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RATIONALITY AND TRUTH

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I

Surely an aim of science is the discovery of the truth. Truth may not be the *sole* aim, as Popper and others have so clearly pointed out, but surely it is *an* aim of our hypothesizing.

Now, clearly, questions of rationality of method are questions about the suitability of means toward accomplishing the ends toward which they are directed. A method is rational if it is suitable (or at least as suitable as any other method, or something like that) for accomplishing the ends to which it is put.

So, if our ends include the truth, surely a method would be rational only if it guaranteed that the theories we accepted were true. Such, of course, is the criterion of rationality of rationalism.

But most of us aren't rationalists. Why not? Because we are convinced that were we to restrict our beliefs to those of whose truth we could be certain, our class of believed hypotheses would be very small indeed; indeed, most of us would say, null.

Now we could accept the consequences of this in the skeptical way. We could, that is, take certainty of truth as requisite for rationality and agree that we have few rational beliefs indeed, and that the bulk of what we had believed all along simply was not rational belief on our part. But most of us will not go this route. Rather we will lower our standards of rationality.

Instead of demanding that what we believe be certain in order that our belief be rational, we might, for example, move in the direction of taking ourselves to be rational if what we believe is certified by our methods to be merely probably true, or likely to be true, or if our methods lead us to beliefs which we may take, in Goldstick's terms as 'apt to be true'.[1]

But we must be careful here. For 'probable truth' or 'likely truth' are not, of course, kinds of truth. Indeed, the connection between being 'probably true and being true, *simpliciter*, is a hard one to fathom.

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Things would be simpler if we relied only on the *a priori* truth of hypotheses. But we do not. Rather we are concerned with the probability of hypotheses relative to our evidence base. Two theorizers with differing evidential bases may quite rightly assign differing probabilities to hypotheses. But, of course, truth, as opposed to probable truth, just doesn't work that way at all.

Indeed, on reflection it becomes difficult to see what we mean by 'probably true' or 'likely to be true' or 'apt to be true' at all, unless we mean by it 'worthy of reasonable belief on the evidence'. And if that is the best analysis we can provide, then what insight have we obtained when we aver that one should rationally believe only those hypotheses which are 'apt to be true' or are 'probably true'?

II

And yet we do sometimes feel that we can distinguish the aim of obtaining true hypotheses from those other aims which characterize our scientific endeavor (simplicity of beliefs? stability of belief? communicability and learnability of theories?). And, perhaps, we can distinguish those elements in our canon of rationality which are there because they are 'marks of truth' of hypotheses from those which are there for other reasons. Let us suppose we can do this. And let us suppose that we can assimilate the former elements into a single probability function which assigns to a hypothesis a real number from zero to one relative to a body of evidence. Under these circumstances is it really the case that a principle like methodological conservatism must be immediately ruled out of court?

Here is an argument that might, initially, make one think so: Hypotheses have probabilities relative to the evidence. Surely we should not believe a hypothesis unless its probability is greater than one-half; that is, unless it is 'more likely to be true than not'. But one couldn't have two incompatible hypotheses both with probabilities greater than one-half. So either there is only one of a set of mutually incompatible hypotheses worthy of belief or none are. In any case there is never *more* than one worthy of belief. So a rule of conservatism, or any other rule designed to select between hypotheses otherwise equally worthy of belief, is never an element of a canon of rational belief.

But, even assuming that we can assign probabilities to hypotheses relative to the evidence, do we *ever* have the probability of a scientific theory greater than one-half? Many have thought not. Consider the problem of getting

universal generalizations (a crucial component of scientific hypotheses) to have a probability greater than zero in a world of (apparently) infinite individuals and unlimited variety. Consider the inductive evidence as to the falsity of our previously preciously and confidently held beliefs. Consider the problem of 'metaphysical' alternatives which are apparently incompatible and yet mutually compatible with all possible evidential data. And, finally, and perhaps most importantly, consider in any given decision which involves choosing a hypothesis to believe, the infinite wealth of alternative hypotheses, which would explain the data to date as well as any we at present have thought up, but which haven't yet even been imagined by the scientific community.

Now we could adopt a 'limited rationalism' which insists that some realistic scientific hypotheses just are more probable than not on the basis of the evidence. Or we could opt for a skepticism which tells us that hypotheses just never are worthy of belief. Or we could, again, lower our standards. We could maintain that even when the probability of a hypothesis is less than one-half there may still be good reason to believe it.

