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HOWARD SMOKLER

ARE THEORIES OF RATIONALITY EMPIRICALLY TESTABLE?

ABSTRACT. Since rationality is a normative ideal, it is difficult to see how a theory of rationality might be subjected to empirical evaluation. This paper explores various aspects of this problem in relation to the work of L. J. Cohen, Amos Tversky and Daviel Kahneman, Ellery Eells, Isaac Levi, and Henry Kyburg. Special consideration is given to its significance for testing systems of inductive logic.

If rationality is only a normative ideal, then it is difficult to imagine how theories of rationality can be empirically tested so as to be confirmed or disconfirmed. How we should think and act is constrained only in the loosest sense by the ways in which we do act and think. On the other hand it is entirely possible to describe the patterns of thought and action manifested by human populations which these populations characterize as rational, and even to give an explanatory account of why these patterns of thought and action are manifested. Hence it is possible to test the empirical adequacy of theories of this second sort.

Two kinds of theories of rationality are distinguishable. The first kind provides an answer to the question:

(1) How should we act or think?

while the second kind provides an answer to the question:

(2) How do we act or think?

Received doctrine states that while theories of the first kind are not accepted or rejected on the basis of experimentally determined data, theories of the second kind can be so accepted or rejected.

Is there any way in which normative theories of rationality are subject to empirical testing? I claim not only that this is possible but desirable. I claim that this is the case when normative theories are conceived of as answers to the question:

(3) How should we think about how we think?

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and that the resulting theory of rationality can be confirmed or disconfirmed by experimental data. Before considering the desirability of such an approach, however, I shall discuss its possibility.

A pioneering attempt to formulate such a methodology is to be found in a recent article by the philosopher L. J. Cohen.[1] This article generates many questions. Cohen advances the thesis that human irrationality can be systematically investigated. If human behavior does not conform to the normative set of standards which define rationality, then irrationality is experimentally demonstrated. These norms are arrived at by a process which Cohen calls the establishment of a "narrow reflective equilibrium". On this approach, which has been developed by Goodman and Rawls,[2] one matches general norms against particular intuitions to arrive at a set of normative principles in harmony with intuitions. Corresponding to these normative theories, and in Cohen's words "read off" from them, are theories of inductive competence. Cohen makes use of the Chomskyian distinction between a person's ideal competence (incorporated in a theory of ideal capacity) and a person's actual performance. He assigns to human performance the character of irrationality on the basis of empirical evidence, but ideal capacity (competence) remains always rational. He does not envision any empirical procedures which put to the test a theory of rational competence. It is an a priori theory.

In fact, there are a number of normative theories of inference that compete for acceptance as the most satisfactory. These theories can be categorized along at least three independent dimensions: (1) they are Bayesian or non-Bayesian theories;[3] (2) they incorporate a folk-psychological or a computational conception of acceptance and belief,[4] and (3) they do or do not incorporate a notion of chance.[5] It is difficult to imagine or specify how competing theories that claim a priori status can vie for acceptance as the best theory. For the presentation of competing a priori intuitions to form the basis of such a choice appears to involve a commitment to false a priori intuitions, a notion which on its face is absurd. Yet some such commitments seem to be at the basis of the argument between adherents of these different theories. So Levi faults Kyburg's system for violating a principle of confirmational conditionalization that Levi claims is intuitively obvious. As a consequence, consensus fails to emerge, though, in the dialectic that occurs, modifications of systems do occur. It is sometimes claimed that theories are not really in conflict because they are topic-specific. Cohen, in fact, has claimed that

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his theory of non-Bayesian probabilities is applicable to legal rather than to scientific subject-matters. But it is difficult to think of a non-circular definition of subject matter that will facilitate this obvious strategy for avoiding conflict. Perhaps systematic psychological data is relevant to theory choice, after all.

Two immediate objections can be raised. A theory of inductive logic, goes the first objection, is a normative theory, and consequently no amount of data, systematically or casually arrived at, can falsify it.

