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## VI\*—INDUCTION, EXPLANATION AND NATURAL NECESSITY

by John Foster

I want to examine a possible solution to the problem of induction—one which, as far as I know, has not been discussed elsewhere. The solution makes crucial use of the notion of objective natural necessity. For the purposes of this discussion, I shall assume that this notion is coherent. I am aware that this assumption is controversial, but I do not have space to examine the issue here.

### I

Ayer is one philosopher who denies that the notion is coherent. But he also claims that even if it were, it would not help in meeting the problem of induction. 'If on the basis of the fact that all the A's hitherto observed have been B's we are seeking for an assurance that the next A we come upon will be a B, the knowledge, if we could have it, that all A's are B's would be quite sufficient; to strengthen the premise by saying that they not only are but must be B's adds nothing to the validity of the inference. The only way in which this move could be helpful would be if it were somehow easier to discover that all A's must be B's than that they merely were so.'<sup>1</sup> And this, Ayer thinks, is clearly impossible. 'It must be easier to discover, or at least find some good reason for believing, that such and such an association of properties always does obtain, than that it must obtain; for it requires less for the evidence to establish.'<sup>2</sup>

Despite its initial plausibility, Ayer's reasoning is fallacious. The first point to notice is that there is a form of empirical inference which is rational, but not inductive in the relevant sense. In the relevant sense, we make an inductive inference when, from our knowledge that all the examined *As* are *Bs*, we infer that all *As* are *Bs* or that some particular unexamined *A* is

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*B*. In such cases the inductive inference is just an extrapolation from the evidence—an extension to all or some of the unexamined cases of what we have found to hold for the examined cases. Not all rational empirical inferences are of this kind. Thus consider the way in which chemists have established that water is  $H_2O$ . No doubt there is a step of extrapolative induction, from the chemical composition of the samples examined to the composition of water in general.<sup>3</sup> But this is not the only step of inference. For the composition of the samples is not directly observed: it is detected by inference from how the samples respond to certain tests. The rationale for such inference is the explanatory power of the conclusion it yields. The conclusion is accepted because it best explains the experimental findings—at least it does so in the framework of a more comprehensive chemical theory which is itself accepted largely on explanatory grounds. Thus the conclusion is reached not by extrapolation, but by an inference to the best explanation.

Now look again at Ayer's argument. Ayer is assuming that since 'All *As* must be *Bs*' makes a stronger claim than 'All *As* are *Bs*', it is no good, in the face of the sceptic's challenge, trying to justify an acceptance of the second via an inference to the first. Now if extrapolative induction is the only form of inference available, Ayer is clearly right. An extrapolation to the stronger conclusion (which associates *A* and *B* across all nomologically possible worlds) already includes an extrapolation to the weaker (which associates *A* and *B* in the actual world) and hence cannot serve to mediate it: any sceptical objection to the smaller extrapolation is automatically an objection to the larger. But suppose we could reach the stronger conclusion by an inference to the best explanation. This would allow an inference to the stronger conclusion to be what justifies an acceptance of the weaker. For it might be precisely because the stronger conclusion is stronger that it has the explanatory power required to make it worthy of acceptance, and thus precisely because we are justified in accepting the stronger conclusion on explanatory grounds that we are justified in accepting the weaker conclusion it entails. This is the possibility which Ayer has missed and which I want to examine in the subsequent sections.

## II

Let us focus on a particular case. Hitherto (or so I shall assume), as far as our observations reveal, bodies have always behaved gravitationally—and here I use ‘gravitational behaviour’ to cover all the various kinds of behaviour, such as stones falling and planets following elliptical orbits, which are normally taken as manifestations of gravitational force. On this basis we are confident that bodies will continue to behave gravitationally in future. But is such confidence well-founded? Does the past regularity afford rational grounds for expecting its future continuation? Here is what strikes me as a natural response. The past consistency of gravitational behaviour calls for some explanation. For given the infinite variety of ways in which bodies might have behaved non-gravitationally and, more importantly, the innumerable occasions on which some form of non-gravitational behaviour might have occurred and been detected, the consistency would be an astonishing coincidence if it were merely accidental—so astonishing as to make the accident-hypothesis quite literally incredible. But if the past consistency calls for some explanation, what is that explanation to be? Surely it must be that gravitational behaviour is the product of natural necessity: bodies have hitherto always behaved gravitationally because it is a law of nature that bodies behave in that way. But if we are justified in postulating a law of gravity to explain the past consistency, then we are justified, to at least the same degree, in expecting gravitational behaviour in future. For the claim that bodies have to behave gravitationally entails the weaker claim that they always do. Consequently, our confidence that bodies will continue to behave in this way is well-founded. The past regularity does indeed, by means of an explanatory inference, afford rational grounds for expecting its future continuation.

