each instance of "P" is assigned a probability of 1/2 independently of all other instances, so that the user of the resulting *Pr* function is certain (in the sense of second order probability) of the probability of an instance of "P," so certain that no number of other instances of "P" will ever change her mind. The probability measure in Wittgenstein's *Tractatus* had this character.[9]

To summarize: Suppose that you give a nonzero prior probability to the hypothesis that the sun always rises. Then the rising of the sun is strongly future-moving projectible over the series of days. On the other hand, suppose that you are absolutely certain that the sun won't always rise. It is still possible for your belief that the sun will rise tomorrow to approach certainty as your experience of new dawns increases without bound. But, assuming (A), it is not possible for your belief that the sun will rise on any number of tomorrows to approach certainty as your experience of new dawns increases without bound.

Another sense of projectibility for predicates sometimes used in the literature[10] is codified in

DEFINITION 5. Relative to *B*, "P" is *somewhat future-moving projectible* over the sequence of individuals $a_1, a_2, \ldots$ *iff* for each $n > 0$, $Pr(Pa_{n+1}/Pa_1 \& \ldots \& Pa_n \& B) > Pr(Pa_n/Pa_1 \& \ldots \& Pa_{n-1} \& B)$.

Under exchangeability (E), "P" is somewhat future-moving projectible unless the measure $\mu(\theta)$ is completely concentrated on some value of Θ, as can be seen by applying the Cauchy–Schwartz inequality. Thus, the case of a closed-minded μ which is not completely closed-minded provides an example where "P" is somewhat but not weakly future-moving projectible. And in general there is no guarantee that projectibility in the sense of definition 5 will have the limiting properties postulated in definitions 3 and 4.

Humean skepticism with respect to future-moving instance induction, weak or strong, stands on unstable ground. If $Pr((\forall i)Pa_i/B)$ is any positive real number, no matter how small, future-moving instance induction must take place, like it or not. Setting $Pr((\forall i)Pa_i/B) = 0$ avoids strong future-moving instance induction, but if past experience, as codified in *B*, does not record a negative instance, then $Pr((\exists i)\neg Pa_i/B) = 1$ says that there is absolute certainty that the future will produce a negative instance, a not very Humean result.

Humeans can escape between the horns of this dilemma either by refusing to conform their degrees of belief to the axioms of probability or else by refusing to assign degrees of belief at all. The first tack is unattractive in view of the 'Dutch book' and other arguments that promote the axioms of probability as rationality constraints on degrees of belief.[11] The second tack seems to lead to something closer to catatonia than to active skepticism.

2. Instance Induction: Standing Pat in the Present While Reaching into the Past

There is a second way of taking the limit as the number of instances accumulates without bound, a way that is, perhaps, more directly relevant to Hume's classic problem of induction. To explain it, suppose as before that

$H, B \vdash E_i$, but now let i range over all the integers so that we have a doubly infinite sequence of instances $\ldots E_{-2}, E_{-1}, E_0, E_1, E_2, \ldots$.

DEFINITION 6. Relative to B, H is *weakly projectible in the past-reaching instance sense* for the sequence $\{E_i\}$ *iff* $\forall n$ $\lim_{j \to +\infty} Pr(E_{n+1}/E_n \& E_{n-1} \& \ldots \& E_{n-j} \& B) = 1$.

DEFINITION 7. Relative to B, H is *strongly projectible in the past-reaching instance sense* for the sequence $\{E_i\}$ *iff* $\forall n$ $\lim_{m, j \to +\infty} Pr(E_{n+1} \& \ldots \& E_{n+m}/E_n \& E_{n-1} \& \ldots \& E_{n-j} \& B) = 1$.

Corresponding senses of projectibility apply to predicates. (Of course, the future versus the past direction of time is not the issue here; rather the point concerns whether the "next instance" lies in the direction in which the limit of accumulating evidence is taken.)

For the future-moving sense of instance induction to be valid, it was sufficient that the prior probability of the universal generalization be nonzero. But not so for past-reaching instance induction. Consider the predicates "P" and "P^*," where the latter is defined by

$$P^*a_i \equiv (Pa_i \& i \leq 1990) \vee (\neg Pa_i \ \& \ i > 1990).$$

We can assign nonzero priors to both H: $(\forall i)Pa_i$ and to H^*: $(\forall i)P^*a_i$ but obviously not even weak past-reaching projectibility is possible for both "P" and "P^*." For P^*a_n is logically equivalent to Pa_n for $n \leq 1990$ and to $\neg Pa_n$ for $n > 1990$, so that if

$$(8) \quad \lim_{j \to +\infty} Pr(Pa_{1991}/Pa_{1990} \& \ldots \& Pa_{1990-j} \& B) = 1$$

then

$$(9) \quad \lim_{j \to +\infty} Pr(P^*a_{1991}/P^*a_{1990} \& \ldots \& P^*a_{1990-j} \& B) = 0.$$

Thus, unlike definitions 3 and 4, definitions 6 and 7 do distinguish between "grue" and "green" in the sense that both cannot be projectible in the past-reaching sense. But the cut between past-reaching nonprojectible versus projectible predicates does not necessarily correspond to the cut between Goodmanlike versus non-Goodmanlike predicates (see sec. 5 below).

