1994

# Why There Can't be a Logic of Induction

Stuart Glennan
*Butler University*, sglennan@butler.edu



## Recommended Citation

Glennan, Stuart, "Why There Can't be a Logic of Induction" *PSA: Proceedings of the Biennial Meeting of the Philosophy of Science Association* / (1994): 78-86.
Available at http://digitalcommons.butler.edu/facsch_papers/145

# Why There Can't be a Logic of Induction[1]

Stuart S. Glennan

Butler University

Carnap's attempt to develop an inductive logic has been criticized on a variety of grounds, and while there may be some philosophers who believe that difficulties with Carnap's approach can be overcome by further elaborations and modifications of his system, I think it is fair to say that the consensus is that the approach as a whole cannot succeed. In writing a paper on problems with inductive logic (and with Carnap's approach in particular), I might therefore be accused of beating a dead horse. However, there are still some (e.g., Spirtes, Glymour and Scheines 1993) who seem to believe that purely formal methods for scientific inference can be developed. It may still then be useful to perform an autopsy on a dead horse when establishing the cause of death can shed light on issues of current concern.

My intention in this paper is to point out a problem in Carnap's inductive logic which has not been clearly articulated, and which applies generally to any inductive logic. My conclusion will be that scientific inference is inevitably and ineliminably guided by background beliefs and that different background beliefs lead to the application of different inductive rules and different standards of evidentiary relevance. At the end of this paper I will discuss the relationship between this conclusion and the problem of justifying induction.

## 1. The Task of an Inductive Logic

An inductive logic is a calculus which allows one to determine the relevance of some set of evidence to establishing the truth of an empirical hypothesis. Carnap conceives the problem of defining such a calculus to be one of defining a conditional probability measure, $c(h,e)$. The parameters $h$ and $e$ are sentences in a scientific language representing the hypothesis and the evidence respectively; $c(h,e)$, is meant to measure the probability of the hypothesis given the evidence. Once one has chosen a particular $c$, the evidentiary relationship becomes completely formal. Since there are infinitely many possible $c$'s, the problem is to choose one which captures our intuitions about evidentiary relations.

Carnap's strategy for choosing a $c$ function has two steps. First Carnap proposed a set of adequacy conditions that any $c$ function must satisfy. These adequacy conditions are meant to explicate our intuitions about inductive inference. They include the requirement that a $c$ function define a fair betting system, and the requirement that the

PSA 1994, Volume 1, pp. 78-86

function should allow one to "learn from experience." The adequacy conditions, while greatly narrowing one's choice of $c$ function, can be satisfied by infinitely many different functions. Carnap's second step therefore was to propose a particular probability measure, $c^*$, that satisfied the adequacy conditions.[2]

Carnap's analysis of inductive inference has been criticized on a variety of grounds. Perhaps the most important involve the justification of Carnap's choice of a particular $c$ function. First, given that the adequacy conditions can be satisfied by a wide range of functions, there seems to be no reason to prefer $c^*$ to other functions satisfying those conditions. Second, Carnap can offer no justification of his adequacy conditions other than that they capture our intuitions about inductive inference. Like Hume's principle of the uniformity of nature, Carnap's adequacy conditions seem to be principles which can be established neither by logic nor experience.

The presumption behind these objections is that Carnap's $c^*$ has succeeded in capturing our intuitions about inductive inference, and that the philosophical problem is that these intuitions cannot be justified. Even those who argue that $c^*$ is an incorrect reconstruction of our intuitions about induction leave open the possibility that an alternative to $c^*$ could be offered, and that then only the problem of justification would remain. If these were the only sorts of objections that could be posed, then further work on inductive logic would be warranted. The problem of the justification of induction could be dismissed as a pseudo-problem, and problems with the particular choice of $c$ function could be met by introducing $c$ functions which meet more refined adequacy conditions.

My objection is more fundamental, for it suggests not merely that a particular $c$ function is inadequate, or that our choice of $c$ functions cannot be justified on empirical or logical grounds, but rather that the very same evidence can lead us to make different inductive inferences, depending upon the context in which we make the inference. If I am right, then there can be no logic of induction, because our inductive inferences inevitably depend upon background beliefs which we have concerning the causes of the observations we use as evidence, and which cannot be completely captured in the calculus.

