Informal Logic
VII.1, Winter 1985

More on Induction and Possible Worlds: Replies to Thomas and Kahane

JOHN NOLT *University of Tennessee*

Howard Kahane and Stephen N. Thomas have raised clear and thoughtful objections to my method of evaluating informal arguments by an intuitive possible worlds semantics. [1] I would like to respond to these objections.

Kahane argues that the possible worlds test is inaccurate with respect to inductive generalization. He considers the following example:

All crows examined so far have been black.
∴All crows whatsoever are black.

"Using Nolt's possible worlds method," writes Kahane, "we conclude that this inference is quite weak, since its conclusion is false in a large percentage of the logically possible worlds in which its premise is true."

True enough. The same point can be made with respect to induction by enumeration (for example, by changing the conclusion of Kahane's example to 'If another crow is examined, it will be black'.) There are too many logical possibilities here; the next crow may be white, green, blue, colorless, etc. Thus induction by enumeration is also weak by the possible worlds test, though not as weak as inductive generalization.

Now Kahane regards inductive generalization (and presumably also induction by enumeration) as inductively strong and hence concludes that the possible worlds test in inaccurate. I disagree. There is no purely logical reason why either form of induction should be regarded as strong. That's what Hume taught us, and the possible worlds method simply illustrates Hume's insight.

In fact, if we lived in a relatively chaotic world, these forms of induction would be wholly unreliable. Only if we assume or presuppose that the world is or is likely to be relatively orderly can we judge either form of induction to be strong. [2]

This assumption or presupposition is generally called the *principle of the uniformity of nature* or *principle of induction*. Its role in induction is a matter of much dispute. Leaving aside the Popperian view, which rejects both inductive generalization and induction by enumeration, there are essentially two positions one can take. The first is to regard some version of the principle of the uniformity of nature as an implicit assumption of inductive reasoning. The second is to count more orderly possible worlds more heavily and thus treat them as more probable. The first method was favored by many older logicians, including (to name two among many) John Maynard Keynes [3] and (at least one point in his career) Bertrand Russell. [4] The second method has been advocated by Rudolf Carnap and his followers, who generally construe possible worlds as state descriptions. [5] Debate continues over whether either method is justifiable, and if so how.

Now how should the logic teacher deal with this admittedly rather embarrassing situation? One certainly cannot show students that inductive generalization and induction by enumeration are strong, and it is hardly illuminating simply to assert dogmatically that they are. That makes perceptive students uneasy. The best way, I think, is frankly to admit Hume's problem and to discuss the role of the principle of the uniformity of nature in induction. It does no harm to admit that this role is a matter of controversy.

The method of possible worlds provides a useful framework in which to do this, as I tried to show in sections 6.5-6.9 of my text, *Informal Logic: Possible Worlds and Imagination*. [6] There I adopted (largely for pedogogical simplicity) the older method of treating the principle of the uniformity of nature as an implicit assumption. (There are also, I believe, sound theoretical reasons for favoring this approach, but this is not the place to discuss them.)

Thomas raises an objection quite similar to Kahane's. In section III of his article he considers this argument:

> There were 50 balls in the urn.
> The first 49, all drawn at random, have been blue.
> ∴The remaining ball is blue.

Thomas observes rightly that by the possible worlds test this argument is weak. But, he says, "If one calculates the probability that the remaining ball is blue given that the first forty-nine drawn at random have been blue, the probability is well in excess of 80%."

This claim seems plausible, but in fact there is no way to calculate such a probability from the information Thomas gives. If in addition we are given the number of blue balls in the urn at the outset, then this probability is easy to determine: if there were 49 blue balls, the probability that the remaining one is blue is zero; if there were 50, it is one. But if we do not know the number of blue balls in the urn initially and we make only the assumptions that Thomas gives, then no calculation will yield the probability that the remaining ball is blue. We can, without violating any mathematical law, assign that proposition any probability we like.

