The Status of Determinism

Jonathan Bennett

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Among the principles which Kant describes as 'regulative' is the determinist principle that every empirical state of affairs follows in accordance with causal laws from some earlier state of affairs. In calling this ' regulative', Kant is making a five-fold claim.

(1) The principle cannot be conclusively proved. Because the future is or may be endless, it is always possible that new states of affairs are going to come about in ways which do not accord with the principle.

(2) The principle cannot be conclusively disproved. Because logical space is limitless, it is always possible, however persistent our failure to find a causal explanation for a state of affairs, that there is an explanation which we have not yet found.

(3) Unlike some statements which can be neither settled nor ruled out by experience, the determinist principle is at least relevant to experience. Although we cannot reach the goal of showing it to be true, or that of showing it to be false, we can and do take small steps towards each goal. We move towards verifying the principle whenever we explain a state of affairs by means of a well-tested and unrefuted hypothesis; and we move towards falsifying it whenever we try but fail to find such an explanation. This sense of direction marks off the determinist principle sharply from such unverifiable and unfalsifiable statements as that all change results from the exercise of the divine will, or that there are intangible, invisible, and impotent goblins.

(4) Sense can therefore be given to the notion of assenting to the determinist principle. For it can be construed as a piece of advice; urging us always to move as far as possible in the direction of verifying the principle, and never to despair of moving further. Assent to the principle is just acceptance of this advice.

(5) The principle gives good advice to anyone who wishes to understand the empirical world, for it advises him to pursue such understanding optimistically and therefore energetically; furthermore, precisely because the determinist principle cannot be falsified, it constitutes good advice *with no risks attached*. Advising someone to assume there is an explanation for every state of affairs is not like advising to assume that there is gold in every river-bed. Acceptance of that advice might engender optimism and therefore industry, but it would also certainly lead to error, for some river-beds are known contain no gold.

I want now to hold in one hand Kant's treatment of the determinist principle as ' regulative', while holding in the other his claim that he has *proved* the so-called 'second analogy of experience' which says: 'All alterations take place in conformity with the law of the connection of cause and effect' (B 232). He makes it abundantly clear that he does not take this to mean merely that it is *safe and sensible* to say that all alterations take place. . . etc. The claim of the second analogy of experience is that to say there are uncaused events is to say something demonstrably false; and this is in conflict with the description of the determinist principle as ' regulative', for that description implies that to say there are uncaused events is to say something which is objectionable because it may induce despondency in the scientific enquirer, *but which cannot be proved to be false*.

In fact, Kant does not succeed in proving the second analogy of experience; but he does (in my view) succeed in proving the more limited conclusion that there could not be intellectually graspable experience which was not experience of a world which manifested a fairly high degree of causal order. (I cannot present his proof of this here: it includes, among much else, all that is valid in Wittgenstein's argument against the possibility of a private language.) This limited conclusion implies that there is no possibility of our finding that the world has become altogether resistant to science; but it leaves open the possibility that some causally inexplicable states of affairs should come about, and so it is consistent with claiming a purely regulative virtue for the statement that there is a causal explanation to be found every state of affairs.

There is, then, a double problem about Kant's handling of the second analogy. Why does he fail to see the gap between the weaker version which he does prove and the stronger version which he claims to have proved? And why does he fail to see that the claim to have proved the stronger version is inconsistent with the attribution, a few hundred pages further on in the *Critique*, of a regulative status to the determinist principle? This is a problem about Kant rather than about philosophy, but I hope to show that it shares with many other problems about Kant the merit that in discussing it we can learn something in philosophy. The real objection to the protective Kantian commentators whose main concern is to defend Kant from charges of error and inconsistency is that they take a real, human, confused, fertile philosopher from whom much can be learned and present him as a mythical, superhuman, infallible, sterile bore. For example, G. Bird (*Kant's Theory of Knowledge*, p. 71) has rebutted the accusation that Kant tells two different and mutually inconsistent stories about the status of the determinist principle, on the grounds that 'Kant 'takes the trouble to distinguish between' the determinist principle which he calls 'regulative' and that of the second analogy for which he claims a more than regulative status. This might be plausible if it were accompanied by an account of what these two principles are, and of how two principles, of which one is regulative and the other not, can both express. some sort of determinism. But Bird tells us nothing about this. Furthermore, his only justification for saying that Kant 'takes the trouble to distinguish' the determinism which is called regulative from the determinism of the second analogy which is not called regulative is that Kant says things about the one which are inconsistent with things he says about the other. But this can be taken as the making of a distinction only if one takes it as literally *axiomatic* that Kant never contradicts himself. I find this axiom unacceptable, but my main objection to this smoothing-over approach is that it teaches us nothing about what is true in philosophy.