I will argue for my conclusion by analyzing a particular thought experiment which Carnap himself discussed. Carnap used this thought experiment to argue for the superiority of his $c^*$ over another possible $c$ function, $c\dagger$.[3] The crucial difference between these two functions is that $c^*$ allows one to "learn from experience," while $c\dagger$ does not. I will argue, contrary to Carnap, that the reason why $c^*$ seems more plausible than $c\dagger$ as a way to make predictions about the outcome of his thought experiment is not that $c^*$ is *universally* rationally preferable to $c\dagger$, but rather that Carnap (and the reader) has made certain implicit assumptions about the nature of the mechanism producing the experimental results. Different assumptions about the experimental mechanism make $c\dagger$ more plausible than $c^*$.

## 2. Two Thought Experiments

In a non-technical article on inductive logic (Carnap 1955), Carnap attempted to make $c^*$ plausible by illustrating its application to a simple experiment involving drawing balls from an urn. We are to imagine an experimental setup in which there are four balls in an urn, each of which has one of two mutually exclusive properties, say being white or black. Suppose that we draw balls one by one from the urn. For each draw from the urn, our inductive method will assign a probability to the hypothesis that the ball will be a certain color. For instance, our inductive method will give the probability that the first ball will be white, or the probability that the third ball will be white, given that the first two were black.

Suppose we make four draws from the urn. There are sixteen possible outcomes, which we can denote by a sequence of Bs and Ws. For example, BWWW represents the outcome of drawing a white ball followed by three black balls. We can completely specify a $c$ function for this experiment by assigning a prior probability to each of these possible outcomes. Carnap calls these possible outcomes *individual distributions*. So long as the sixteen values sum to one, the $c$ function will satisfy the minimum condition of being a probability measure. If there really is a logic of induction, then there should be some assignments that are better than others, and ideally one assignment which we can offer reasons to prefer.

Carnap of course thinks that some assignments are better than others. To make his point he considers two possible assignments which I shall call method † and method *.[4] Both of them apply the classical principle of indifference to the set of possible outcomes, but they do so in different ways. In the first case, Carnap assumes that, in the absence of further evidence, each of the sixteen individual distributions are equiprobable, assigning them probability 1/16. This is the probability assignment that would be made by the measure $c\dagger$. In the second case, we apply the principle of indifference twice. First we aggregate the individual distributions according to the number of black balls they contain. Carnap calls these aggregates *statistical distributions*. There are five such distributions, each of which we assign the probability 1/5. Next, we apply the principle of indifference to the individual distributions comprising each statistical distribution, assuming that the probability of 1/5 is divided evenly among each of the constituent individual distributions. This method yields probability assignments identical to those that would be made by $c^*$. The assignments of prior probabilities to individual distributions are summarized in the table below:[5]

Once we have specified the prior probabilities, it is possible to measure the probability of any hypothesis given any evidence. To calculate the prior probability of a hypothesis, one merely sums the probabilities for all of the individual distributions in

| Statistical Distributions | | Individual Distributions | Method † | Method * | |
|---|---|---|---|---|---|
| Black | White | | Probability of Individual Distributions | Probability of Statistical Distributions | Probability of Individual Distributions |
| 4 | 0 | BBBB | 1/16 | 1/5 | 1/5 |
| 3 | 1 | BBBW | 1/16 | 1/5 | 1/20 |
| | | BBWB | 1/16 | | 1/20 |
| | | BWBB | 1/16 | | 1/20 |
| | | WBBB | 1/16 | | 1/20 |
| 2 | 2 | BBWW | 1/16 | 1/5 | 1/30 |
| 2 | 2 | BWBW | 1/16 | | 1/30 |
| | | BWWB | 1/16 | | 1/30 |
| | | WBBW | 1/16 | | 1/30 |
| | | WBWB | 1/16 | | 1/30 |
| | | WWBB | 1/16 | | 1/30 |
| 1 | 3 | BWWW | 1/16 | 1/5 | 1/20 |
| | | WBWW | 1/16 | | 1/20 |
| | | WWBW | 1/16 | | 1/20 |
| | | WWWB | 1/16 | | 1/20 |
| 4 | 4 | WWWW | 1/16 | 1/5 | 1/5 |

which the hypothesis holds. To calculate the probability of a hypothesis given some evidence, one finds the sum of probabilities for individual distributions in which both the evidence and hypothesis hold, and divides it by the sum of probabilities for individual distributions in which the evidence holds.