In essence, Thomas' example is another case of inductive enumeration. We intuitively feel that the probability of the 50th ball being blue ought to be high, given the premises; but this feeling is based on what Hume would have called our "habit" of extrapolating constant conjunction. It is based, in other words, on our implicit assumption of the uniformity or likely uniformity of the urn's contents, not on any mathematical principle.

I think it is illuminating to notice this assumption, which is just what the possible worlds test helps us to do. Having noticed it, we may try to state it explicitly and add it to the argument. If we succeed, the conclusion may well be true in most of the worlds in which the premises (including this uniformity assumption) are true, so that the inference may legitimately count as strong. (But then, of course, Humean sceptics will doubt the uniformity assumption.) If we do not make such an assumption, however, then we have no basis for thinking that the color of the first 49 balls is in any way relevant to the color of the 50th. Since there are many possible colors other than blue, the inference is genuinely weak.

I have thus far neglected Thomas's stipulation that the balls were selected at random. Doesn't this affect the case? No, in fact it doesn't. To say that objects are selected at random is to say that they are selected by a process which insures that each member of the population to which it is applied has an equal chance of being chosen. Thus if we randomly sample 49 of a population of 50 objects, for each of these objects the probability that it is chosen is 49/50 and the probability that it is not chosen is 1/50. Hence if there is a non-blue ball, it is unlikely (probability

1/50) that it would not be drawn. But it does not follow that it is probable, given that 49 blue balls have been drawn, that the remaining ball is blue. For it is equally unlikely of any particular blue ball (if there are 50 blue ones) that it should be the last in the urn. Thus regardless of whether the remaining ball is blue, it is equally unlikely to have been the one remaining in the urn. The stipulation of randomness favors neither the conclusion that the remaining ball is blue nor the conclusion that it is not. It simply has no effect. Thus in the absence of any uniformity assumption, Thomas's inference is indeed quite weak, as the possible worlds test indicates. [7]

Thomas raises a quite different set of objections in sections IV and V of his paper. He correctly points out that by the possible worlds test the following argument should be judged weak, but not nil, since its conclusion is true in some, though not many, of the worlds in which its premises are true:

(A) Some roses are red.
Some violets are blue.
∴ Buddy still loves Peggy Sue.

On Thomas's view, this argument deserves a "nil" rating.

He notes further (again correctly) that the possible worlds test rates the following argument as strong, since its conclusion is true in most of the worlds in which its premises are true:

(B) Some roses are red.
Some violets are blue.
∴ Buddy does not still love Peggy Sue.

Thomas holds that this argument is not strong.

Finally, Thomas criticizes the general principle (which he thinks my view entails) that if a set of premises gives n% support to a conclusion, then the same premises give (100 - n)% support to the conclusion's negation.

In fact, my view entails this principle, only if by 'support' one means inductive probability. Inductive probability is a kind of conditional probability, the probability of a conclusion, given a set of premises. It is inductive probability which the possible worlds test is designed to estimate.[8] And it is obvious, I think, that the probability of the conclusion of (A), given its premises, is not nil, though it is not very high. That the inductive probability of (B) is high is perhaps not obvious initially; we shall return to (B) shortly. In any case, it is a consequence of the probability calculus, not merely of my view, that (where probabilities are expressed as percentages) the probability of a statement C given a set of statements S is 100% minus the probability of not-C given S.

Thomas obviously means something other than inductive probability when he uses the term 'support,' since support in his sense does not obey this law. And I wholly agree; it seems to me that by 'support' we do commonly mean something other than inductive probability.

But what do we mean? The best answer I know is that support is a combination of at least two factors: inductive probability and relevance. The following arguments will serve to illustrate this claim:

(C) 99% of all men love Peggy Sue.
Buddy is a man.
∴ Buddy loves Peggy Sue.

(D) Some roses are red.
Some violets are blue.
∴ Peggy Sue does not have exactly 7,127,368 teeth.

(E) All violets are colored.
∴ All violets are blue.

(F) Some roses are red.
Some violets are blue.
∴ Everyone loves Peggy Sue.