If exchangeability (E) holds for "P," then past-reaching projectibility for "P" is equivalent to future-moving projectibility. Thus, if we assign nonzero priors to both $(\forall i)Pa_i$ and $(\forall i)P^*a_i$, exchangeability cannot hold for both "P" and for "P^*." Or if exchangeability does hold for both, then for at least one of them the measure μ in De Finetti's representation must be closed-minded.

This is more or less what one would have expected since in the present setting exchangeability functions as one expression of the principle of the

uniformity of nature.[12] What is interesting is that there is a principle of induction—weak and strong future-moving instance induction—whose validity does not depend on a uniformity of nature postulate. Furthermore, uniformity of nature in the guise of exchangeability is precisely what one does *not* want in order to make true some of the truisms of confirmation theory, such as that variety of evidence can be more important than sheer amount of evidence. Return to formula (1) used to prove Jeffreys's theorem and note that the more slowly for given n the factor $Pr(E_{n+1}/E_1 \& \ldots \& E_n \& B)$ goes to 1, the smaller the denominator on the right-hand side of (1) and, thus, the larger the posterior probability of H. Intuitively, the more various (and nonexchangeable) the E_is, the slower the approach to 1 is. Perhaps this intuition can be turned round to yield an analysis of variety of evidence, but I will not pursue the matter here.

Crudely put, the problem of future-moving instance induction concerns whether the future will resemble the future, while the problem of past-reaching instance induction concerns whether the future will resemble the past. The former problem can be posed and solved without much attention to the form the resemblance is supposed to take; for *any* predicate, "genuine" or "Goodmanized," will, irresistibly, lend itself to future-moving projectibility as long as a nonzero prior is assigned to the universal generalization on the predicate, and there is no danger of being led into inconsistency as long as the initial probability assignments are coherent. But the latter problem, as Goodman's examples have taught us, requires scrupulous attention to the form of resemblance if inconsistencies are to be avoided. Future-moving instance induction leaves only narrow and unstable ground for the skeptic to stand on. By contrast, past-reaching instance induction provides the grounds for but does not require a blanket skepticism, while the strongest form of general induction virtually begs for skepticism. It is to general induction that I now turn.

3. General Induction

Still assuming that $H, B \vdash E_i$, $i = 1, 2, 3, \ldots$, we can say that

DEFINITION 8. Relative to B, the hypothesis H is *weakly projectible* on the basis of instances $E_1, E_2, \ldots$ *iff* the probability of H is increased by each new instance, i.e.,
$Pr(H/E_1 \& \ldots E_{n+1} \& B) > Pr(H/E_1 \& \ldots \& E_n \& B)$ for each $n > 0$.
Claim: The necessary and sufficient conditions for H to be weakly projectible are that $Pr(H/B) > 0$ and that $Pr(E_{n+1}/E_1 \& \ldots \& E_n \& B) < 1$.
Proof: Write out Bayes's theorem.

The price for weak projectibility of H is low; but what we buy may be unexciting since there is no guarantee that the increases that come with increasing instances will boost the probability toward 1. Thus, we also formulate

DEFINITION 9. Relative to B, the hypothesis H is *strongly projectible* on the basis of the instance $E_1, E_2, \ldots$ *iff* $\lim_{n\to\infty} Pr(H/E_1\& \ldots \&E_n\&B) = 1$.

Claim: H is *not* strongly projectible if there is an alternative hypothesis H' such that (i) $B \vdash \neg(H\&H')$, (ii)$H', B \vdash E_i$ for all i and (iii) $Pr(H'/B) > 0$.

Proof: Assume that H is strongly projectible and assume that there is an H' satisfying (i) and (ii) and show that (iii) is violated. By Bayes's theorem and (*ii*),

$$(10) \quad \frac{Pr(H/E_1\& \ldots \&E_n\&B)}{Pr(H'/E_1\& \ldots \&E_n\&B)} = \frac{Pr(H/B)}{Pr(H'/B)}$$

