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## *Undermining and Admissibility*

MICHAEL THAU

Our best physical theories seem to tell us that the world is an objectively chancy place. Some possible futures have some chance of being actual, and this is the most that can be said. More knowledge about the way the world is now and what the laws are won't yield complete information about what will be. Of course, our best theories may be wrong, but they are not wrong merely because they advert to the notion of objective chance. Quantum mechanics may be incorrect because it postulates entities or properties that there aren't; it might even be wrong because it makes no sense; but it isn't wrong because the notion of chance it so freely makes use of makes no sense. Whether our best theories are to be believed or not, they force upon us the conceptual possibility of objectively chancy events.

Given this, one might wonder whether the totality of facts about the world which aren't about chance determines the chances, whether matters of chance supervene on matters of non-chance. We do not learn about the chances directly, rather we make hypotheses about what the chances are based on information about relative frequencies. Induction justifies us in thinking that the relative frequencies we have observed will continue throughout world history. But if supervenience fails we have reason—in *addition* to general worries about induction—to be sceptical concerning our knowledge of the chances. For if supervenience fails, the correctness of our views on what there is and how it behaves won't suffice to determine the chances. So one reason to hope for a successful reductive analysis of chance is that it would show that chance does supervene on matters of non-chance.

One way that matters of chance might supervene on matters of non-chance is through Humean supervenience, the view that all supervenes on local matters of particular fact. David Lewis is the view's architect, but he has also given reason to think that chance cannot supervene in a Humean manner (Lewis 1994). He begins with a vague worry that does not itself force the Humean to give up hope. However, by adding an extremely plausible premise concerning the relationship between chance and credence, he turns the worry into a proper argument.

It has gone unnoticed that the trouble Lewis raises is not restricted to Humean reductions of chance. The argument—if sound—destroys the hope of virtually *any* reductive analysis of chance. Indeed, the argument even raises difficulties for some views according to which chance is brute and unanalysable. The primary purposes of this paper are to show how broad the scope of the argument is, and then to offer a response.

In § I, I will quickly review Lewis's vague worry, and show that it extends far beyond Lewis's particular Humean concerns. In § II, I will explain how the worry gets turned into a proper argument. In § III, I will discuss the significance of the argument. And, in § IV, I will show that the proper argument is unsound. Finally, in § V, I consider worries about the relationship between credence and chance that arise from my response to Lewis's argument.

## I

Science doesn't specify chances outright, but rather gives us conditionals about chance. We know that *if* a tritium atom were to come into existence now its chance of decaying within a year would be such-and-such. For convenience, I will use the term "world history" to denote all of world history that does not concern chance. Let a *history-to-chance conditional* be any conditional whose antecedent specifies world history up to a time  $t$ , and whose consequent gives chances at  $t$ . Let  $T$  be the complete theory of chance<sup>1</sup> for this world.  $T$  is the conjunction of all true history-to-chance conditionals. If the chances supervene on world history, then the actual world history implies  $T$ . However, if ours is an indeterministic world then there is now some chance that world history will turn out differently than it actually does. Now, suppose that chance supervenes in a way which implies that the chances must, to a very great extent, reflect limit relative frequencies throughout all of time. In this case, the indeterminism in the world's future means that patterns which make  $T$  false nevertheless have some chance of occurring—whence  $T$  implies that these patterns will not occur, even though by  $T$ 's own lights they are probabilistically possible. So it seems that the theory of chance talks out of both sides of its mouth. On the one side  $T$  says that certain patterns are not possible, on the other it says that these very patterns have some chance of occurring. This is Lewis's vague worry.<sup>2</sup>

The worry applies to any reduction according to which the chances must, to a very great extent, reflect the limit relative frequencies. Lewis believes that any adequate Humean construal of chance will have this feature. His reasoning involves the Principal Principle, which connects degree of credence with credence about chance, and which he claims captures all that we know about objective chance. The Principle states:

Let  $C$  be any reasonable initial credence function. Let  $t$  be a time. Let  $X$  be the proposition that the chance, at time  $t$ , of  $A$ 's holding equals  $x$ . Let  $E$  be any proposition compatible with  $X$  that is admissible at time  $t$ . Then  $C(A/XE) = x$ . (Lewis (1966, p. 87))

<sup>1</sup> Throughout the paper, "theory of chance" is used to denote the theory which gives what the chances are, while "view of chance" is reserved for philosophical theories about chance itself. Quantum mechanics is a theory of chance, Humean supervenience is a view of chance.