Arguments (C) and (D) have high inductive probabilities and would therefore score high on the possible worlds test. But the reason why the conclusion of (D) is highly probable, given (D)'s premises, is that it is highly probable in itself. It is such a weak statement that it contains virtually no information; hence it is likely to be true virtually regardless of what we assume. Arguments (E) and (F), by contrast, have very low inductive probabilities.

The premises of (C) and (E) are relevant to their conclusions—the premises of (C) perhaps more so than the premise of (E). Those of (D) and (E) are wholly irrelevant. Thus it is clear that high inductive probability and relevance can each occur with or without the other.

Only in (C) do the premises strongly support the conclusion. It follows, then, that neither high relevance nor high inductive probability alone is sufficient for strong support. Yet both seem necessary. It is difficult to envision anything we would call strong support in the absence of either.

Are there other necessary conditions? I'm inclined to think not; it seems to me that strong support just is is high inductive probability together with high relevance. But there is no need to decide that issue here.

Both high inductive probability and high relevance are necessary for strong support, not only in inductive logic, but in deductive logic as well. In deductive logic, strong support requires validity, and valid deductive arguments have inductive probabilities of one (or 100% if we prefer percentages).[9] But not every valid deductive argument supports its conclusion, for support also requires relevance. An argument consisting of a tautologous conclusion inferred from irrelevant premises is valid but gives its conclusion no support.

Argument (D) and Thomas's (B) are instances of an analogous phenomenon in the realm of induction. The conclusion of (B), like the conclusion of (D), is inherently probable. Just as a tautology (an empty and therefore inherently certain statement) is deductively implied by any set of premises, so an inherently probable statement (one so weak as to be probable in the face of almost any evidence) is inductively implied by almost any set of premises.[10]

In summary, the term 'support' seems to designate a combination of at least two factors: inductive probability and relevance. The possible worlds test estimates only inductive probability. It is therefore an analogue for inductive logic of classical tests for deductive validity in that, like classical deductive logic, it disregards relevance. Thomas's method, by contrast, seems to be an analogue for inductive logic of relevance logic in that, like relevance logic, it requires relevance *in addition to* validity in the classical sense. (All inferences valid by relevance logic are also classically valid.)

I think it is useful to consider inductive probability and relevance separately (as classical deductive logicians have always done), because doing so permits more articulate analysis. Consider, for example, the following argument;

(G) My roommate says that the creation did not occur exactly as it is described in **Genesis**.
∴ The creation did not occur exactly as it is described in **Genesis**.

Like (B) and (D), this argument exhibits high inductive probability and low relevance. (Indeed, it would typically be classified as a fallacy of relevance: the fallacy of appeal to authority.) But it is worthwhile to note that even though the argument is fallacious in this sense,[11] its conclusion is still highly probable, given its premise. This is so not because a vast body of scientific evidence contradicts the *Genesis* account; that, of course, is true, but it is not at issue in this argument. (Undoubtedly it should be, but it's not.) Rather, the inductive probability of the argument is high because its conclusion is so very weak. The conclusion is true in any possible world in which creation varies one iota from the *Genesis* account.

On the standard informal fallacies approach, and on Thomas's method as well, one rejects arguments like (G) out of hand. They are fallacious or weak, and that's that. The possible worlds method invites us to think more deeply. In this case, it reveals that it is far more rational to accept the conclusion of (G) than to reject it, even though (G) is fallacious, and even if we (perversely) ignore the evidence of science. [12] That, I think, is a fact too little appreciated by creationists and logic teachers alike.

Use of the possible worlds test, which is insensitive to relevance, by no means entails the view that relevance is unimportant. Indeed, on my view, it is just as important for good reasoning as high inductive probability is.[13] The possible worlds test should be understood, not as a way to neglect relevance, but as a way to separate it from inductive probability for purposes of analysis.

Thomas's remaining objection (actually the one he states first) is perhaps the most formidable. It concerns the inference:

(H) The Earth has at least one moon.
∴ The Earth has more than one billion moons.