By (i), $Pr(H/X\&B) + Pr(H'/X\&B) \leq 1$. So if H is strongly projectible, the limit as $n \to \infty$ of $Pr(H'/E_1\& \ldots \&E_n\&B)$ is 0. Thus, taking the limit in (10) gives

$$(11) \quad +\infty = \frac{Pr(H/B)}{Pr(H'/B)} \to Pr(H'/B) = 0.$$

Philosophers of science routinely claim that any amount of data can be covered by many, possibly an infinite, number of hypotheses. Strictly speaking, this is not so if it means that there are many H's satisfying (i) and (ii) above. Take the E_i to be Pa_i and take H to be $(\forall i)Pa_i$. Then H admits of no logically consistent alternatives satisfying (i) and (ii) and, hence, no alternatives satisfying (i)–(iii). Such lowly empirical generalizations escape the above negative result, and if (A) and (C) hold, so does strong projectibility. For if (A) and (C), then

$$(12) \quad \lim_{n\to\infty} Pr((\forall i)Pa_i/Pa_1\& \ldots \&P_n\&B) = \lim_{n\to\infty} [Pr((\forall i)Pa_i/B)/Pr(Pa_1\& \ldots \&Pa_n/B)] = 1.$$

We can also consider a doubly infinite sequence of individuals $\ldots a_{-2}, a_{-1}, a_0, a_1, a_2, \ldots$ and demand strong projectibility in the past-reaching sense, i.e.,

DEFINITION 10. Relative to B, $(\forall i)Pa_i$ is *strongly projectible in the past-reaching sense iff* for all n $\lim_{j\to+\infty} Pr((\forall i)Pa_i/Pa_n\&Pa_{n-1}\& \ldots \&Pa_{n-j}\&B) = 1$.

If (C) holds along with exchangeability and the natural generalization of (A), viz., for all n

$$(A') \quad \lim_{j\to+\infty} Pr(Pa_{n+j}\&Pa_{n+j-1}\& \ldots \&Pa_n\&Pa_{n-1}\& \ldots \&Pa_{n-j}/B) = Pr((\forall i)Pa_i/B),$$

then definition 10 is satisfied. In effect, exchangeability has the flavor of "If you've seen an infinite number of them, you've seen them all."

Once we move beyond direct observational generalizations to theories that outrun the data, it is surely true that there are many rival theories that cover the same data. For such a theory strong projectibility on the basis of its instances is impossible unless the dice have been completely loaded against all the alternatives.

We might then hope for a more modest form of projectibility, as given in

DEFINITION 11. Relative to B, H is (r, s) *projectible* on the basis of its instances $E_1, E_2, \ldots$ *iff* $Pr(H/B) = r < .5$, but there is a sufficiently large N such that $Pr(H/E_1\& \ldots \&E_N\&B) = s > .5$.

Claim. H is *not* (r, s) projectible for any r and s if there is an H' such that (i) $B \vdash \neg(H\&H')$, (ii) $H', B \vdash \neg E_i$ for all i, and (iii) $Pr(H'/B) \geq Pr(H/B)$.

Proof. Use (10) with $n = N$. If H is (r, s) projectible and there is an H' satisfying (i) and (ii), the left side of (10) is greater than 1. But if (iii) holds, the right-hand side is less than or equal to 1.

For this more modest form of general induction to work we don't have to load the dice completely against all rivals covering the same instances, but we still need to load them.

Although the Bayesian apparatus has shown itself to be very useful in clarifying issues about confirmation and induction, it proves to be idle machinery when it comes to testing nonstatistical scientific theories. Such a theory can have its probability boosted above .5 and toward 1 by finding evidence that falsifies rival theories. But in such cases simple eliminative induction suffices; and when eliminative induction does not work, then neither does Bayesianism, unless the dice have been loaded against all rival theories.

4. Russell on Induction

Having begun with Russell's formulation of the problem of induction, I now want to return to *Human Knowledge* to see what progress Russell made on the problem. Given that the book is the product of one of the great minds of Western philosophy, the results are more than a little disappointing. Here are four interrelated reasons for disappointment.

First, Russell did not distinguish between the past-reaching and future-moving senses of instance induction. When he gets specific about what instance induction means he tends to make it sound like the future-moving variety, as in "Let $a_1, a_2, \ldots, a_n$ be the hitherto observed members of α, all of which have been found to be β, and let a_{n+1} be the next member of α."[13] This is the easiest and most neatly "solvable" case, but Russell makes little progress toward its "solution," despite the fact that some of his reasoning is close to that later used by Jeffreys[14] to prove that the probability of the next instance approaches 1 (see the third comment below). One can speculate that Russell, having already decided that the validity of induction requires an extralogical principle not justified by experience, was not on the lookout for the kind of result provided by Jeffreys and Huzurbazar.

