Assessing Inductive Logics Empirically[1]

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Philosophers of science have recently been urged by Arthur Fine to collaborate with physicists and with other scientists in constructing scientific theories.[2] What I am proposing is a collaboration at the other pole of scientific activity; the pole of experiment.

I consider this effort to be part of a tendency within philosophy to naturalize epistemology. The banner of naturalistic epistemology has attracted such men as Quine and Goldman. I consider the effort as one small part of that program which involves not only theoretical integration of at least portions of the two corpuses of knowledge, but the employment of methodologies which are common to them both.

The theory of inductive logic today is in stasis. A number of highly ingenious and very sophisticated theories of inference vie for primacy. As in the case of many philosophical theses, no closure seems in sight. No theory or its variant commands the adherence of the bulk of the community's members. In this situation a number of responses have been suggested.

1. Accept the situation as one which is characteristic of philosophical thinking and expect no closure.

2. Hope that the dialectical process in which experts engage will result in an outcome in which there is agreement on closure for at least a time.

3. Make use of the resources of science to aid in the process of closure.

These strategies are not all that different from one another. Except for the first they put great store in the value of closure. None of them offer complete assurance that they will have successful outcomes. But the movement to naturalize epistemology sees value in employing the resources of science to clarify problems which have proved resistant to more traditional methods, and it is in this spirit that I offer this essay.

As the first approach is from my point of view defeatist, I do not consider it. The history of science demonstrates many times that problems formerly considered to be only philosophical are in fact scientific.

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Let us turn to the second approach. There are some cases in which holders of one view have attempted to argue that those of another hold a viewpoint at variance with intuitions-the Carnap-Popper debate of the fifties is one example-but neither side seems to be able to convince the other.

Might there not exist a more suitable way to choose the best theory of inductive inference than to ground that decision upon the claims of logicians that their intuitions are closer to the truth than those of their rivals. Should not a more scientific methodology be employed in this type of theory choice?

Some evidence already exists for the claim that the rudiments for such a scientific choice exist. The fact is that alternative theories of induction have been created which differ significantly from one another in the following sense: they differ in the inferences which they license.

Let me offer some evidence for this claim. Consider the inductive systems of Henry Kyburg(1983) and of Isaac Levi(1980). Each of the following premisses is formulable within each of the two systems:

1. %(Swedes who are Protestants)=.9

2. %(Swedes who are residents of Malmo and who are Protestants)=.85 or (Swedes who are residents of Malmo and who are Protestants)=.91

3. Peterson is a Swedish resident of Malmo.

For Kyburg

4. Probability (Peterson is a Protestant)=.9

For Levi

4A.Probability (Peterson is a Protestant)={0,1}[3] (Levi, 1984, pp.192-213
This is so because Levi requires, as a premiss in the argument from which (4A) can be inferred, the statement:

1A. The chance of a Swede being a Protestant=.9

Kyburg permits no such statement in his corpus of knowledge. Here is another case. A probability function, structurally different from Bayesian probability has been developed by L.J. Cohen.(1977) Additive probabilities are not permitted in this system[4,5]

(1) $Prob_I(S)>0$

(2) If $Prob_I(s)>Prob_I(S')$, then $Prob(S\&S')=Prob(S')$

(3) If $Prob(S)>0$, then $Prob(7S)=0$

This notion of inductive probability is in contrasted with the standard Bayesian notion of probability According to the I system:

(A) $Prob_I$(Peterson is Protestant)=.9
$Prob_I$(Peterson is not Protestant)=0

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According to the Bayesian system

(B) Prob_B(Peterson is Protestant)=.9
Prob_B(Peterson is not Protestant)=.1

The existence of conflicting inference patterns which are sanctioned by alternative theories shows that these theories are not logically equivalent to one another, unless we believe that any differences in sanctioned inferences are accounted for by the fact that the schemes are appropriate for different subject/matters of theoretical investigation. A principle of selection between these competing theories is called for:

A number of logicians have in fact relied on their intuitions to determine which theory is correct. This includes Carnap, Hintikka, Kyburg, and Levi. An instance of this manner of argument is given by Levi in some comments he provides about Kyburg's logic:

> "Thus I strongly resist Kyburg's amusing suggestion that our disagreements over the indispensability of knowledge of chances in direct inference is based on a difference in our "intuitions" concerning what are to count as legitimate direct inferences...Our intuitions undoubtedly differ. But I claim that my intuitions ar sound and Kyburg's mistaken because his imply violation of confirmational conditionalization."[6] Levi(1978) p.73.