This is just one case. But it illustrates what is arguably a quite general solution to the problem of induction—a solution which is summarized by the following three claims:

- (1) The only primitive rational form of empirical inference is inference to the best explanation.

(2) When rational, an extrapolative inference can be justified by being recast as the product of two further steps of inference, neither of which is, as such, extrapolative. The first step is an inference to the best explanation—an explanation of the past regularity whose extrapolation is at issue. The second is a deduction from this explanation that the regularity will continue or that it will do so subject to the continued obtaining of certain conditions.

(3) A crucial part of the inferred explanation, and sometimes the whole of it, is the postulation of certain laws of nature—laws which are not mere generalizations of fact, but forms of (objective) natural necessity.

How this solution works out in detail will, of course, vary from case to case. Sometimes the nomological postulates will form the whole explanation (when the past regularity is a consequence of the laws alone) and sometimes only part (when the past regularity is only a consequence of the laws together with the obtaining of certain specific conditions). Sometimes the predictive conclusion deduced from the explanation will be categorical ('the regularity will continue') and sometimes hypothetical ('the regularity will continue if such and such conditions continue to obtain'). And most importantly, in any particular case, our choice of the *best* explanation will depend to a large extent on what other explanatory theories we have already established or have good reason to accept.

To gain a better understanding of the proposed solution—let us call it the *nomological-explanatory solution* (NES)—three points must now be noted.

(i) Some philosophers hold, contrary to claim (1), that extrapolative induction is a primitive form of rational inference (i.e. *inherently* rational) and that consequently any attempt to justify it by its reduction to other forms of inference is misconceived. I cannot accept this view. Suppose (perhaps *per impossibile*) we knew that there were no laws or other kinds of objective constraint governing the motions of bodies and thus had to interpret the past consistency of gravitational behaviour as purely accidental. Such knowledge is logically compatible with the belief that the regularity will continue. But clearly we

would have no grounds for thinking that it will. For in knowing that there were no constraints, we would know that on any future occasion any form of behaviour was as objectively likely as any other, and this would deprive the past consistency of any predictive value. This result is in line with NES, since the envisaged knowledge explicitly blocks the explanatory inference: if we know there are no laws, we cannot offer a nomological explanation of the past regularity. But how can the result be explained by those who hold induction to be inherently rational? Why should the envisaged knowledge undermine the extrapolative inference unless the rationality of that inference depends on some further inference with whose conclusion the knowledge logically conflicts? I can see no answer to this.

(ii) As indicated in (3), the postulated laws are forms of objective natural necessity. This is crucial. If the laws were mere factual generalizations, or such generalizations set in the perspective of some attitude we have towards them,<sup>4</sup> they would not be explanatory in the relevant sense. In particular, their postulation could not be justified by an inference of a non-extrapolative kind. Thus suppose we construed the law of gravity as merely the fact that bodies always behave gravitationally. There is, I suppose, a sense in which the postulation of this 'law' might be taken to explain the past consistency of gravitational behaviour—the sense in which to explain a fact is to subsume it under something more general. But it cannot be this sort of explanation which is involved in NES. For if it were, the inference to it would be an ordinary step of extrapolative induction and hence vulnerable to the sceptic's attack. In subsuming the past regularity under a universal regularity we would not be diminishing its coincidental character, but merely extending the scope of the coincidence to cover a larger domain. And it is just this kind of extension which the sceptic calls in question. The reason we can hope to do better with laws of a genuinely necessitational kind is that, arguably, their postulation can be justified by reasoning of a quite different sort. Thus, arguably, we are justified in postulating a law of gravity, as a form of objective natural necessity, because it eliminates what would otherwise be an astonishing coincidence: it enables us to avoid the incredible hypothesis that the past consistency of gravitational behaviour, over such a vast range

of bodies, occasions and circumstances, is merely accidental.

(iii) It may be wondered whether past regularities really do call for explanation. Suppose I toss a coin 1000 times, randomizing the method and circumstances of the tossing from occasion to occasion, and each time it comes down heads. Let  $H$  be the hypothesis that the coin is unbiased, i.e. (in effect) that, for an arbitrary toss, its chances of heads and tails are equal. On the supposition of  $H$ , the antecedent probability of the run of heads was astronomically small:  $\frac{1}{2}^{1000}$ . But while astronomically small, it was no less than the antecedent probability of any other of the possible sequences of outcomes: for 1000 tosses, there are  $2^{1000}$  possible sequences and on the hypothesis of no bias each has the same probability. This may lead us to suppose that the occurrence of the run does not count as evidence against  $H$  and hence does not call for any explanation. For it seems that on the supposition of  $H$  we should be no more surprised at the run of heads than at any other sequence which might have occurred. In the same way we may be led to suppose that the past consistency of gravitational behaviour calls for no explanation—that on the supposition of no laws or constraints this consistency should seem, in retrospect, no more astonishing than any other determinate sequence of behavioural outcomes.