Second, Russell recognized Goodman's "new problem" of induction; and then again he didn't. He did because he used examples of Goodmanized hypotheses (see the fourth comment) and because he says that β

> must not be what might be called a "manufactured" class, i.e., one defined partly by extension. In the sort of cases contemplated in inductive inference, β is always a class known by intension, but not in extension except as regards observed members . . . and such other members of β, not members of α as may happen to have been observed.[15]

But then again he didn't because he didn't recognize that there is a distinction to be drawn between past-reaching and future-moving instance induction and that it is only for the former that Goodman's "new problem" arises.[16]

Third, Russell formulated the problem of induction in part V. Part VI discusses Keynes's attack on general induction. Assuming as before that $H, B \vdash E_i$, we can apply a result from Keynes's *Treatise on Probability*[17] to conclude that

$$(13)\quad Pr(H/E_1\& \ldots \&E_n\&B) = \frac{1}{1+[Pr(\neg H/B)/Pr(H/B)] \times [Pr(E_1/B\&\neg H)x \ldots xPr(E_n/E_1\& \ldots \&E_{n-1}\&B\&\neg H)]}$$

Set $Q_n \equiv Pr(E_n/E_1\& \ldots \&E_{n-1}\&B\&\neg H)$ and $q_n \equiv Q_1x \ldots xQ_n$. Then if $Pr(H/B) \neq 0$, the posterior probability of H will go to 1 in the limit as $n \to \infty$ if $q_n \to 0$. Russell comments:

> If there is any number less than 1 such that all the Q's are less than this number, then the product of n Q's is less than the nth power of this number, and therefore tends to zero as n increases.[18]

The reasoning here is similar to that used to prove Jeffreys's theorem on future-moving instance induction, but Russell does not make the connection. When H is a simple empirical generalization, e.g., $(\forall i)Pa_i$, and the E_i's are Pa_i, Russell says that "it is difficult to see how this condition [as quoted above] can fail for empirical material."[19] When i runs from 1 to $+\infty$ and the continuity axiom (A) holds, the factor $Pr(Pa_1\& \ldots \&Pa_n/B\&\neg(\forall i)Pa_1)$ in the denominator of the Keynes formula (13) must go to 0. But when i ranges from $-\infty$ to $+\infty$ and the instances accumulate in the past-reaching sense, this factor cannot be shown to go to 1, unless by "empirical material" Russell means material for which exchangeability or the like holds.

Fourth, the difficulty with general induction to theoretical hypotheses can be seen from a simplified version of Keynes's formula, viz.,

$$(14)\quad Pr(H/E\&B) = \frac{1}{1 + [Pr(\neg H/B)/Pr(H/B)] \times Pr(E/\neg H\&B)}.$$

Suppose that $Pr(H/B)$ is nonzero but small. Then in order for $Pr(H/E\&B)$ to be large, E must be such that it would be improbable if H were false ($Pr(E/\neg H\&B)$ small). But as Russell notes, it may be hard to find such evidence. Take, for sake of illustration, H to be Newton's theory of gravitation and E to be the discovery of Neptune. Then there are many alternatives to H "which would lead to the expectation of Neptune being where it was"; for example, take H' to be the hypothesis that Newton's law of gravitation holds up to the time of discovery of Neptune but not afterward.[20] Russell scores a point with his Goodmanian illustration, but the point obscures the fact that the general problem arises even when Goodmanian alternatives are not at issue.

5. Prospects for a Theory of Projectibility

From the perspective of the preceding approach some philosophical theories of projectibility appear to be confused as to purpose, or false, or both. Consider the most ambitious and widely discussed philosophical theory of projectibility, Goodman's entrenchment theory.[21] Conditions couched in terms of relative entrenchment of predicates seem irrelevant to some of the questions of projectibility distinguished here and inadequate to others. For example, any hypothesis, no matter how ill entrenched its predicates, is weakly projectible on the basis of its positive instances if it has a nonzero prior—that is a theorem of probability. To claim that H gets a zero prior if it conflicts with an H' that is supported, unviolated, and unexhausted, that uses better-entrenched predicates than those of H, and that conflicts with no still better entrenched hypothesis, is to make a claim that is constantly belied by actual scientific practice where new hypotheses using new predicates are given a "fighting chance" of a nonnegligible prior. On the other hand, strong projectibility of a hypothesis, even if all of its predicates are supremely well entrenched, may be provably impossible if rival hypotheses are given a fighting chance, even when the rivals use ill-entrenched predicates. The most obvious application of the entrenchment notion is to what I called the problem of past-reaching instance induction. Of course, the general problem is independent of the direction of time and, more importantly, of the time dimension, for parallel problems arise for projecting from one side of a division of the range of a nontemporal parameter into the other side (say, from cases where $(v/c) < 1$ to cases where v is near c). But as Rosenkrantz[22] has emphasized, there are numerous cases in the history of science where scientists project predicates that are unentrenched and that, from the perspective of entrenched theory, appear to be Goodmanized because they agree with the old entrenched predicates to a good degree of approximation in the well-sampled side of the division but diverge on the other side.