I hope it will not prove too exasperating if I set about solving (and learning from) this problem about what Kant is up to by raising first the different but related problem of what he is up to when he says that there is a radical error in Hume's analysis of the concept of cause. Of a group of concepts which includes that of cause, Kant says (A 112):

> All attempts to derive these. . . concepts. . . from experience, and so to ascribe to them a merely empirical origin, are entirely vain and useless. I need not insist upon the fact that, for instance, the concept of a cause involves the character of necessity, which no experience can yield. Experience does indeed show that one appearance customarily follows upon another, but not that this sequence is necessary. . .

Kant denies that the concept of cause can be derived from experience because derivation requires thought, and one can think about one's experience only if one already has a use for the concept of cause. But his concern here is less with the Humean account of how we *acquire* the concept of cause than with the Humean *analysis* of this concept. Because 'the concept of cause involves the character of necessity, which no experience can yield', no analysis of the concept in purely experiential terms can be correct.

Some writers have tried to fault Hume's analysis by producing statements which, they say, Hume is committed to calling causal laws but which we know perfectly well not to be causal laws: every time the 5 o'clock whistle blows at Dagenham, the workers at Coventry their tools and go home. Such counter-examples may tell against specific empiricist analyses of causality, but they cannot show the wrongness of the empiricist *programme* for such an analysis. If we know perfectly well that this whistle's blowing does not cause those workers to down tools, the empiricist has only to ask *how* we know this, and to amend his analysis in the light of the answer. Kant makes mistakes, but he does not make the mistake of trying to show, by the production of empirical counter-examples to specific empiricist analyses, that no empiricist analysis can succeed. His position is accurately stated by Körner (*Kant*, p. 74): 'While Kant insists that the notion of causality is not *equivalent* to that of regular succession, he holds that unless it *implied* this notion it could not refer to anything in perception.' But although Kant thus seems to claim the true notion causal necessity to be narrower than Hume says it is, he rightly refrains from any attempt to prove or illustrate the over-generosity of Hume's concept by pointing to universal statements which Hume would have to call causal but which are obviously not so.

Just because Kant does not attack Hume in this misguided way, there a problem over what attack he does think he is launching. The solution to this problem is twofold. One part of it is well known to commentators; in respect of the other part—which alone is relevant to regulativeness—I claim a certain mild originality.

The first way in which Kant thinks that Hume cannot do justice to the necessity of causes brings us back to the part of the second analogy which Kant does succeed in proving. Hume presents us as discovering moment by moment that our world continues to be fairly orderly, and gives the impression that the onset of total chaos is a possibility which might at any moment be realised in our experience; Kant, on the other hand, knows that causal order is not dispensable to this extent. It is often said that the burden of Kant's complaint against Hume is just this: Kant is accusing Hume of implicitly denying that *causal laws are necessary* in the sense that *there must be (known) causal laws* if there is to be graspable experience at all. But this is different from saying that *causal laws are necessary* in the sense that *every causal law expresses or involves some non-empirical element of necessity*. What looks like a complaint against Hume's analysis of the concept of cause is here presented as a complaint which is compatible with any analysis, namely that Hume has exaggerated the intellectual dispensability of causal order. Kant does sometimes seem to conflate these two complaints, but that is no excuse for his commentators' doing so. To take an example from one of the best of them, W. H. Walsh (*Reason and Experience*, p. 153) says: 'Kant is emphatically not saying that some kind of inner necessity binds cause to effect. . . The necessity which marks the causal relation is. . . derived wholly from the necessity of the principle of causality itself.' This says that Kant brings only one charge against Hume, namely that concerning the dispensability of causal order; yet Walsh expresses this in terms of the phrase 'the necessity which marks the causal relation', which is appropriate only to the other anti-Humean charge which Walsh explicitly (and rightly) denies Kant to be making.