However, this reasoning is fallacious. Suppose I selected the coin at random from a bag of coins, knowing that half are unbiased and half are very strongly biased in favour of heads. Prior to the series of tosses, I could assign equal epistemic probabilities to  $H$  and to the alternative hypothesis ( $H'$ ) that the coin is heads-biased. If the reasoning above were sound, the subsequent run would not alter these probabilities: that is, even after the run I should have no more reason to accept  $H'$  than  $H$ , since on the supposition of  $H$  the antecedent probability of the run was no smaller than that of any other possible sequence. But this is clearly wrong. Obviously I have very strong grounds for accepting  $H'$ . If I were to make a habit of betting on  $H'$  in such circumstances, I could expect to win almost every time. For what matter here are not the relative antecedent probabilities of alternative sequences on the supposition of  $H$ , but the relative antecedent probabilities of the run on the alternative hypotheses. What makes  $H'$  overwhelmingly more credible given the evidence of the run is that, antecedently, the run was

overwhelmingly more probable on the supposition of  $H'$  than of  $H$ . Another relevant factor, of course, are the relative epistemic probabilities of  $H$  and  $H'$  prior to the evidence of the run. Had we set the initial probability of  $H$  higher than  $H'$ , this would have reduced the strength of the subsequent grounds for accepting  $H'$  on the evidence. But to make any practical difference, we would have had to set the initial probability of  $H'$  astronomically low, simply because of the extreme difference in the antecedent probabilities of the run on the two hypotheses.

Let us now apply these considerations to the gravitational case. One hypothesis ( $H_1$ ) is that, in the respects which concern us, the behaviour of bodies is not subject to any laws or constraints, so that any consistent pursuit of gravitational behaviour would be purely accidental. What makes the past consistency count so strongly against  $H_1$  is not just that its antecedent probability would be astronomically small on the supposition of  $H_1$  (for this would be true of each possible sequence of behavioural outcomes), but that there are alternative hypotheses on which this probability would be substantially higher and which do not, on the face of it, have a sufficiently lower initial probability to balance this difference. In particular, there is the hypothesis ( $H_2$ ) that it is a law of nature that bodies behave gravitationally. On this hypothesis, which has been proposed as the best explanation of the consistency, the consistency would be antecedently inevitable. As far as I can see, the only way in which one could rationally retain  $H_1$  in the face of the evidence would be by maintaining that the very notion of natural necessity is incoherent. This is an arguable position (though I think it is mistaken), but, as I said at the outset, I am discounting it for the purposes of the present discussion.

### III

NES is beginning to look very plausible. However, there are two major objections to it—in effect, two versions of a single objection. I shall consider one in this section and the other in the next.

The past consistency of gravitational behaviour would indeed be an astonishing coincidence if it were merely accidental. Let us agree, then, that we are justified in taking it to be the product



of natural necessity: bodies have always behaved gravitationally, within the scope of our observations, because they had to. But why should we suppose that this natural necessity holds constant over *all* bodies, *all* places and *all* times? Why should we suppose that there is a *universal* law of gravity rather than one which, while covering our data, is restricted in scope to some particular set of bodies or some particular portion of the space-time continuum? For example, with *t* as the present moment, consider the following three nomological hypotheses:

- (A) It is a law *for all times* that (alternatively,<sup>5</sup> it is a law that *at all times*) bodies behave gravitationally.
- (B) It is a law *for all times before t* that (alternatively,<sup>5</sup> it is a law that *at all times before t*) bodies behave gravitationally.
- (C) (B) and there is no more comprehensive gravitational law.

To justify our belief that bodies will continue to behave gravitationally in future, we have to justify an acceptance of (A) in preference to (C). But how can this be done by an explanatory inference? For both (A) and (C), by including (B), account for the gravitational regularity so far. It seems that to justify an acceptance of (A) we have to fall back on extrapolative induction, arguing that because gravitational behaviour has been necessary hitherto, it is likely to be necessary in future. But if so, we have not answered the sceptical problem. Nor, indeed, do we seem to have made any progress at all. For if we have to resort to induction at this point, we might just as well apply it directly to the past regularity without bringing in nomological explanation at all.