It is time to pause to ask what can be expected from a "theory of projectibility." A minimalist theory would be established by finding sharp and interesting necessary and sufficient conditions for the various notions of projectibility. The results reported here take us only part of the way toward this minimalist goal. But once the goal is reached, what more remains to be done? A more grandiose theory of projectibility would, presumably, consist of descriptive and/or normative rules for determining when the conditions developed in the minimalist theory are or ought to be met. The prospects for constructing such a theory with the tools of analytic philosophy seem to me dim.

To make this skeptical conclusion plausible, it suffices to focus on cases where we found that projectibility turns largely on prior probability considerations. Objectivist accounts of prior probability assignments have been offered by Reichenbach,[23] in terms of frequency counts, by Jaynes,[24] in terms of maximum entropy calculations, and by others. But in every instance there are serious if not crippling difficulties with the proposed method of assignment.[25] Without assigning specific prior probabilities we could seek a theory to justify assigning some nonzero priors to a class of favored hypotheses. Keynes's "principle of limited variety" was designed for just this purpose. In *Human Knowledge* Russell attacks Keynes's theory (and rightly so, I think). But Russell's own five 'postulates of induction,' designed he says to "provide the antecedent probabilities required to justify induction,"[26] are just as unattractive. Separability and continuity of causal lines, common causes for similar structures ranged around a center, etc., have a certain intuitive appeal, but they involve contingent assumptions that may not hold in the actual world if it is anything like what the quantum theory says it is like. For the subjectivist school of probability, as represented by De Finetti and followers, the envisioned theory of projectibility would consist of a psychological account of how people in fact distribute initial degrees of belief consistent (hopefully) with the axioms of probability. This is a task for cognitive psychology, not armchair philosophy. Of course, I expect that psychology will find that entrenchment and other considerations suggested by philosophers will play some role in the account, but I do not expect that the account will consist of a neat set of rules of the type envisioned in the philosophical literature.

Goodman has charged that the problem of induction and its solution have been misconceived. I agree, but I think the misconception extends further than Goodman would allow. In any case, it is curious that philosophers have reached for more grandiose theories of projectibility before getting a firm grip on minimalist theories. In addition to filling in the gaps in the results reported here, it would be nice to have results based on alternatives to exchangeability.[27] One would also like to have information about how fast the posterior probability increases and whether, as Keynes worried, we are all dead before the value gets anywhere near 1.[28]

Postscript (July 1992)

Nelson Goodman's projectibility puzzle was first stated in print in his article "A Query on Confirmation," *Journal of Philosophy 43* (1946): 383–385. Starting before the publication of the Query and continuing for some years thereafter, there was an intense series of three-way conversations among Goodman, Carnap, and Hempel regarding the issues raised in Goodman's Query. Some fascinating glimpses of these conversations are preserved in documents in the Carnap *Nachlass,* which is part of the University of Pittsburgh Archives of Scientific Philosophy. One of the documents is entitled "Survey of comments and objections made by Nelson Goodman concerning Carnap and the H_2O theories of degree of confirmation."[29] It is dated 1/27/46, and consists of eight handwritten pages, numbered 3 through 10. At the top of the first page (numbered 3), Carnap has written in red a large "H," his indication that the author is Hempel. "H_2O" evidently refers to the confirmation theories of Hempel, "Studies in the Logic of Confirmation," *Mind 54* (1945): 1–26, 97–121, and of Helmer and Oppenheim, "A Syntactical Definition of Probability and Degree of Confirmation," *Journal of Symbolic Logic 10* (1945): 25–60. Characteristically Carnap underlined what he took to be important passages, and in the margins and between lines he scrawled comments in his barely decipherable shorthand notation. I will quote from what I take to be the most interesting part of Hempel's missive.

"Question: How if at all are we to distinguish between permissible and non-permissible predicates?"[30] Answering in the third person, Hempel responded to his question as follows:

> The difficulty here indicated is closely related to the one which struck Hempel many years ago in connection with certain descriptions of the basic idea of inductive procedures; the latter is sometimes characterized thus: Induction consists in assuming that those regularities which have been found to be satisfied in all cases [of] past experience will continue to be satisfied by future experiences.[31] But this 'assumption' is contradictory: Any given finite evidence always satisfies several incompatible general regularities, which lead to different predictions for future cases. This is illustrated by . . . the fact that any finite set of points (results of measurements of 2 magnitudes) lies on many different curves (representing different laws of ??? for the two magnitudes) which determine incompatible predictions for new values still to be measured.[32]

There are two and possibly three morals to be drawn from 'grue.' Writing before the term 'grue' was coined, Hempel had one of the morals right in advance: in part, 'grue' is an illustration of the familiar point that any given finite evidence always satisfies several incompatible hypotheses that lead to inconsistent predictions about new instances. The inductive machinery Carnap was developing was in no way idled by this commonplace since it was not designed to say which one of the competing hypotheses was confirmed (period) but rather to specify the degree to which each is confirmed by the evidence.