We could leave it at that, and conclude that Kant has really nothing to say about Hume's analysis of cause but only about his failure to see how far our ability to grasp and be aware of our experience depends upon the obedience of experience to causal laws. I was myself inclined to leave it at that, until I began to pay close attention to a passage (A 91) in the *Critique* from which emerges a new line of thought altogether. I number its sentences for purposes of reference; and, since I am taking the passage rather seriously, I should mention that the translation is Kemp Smith's and—as almost always with this translator—is beyond reproach:

> **(1)** If we [said] that experience continually presents examples of . . . regularity among appearances and so affords abundant opportunity of abstracting the concept of cause, and at the same time of verifying the objective validity of such a concept, we should be overlooking the fact that the concept of cause can never arise in this manner. . . **(2)** For this concept makes strict demand that something, A, should be such that something else, B, follows from it *necessarily and in accordance with an absolutely universal rule*. **(3)** Appearances do indeed present cases from which a rule can be obtained according to which something usually happens, but they never prove the sequence to be *necessary*. **(4)** To the synthesis of cause and effect there belongs a dignity which cannot be empirically expressed, namely, that the effect not only succeeds upon the cause, but that it is posited *through* it and arises *out of* it. **(5)** This strict universality of the rule is never a characteristic of empirical rules; they can acquire through induction only comparative universality. . .

The first two sentences seem to voice the old accusation—for which I have sought and failed to find a justification in Kant's pages—that Hume's kind of analysis omits some essential element of necessity from the concept of cause. To take these sentences in this way would involve taking the stressed phrase ' necessarily and in accordance with an absolutely universal rule' to mean ' not only in accordance with a universal rule (for which Hume can allow) but also necessarily (for which Hume cannot allow)'. But in sentence **(3)** there is an indication that Kant believes that there is also an issue about universality between himself and Hume, for he there seems to accuse Hume of fobbing us off with statements about regularities which '*usually*' hold. Also, in sentence **(3)** there is a suggestion that the issue over universality *is* the issue over necessity; for Kant seems to treat the idea of a regularity which *usually* holds as though it were the obvious antithesis of the idea of a regularity which *necessarily* holds, thus suggesting that the only strictly true universal statements are those which express some sort of necessity. This suggestion is reinforced by the transition from sentence **(4)** to sentence **(5)**. The subject-matter of

(4) is the idea of something's not just succeeding upon but arising *out of* something else, which Kant clearly thinks to involve the necessity which escapes Hume. Sentence **(5)** purports to continue discussing this notion of necessity, yet its opening words are 'This *strict universality* of the rule is never a characteristic of empirical rules. . . '

[Added in 2012: When I wrote that Kemp Smith translates this passage perfectly, I must have been guessing and intending to check the guess before publishing. Careless! In fact there's a serious flaw in the translation. The start of **(5)**:

> 'This strict universality of the rule is never. . . '

is a mistranslation of

> *Die strenge Allgemeinheit der Regel ist auch gar keine*. . .

which means

> 'Strict universality of the rule is also never. . . '.