There is a second objection. The charge of circularity can still be raised against the procedure of confirming a theory of induction. For if data is to be relevant (or irrelevant) to a theory, then that relationship can best be described as an inductive one, the subject matter of an inductive logic. So in order to test an inductive logic, a correct inductive logic must be presupposed. This difficulty is also encountered in the various attempts to justify induction. How should we deal with these objections?

Logical norms, including those we classify as inductive, are traditionally entertained as analytic statements, i.e., as true or false because of the meaning of their constituent terms. Carnap later characterized these inductive norms as based upon our intuitions. It is not difficult to consider the evidence garnered by psychologists as systematically expressing the intuitions of human populations. Psychologists can determine the inferences or estimations a population thinks should be made under determinate circumstances. Norms – generally those of a singular character, such as that 'the probability of *this* event should = .N' – are the subject matter of psychological inquiry. Yet these norms are themselves of the same logical character as the general principles that they test and are themselves in a dialectical relationship with those principles. This in fact is the process of finding a reflective equilibrium. There no longer exists a logical gap between statements that specify what ought to be the case and statements as to what is or is not the case.

What about the circularity that seems to accompany all our attempts to investigate the foundations of inductive logic? Many inductive logicians accept this circularity without considering it to be crippling. As Ellery Eells, for example, has remarked,

> To the extent that a theory implies and explains what is already known or believed about its subject matter, the theory is confirmed. And this should hold true for theories about confirmation. Thus the Bayesian theory of confirmation will be confirmed if it can be shown that it is able to explain and justify accepted forms of inductive inference.[6]

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From this point of view, a theory of confirmation can itself be confirmed. Eells endorses the thesis that a theory is confirmed to the extent that it explains some underlying data not previously explained. The theories of confirmation we consider as candidates for confirmation themselves are presupposed as true; otherwise no confirmation could be established for them. But in this context, this is not a relevant objection. Consider the procedure as a bootstrapping process. If we assume the correctness of some leading principles of confirmation, we may subsequently have to modify our leading principles, but the situation is analogous to the situation of a naturalized epistemology. These standards of knowledge are utilized by our sciences of nature. But the knowledge they provide can only be justified in terms of our standards of knowledge. The circuitous quality of the circle in this case is responsible for the lack of a feeling of paradox. In the case of inductive principles, these is also virtue in the circle. Only some rather general principles are assumed *ab initio* in order to confirm the logic of induction. We do not know whether these principles are exactly those that are to be confirmed or disconfirmed. In the strict case of vicious circularity, exactly the proposition that is to be correctly inferred must be assumed as a premise. As Black has shown in his defense of an inductive justification of an inductive rule, even when the rule to be inferred is itself employed as a rule of inference, the circularity is not of this strict kind. Much less so is the circularity of confirmation in the case I am discussing in this paper. Thus can we investigate empirically the adequacy of logical theories of induction.

We do not know whether the use of psychological data will advance the choice of a theory. But we know that the systematic collection of empirical data has frequently been of value to indicate some possible modes of inquiry. Yet two caveats ought to be acknowledged.

First, it should be stated that what we are attempting to confirm or disconfirm are normative theories of inductive inference. Some writers on this subject have claimed that inductive logic represent innate human competencies and that, under determinate conditions, performances can be lacking in competencies. Such a view is surely one to be established or rejected empirically, but it is not the question at issue here. Of course, if this theory is eventually accepted, then our views about the normative interpretation of logic will be affected. But we need not speculate on these matters now.

Second, the evidence elicited by psychological investigation consists

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of individuals providing an inquirer with reports of their particular intuitions regarding inductively established beliefs or inferences. In a very well-known investigation conducted by Amos Tversky and Daniel Kahneman, persons were given the following information:[7]

A cab was involved in a hit and run accident at night. Two cab companies, the green and the blue, operate in the city. You are given the following data:

(A) 85% of the cabs in the city are green, and 15% are blue.