I hope that there is a better way to arrive at a consensus than this one. It is at least worthwhile to spend sometime in this quest.

In an earlier paper I argued for an alternative to the method of basing the choice between inductive logics on intuitions (Smokler, 1989). To my mind there seem two alternatives to this intuitive approach. One is mentioned by Cohen (1981). It is to take some on-going set of inferential practices say scientific inference, and determine which theory of inductive inference best accounts for this set. I propose another approach, but I do not want to make it appear that I reject the method which Cohen has suggested, He has argued for the explanatory superiority of his Baconian probabilities for the law of evidence. Others including Howson and Franklin(1984) have argued that inferences in natural science are best explained within the Bayesian framework. I argue for an experimental approach to this question. More precisely, I argue for the use of psychological methodology to sample human species-capacities for inductive inferences. The theory most consistent with all the data is to be considered the theory to be chosen. Of course, a possible outcome may turn out to be that no such theory satisfies the loose criterion for choice just enunciated.

This can be put another way. if we consider inductive logics to be theories in the given that term by philosophers, we examine a set of theories:

$$T_1, T_2, \ldots T_n$$

Ideally, the theories license a set of judgments which are at least contraries of one another:

T_1 logically Implies J_1
" " " " "
T_2 logically Implies J_2
" " " " "

where {J1, J2, Jn} are pairwise contraries. Hopefully by a process of elimination (on

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the basis of evidence resulting from psychological investigation) all but one of the theories proposed will be rejected.

Before I turn to the main body of this paper which describes and analyzes some of the empirical evidence which psychologists have collected, I want to offer some brief remarks in support of the approach I am advocating.

Several traditional objections have been offered against the research program which I propose. I will state them briefly. And then I will offer some considerations designed to defuse the force of these arguments:

(1)Theories of inductive logic are composed of normative propositions which license inferences from factual propositions to factual propositions. But how people in fact make inferences is not relevant to the truth or falsity of the normative statements which prescribe correct inference. In other words, the actual outcomes of the inferential process cannot stand is irrelevant to theory choice.

(2)It is possible on analytic grounds alone to specify a unique normative system which is rational. This solution of the problem of theory choice obviates any need to employ alternative methods to determine a decision. This analysis has come to be known as the Dutch-Book argument.

(3)No systematic scientific methodology for determining inductive theories can escape the charge of circular reasoning. For scientific methodology is but another name for a system of inductive logic. Such circular reasoning is inadmissible.

How does a proponent of a naturalizing epistemology deal with these objections. Much of the discussion regarding these points makes use of the notion of 'intuition'. The word is used in a non-classical sense as a belief which is widely shared and which is quickly assented to, a kind of natural judgment about which no great mental effort is required for its assent. Intuitive judgments are considered to be so basic as not to require justification there is sometimes the suggestion that what enters into the justification of an intuition is a kind of practical knowledge not formulable in propositional form. All of these senses of the term are in opposition to its classical philosophical meaning as judgement whose truth is necessarily evident in its very articulation. I shall employ the term to indicate a belief which is psychologically certain to most persons and which requires no great thought to garner assent.

The general consensus of opinion is that the intuitive judgements of experts in a field is more valuable than are the comparative judgements of ordinary folk. More specifically, a greater proportion of their judgements are considered to be true or approximately true, expertise, of course is constituted by much more than by this supposed characteristic. But whether or not this supposed characteristic does, in fact hold, is difficult if not impossible to determine.[7]

How about the normative propositions which lie at the heart of inductive theories. On what basis are these theories to be chosen. Not by the accumulation of empirical evidence. The reason for this is that the verifying on falsifying conditions for normative propositions can not be specified in terms of factual statements. A norm can be true whether or not the corresponding factual statement is true or false. In practice this means that the judgements which people state as the consequence of their inferences are of no logical relevance to the truth of those principles which sanction the inferences.

This seemingly insuperable problem is avoided, if one adopts the point of view advocated four decades ago by Nelson Goodman(1983) and later called by John Rawls(1971) 'The Method of Reflective Equilibrium." Nelson Goodman stated the essence of this method for the case of inductive inference in the following words:

> An inductive inference too, is justified by conformity to general rules and a general rule by conformity to accepted inductive inferences. Goodman(1983), p.63.

We can systematically study inferences and find some way of bringing their conclusions into a consistent relationship with the general rules of which Goodman speaks.