<sup>2</sup> For a more detailed discussion of the worry see Lewis (1994).

A proposition is admissible at  $t$  if it doesn't provide direct information about the outcome of chancy events that occur subsequently to  $t$ . The Principal Principle says roughly that credence must reflect beliefs about chance if you have no direct (i.e., inadmissible) evidence about the relevant outcome. If you believe the chance of heads is 0.5, and have no direct information about how the toss turns out, your credence in heads should be 0.5. Lewis accepts that chances must reflect limit relative frequencies because he sees it as the only way to explain the Principal Principle—the only way to explain why it is often rationally required that credence reflects beliefs about chance.<sup>3</sup> However, there are other reasons to think that *any* successful reduction of chance must imply that the chances reflect the relative frequencies, and thus be vulnerable to Lewis's worry.

Philosophers seek reductive analyses for at least two reasons. Sometimes—in the case of attempted analyses of knowledge—reductions are a means toward clarifying concepts which are not seen as problematic. In other cases—such as attempts at reductive analyses of the mental—the reduction is deemed necessary at least in part *because* the candidate for reduction seems problematic. In these latter cases, one worry is almost always epistemological: if mental states are logically distinct from behaviour, then how is it that we come to know another's mind strictly via observation of his behaviour? An analysis of chance will likely be of this latter type, to arise at least in part because chance seems an epistemologically mysterious entity. But any analysis which addresses this motivation will have to imply that the chances must, to a great extent, reflect the relative frequencies; for it is via information about relative frequencies that we posit what the chances are. A reductive analysis of chance which did not have this feature would be like a reductive analysis of the mental which failed to tie pain to pain behaviour; one would wonder what the point is. The above is only meant to show that given a likely motivation for attempting a reductive analysis of chance, such an analysis will be subject to Lewis's worry. There is, however, a related reason to believe that regardless of the motivation, any analysis of chance which failed to make the chances reflect the relative frequencies would be a failure.

As mentioned above, Lewis believes that the only way to explain the rational requirement of the Principal Principle is to explain what chance is in a way which guarantees that the chances reflect the relative frequencies. While Lewis believes that such an explanation is highly desirable, some will respond that chances are brute facts, metaphysically independent of the relative frequencies. Such a person will certainly not be able to justify the rationality of the Principal Principle in Lewis's manner, and very likely won't be able to justify it at all. I do not think that such a person is thereby irrational. However, I will argue that if you are in the business of analysing chance, then you must have an explanation of the rationality of the Principal Principle, and that there is more that you must explain.

The Principal Principle gives us one aspect of how the concept of chance figures in rational thought, but there are other aspects as well: it is rational to posit chances which mirror the relative frequencies, and, other things being equal, irra-

<sup>3</sup> See Lewis (1994).