(B) A witness identified the cab as blue. The court tested the reliability of the witness under the same circumstances that existed on the night of the accident and concluded that the witness correctly identified each one of the colors 80% of the time, and failed 20% of the time.

Then the subjects of the experiment are asked: "What is the probability that the cab involved in the accident was blue rather than green?"

A Bayesian analysis would sanction the particular inference,

(1) $P(B/W) = 0.41$.

Persons' intuition differed, however. The median and mode intuition was,

(2) $P(B/W) = 0.8$.

What psychologists are investigating, of course, are persons' particular intuitions. These may be compared with the judgments mandated by a system of logic, or even with the general intuitions that form the basis for that system.

The kind of experiment that I have described is only one of a series, dating back over thirty years, in which psychologists have "tested" persons' rationality by subjecting them to experiments. Ward Edwards, Amos Tversky, and Daniel Kahneman are perhaps the best-known of this group. Edwards confirmed that people were conservative Bayesian; Tversky and Kahneman revised this finding to show that people are not Bayesians at all. To Cohen this data is seen as relevant to the judgment of rationality. But there is another way to view these investigations, and that is to see these systematic studies of intuition as confirming or disconfirming theories of induction. Thus we may examine the experiment performed by Tversky and Kahneman. It seems to

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refute naive Bayesianism. But does it support any alternative theory? As Tversky and Kahneman show, when (A) is replaced by the following: (A′) Although the two companies are roughly equal in size, 85% of the cab accidents in the city involve green cabs and 15% involve blue cabs, they report "The answers to this problem were highly variable, but the base rate was no longer ignored. The median answer was 0.60 which lies between the reliability of the witness (0.80) and the correct answer (0.41)".

Since (A′) involves a notion of chance, it seems that here is some evidence for a system of inductive logic that incorporates a notion of chance (like Levi's system) rather than one that makes no use of such a notion (like Kyburg's system). This is not positive proof that a theory incorporating a conception of chance must be chosen. But it suggests the relevance of psychological data to the choice of theory. Data not only can but should be used to test the comparative empirical adequacy of competing logical theories. The experiments that have been performed, therefore, offer the beginnings of a research program that to my knowledge has not yet been carried out. This research program may be successful or it may fail, for it may be impossible to differentiate between logical theories in terms of their empirical adequacy.

I have made the claim that there are competing theories of inductive logic, and I wish to say more about this claim at this time. It seems to me that there are at least two ways in which such theories can be compared:

(A) In some cases theories license conflicting particular judgments. A case of this sort will subsequently be discussed.

(B) In most cases, fundamentally different conceptions of the inductive process are made, at least by different classes of theories. Thus, among the fundamental ways in which inductive theories differ are the following:

(1) As to whether associated with each person is a corpus of beliefs into which are accepted other beliefs in accordance with rules of acceptance that are part of the logic;

(2) As to the correct measure of inductive support;

(3) As to the assumption of chance.

There may be other factors as well. A three-fold table of theories

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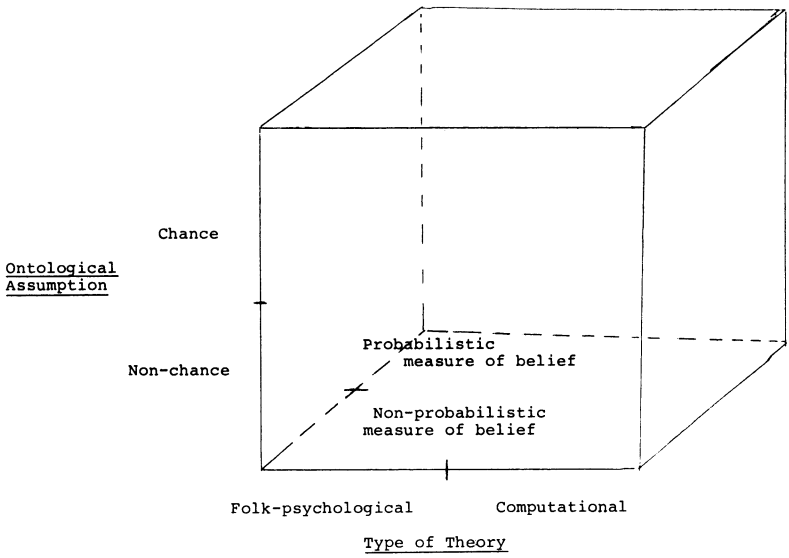

Fig. 1.

classified as to these three dimensions looks something like that shown in Figure 1.