If we treat particular probability judgements as normative in character, then the logical problem we noted above no longer presents itself to us. But I see no problem in treating particular probability judgements as normative ones. They represent what people think *should* be the probabilities to be assigned.

The method of reflective equilibrium as practised by Goodman consists in testing general intuitions by accepted inferences. We can expand this notion in two directions (1) by considering a set of alternative theories in relationship to the set of the accepted inferences of a community and (2) by expanding the notion of a process of reflective equilibrium to that of a community. It is unclear whether Goodman considers the method one to be employed individually or within a community, but it is easy to imagine a situation in which a series of alternative theories, each held by at least one member of that community are subject to the process of reflective equilibrium.

The method of reflective equilibrium is a systematic way to arrive at normative judgments. When intuitions differ, there is no better way to reconcile differences in than by dialectically challenging each of these conflicting general intuitions by particular ones. Particular intuitions-which, in this context can be equated with normative judgements can be empirically determined by systematic questioning of a population. The whole panoply of psychological methods can be used to elicit beliefs and the whole range of statistical techniques can be employed for estimating the reliability of the sample. In other words, there are clearly ways to systematically elicit inferential intuitions on the part of an individual or collectivity. These intuitions may turn out to be coherent or incoherent both for an individual and a collectivity. They may turn out to be implied by or contradictory to a purported theory of induction. They may turn out to be irrelevant to that theory. Through a dialectical process a scholar can adjust theory and experiment. One can view theory choice in scientific contexts as a variant of the method of reflective equilibrium. A theory and its alternatives purport to explain some event or its negation. The production of that event (evidence) results in the acceptance or rejection of the theory, but sometimes the theory is so firmly accepted that experimental evidence is rejected. Experiment and theory are dialectically related in the way that the method of reflective equilibrium would require them to be.

It seems to me that reflective equilibrium is achieved in science. Here it is called the experimental method, and when the theoretical aspect of science is stressed, it is called 'the hypothetico - deductive method.' When experimental data indicates the ser of theoretical and experimental beliefs to be inconsistent, a new equilibrium point is sought. When it confirms expectations it strengthens some theoretical beliefs. Experiments can sometimes determine a choice. On the other hand, experimental findings are sometimes rejected in the interests of consistency. The method is well accepted within the scientific community.

There is a standard objection to this viewpoint. It is this: people's intuitions are frequently wrong, i.e. That assent to a intuitive proposition in no way guarantees the truth of the proposition. Thus, Stephen Stich argues that the following intuition shared by many people, amongst them experts, is obviously false:

> In a fair game of chance the probability of a given sort of outcome occurring after n+1 consecutive instances of non-occurrence is greater than the probability of its occurrence and n consecutive instances of non-occurrence. Stich and Nisbet (1980), p. 192.

The inappropriateness of believing in the truth of this inferential principle is expressed in strong terms by Stich:

> Faced with this argument the friends of reflecting equilibrium may offer a variety of responses. The one I have the hardest time understanding is simply to dig in one's heels and insist that if the gambler's fallacy (or some other curious principle) is in reflective equilibrium for a given person or group. (Stich 1988), p. 398.

Of course, the "Gambler's Fallacy" rests upon the assumption that the sequence of events for which the intuition is held are non-independent and have an unusual causal or probabilistic relationship to one another. Prima facia there is no incoherence in accepting the correctness of a scheme of probability and then adding to it the so-called gambler's fallacy as an additional inference scheme.

Intuitions can be wrong even though they are nearly universally held. After all an intuition is a belief held with strong conviction and frequently with no real justification. If we have reason to doubt the intuition, which is after all corrigible, this merely indicates that a new reflective equilibrium is to be sought. It is by no means a fundamental criticism of the method itself, Foundationalists would not accept the method of reflective equilibrium but most of those who enter into this present debate are not foundationalists.

Still another objection which is voiced in this respect is that the process of reflective equilibrium as I have employed the term is itself an inductive method and consequently a kind of circularity of reasoning is manifest. But this need not concern us. Indeed the fact that an inductive procedure is employed as a way to confirm an inductive proceeding lends credence to the correctness of choice.