By adding 'This' and suppressing *auch* = also, Kemp Smith presents two wrong indications that Kant is *identifying* necessity with strict universality. This seems to be a case where the commentator borrowed the translator's pen.**]**

If Kant is saying that the only true universal statements are those which are in some way necessary, then he is wrong. If he is saying more modestly that until we know that a universal statement is necessary we cannot know that it is true, then he is still in trouble because, on his own showing, the necessity in question cannot be discovered to obtain in any particular case and therefore it cannot be used as a guarantee of truth or of anything else. There is, however, a different way of associating Hume's position with a certain kind of permissiveness about 'strict universality'; and I am confident that it is part of what is expressed in the passage I have been discussing as well as in some others, though Kant seems not to have isolated and crystallised it in his own mind sufficiently to be able to spell it out clearly, or to see how misleading is its formulation in terms of 'necessity'. The point is as follows:

While Hume thinks of causal laws as universal statements which have no exceptions at all, his account of the sort of thing a causal law is and of the ways in which causal laws are useful to us is such as to allow that we could make almost as much use of statements which were like causal laws except that they were known to have just a few counter-instances. He shares the common belief that when we are inclined to accord a causal status to a lawlike statement which then lets us down by proving false in a single instance, the statement ought to be dropped entirely because **(a)** it has, or could be made to have, an unlimited number of counter-instances, and **(b)** the facts which it covers are also covered by some other (still unformulated) universal statement which has no counter-instances at all. But just suppose that we believed—for no matter what reason: perhaps because God told us—that the nearest we could come to exceptionless universal statements was to establish universal statements which were true in almost every instance, what should we say then? To put it in another way, what should we say about the possibility that we might have to settle for a weakly quantified science—i.e. one whose hypotheses were all stated in terms of a quantifier which had the force of ' For very nearly every value of x. . . '? (Like Kant and Hume, I say nothing about hypotheses which say that all but a certain small *proportion* of the members of a certain class belong to a certain other class. That raises entirely different issues, on which Kant said nothing because they never occurred to him.)

Hume would answer that such a science could be useful in the same kind of way, and to nearly the same extent, as a science consisting of genuinely universal statements. He would have to concede a modest loss of utility: predictions would be made a little less confidently, and there might also be some attenuation of our sense of having explained, or understood, an event by relating it to earlier events in accordance with the statements of our science. But Hume has no reason for denying that predictions and explanations in terms of a weakly quantified science would still be genuine predictions and explanations. For him, all this must be a matter of degree: if God tells us that the best laws we can find will have exactly *fifty* known exceptions each, then things are moderately bad; and if He tells us that the best laws we can find will have exactly *one* known exception each, then things are only one-fiftieth as bad. Strict universality is the ideal, but only as the maximum of something which it is desirable to have in as high a degree as possible.

In contrast with this, it is Kant's view that there is a great gulf between exceptionless universal statements and universal statements which have one counter-instance each; but that the difference between 'one counter-instance' and 'two counter-instances' or 'one hundred and seventeen counter-instances' is of no theoretical importance at all, because the real damage is done by the admission of a single counter instance. This is because an 'explanation' of an event which appeals to a statement of the form 'Very nearly every time A occurs, B follows' *is not an explanation at all*. The production of such a statement might soothe someone into saying 'You have explained the event to me' or 'Now I know why it occurred' or 'Now I understand'; but the event would *not* in fact have been explained; the hearer would *not* know why; he would *not* understand. It is of the essence of the notions of *explanation*, of *understanding*, of something's being so *because* something else is so, of an event's being *made* to occur, that these are all expressible in terms of '*an absolutely universal rule*'. The flaw in Hume's position is that he cannot do justice to this fact. Hume's only reason for connecting 'explain', 'understand', 'because' and the rest with exceptionless universal statements is his *contingent* belief that there are exceptionless rules to be found and that any rule which has one exception can be made to have indefinitely many exceptions. Just because he thinks that the concept of causality can be analysed in purely empirical terms—and crucially in terms of regularity—Hume does not *and cannot* attach a fundamental philosophical importance to the difference between a rule which always holds and one which nearly always holds.

I wish now to bring out two connections between the notion of regulativeness and Kant's view that genuine explanations demand a strongly quantified science. One of them involves using Kant's view about strong quantification to clear up a difficulty about his handling of regulativeness; the other reverses the order and uses the notion of regulativeness as an aid to adjudicating between Kant and Hume in their conflict over strong quantification.