If we call these notions the elements of a conceptual scheme, then inductive theories have different conceptual schemes.

(I) The present situation in the field of inductive investigations is that a number of alternative theories have been proposed and compete for centrality. They are not notational variations of one another. And, at least for certain classes of problems, they warrant differing inferences. Disputes sometimes have been settled when:

(1) A contradiction is discovered in a system and the system is revised or is repaired (e.g., a contradiction in Kyburg's system pointed out by Schick led to some ad hoc repairs by Kyburg).

(2) Fundamental differing intuitions are compared. Levi thinks that a principle of confirmational conditionalization, as he calls it, is fundamental to any system of inductive logic. Kyburg disagrees. But no resolution is in sight.

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An alternative methodology is called for. In effect I call for the replacement of expert intuitions by both expert and inexpert ones. We should assume that, if rationality is a universal human ability, then everyone (excluding infants, the mentally deficient, and the senile) should serve as the population to sample.

If we consider the kinds of ways that inductive theories differ from one another, we have some ways in which to test their empirical adequacy:

(A) We may attempt to find conflicting predictions made by different theories and determine whether or not, in any systematic way, persons agree with the one prediction or its conflicting alternative.

(B) In the absence of direct conflict in predictions based upon diverse theories, the systematic study of person intuitions about conceptual frameworks may support one theory better than another.

As an example that illustrates my approach, let me suggest the following test between Levi and Kyburg's systems: Levi claims that probability estimates are interval estimates. One can estimate the probability of an event as being in the interval [0.4, 0.6]. The limit case is 0.1. (Kyburg also accepts an interval conception of probability.) Both of them also consider a principle of direct inference essential to an inductive logic, i.e., a rule that assigns a degree of belief (an epistemic probability) to a proposition that predicates of an individual a certain property, when some systematic knowledge about all like individuals is known. But while Levi requires a statement of chance as part of an argument correctly employing a rule of direct inference Kyburg does not. Levi asserts that in the following case:

(1) 90% of Swedes are Protestants.

(2) Either 85% of Swedish residents of Malmo or 91% or 95% are Protestants.

(3) Peterson is a Swedish resident of Malmo.

By direct inference.

Levi: Degree of belief (Peterson is a Protestant) = 0.1; but for Kyburg this is the case.

Kyburg: Degree of belief (Peterson is a Protestant) = 0.90.[8]

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No statement regarding chance has been made. Yet conflicting inferences are warranted by these two theories. No one claims that results in favor of either theory definitively settle the matter of choice. But surely it would be relevant evidence to discover that people made point estimation judgments, of say, 0.9 in this case. More conflicts of this kind need to be investigated. In addition, intuitions about conflicting conceptual schemes should be studied. Indeed, there has already been some unsystematic study of this in the Kahneman and Tversky experiment I have described. Other experiments by Kahneman and Tversky, moreover, show that the conjunction rule for independent probabilities is not adhered to. This may shed light upon the question of a proper metric for inductive support. I believe that this path offers much hope for understanding and testing systems of inductive logic.

NOTES

[1] L. J. Cohen (1981).
[2] Rawls (1972) and Goodman (1985).
[3] Jeffrey (1983) vs. Shaefer (1976).
[4] Levi (1984) vs. Jeffrey (1983).
[5] For example, Levi (1980) vs. Kyburg (1961)..
[6] Eells (1982).
[7] Kahneman and Tversky (1982).
[8] Levi (1984).

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