There is another criticism which is offered against the naturalizing methodology which I have outlined. It is this. By conceptual analysis alone the necessary and sufficient conditions for rational belief can be specified. This determines an inductive logic for all people, viz Bayesian logic.[2] A basic assumption of such an argument is that any logic which a person would find rational must be associated with a set of acts which are rational. So any assignment of degrees of partial beliefs to an individual which guarantees that the person loses money (or utility) is not rational, since it is associated with an irrational act. Such an eventuality is called a Dutch Book, and an assignment of partial beliefs which results in a Dutch Book is called incoherent. Therefore a coherent assignment is one in which no Dutch Book can be made. DeFinetti and others have shown that all and only coherent assignments of degrees of belief to propositions obey the Kolmogorov axioms and therefore the Bayesian logic.

What is the relationship between rationality and Bayesianism? If somehow it could be shown that a person's set of beliefs is coherent if and only if he is rational then the choice of a version of Bayesianism as the universal inductive logic would be but a small inferential step. I am not so clear that such an equivalence has been shown. The coherence of any set of beliefs would have to be a necessary and sufficient condition for a person's rationality. We will not trouble with the plausibility of the sufficiency condition.

(SUFF) If a person has a coherent set of beliefs, then he is rational but we question the a priori character of the necessary condition.

(NEC) If a person does not have a coherent set of beliefs, then he is not rational.

I think that one purported justification for (NEC) goes as follows: Any person who knowingly assents to a situation in which he loses money (or utility) is irrational. For he is acting in such a way as not to have as an aim which would be best for himself and which makes his life go for him as well as possible. This is a principle under which and only under which a set of coherent bets is rational. Otherwise what difference would it make to the believer if he lost money on the basis of his betting behavior? It is also clear that it is a rational policy to assign degrees of belief which do not necessitate loss since before the bet is paid off, it is reasonable to expect that a person's general aim of doing what is best for himself can be fulfilled. I know of no other general principle under which the relationship of rationality to coherence makes sense.

But the theory of rationality which makes sense of this connection is in fact one that Derek Parfit in his brilliant book, *Reasons and Persons* (1984 pp.130-132) advances and then shows very clearly to be false. He calls it the S-Theory: it is a theory of rationality. It places the obligation upon each of us of acting rationally. It postulates that, "For each person, there is one supremely rational ultimate aim, that his life go, for him, as well as possible." Now the falsity of this S-Theory is shown by producing a counter-instance to it. Parfit does this. I shall adopt the example to the particular instantiation of the theory which the coherence criterion exemplifies.

A person chooses to bet in such a way that will insure that the person he bets against will necessarily gain a certain amount of money. The person knows the facts and thinks clearly about them. He knows that the money he is providing is desperately needed by the other person and that to give the money directly to that person would so psychologically devastate him so as to make the transfer psychologically disastrous for the other person. He is doing what will be worse for him. If he did not give the money he would not be haunted by remorse. The rest of his life would be well worth living.

Does the production of a case where a person is being rational yet incoherent prove that Bayesianism is wrong? No, it only proves that it is not self-evident. I have shown that a theory of rationality of which Bayesianism is a instantiation is not self evidently true. I do not claim that Bayesian logic is false or irrational. But it is not true or rational on the basis of the supposed fact that this S-Theory is intuitively true and that as a deductive consequence of the theory it also must be true and rational.

Bayesianism is not the only rational inductive logic though surely its correctness is *not* excluded by the argument just offered. All that is meant to be shown is that there exists no a priori basis for a choice of inductive system; on the contrary there are good reasons for making the choice on scientific grounds.

Let me now turn to the practical possibilities of investigating inductive theories. If the strategy I have outlined makes sense, than the empirical work of investigation remains to be done. Firstly, the class of conflicting theories must be indentified, and secondly, systematic observations of the range of normative judgements regarding probability must be set against the inferences warranted by the conflicting theories. Then and only then can the process of reflective equilibrium be set in motion.

As of now, only several theories have been shown to be in conflict. We have seen Levi's system of modified Bayesianism and Kyburg's logistic system warrant conflicting conclusions. We have seen that Cohen's system warrants different conclusions from that of a Bayesian one. For all we know, there may be many other conflicts as well.

Poised against these theoretical disputes are a handful of empirical investigations, most of which presuppose the correctness of Bayesianism, only one of these experiments have been instituted with the idea of testing one theory of inductive inference against another with the aim of seeing how peoples' judgement coincided or conflicted with one or more of the theories.

Conflicting theories exhibit several dimensions of difference about which empirical evidence could be gathered. Here are four dimensions which come to mind:

(1) Whether or not a principle of conditionalization is part of the system.

(2) Whether or not a notion of chance is incorporated within the system.