But if we adopt this last stance we may be faced with incompatible hypotheses of equally high probability, one of which we deem more worthy of belief than the other. We deem this to be the case on grounds other than a differential possession of the 'marks of truth' of the hypotheses, of course. We might do so on the grounds that while truth is an aim of science it isn't the only aim. The other aims give us different rules, rules for choice over and above those based on the possession of 'probability' by the hypothesis. A rule of simplicity might be such a canon for belief. Or, we might do so on purely methodological grounds. We might, for example, deem it appropriate to be as stable in our beliefs as we can be without doing injustice to new hypotheses on other grounds. That is, we might be methodological conservatives.

But can we really be said to rationally believe hypotheses of which we cannot affirm that it is 'more likely than not that they are true'? And which we choose merely because they are simple? Or because they were earlier chosen and we don't want to give them up without good reason? There are a number of options we could take here.

We might try to retrench in the hypotheses we consider. If none of the ones we have been considering is 'more probable than not' perhaps there are other, weaker, ones which are 'more probable than not'. If I am far from certain, for example, that general relativity is correct, am I not at least nearly

certain that some theory which allows for a range of consequences centered around the predictions of general relativity is correct? And isn't it this latter, weaker, theory the one I should believe? But am I ever nearly certain that even such weakened theories are correct?

Another thing we could do is to say that we *accept* scientific hypotheses, but we don't believe them. We 'tentatively take them as best to date' but we don't really believe them to say what is the case. But can we really draw this distinction in such a way that any role whatever is left for 'genuine belief' as opposed to 'mere acceptance'? Or has belief then become something we are never justified in having, and, hence, a mere detached and inoperative part of our conceptual machinery? Aren't we just trying to have our skeptical cake and eat it too?

Finally we could just lower our standards for belief, allowing that we do and should, believe hypotheses on grounds which we can't appropriately identity with those which may properly be called 'marks of truth'. Is such a position patently absurd?

Of course such a position has its own difficulties which will need to be met. The earlier requirement of high probability had the danger that we might always be led by our criteria to withhold belief. The new more permissive rules, if they are not suitably constrained, might lead us never to withhold belief. But there are, of course, times when none of the available hypotheses seems suitable to us.

III

Goldstick offers a moral analogy to convince us of the irrationality of conservatism. Suppose two actors act differently and incompatibly, all morally relevant features of their situation being the same. Suppose someone now deems each to have acted justifiably – and judges that they each would have been unjustified had they acted otherwise – on the basis that the first did what he did and the second did what he did. Goldstick appeals to the reader's intuition that such a method of evaluation is "excluded by the very concept of what it is to be justified".

Perhaps so. But is the analogy fair to conservatism? Or is this analogy more like it: For whatever reasons two actors have, all along, been acting in different, incompatible ways. Nothing in their situation distinguishes them in a morally relevant way. Someone now deems it morally right for each to

continue to act as he always has, on the basis of the reasoning that there is nothing in either's moral environment which militates a change, and under those circumstances continuing to do what you have been doing seems the best course. Does that offend moral intuition? And isn't conservatism a doctrine about continuing to do what you have been doing (continuing to hold a belief already in your corpus of beliefs) and not a thesis about initiating some new action (adding a new belief to your corpus of beliefs)? Conservatism is not 'permissivism', and it is toward a (not plausible) version of the latter that Goldstick's analogy is directed.

Goldstick describes the paper he is replying to as a defense of conservatism. It isn't really. It explores one kind of conservatism. It finds it not *logically* absurd. But it doesn't find it very important. It briefly mentions another doctrine which might be called conservative, but finds the doctrine obscure. Now the one kind of conservatism, the kind Goldstick was discussing in his original paper and the kind first discussed in the paper to which he is replying in this journal, may be absurd. It may conflict with some intuitions so strongly nothing we could say in its defense would help make it more plausible. "Surely", some will say, "a ground for belief which is not a ground for taking something to be true is no ground for belief at all". And they may claim that this is, if not a purely logical truth, at least an 'analytical' truth given the meaning of 'belief'.

But we do talk about scientists believing hypotheses. And a description of how they do behave, even the best of them, may show us that they are fixing their beliefs by means of rules like conservatism (or simplicity, or probability resting upon purely subjective *a priori* probability assignments) which, on inspection, seem to have no warrant in terms of 'aptness toward truth'. We could say that they don't really believe any hypotheses at all. Or we could say that they are irrational. But is it clear that we shouldn't say that the criteria of rationality in belief may outrun those which 'aim at the truth' in the narrow sense?

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NOTE

[1] See Daniel Goldstick, 'More on Methodological Conservatism', *Philosophical Studies*, this issue, pp. 193–195.