Firstly, let us return to the question of why, having proved that all graspable experience must have a fairly high degree of causal order, Kant claims to have proved that total determinism is strictly true.

There are two ways in which it is *prima facie* possible that a fairly orderly world should fall short of being totally causally determined. **(i)** It might manifest a causal order which is fully expressible in strongly quantified statements which do not claim all the territory—that is, which give no answers at all to some questions about what, in given fully described situations, will happen next. **(ii)** It might manifest a causal order which is expressible in statements which give answers to every question about what will happen next, but only weakly quantified statements of this sort. If we think only in terms of strongly quantified science, then these two *prima facie* possibilities are the possibilities **(i)** that a world might admit only of an *uncomprehensive* science and **(ii)** that a world might admit only of a slightly *inaccurate* science.

I have no reason to think that the first of these ever occurred to Kant as a possibility, but in the present context this oversight is not a damaging one. There could of course be an accurate but uncomprehensive science; but I doubt if there could be a partially ordered world the whole of whose order was expressible in an accurate, strongly quantified science—so that events which were not predictable by this science could not be brought under any generalisations at all, even weakly quantified ones. If I am right in thinking that this is not possible, then any world which was not totally determined would have to owe at least part of the incompleteness of its causal order to the second of the two possibilities, that is, to its admitting only of a slightly inaccurate science or a weakly quantified one.

Now, Kant's arguments for the second analogy all turn on such notions as those of *explanation*, *retrodiction*, *reasons* for believing, the way in which something *must* have happened, and so on. What his arguments show is that we cannot have graspable experience at all unless we have experience to which we can apply notions like these. But his complaints against Hume include (I have argued) the claim that we are not using these notions properly unless we are using them in connection with a true, strongly quantified science; and I have suggested that a fairly orderly world can escape being totally determined only if its science is either not-quite-true or weakly quantified. And *this*, I submit, is why Kant thinks that there is no gap between the notion of a world which is orderly enough for the notions of explanation etc. to be applicable to it and that of a world which is totally determined; it therefore provides the reason for Kant's exaggerated description of what he has achieved in his arguments for the second analogy of experience. Qed.

By speaking of 'exaggeration', I commit myself to taking Hume's side against Kant on the issue of weakly versus strongly quantified science. I believe that most people find Kant's view here the initially attractive and plausible one, and I hope that this is so because I do not wish to take up space here trying to increase its attractiveness before showing it to be false.

Let K be a scientist whose working assumption about what kinds of causal laws can be found is the strictly determinist one which Kant thinks to be a *sine qua non* of scientific respectability. Let H be another scientist whose working assumption is that the world admits of a weakly, but not of a strongly, quantified science. I wish to examine the ways in which their respective assumptions affect the behaviour of K and H as scientists, in order to see whether Kant is right in withholding from H the cachet of intellectual respectability which he confers upon K.

H thinks that some events are causally inexplicable, while K denies this. Suppose them to be confronted by an event for whose occurrence they have no explanation. It is not an event upon which received science is *silent*, but one whose occurrence is *in conflict with* some hypothesis which has hitherto been a part of received science. A short description of what then happens would be as follows: K says that the hypothesis in question must be dropped and a replacement for it sought, and he maintains this in face of persistent failure to replace the hypothesis; H on the other hand regards it as possible that the challenged hypothesis is the best hypothesis of its kind of which the world admits, and therefore that the event whose occurrence conflicts with it is simply an illegality which cannot be coped with in science at all.

K stands a chance of discomfiting H—by finding an explanation for the event which H has conjectured to be inexplicable—while H has no comparable way of discomfiting K. Those of us who were brought up to think of vulnerability as a virtue may well regard this as a point in favour of H's position. But there is a reason which might be given for awarding the honours the other way, namely that although H and K both make acts of faith, it is K who makes the optimistic, industry-urging act of faith. To give this reason for favouring K's strict determinism is to appeal to its alleged merit as salutary advice, that is to its alleged acceptability as a regulative principle. More than this is needed to justify Kant's position, for he says that a belief in illegalities is not just mildly bad for the scientist who holds it but is radically incompatible with the rational understanding of the world. Nevertheless, I wish to discuss the weaker claim for the regulative virtue of strict determinism, as a discussion of this will give us all we need for a refutation of Kant's stronger claim that strict determinism is not only salutary for the scientist but is mandatory upon him.