tional to do otherwise.<sup>4</sup> Call the rational constraints on the concept of chance the *normative force* of chance. Again, if chance is brute, there will be no explanation for why the concept of chance has the normative force that it does. But consider someone who proposes a reductive analysis of chance. For this person, the fact that the chance of heads is 0.5 reduces to some proposition *R* not directly about chance. But *R* already has constraints on how it can be rationally employed, so if *R* is to be the truth-maker for the proposition that the chance of heads is 0.5, then these constraints should dictate that it is reasonable for *R* to figure into the Principal Principle the way that the proposition that the chance of heads is 0.5 does; and it should be reasonable to infer *R* from the hypothesis that the limit relative frequency of heads is 0.5. If *R* itself gives reason to believe that the long term relative frequency of heads is 0.5, then *R* can comfortably play both roles; if not, then *R* can play neither. Thus, any analysis of chance will have to entail that the chances, to a very great extent, reflect the relative frequencies. For if it is irrational that the concepts of the analysans be employed in the way that the concept of chance is rationally employed, then the analysis will require that new and hitherto irrational modes of reasoning be employed. And this goes far beyond the mandate of analysis. An analysis of causation which says that *A* causes *B* just in case “*A* causes *B*” is inscribed in a book buried under Mount Sinai is wrong at least in part because it would be irrational to use the concept of inscriptions in buried books to explain why your car won’t start.<sup>5</sup> So any acceptable analysis of chance will have to imply that the chances, to a very great extent, must reflect limit relative frequencies, making it subject to Lewis’s worry. However, the worry extends even to some views of chance according to which chance is brute. This can best be seen if we first take a closer look at Humean supervenience.

The Humean’s view of chance implies an epistemology of chance. Given any complete world history, the Humean will be certain that one theory of chance is true because on his view any complete history will entail a single theory of chance. One could hold the view that chance is brute—and hence that the Humean is wrong about the entailment—but still adopt the Humean’s epistemology. That is, one might still give full credence to a theory of chance given some complete history that the Humean believes entails that theory. But any such view will be subject to Lewis’s worry.

<sup>4</sup> Lewis (1980) attempts to derive this requirement from the Principal Principle. I include it as a separate requirement because—as we shall see—the derivation depends on an illicit assumption.

<sup>5</sup> The point mirrors a popular reconstruction of Moore’s open question argument. Rather than questioning whether the proposed analysans of the analysis of *good* is good, the reconstruction asks whether the analysans plays the role in our reasoning that the concept good does: that is, is it irrational not to seek and promote that to which the analysans applies? The reconstruction—like Moore’s original argument—is intended to show that goodness is irreducible and brute. Its success depends on two things: one, that an analysans must have the same normative force as its analysandum, and two, that no natural property has the same normative force as goodness. For a discussion of this reconstruction of Moore see Darwall, Gibbard, and Railton (1992, p. 117).

Let  $T$  be some theory of chance. Let  $H$  describe the actual course of history up till now, and let an *undermining future* be any future  $F$  such that (i)  $F$  has a non-zero chance of occurring according to present chances as fixed by  $T$  and  $H$ , but (ii) there is some reasonable initial credence function  $C$ , such that  $C(T'/HF) = 1$ , where  $T'$  conflicts with  $T$ . Undermining futures are just the possible futures that raise difficulties for the Humean, since on his view  $HF$  entails  $T'$ , implying that (ii) holds for any reasonable initial  $C$ ; and hence that  $C(THF) = 0$ , contra (i). But undermining futures deserve the name even if the inconsistency is of the probabilistic and not the logical sort, that is, if there is some reasonable initial  $C$  such that  $C(THF)$  is 0 even though  $THF$  is logically consistent. In some sense, the worry concerning reductive analyses of chance isn't that they posit an analytic connection between world histories and theories of chance. The fact that such a connection is posited (in conjunction with the fact that chances must to a great extent reflect relative frequencies) is enough to entail that there are probabilistically possible futures  $F$  such that  $C(T/HF) = 0$ . But analytic connection isn't the only way that this could be the case. If the levels of certainty that result from a Humean analytic connection between chance and world histories are reasonable, then it isn't unreasonable to believe that there are probabilistically possible futures which would undermine the theory of chance. Undermining futures are a problem if the Humean's epistemology is reasonable.

For convenience, let us call any view according to which certainty about complete world history can justify certainty about the chances a  $JC$  (for justified certainty) view of chance. Obviously, any reductive analysis of chance will be a  $JC$  view of chance. Given that some  $JC$  view of chance is correct, there is no reason to think that Humeanism can force an agent who has complete knowledge of world history to be unreasonable in his assessment of what the chances are; that is, there is no reason to think the Humean gets the epistemology of chance wrong. Hence, if it is reasonable to be certain about the chances given knowledge about complete world history, it surely must be reasonable to be certain in just the way that a Humean would be, that is, to adopt a Humean epistemology. Thus, Lewis's worry applies to *any* acceptable  $JC$  view.