Why then did 'grue' become so controversial? In part the answer lies with the other (alleged) morals I will discuss below. But a significant segment of the controversy was an artifactual result of the constrained setting in which the inductive logicians of the 1940s were working. For example, Hempel's account of qualitative confirmation was designed for hypotheses formulated in first order predicate logic. In this setting it is not so easy to concoct examples of a consistent evidence set, consisting of atomic observation sentences or negations of such sentences, that simultaneously satisfies two or more general hypotheses which are also couched in observational vocabulary and which make incompatible predictions about new cases. Indeed—and this helps to explain why 'grue' generated controversy—to produce such examples in this constrained setting, it is

necessary to resort to what many interpret as logical sleight of hand. And thus has arisen the impression that what is needed to resolve the grue problem is some clever bit of countermaneuvering. Perhaps 'grue' can be squashed by some clever philosophical tap dancing. But if the fundamental problem to which 'grue' averts is the one identified in Hempel's missive, a solution is not to be had by squashing of Goodmanized predicates.

To understand the second moral, recall that the inductive logicians of the 1940s took the 'logic' of inductive logic seriously in that they saw their task as one of creating an inductive logic that complements but lies parallel to deductive logic, and they took the parallelism to mean that they should produce a purely syntactical definition of confirmation. 'Grue' and related examples showed that this goal had to be abandoned.

Here it is interesting to trace Carnap's own evolving reactions to 'grue' and company. His first response was to deny that Goodman's objections affected Carnapian inductive logic as it was then conceived. All of Goodman's examples, as reported to Carnap by Hempel, seemed to involve an appeal to temporally or spatially ordered individuals. This allowed Carnap to write on March 9, 1946, that such examples are "outside my theory, because my theory applies only to a simple language with a non-ordered universe."[33] But even in a relatively simple language gruelike predicates can be defined, a point made by Goodman on March 13, 1946, in a document humorously entitled, "Notes on Notes on Carnap's Notes on Hempel's Notes on my Notes."[34] Goodman noted that the evidence that all marbles observed so far are red confirms not only the hypothesis that other marbles are red but also $(x)Tx$, where 'Tx' is a predicate that ascribes redness to observed marbles and nonredness to other marbles. So, for example, if the observed marbles were placed on desk D and other marbles are to be placed on shelf S, one could take 'Tx' to mean that x is on D and is red or x is on S and is green. Carnap acknowledged the point in correspondence and in his published reply to Goodman ("On the Application of Inductive Logic," *Philosophy and Phenomenological Research 8* (1947): 133–147) by placing restrictions on the primitive predicates of the system—in particular, requirements of simplicity and completeness. Hempel was skeptical of this approach, essentially for Carnapian reasons! His reservations were expressed in a letter to Carnap dated January 29, 1947:

> I share Nelson's [Goodman's] skepticism concerning the concept of absolute simplicity and the idea of a non-relativized analysis of attributes. Would you not have characterized these ideas some years ago, as reflecting—at least!—the material mode of speech, and as requiring a restatement which would make them rel tive to the language under consideration and to the logical means of analysis available in it?[35]

Carnap's response showed just how far he had moved from his position of the Vienna Circle days:

> I do not myself like at all this absoluteness of simplicity and completeness, but at the present moment I do not see a way of avoiding it. It is not meaningless, I believe. I do not condemn the material mode as strongly as earlier, but would merely warn against its possible dangers. . . . Semantics has removed some of the earlier fears of speaking about the world and the properties in it. Once we admit property variables in the metalanguage (which it is true, involves serious problems, but is anyway necessary for certain purposes of science), then we can say that *all* properties (in the universe in question) are expressible in the language. This is 'ontological' only in the new Quine sense, not in the traditional, metaphysical sense.[36]

I turn finally to the third and most controversial moral which Goodman wanted to draw from 'grue'; namely, that projectibility turns not only on matters of syntax and semantics but also on practical criteria, such as what predicates have in fact been successfully projected in past practice. The Carnap of the 1940s would have found this moral quite uncongenial. Once the appropriate restrictions on the primitive predicates (simplicity, completeness, or whatever) were in place, questions of degree of confirmation and, thus, of projectibility were for Carnap just a matter of calculation, a matter of applying *the* correct *c*-function, c^* being his then favorite candidate for that role.