The suggestion before us, then, is that when K insists that there is an explanation to be found for the as-yet-unexplained event, he is saying something which is healthier, because less conducive to complacency, than H's conjecture that the event is an illegality. The short reply to this is that it is a libel on H to assume that he is not prepared to test his conjecture; and to test it he will to have do exactly what K does, namely to search as hard as he can for an explanation for the event in question. At any level of science, the behaviour of the honest friends of an hypothesis is the same as that of its earnest enemies.

'Still,' it may be said, 'H will eventually give up the search for an explanation. He lacks K's ultimate commitment to a belief in the total intelligibility of the world, and this failure of faith will sooner or later sap his will and lead him to abandon the pursuit.' This is correct; but *it is not a difference between H and K.*

It must be remembered that the problem concerns a *single* event, not a large *class* of events of a certain kind. H and K have adopted a hypothesis about (say) sulphates and then discovered not that it fails for (all samples of) copper sulphate, but that it failed for *one particular sample of copper sulphate on one particular Wednesday morning.* If their difficulty arises from an experiment which can be reproduced at will, and always with the same result, then H and K do have a pair of strictly universal statements which cover the known facts: one about sulphates other than copper-sulphate-under-conditions-C, and one about copper-sulphate-under-conditions-C. Much time may be spent in trying to derive both hypotheses from some more general hypothesis; but even if they are not as general as H and K might wish they are nevertheless 'strictly universal' and so suffice to ward off our Kantian problem. Our problem arises only when a hypothesis is challenged by a single datable, locatable, unrepeatable experiment, not when it is challenged by a kind of experiment of which there are many instances.

There is, I think, universal agreement amongst scientists and philosophers of science. that unrepeatable experiments do not count against well-corroborated hypotheses. So, when H—faced with the stubborn failure of his attempts to repeat the experiment—says that the challenged hypothesis ought to be retained and the challenging experiment relegated to limbo, he is only saying what K too will say if he does not wish to make an ass of himself in the eyes of the scientific community, The only difference between them is that H can give a better account than K can of why he refuses to give up a hypothesis which is challenged by an unrepeatable experiment. For H can say that he is acting on the conjecture that the experiment was an illegality and that the challenged hypothesis has that 'true with very few exceptions' status which is the most that we can hope for in science. K of course cannot say this; but what can he say?

He may say that he does regard the hypothesis as refuted, but that he continues to treat it as though it were unrefuted because he has nothing to put in its place. But if he says this, he will find himself estranged from the rest of the scientific community: not because he is so lax as to treat a hypothesis as true which he believes to have been refuted, but because he is so silly as to believe a hypothesis to have been refuted by an unrepeatable experiment.

I shall suppose him, then, to say whatever the other scientists and the philosophers of science say about the negligibility of unrepeatable experiments. The trouble is that most of them say nothing; and those who do pronounce on the subject are quite unclear about the particular point which I am raising. However, if we can see clearly what are the *possible* positions on this matter, we can afford to ignore the question of which philosophers have in fact taken which position. Stripping the issue to its bare essentials, we can say that if a well-corroborated hypothesis of the form '$(\forall x)(Fx \supset Gx)$' is challenged by the production of an experimental report of the form 'Fa & Ha & $\neg Ga$', and all attempts to repeat the experiment described therein meet with failure, there are just three possibilities: **(1)** To say that the hypothesis is refuted by the single experiment; **(2)** To say that the experimental report is not true; and **(3)** To say that the experimental report is true but the hypothesis is unrefuted—this is what H says, and it commits anyone who says it to reading the hypothesis as weakly quantified.