Thomas argues that since there are infinitely many numbers greater than one billion and only finitely many less than or equal to one billion, the conclusion is true in virtually all of the possible worlds in which the premise is true. Thus by the possible worlds test the argument is very strong (i.e., its inductive probability is very high). This, he thinks is a mistake.

Thomas's argument seems to rely on the assumption that there is exactly one possible world corresponding to each number of moons that the Earth might have—or at least there are equally many worlds for each number of moons that the Earth might have. I can see no good grounds for that assumption.

But I must also admit that it is far from clear to me that Thomas is wrong. Perhaps it *is* highly probable that the Earth has a billion moons, given that it has *at least* one. The idea loses some of its air of paradox when we reflect that 'at least one' means from one to infinity—not to mention the rather large number of transfinite possibilities!

How we settle this question depends on how we count worlds, i.e., on how we individuate them. I have no precise recommendation on how to do that. For most familiar forms of reasoning (as I tried to show in my book) the exact method of individuation does not matter. Any fairly natural method will do. In such cases we can get by well enough (and reach a high level of agreement) using only common intuitions. The possible worlds test was never intended, after all, as a precise way of measuring inductive probability—only as a heuristic tool for obtaining a rough estimate of it.

Issues such as the one raised by (H), however, cannot be settled without making technical choices. In such cases, the possible worlds test raises more questions than it answers, and thus it fails in its role as a heuristic tool. Fortunately, most of these failures occur in cases which, like (H), are artificially contrived and not of much practical interest. But the method also flounders in some fairly natural cases. It is conceivable that such failures could be remedied by sufficiently ingenious technical elaborations, but that would defeat the method's purpose. The point is to have something nontechnical that students can learn to apply quickly and effectively. Consequently, some of the method's flaws are, given its aims, apparently irreparable. That would be a good reason for rejecting it, if we had something that achieved the same goals more effectively. But I'm not convinced that we do.

Notes

[1] Howard Kahane, "John Nolt's Inductive Reasoning Test," and Stephen N. Thomas, "Degrees of Validity and Ratios of Conceivable Worlds," *Informal Logic*, vi, 3 (December 1984), 30-34. Both are responses to my "Possible Worlds and Imagination in Informal Logic," *Informal Logic*, vi, 2 (July 1984), 14-17. (Incidentally, I'd like to apologize to Thomas for my careless misspelling of his name.)

[2] For an excellent discussion of this point, see Brian Skyrms, *Choice and Chance: An Introduction to Inductive Logic*, 2nd ed., (Encino and Belmont, Calif., Dickenson Publishing Co., 1975), Ch. II.

[3] *A Treatise on Probability* (London, Macmillan and Co., 1921), Part III.

[4] *The Problems of Philosophy* (London, Oxford University Press, 1959), Ch. VI.

[5] Rudolf Carnap, *The Continuum of Inductive Methods*, (Chicago, University of Chicago Press, 1951).

[6] (New York, McGraw-Hill Book Co., 1984).

[7] It is, of course, not always true that randomness has no effect. For a discussion of arguments in which randomness does play a crucial role, see section 6.4 of my *Informal Logic, ibid.*

[8] In the paper which Thomas is criticizing and in my book, I use the term 'strength of reasoning' as a synonym for 'inductive probability'. This may well have been confusing to many readers. In both works I avoided the term 'support'.

[9] Arguments with contradictory premises may be an exception. On some theories of inductive probability, they have no inductive probability.

[10] For further discussion of this point, see section 6.3 of *Informal Logic, ibid.*

[11] In *Informal Logic* I used the term 'fallacious' in a narrower sense; there it meant having low inductive probability.

[12] The matter would be otherwise, of course, if we had strong evidence in favour of the *Genesis* account. But the claim that the creation took place *exactly* according to this account is so strong that any evidence that rendered it probable would have to be extremely strong indeed.

[13] This is a view which has developed since the publication of *Informal Logic*. I now think that I devoted far too little of that book to relevance, a fault which I hope to rectify if there is a second edition.

Dr. John E. Nolt, Philosophy Department, 801 McClung Tower, University of Tennessee, Knoxville, TN 37996-0480. □