Let H1 be the hypothesis that the first ball picked is white and H2 the hypothesis that the first two balls picked are white. Using the above table we can calculate for each method the probability of H1, H2 and H2 given H1:

| Hypothesis | Method † | Method * |
|---|---|---|
| H1 | 1/2 | 1/2 |
| H2 | 1/4 | 1/3 |
| H2 given H1 | 1/2 | 2/3 |

Both of these methods yield coherent sets of expectations. The problem is to decide which to apply. Carnap argued for method * over method † because method * allows one to learn from experience. We can illustrate this point by reference to the table above. Method † says that the probability that the next ball picked will be white is 1/2, regardless of what picks have gone before. On the other hand, according to method *, the probability of a second white ball given a first white ball is higher (2/3) than the probability of the first ball being white (1/2). It is important for the subsequent argument to notice that these methods are *formal* in the sense that the values calculated do not depend upon of our interpretation of the B's and W's.

The requirement that an inductive method allow us to learn from experience is one of Carnap's adequacy conditions. Carnap states the principle as follows:

> Inductive thinking is a way of judging hypotheses concerning unknown events. In order to be reasonable, this judging must be guided by our knowledge of observed events. More specifically, other things being equal, a future event is to be regarded as the more probable, the greater the relative frequency of similar events observed so far under similar circumstances. This *principle of learning from experience* guides, or rather ought to guide, all inductive thinking in everyday affairs and in science (Carnap 1955, 286).[6]

Elsewhere (Carnap 1945, §16) Carnap argues for this claim using Reichenbach's argument that a method which allows one to learn from experience is rational, because only such a method will in the long run allow one to improve one's ability to predict things in the world, supposing that the world is predictable at all. Much of the appeal of Carnap's $c^*$ however derives from its intuitive plausibility as applied to concrete cases like the urn experiment. If the first three balls that we draw out of the urn are white, it would seem foolish (at least according to Carnap's intuitions) to insist that the probability of the next one being white was only 1/2.

I believe we have been cheated here. We have been asked to endorse the principle of learning from experience largely on the basis of its plausibility as applied to particular thought experiments like this one. It is possible however to construct a formally identical thought experiment which yields quite different intuitions. Suppose that rather than drawing four balls from an urn, our experiment consists of flipping a coin four times. Let "W" stand for our getting a heads on a particular toss, and "B" stand for our getting tails. Then, using the same nomenclature as in the first experiment, we can describe a particular sequence of coin tosses by a sequence of letters such as "WWWB". Using table one, we can apply the method discussed above to calculate inductive probabilities. Let H1' be the hypothesis that the first coin toss turns up heads, and H2' be the hypothesis that the first two coin tosses turn up heads. Then we get the same probabilities as we calculated for H1 and H2. I think that our intuitions strongly suggest that we should ignore evidence from the first coin toss in predicting the outcome of the second coin toss; that is, we should choose method † over method * as a method for predicting the outcome of this experiment.

We are left in the following intolerable situation: We have two experiments which are described by structurally identical languages, and whose spaces of possible outcomes are isomorphic. We nevertheless have strong intuitions on the basis of information not formally described in our language that make us choose method * for the first thought experiment, and method † for the second. These experiments show that there is no inductive method that applies to all situations. We choose an inductive method appropriate to a particular situation on the basis of background knowledge or beliefs about that situation.

I will discuss several responses to these objections in the next section. For the moment though, I would like to consider the character of the background beliefs which make method * intuitively plausible in one case, but not in the other. In the first case, the experiment involves drawing balls from an urn. What we do in the experiment is to sample from an underlying distribution. We do not know what this distribution is, but we assume that it is fixed. If we believe that our sampling method is unbiased, we expect our sample to approximate (within some margin of error) the underlying distribution. Because of our understanding of the causal mechanism producing the evidence, we believe that past experience (our sample) should be a guide to future expectations.[7]

In the second case, the situation is quite different. Rather than thinking that there is an underlying distribution from which we sample, we think of the evidence as being generated by a sequence of independent coin tosses. What happens on the second trial has nothing to do with what happens on the first, beyond the mere fact that we used the same coin. What matters is only our judgment that we are dealing with a fair coin; and even if we believe that our coin is not fair, we should, in virtue of the independence of the trials, assign the same probability of heads to each trial.