I have no solution to Lewis's worry, unless the suggestion that it isn't yet very worrisome counts as a solution. It sounds a bit odd to say that the theory of chance might rule out certain futures which are nonetheless probabilistically possible, but only a bit odd. Compare: we might have thought that to believe the chance of an event occurring is 0 is to rule out that event occurring. On standard probability theory, however, 0 chance does not imply non-occurrence, and this is easy to get used to. This suggests that our naive views about the connection between chance and possibility are suspect, and that at this stage we shouldn't be too worried about Lewis's worry. The worry becomes problematic, however, because of the ease with which it can be turned into a proper argument against any acceptable  $JC$  view of chance.

## II

The Principal Principle states that for any reasonable initial credence function, conditionalising on the proposition that the chance of  $A$  is  $x$ , and any other proposition which is admissible, yields that credence in  $A$  is  $x$ . We can now construct the argument against any *JC* view of chance. Let  $T_w$  be the complete theory of chance for world  $w$ .  $T_w$  is the conjunction of all history-to-chance conditionals that are true at  $w$ . Let  $H_{tw}$  be the history of  $w$  up to time  $t$ . Assume that both  $T_w$  and  $H_{tw}$  are admissible. With  $C$  a reasonable initial credence function,  $P_{tw}$  the function that gives the chances for  $w$  at time  $t$ ,  $P_{tw}(A) = x$ , and  $X$  the proposition that the chance of  $A$  is  $x$ , the Principal Principle yields

$$C(A/XT_wH_{tw}) = P_{tw}(A).$$

However, since  $T_w$  is the complete theory of chance at  $w$  and  $H_{tw}$  is the history of  $w$  up to time  $t$ ,  $T_w$  and  $H_{tw}$  imply  $X$ . Hence we have a special case of the Principal Principle

$$C(A/T_wH_{tw}) = P_{tw}(A).$$

Whenever  $T_w$  and  $H_{tw}$  are admissible, one's credence in  $A$  given  $T_w$  and  $H_{tw}$  should equal the chance of  $A$  according to  $T_wH_{tw}$ . Lewis argues that both  $T_w$  and  $H_{tw}$  should be admissible at  $t$ , since  $H_{tw}$  concerns only events prior to  $t$ , and  $T_w$  is just a conjunction of history-to-chance conditionals each of which is admissible.

*JC* views of chance are committed to the fact that it might be reasonable for an agent to give no credence to a certain course of future history  $F$ , although the theory of chance he holds gives  $F$  some non-zero chance of occurring. In other words: (i)  $C(F/T_wH_{tw}) = 0$ , but (ii)  $P_{tw}(F) = r$ , where  $r$  does not equal 0. But by the special case of the Principal Principle (ii) implies that  $C(F/T_wH_{tw}) = r$ , contradicting (i). So it seems that any *JC* view of chance is refuted by this argument.

## III

My response to this argument will engender further worries about *JC* views of chance. In the end these worries do not rule out such views, but seeing this requires a more detailed discussion of the force of the above argument.

Suppose that Ben proposes an analysis of chance according to which it supervenes on matters of non-chance, while Felix believes that non-supervenience is constitutive of chance. Felix does not accept Ben's analysis, but how is his non-acceptance to be characterized? What is the nature of the disagreement between Ben and Felix? We might just say that Felix takes Ben's analysis to be false. While this is how we tend to characterize such disputes, I think that it misses

distinctions of some philosophical import. A distinction due to John Burgess<sup>6</sup> will help us see some of what is left unsaid.