(3) Whether this or that measurement system is appropriate for the representation of the relation of probabilistic inference.

(4) Whether or not rules of acceptance are part of the system.

In this paper, I will concentrate on the available evidence for (3). It may turn out that the empirical data is inconclusive. I hope not.

What systematic evidence do we have? There are a number of studies which are relevant to the inductive systems which I have enunciated. With one exception the studies were not done with the idea of testing one system against the other ones. Psychologists have generally presupposed the normative correctness of one of these systems and have investigated the question of how closely the subjects tested conform to these normative standards. In cases where such conformity is clearly not present, some of these psychologists have postulated distorting mechanism's which account for this gap. One thinks immediately of Amos Tversky and Daniel Kahnman. This initial presupposition is one I do not share. So it is unlikely that these investigations will bear directly on the point which I raise in my paper: the choice between theories with regard to their metrical representations.

Let me list the experimental results:

1. W. Edwards(1967) work on conservatism of judgement showed that persons asked to evaluate probabilities on the basis of prior probabilities, i.e. to estimate the posteriors of the basis of Bayes Theorem overwhelmingly underestimated those probabilities.

2. Kahneman & Tversky(1983) showed that persons, under plausible conditions, frequently violate the conjunction rule of Bayesian probability:

What their experiment shows is that most people reject the principle of Bayesian probability:

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Prob(AB)=Prob(A) X Prob(B/A)

In other words, it could be taken as evidence of the inappropriateness of Bayesian probability as an inductive logic.

Schum and Martin(1987) are psychologists who are interested in legal reasoning and therefore in Cohen's system. He compared peoples evaluation of guilt and innocence in simulated court cases with respect to the adequacy of the Bayesian system in contrast with the Baconian system. His methodology is rather complex; in general it depends upon offering subjects stories involving the introduction of evidence relevant to the question of guilt or innocence of a party. Such judgments of guilt and innocence change over time. As I have already indicated, Bayesian probability theory would, once a proposition is assigned minimal probability, not permit revision of that probability, and it would mandate the negation of a proposition to be assigned a probability which when added to the original proposition sums to the maximal value. In qualitative terms this would amount to the probability of the guilt judgment varying inversely over time as does the judgment of innocence and vice versa. This does not happen.

Schum and Martin studied patterns of inference from twenty subjects. They found evidence that the subjects tested in the context of appraising legal evidence adhered to the Baconian model of probabilistic inference.

So far the record of experimental work is suggestive from the standpoint of the questions I have posed. Here are some problems:

1. Except for the work by Schum no investigation has been made comparing two inductive theories for the comparative adequacy of the explanation of the data.

2. It is not clear that adequate statistical techniques have been used in the selection and analysis of the data. The question of whether twenty undergraduate students stand as surrogates for the human race is raised by these samples, though it is clear that Edwards, Schum, and Tversky are well aware of these problems. All experiments should be conducted in accordance with the best statistical methodology available at the time.

3. If the goal of the investigations is to sample the human population then qualitative versions of the differences between the consequences of the theories should be sought. It is implausible to think that most persons can, for example, compute likelihood values and binomial distributions yet this is the information required for a person computing the hypothesis about bags in Edwards' investigation.

As I have indicated there are still other dimensions of this question which have not been touched on. One is whether the theory is committed to a view of belief as binary or graded. This is related to the question (4) which I posed earlier in this paper. Goldman in his book *Epistemology and Cognition* has written on these subjects. He attempts to marshall the empirical evidence for some of the other questions I raised earlier in this paper. I shall not enter into this question here.

What is required is the collaborative efforts of psychologists and philosophers, working in close cooperation, to devise testing procedures which will pass the standards, of both communities and bring progress to an area of inductive logic which has been stalemated these many years.

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Notes

[1]I wish to express my appreciation to Jonathan Dancy and Allan Franklin for encouragement and criticisms. None of the errors however are their responsibility.

[2]In his presidential address to the meeting of the PSA, October 1988.

[3]I take it that however one interprets a probability measure (0,1), the two probability judgements are in conflict. Levi surely takes his system to warrant a judgement in conflict with that of Kyburg.

[4]Cohen(1977). pp.224-229.

[5]From the time of the earliest development of the ideas of mathematical probability, the notion of non-additive probabilities has been present. These matters are discussed in Daston(1988).

[6]Levi(1978)

[7]Psychologists have determined in a number of studies that expert intuitive judgement is as much in error as that of the non-expert.

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