In his later years Carnap tended towards, without ever quite reaching, the point of view of Bayesian personalism. Here talk of *the* degree of confirmation of H and E is replaced by talk of a person's degree of belief in H on E, the only synchronic constraints on such degrees of belief being the axioms of probability and the only diachronic constraint being the rule of conditionalization. From this perspective the ultimate moral of Goodman's grue problem—the descriptive nature of the problem of induction—returns with a such a vengeance that even the Goodman of the Query might have flinched. Goodman assumed that as a matter of actual fact we do largely agree on matters of projectibility and that the remaining task of induction is to describe how the agreement is manifested in rules of projectibility. By contrast the Bayesian personalist is prepared to find that such rules are a shimmering mirage. There will, of course, be rules in the sense of truisms of confirmation consisting of theorems of probability. But it is dubious that simple and general substantive rules about what degrees of belief to assign to new instances on the basis of already observed ones can be extracted from the set of degree of belief functions of all actually existing people. Perhaps such rules can be extracted from the more circumscribed set of belief functions of the members of a scientific community, the idea being

that the community would not remain a community for long unless the members experience rapid merger of opinion on the relevant range of hypotheses. This idea is in danger of turning into a tautology unless communities can be identified independently of merger of opinion behavior. We have arrived at an interesting set of issues. But they are a far cry from what most of the literature takes the grue problem to be about.[37]

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NOTES

1. Bertrand Russell, *Human Knowledge: Its Scope and Limits* (New York: Simon and Schuster, 1948), p. 401. See also Russell's *A History of Western Philosophy* (New York: Simon and Schuster, 1945), chapter 17 ("Hume"), and *Problems of Philosophy* (Oxford: Oxford University Press, 1956), chapter 6 ("On Induction").

2. H. Jeffreys, *Scientific Inference,* 2nd ed. (Cambridge: Cambridge University Press, 1957).

3. V. S. Huzurbazar, "On the Certainty of Inductive Inference," *Proceedings of the Cambridge Philosophical Society* 51 (1955), 761–62.

4. Paul Horwich, *Probability and Evidence* (Cambridge: Cambridge University Press, 1982), gives an interpretation of Russell's principle of particular induction that corresponds to our definition 3. However, since Russell's discussion was directed toward Hume's problem, definition 6 (given later) may provide a better interpretation.

5. Or "goy" from "boy" as in R. C. Jeffrey, *The Logic of Decision* (New York: McGraw-Hill, 1965), 175–77. But see Jeffrey's later discussion in the second edition of *The Logic of Decision* (Chicago: University of Chicago Press, 1983), 188–90. See also sec. 2 below. And compare with the confused discussion in K. Popper, *The Logic of Discovery* (New York: Scientific Editions, 1961), appendix vii.

6. A. Kolmogorov, *Foundations of Probability* (New York: Chelsea, 1956). In measure-theoretic terms, continuity requires that if $A_1 \supseteq A_2 \supseteq \ldots$ is a sequence of μ-measurable sets and $A = \bigcap_{n=1}^{\infty} A_n$, then $\mu(A) = \lim_{n \to \infty} \mu(A_n)$. In the presence of finite additivity, continuity is equivalent to countable additivity, requiring that if $\{B_i\}$ is a sequence of pairwise disjoint sets, then

$$\mu(\bigcup_{i=1} B_i) = \sum_{i=1}^{\infty} \mu(B_i).$$

7. R. Carnap, *Logical Foundations of Probability* (Chicago: University of Chicago Press, 1950), and *The Continuum of Inductive Methods* (Chicago: University of Chicago Press, 1952).

8. B. De Finetti, "Foresight: Its Logical Laws, Its Subjective Sources," in H. Kyburg and H. Smokler, eds., *Studies in Subjective Probability* (New York: Wiley, 1964). μ is uniquely determined by the Pr values if, as we are assuming, there are an

infinite number of individuals. Instead of exchangeability, Carnap speaks of "symmetric *c*-functions."

9. L. Wittgenstein, *Tractatus Logico-Philosophicus,* trans. D. F. Pears and B. F. McGuiness (London: Routledge & Kegan Paul, 1961), 5.15–5.152.

10. See Horwich, *Probability and Evidence,* and Jeffrey, *The Logic of Decision.*

11. See De Finetti, "Foresight," and A. Shimony, "Scientific Inference," in R. G. Colodny, ed., *The Nature and Function of Scientific Theories* (Pittsburgh, University of Pittsburgh Press, 1970). These Dutch book arguments are based on a finite series of bets and do not suffice to justify countable additivity or continuity. Extensions to an infinite series of bets are studied in E. Adams, "On Rational Betting Systems," *Archiv für mathematische Logik und Grundlagenforschung* 6(1961), 7–29, 112–28.

12. Carnap did not interpret exchangeability (or symmetry of *c*-functions) in this way since he specified that the subscripts on the individuals are to have no spatiotemporal significance.

13. Russell, *Human Knowledge,* 404.

14. Jeffreys's result was not available when Russell was writing *Human Knowledge.* Or at least the result did not appear in the first edition (1931) of *Scientific Inference* or the 1937 reissue; it is in the 1948 second edition.