## 3. Carnapian Rejoinders

I would like to consider two rejoinders which Carnap might offer to the claim that these thought experiments show that there is no logic of induction, in the sense of no uniquely determined *c* function. The first is to maintain heroically that method * is the correct method even for the second experiment. The second is to argue that the apparent inadequacy of method * results from our failure to take into account all relevant evidence.

Before turning to these rejoinders, I would like to indicate more precisely what challenge has been posed by my argument that there are situations in which method † is the right method. Carnap admits that there is a continuum of inductive methods from which we can not single out one *a priori*. One might think that in indicating that we cannot prefer method * for the coin tossing experiment, I am holding Carnap to a standard which he himself did not hold. This is not, however, the case. First, I am not arguing that we cannot justify the choice of method * over method † on *a priori* grounds; rather, I am suggesting that there are sound reasons related to our background knowledge about experimental setups which dictate the choice of method † over method *. Furthermore, according to Carnap my preference for method † over method * cannot be licensed either by experience or subjective whim because it violates *a priori* axioms for *all* acceptable *c* functions.[8]

**The heroic defense of method * –** Carnap's first line of defense is to argue that, contrary to appearances, method * is the correct method to apply to the coin toss case. One could construe the coin toss experiment as an experiment to determine whether the coin is fair. After all, how can we know that a coin is fair except by experience? The problem with this rejoinder is that it should take many more than four trials to shake our confidence in the fairness of the coin. In the first experiment, we identify the probability of a black ball with the underlying distribution of black balls, but in the second, we do not identify the probability of a heads with the distribution of heads in a sequence of experiments.

I am not denying that in the long run experience might lead us to doubt the fairness of the coin. If one gets 100 heads in a row, then it would be plausible to doubt the fairness of the coin. The difficulty with Carnap's reconstruction of the situation is that it does not show the way in which such evidence is brought to bear. There is no single method of induction which we apply uniformly to our experience. Rather, given antecedent beliefs about the nature of the mechanisms producing the states of affairs we take as evidence, we choose a particular inductive method. If our expectations are repeatedly not borne out by experience, then at some ill-defined point, we begin to doubt our beliefs about the mechanisms which bring about those states of affairs. We eventually consider using different inductive methods.

**The requirement of total evidence –** A second and related way in which Carnap could seek to undermine my counterexample would be to invoke what he calls the principle of total evidence:

> For an application of inductive logic by an observer *X* at a certain time *t* the following holds: ... If *X* wishes to apply a principle or theorem of inductive logic to his knowledge situation then he must use as evidence his total observational knowledge *K(t)* (Carnap 1963, 972).

Presumably there must be some observational evidence for our belief that the mechanism producing the sequence of experimental outcomes is of a certain kind. It may well be that the probability of H2' given H1' *alone* is 2/3, but when we consider as evidence both H1' and our evidence for the fairness of the coin, we could very well get a probability of H2' around 1/2. The failure of method * to give a plausible value comes not from a defect in or limit in the applicability of that method, but rather from a failure to consider all relevant observational evidence.

There is something correct in this rejoinder. If we have any reason to believe that the experimental outcomes are being produced by a fair coin, it is because we have at some time had evidence to that effect. It would not be fair dealing to expect an inductive method to give us the intuitively "right" answer without taking this evidence into account. The requirement of total evidence is, however, implausibly strict, and is not acceptable even as a rational reconstruction of good science. Leaving aside the objection that it is unclear what would count as total evidence, it is simply not the case that we ever could (or should want to) take into account all observational evidence in evaluating each hypothesis. Furthermore, when we take all evidence into account, we will in general invoke more than observations. We will also invoke background theories which refer to unobserved (and unobservable) entities.