Option **(1)** is eccentric and uninteresting, and I shall discuss it no further; the important and interesting clash is that between options **(2)** and **(3)**. I do not think it has been generally recognised that the orthodox view about unrepeatable experiments confronts us with the disjunction of **(2)** and **(3)**, but there are signs in the literature that this has been half-recognised: some writers speak of unrepeatable experiments in terms appropriate to the adoption of **(2)**, and the only reason I can think of for adopting **(2)** as necessarily the right account is a Kantian belief that to adopt **(3)** is in some way to let the side down. The trouble with option **(2)**—that is, with saying that the experimental report is untrue in some relevant respect—is that there *could* be cases in which it was entirely implausible. It does not matter whether there have been such cases: their mere possibility is sufficient to show that the insistence upon a strongly quantified science, which debars one from ever taking option **(3)** when dealing with an unrepeatable experiment, is irrational and unsatisfactory.

In saying that there could be cases in respect of which option **(2)** was implausible, I am not calling attention merely to those cases where we have no reason to suspect the integrity and basic professional competence of the author of the experimental report. A report may be deemed untrue without the honesty or basic competence of its author being called into question, if the doubts do not concern those parts of the report which say what apparatus was used, what readings were taken, what kind of graft was made, etc., but rather those parts which say that the sample used was pure, that the meteorological conditions did not vary during the experiment, that adequate safeguards were taken to prevent cross-pollination. Claims of this sort will always be needed if the reported experiment is to be made relevant to a received hypothesis, and a doubt which is cast upon any such claim in an experimental report is a doubt as to the truth of the report *in so far as the report says that something happened which was in conflict with a received hypothesis*. It is in connection with this sort of doubt, I think, that it has become customary to dismiss unrepeatable experiments as involving some sort of 'experimental error'.

But it is just dogmatism to say that wherever an experiment turns out to be unrepeatable it is *ipso facto* reasonable to say that it involved some 'experimental error' of this sort. If the claim that there was an experimental error has no backing except for the fact that the experiment has turned out to be unrepeatable, then the imputation of experimental error is just a face-saving, word-spinning device which has much less to recommend it than has the alternative of saying that the hypothesis under challenge must be taken as weakly quantified. The impression is sometimes given in the literature that it is always reasonable to take option **(2)** in connection with an unrepeatable experiment, because repeatability is a necessary condition of the experiment's counting as an 'objective' happening, or of its not counting as 'occult' or 'chimerical'. The picture which this evokes is that of a scientist ending his experimental report with something like '. . . and then I saw a blue flash; no-one else was in the laboratory at the time, but I swear that I saw it'. But of course the circumstances of the experiment do not have to be like this at all: there can be witnesses, research assistants, photographs, recorded running commentaries—a whole host of sources of evidence that the experiment did take place as reported. In these circumstances, it would be merely arbitrary to insist that nevertheless there *was* an ' experimental error' in the relevant sense, or that what happened was ' chimerical'; but it would not be arbitrary to dismiss the experiment as negligible on the grounds of its unrepeatability. In such a case, the only person who could give a sensible account of why the experiment was negligible would be the one who—like my scientist H—does not insist upon a strongly quantified science.

In conclusion, then, I maintain that a preparedness to accept a weakly quantified science is not only permissible but is mandatory upon any scientist who wishes to be able to cope sensibly with a really well-attested but unrepeatable experiment, if one should occur. Kant's insistence upon 'absolutely universal rules' is not just the contradictory, but a contrary, of the truth.

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[Added in 2012:]

At a meeting of the British Society for the Philosophy of Science where I presented this paper its conclusion was met with scepticism unsupported by arguments. As evidence that the conclusion isn't merely *weird*, I remarked that Popper at least agreed with it. This produced a storm of disagreement; but Imre Lakatos produced a copy of *The Logic of Scientific Discovery*, in section 22 of which this is said:

> We say that a theory is falsified only if we have accepted basic statements which contradict it. This condition is necessary, but not sufficient; for we have seen that non-reproducible single occurrences are of no significance to science. Thus a few stray basic statements contradicting a theory will hardly induce us to reject it as falsified.