15. Russell, *Human Knowledge,* 404.

16. In fairness to Russell it should be noted that it is not clear that Goodman himself passed this test.

17. J. M. Keynes, *A Treatise on Probability* (New York: Harper & Row, 1962), 235–37.

18. Russell, *Human Knowledge,* 424.

19. *Ibid.*

20. *Ibid.,* 411.

21. N. Goodman, *Fact, Fiction and Forecast,* 3rd ed. (Indianapolis: Hackett, 1979).

22. R. Rosenkrantz, "Why Glymour Is a Bayesian," in J. Earman, ed., *Minnesota Studies in the Philosophy of Science,* vol. X (Minneapolis: University of Minnesota Press, 1983), 69–97.

23. H. Reichenbach, *Theory of Probability* (Berkeley: University of California Press, 1971).

24. E. T. Jaynes, "Prior Probabilities," *IEEE Transactions on Systems Science and Cybernetics,* SSC-4, no. 3 (1968), 227–41.

25. Difficulties with Reichenbach's approach are well known. For criticisms of Jaynes's approach, see K. Friedman and A. Shimony, "Jaynes' Maximum Entropy Prescription and Probability Theory," *Journal of Statistical Physics* 3 (1971), 381–84, and T. Seidenfeld, "Why I Am Not an Objective Bayesian: Some Reflections Prompted by Rosenkrantz," *Theory and Decision* 11 (1979), 413–40.

26. Russell, *Human Knowledge,* 487.

27. Generalizations of De Finetti's representation theorem are discussed in B. Skyrms, *Pragmatics and Empiricism* (New Haven, Conn.: Yale University Press, 1984), and J. von Plato, "The Significance of the Ergodic Decomposition of Stationary Measures for the Interpretation of Probability," *Synthese* 53 (1982), 419–32.

28. I am grateful to C. A. Anderson, G. Hellman, P. Kitcher, B. Skyrms, and W. Sudderth for helpful comments on an earlier draft of this paper.

29. Doc. # 084–19–34, Archives of Scientific Philosophy, University of Pittsburgh.

30. Here Carnap has jotted: "Nicht klar. Bezeiht sich dies auf das *vorangehende* Beispiel oder auf das folgende ('projectible')" [Not clear. Does this refer to the *preceding* example or to the following ("projectible") following?]. The "preceding example" refers to the following case. Consider 99 individuals, denoted by a_1, $a_2, \ldots, a_{99}$. Let the evidence concerning them be E: $\neg Pa_1 \& \neg Pa_2 \& Pa_3 \& \neg Pa_4 \& \neg Pa_5 \& Pa_6 \& \text{etc} \& \neg Pa_{98} \& \neg P_{99}$, where '$P$' is a monadic predicate (say, is red). And let the hypothesis be H: Pa_{99}. Then we get $dc(H, E) = 1/3$ (Helmer and Oppenheim) and $c^*(H, E) \approx 1/3$ (Carnap). Now let a_1 refer to the first triple, a_2 to the second triple, etc., and let 'P' now denote the property of trios that exhibit green, green, red. With E': $Pa_1 \& Pa_2 \& \ldots \& Pa_{32}$ and H': Pa_{33} we get $dc(H', E') = 1$ and $c^*(H', E') \approx 1/3$. I am indebted to Gerald Heverly for deciphering Carnap's shorthand and to Pirmin Steckler-Weithofer for the English translation. The passage is quoted by permission of Prof. C.G. Hempel and the University of Pittsburgh.

31. Carnap underlined "will continue," and above the line he jotted: "Das is eine ungenugende Formulierung; Sie wird ersetzt durch *Def* fur *dc* bzw. c^*. Dann ist keine Inkompatibilitat mehr da. [This is an unsatisfactory formulation; it is replaced by [the] *Def* for *dc* respectively c^*. Then there is no longer any inconsistency.]

32. Carnap has underlined "lies on many different curves," and above the line he has written: "Ja, das ist grosse Schwierigkeit; aber erst zu losen spater, *für quantitative Sprache!* (Ich habe einige Ideen [?], in Anschlus an Jeffreys [?] aber noch keine Losung.)" [Yes, this is a great difficulty; but only to be resolved later *for a quantitative language!* (I have some ideas in connection with a proposal of Jeffreys, but not yet a solution.)] Presumably the reference here is to work of Harold Jeffreys.

33. Doc. # 084–19–31. Quoted by permission of the University of Pittsburgh.

34. Doc. # 084–19–30.

35. Doc. # 084–19–20. Quoted by permission of Prof. C.G. Hempel and the University of Pittsburgh.

36. Doc. # 084–19–06. Quoted by permission of the University of Pittsburgh.

37. I am grateful for helpful comments from Carl Hempel, Richard Jeffrey, and Wesley Salmon.