The fundamental mistake in Carnap's view is that Carnap conceives of inductive inference as a process of formal calculation. He believes that at least ideally we should assess the probability of a hypothesis *h* by calculating the value of our preferred *c* function with *h* and our total observational evidence *K(t)* as arguments. My claim is that the connection between *h* and *K(t)* must be mediated by background theory. In order to bridge the logical gap between $h$ and $K(t)$ we must invoke some set of background hypotheses $T$. We believe $T$ in light of $K(t)$ and further background hypotheses $T'$, and so on. We see why we must invoke background theory by considering cases like the experiments described above where implicit assumptions about the mechanisms producing the evidence determine what method for calculating the probability of a hypothesis we should use.[9]

The view I am suggesting is reminiscent of Glymour's (1980) account of bootstrapping. Glymour argues that we must use auxiliary hypotheses to make the logical connection between evidence and hypothesis. The difference between his view and mine is that Glymour thinks that all auxiliary hypotheses can be made to explicitly enter into one's calculations of evidentiary relevance, while I am arguing that they may implicitly guide one's choice of an inductive method as well. This difference is significant, because if Glymour is right, it should be possible to define a three-place relation $c(h, e, T)$ between hypothesis and evidence with respect to some body of background theory. This would still be a logic of induction, albeit a more complex one than Carnap envisioned.

There are reasons why this proposal cannot succeed. First, it would be impossible to actually write down the rules for a plausible three place $c$ function. Given that different background theories determine different relations between hypothesis and evidence, writing down the three place function would in essence require one to list the two place $c$ functions determined by each possible background theory. However, there are infinitely many such theories, so we could never write them all down. Such a function, if it exists, would not be computable (at least in any practical sense). Furthermore, we will run into the same problem as we did for two place $c$ functions of determining the relation between evidence and hypothesis in the absence of background theory (i.e., for null $T$). If all our theoretical claims are ultimately based only upon our inductive method and our total observational knowledge, then there must be some theories which do not require background theory for confirmation. A three place $c$ function must consequently determine probabilities of $h$ given $e$ and null $T$. But what values should it choose — those given by $c^*$ or $c\dagger$, or some other set of values? My analysis suggests that in the absence of any background theory there is no answer to this question. Prior probabilities cannot be assigned on *a priori* grounds, but only on the basis of hypotheses concerning the mechanisms producing the evidence in question.

## 4. The Justification of Induction

Carnap belongs to a tradition beginning with Hume that regards the justification of induction with considerable indifference. While members of this tradition admit that principles of inductive reasoning do not admit of either empirical or deductive justification, they point out that we use such principles with confidence and practical success. The point of skepticism about induction is merely to exorcise our rationalistic pretensions. Beyond that point, they believe we can and must ignore it.

Carnap says surprisingly little about the justification of induction, but what he does say supports the theory that he took it to be a pseudo-problem. In Carnap 1945 he remarks that "the situation [regarding induction] has sometimes been characterized by saying that a theoretical justification of induction is not possible, and hence that there is no problem of induction. However, it would be better to say merely that a justification in the old sense is not possible" (Carnap 1945, §16). The new kind of justification involves showing (1) that the logic we have given accords with our inductive practices; and (2) that these practices are guaranteed a certain measure of success (provided that the universe is at all predictable). We might rest comfortably at this point so long as we believe that there is more or less a single set of intuitive judgments about inductive inference upon which we can and do successfully rely.

The point of my thought experiments is to challenge this assumption. They provide an example of structurally identical experiments which produce conflicting intuitions concerning what can be inferred from their outcomes. My challenge is analogous to the challenge that Goodman posed with his new riddle of induction. The new riddle of induction replaced the question "What justifies our principle of induction?" with "Why is it that we choose some inductive principles instead of others?" Like Goodman's new riddle, my argument shows how formal properties of the language describing hypothesis and evidence are insufficient to answer this question.