Is this objection decisive? Well it is certainly true that (B), and hence both (A) and (C), offer explanations, in the relevant sense, of the past regularity. But this alone is not enough to sustain the objection. What the objector must show is that, as explanations, (C) is not inferior to (A); or put another way, that (B) is not inferior to (A) as a terminus of explanation. And it is on this point, I think, that the defender of NES has a reasonable case. For it seems to me that a law whose scope is restricted to some

particular period is more mysterious, inherently more puzzling, than one which is temporally universal. Thus if someone were to propose (C), our response would be to ask why the fundamental law should be time-discriminatory in that way. Why should  $t$  have this unique significance in the structure of the universe that bodies are gravitationally constrained in the period up to  $t$  but not thereafter? Barring the postulation of a malicious demon, these questions are unanswerable: any answer we could receive would only serve to show that the fundamental laws were not as suggested—that there was a deeper explanation in terms of time-impartial laws and a difference, relevant to the operation of these laws, in the conditions which obtain in the two periods. It is because these questions seem pertinent and yet are *ex hypothesi* unanswerable that we are left feeling that, as hypothesized, nature is inherently puzzling and precludes an explanation of our empirical data which is both correct and, from the standpoint of our rational concerns, fully satisfactory. And it is for this reason that, presented with the data (the past gravitational consistency) and the alternatives (A) and (C), we are justified in preferring (A). We are justified in preferring (A) because it is the *better* explanation, and it is the better explanation because, unlike (C), it dispels one mystery without creating another: it dispels the mystery of past regularity without creating the mystery of capricious necessity. For the same reason we are justified in preferring (A) to other hypotheses of a similar kind to (C) such as those which restrict the scope of the gravitational law to some particular set of bodies or some particular region of space.

The objector might reply that I am guilty of double standards. I am claiming that in the case of behaviour we should avoid unexplained regularity, while in the case of necessity we should avoid unexplained caprice. What I hold to be problematic is, in the one case, a behavioural uniformity not explained by laws and, in the other, a variation in behavioural constraints not explained by a difference in the relevant conditions. But why should our expectations for behaviour and necessity be so strikingly different? If there is no problem in expecting irregular behaviour when there are no laws to forbid it, why should there be a problem in building a measure of irregularity into the laws themselves? Conversely, if it is reasonable to expect the laws to

be uniform over bodies, space and time, given no positive evidence against it, why should it not also be reasonable to expect uniformities of behaviour without the backing of laws? It seems that I am relying on opposite standards of rationality in the two cases.

Well in a sense I am. But that is just because the cases are quite different. What makes them quite different is that, unlike the concept of behaviour, the concept of necessity has some notion of generality built into it. Thus try to imagine a world in which there are no conspicuous uniformities, but in which for each object  $x$  and time  $y$  there is a separate law prescribing how  $x$  is to behave at  $y$ . In such a world everything that happens has to happen, by natural necessity, but there is no uniform system of necessity, or anything remotely resembling one, which imposes the same constraints on situations of the same kind. Each law is concerned with the behaviour of a unique object at a unique time. Now it seems to me that such a world is not possible, not because we cannot conceive of such randomness in behaviour, but because we cannot conceive of such singularity in the scope of the laws. And this is not just a trivial point about the meaning of the word 'law'—a point which we could avoid by choosing another term. Rather, we cannot make sense of the claim that it is naturally necessary for a particular object to behave in a certain way at a particular time except as a claim which is implicitly more general, concerning how it is naturally necessary for objects of a certain type to behave in situations of a certain kind. This is not to say that we cannot conceive of laws (i.e. natural necessities) which are to some degree restricted by some singular reference. We can, I think, conceive of the law postulated by (C), whose scope is restricted to a certain period. But this is only because the restriction leaves room for enough generality of scope for the notion of law to gain purchase. In itself a singular restriction is something which runs counter to the direction of nomological explanation. This is why we serve the purposes of explanation better, if there is a need for explanation at all, by postulating laws without such restrictions, if we can do so compatibly with our data. And in particular, this is why, given the past consistency of gravitational behaviour, we rightly regard (A) as a more satisfactory explanation than (C) or any other explanation of a similarly restricted kind, whether the

restriction is to a period, to a region or to a sample of bodies. None of these considerations which apply to our concept of natural necessity carry over to our concept of behaviour. There is no implicit notion of generality in our concept of an object's behaving in a certain way at a certain time. Indeed, our rational expectation is that without the backing of laws the total pattern of behaviour will be more or less random, not because there is anything to ensure this, but because there is nothing to ensure regularity and because, if it is left to chance, the probability of any significant regularity is exceedingly small. In short, there is something *a priori* perplexing about an arbitrary restriction in the laws and something *a priori* surprising about a coincidental regularity in behaviour.