A true logic of induction would be a calculus which we ascertain *a priori* and which we can use to calculate the weight which our experience lends to any hypothesis. There is no such *a priori* method. Our empiricism must be more thoroughgoing, allowing that even our methods of inference are determined by empirical considerations. Up to a certain point Carnap is sympathetic to this idea. In discussing the reasons for choosing one among the continuum of inductive methods (by choosing a particular value for a parameter $\lambda$), Carnap remarks:

> An inductive method is ... an instrument for the task of constructing a picture of the world on the basis of observational data and especially of forming expectations of future events as a guidance for practical conduct. X may change this instrument just as he changes a saw or an automobile, and for similar reasons. ...[A]fter working with a particular method for a time, he may not be satisfied and thereby look around for another method. ... Here, as anywhere else, life is a process of never ending adjustment; there are no absolutes, neither absolutely certain knowledge about the world, nor absolutely perfect methods of working in the world (Carnap 1952, 55).

This remark is characteristic of what Howard Stein has called Carnap's dialectical attitude. My suggestion is that we must extend this attitude further. Empirical considerations guide more than our choice of a single parameter; they infect all of our assumptions about the significance of the evidence we collect. Our inductive methods cannot claim the title of inductive logic because we choose our inductive methods on the basis of our understanding of the world which we investigate.

## Notes

[1]I would like to thank Erich Reck and Mike Price for comments on earlier drafts of this paper, and Howard Stein for discussions on Carnap's views on induction.

[2]Carnap discusses adequacy conditions in a number of places, e.g., Carnap 1950. Kemeny (1963) has provided a succinct list which Carnap endorsed. For a definition of $c^*$, see Carnap 1945 or the appendix to Carnap 1950. Carnap ultimately gave up $c^*$ (Carnap 1963, 974), but the revised function he proposed suffers from the same difficulties that I shall discuss here.

[3]$c\dagger$, which Carnap attributes to Wittgenstein, is defined in Carnap 1952, §13.

[4]These methods correspond to c† and c*, but are defined only for the experiment under discussion. They should not be confused with c* and c† which are functions defined for arbitrary sentences in a first order language.

[5]This table is based upon a figure from Carnap 1955.

[6]Page references to Carnap 1955 refer to the version reprinted in Brody and Grandy 1989.

[7]For a discussion of the nature of causal mechanisms and their significance for confirmation see Glennan 1992.

[8]Specifically, it violates what Carnap calls the axiom of instantial relevance and the axiom of convergence (Carnap 1963, 976). These axioms in turn are justified by the condition that an inductive method must allow us to learn from experience.

[9]In chapter 5 of Glennan 1992, I argue that the background knowledge needed to legitimate inference concerns the mechanisms which produce the states of affairs we take as evidence. See also Helen Longino's argument for the ineliminability of background theory in evidentiary reasoning (Longino 1990, chapter 3). Longino offers examples of situations in which differing background assumptions make the same states of affairs take on different evidential significance.

## References

Brody, B.A., and Grandy, R.E. (ed.) (1989), Readings in the Philosophy of Science. 2nd ed. Englewood Cliffs, NJ: Prentice Hall.

Carnap, R. (1945), "On Inductive Logic", *Philosophy of Science*. 12: 72-97.

_______. (1950), *Logical Foundations of Probability*. Chicago: University of Chicago Press.

_______. (1952), *The Continuum of Inductive Methods*. Chicago: University of Chicago Press.

_______. (1955), *Statistical and Inductive Probability*. Brooklyn, N.Y.: Galois Institute of Mathematics and Art. Reprinted in Brody and Grandy 1989.

_______. (1963), "Replies and Systematic Expositions", in P. Schilpp (ed.), *The Philosophy of Rudolph Carnap. The Library of Living Philosophers*, Vol. XI. LaSalle, IL: Open Court, 859-1016.

Glennan, S. S. (1992), "Mechanisms, Models and Causation". Ph.D. Dissertation, the University of Chicago.

Glymour, C. (1980), *Theory and Evidence*. Princeton: Princeton University Press.

Kemeny, J. (1963), "Carnap's Theory of Probability and Induction", in P. Schilpp (ed.), *The Philosophy of Rudolph Carnap. The Library of Living Philosophers*, Vol. XI. LaSalle, IL: Open Court, 711-738.

Longino, H E. (1990), *Science as Social Knowledge: Values and Objectivity in Scientific Inquiry*. Princeton: Princeton University Press.

Spirtes, P., Glymour, G., and Scheines R. (1993), *Causation, Prediction, and Search. Lecture Notes in Statistics*, Vol. 81. New York: Springer-Verlag.