Philosophers often try to dodge ontological commitments. For example, one might be inclined to view talk about abstract entities with suspicion, but also be inclined to view talk of numbers as indispensable. The two inclinations combined will very likely further incline one to look for some view of numbers according to which they are not abstract entities. Consider someone so inclined—call him “Nominalist”—who goes on to give a characterization of numbers according to which they are concrete. I have left much out of the description: no clue has been given as to what Nominalist’s characterization of the numbers is; nor has Nominalist’s motivation for viewing abstracta with suspicion been given. But something still more fundamental has been left out: we don’t yet know exactly what he means to be doing with his characterization of the numbers. Burgess has noted two alternatives. On the one hand, Nominalist might intend his characterization to capture what we’ve meant by our number talk. According to him the number 4 is actually some concrete entity *F*. Nominalist might think that—possibly unbeknownst to us—*F* is what has been meant by “4” all along. Burgess calls this strategy “Hermeneutic Nominalism”. On the other hand, Nominalist might think that we have intended “4” to refer to some abstract entity. Since he is suspicious of such entities, he does not wish to use “4” in this way. His characterization of 4 is meant to replace a conception which, for whatever reason—he rejects. Burgess calls this strategy “Revolutionary Nominalism”. The hermeneutic/revolutionary distinction is not restricted to mathematical entities. *Any* attempt at dodging ontological commitment by characterizing the suspect entity as something thought to be unproblematic requires some elucidation of the relationship between the old concept and the new characterization.

David Lewis has suggested (in discussion) that the hermeneutic and revolutionary strategies don’t exhaust the alternatives for that relationship. Rather, they merely represent the extreme cases, with much ground between. The intermediate strategies wouldn’t have the character of full blown replacement, nor would they be simply an elucidation of what was meant all along. The possibility of this middle ground emerges because our concepts are resilient in the face of certain changes; sometimes there is indeterminacy, and making determinate what was once indeterminate needn’t count either as replacing concepts, or as revealing what was implicit in them all along.

Consider a law enacted in the 18th century which prohibits vehicles from the park. Does this law exclude wearing roller skates in the park? Is it at all plausible to think that the 18th century conception of vehicle determinately excluded or

<sup>6</sup> I have slightly altered Burgess’s characterization of the revolutionary and hermeneutic positions. Burgess is concerned with nominalist who attempts to show that *science* can be done without abstract entities, hence he characterizes hermeneutic nominalism as the view that “science, properly interpreted, already does dispense with mathematical objects”. He characterizes revolutionary nominalism as the view that “there are scientific reasons why current scientific theories should be replaced by alternatives dispensing with ...” abstract entities (Burgess 1983, p. 96).

included roller skates?<sup>7</sup> And if it comes about—through conscious decision or unconscious conceptual evolution—that for us roller skates will (or won't) count as vehicles, need we say that the concept we attach to "vehicle" is different from the concept attached by 18th century law-makers? The same point applies to the commitment dodger. Maybe our conception of number is indeterminate in certain respects. Perhaps prior to the reduction of numbers to sets it was indeterminate whether numbers were sets, so perhaps the set theoretic reduction doesn't count as replacing concepts. But very likely it is false that there was something implicit in our prior use of number talk which implied that when we talked of numbers we were talking about sets. So the set theoretic reduction isn't strictly a hermeneutical move either.

I think it is clear that the reductive commitment dodging discussed by Burgess is just a special case of reductive analysis, and that the Burgess-Lewis spectrum applies to analyses in general. For whatever reason, Ben finds the notion of non-supervening chances unacceptable, and for whatever reason Felix finds the notion of supervening chances just as unacceptable. Each of them may take their viewpoint to be constitutive of our notion of chance as it is used pre-analytically, in which case they simply disagree over whether Ben has accurately captured our pre-analytic conception of chance. But their disagreement may be different—and more philosophically interesting—than this.

The concept of chance has a function; the Principal Principle is an expression of part of that function. While Lewis may not have been right that the Principal Principle captures all that we know about chance, it certainly tells us much that is functionally important about chance. Felix claims that it is constitutive of our concept of chance that the chances do not supervene; perhaps he is right, and perhaps—contrary to Lewis—this is something we know about chance that isn't captured by the Principal Principle. If this is the case, then Ben's claim to a hermeneutical analysis is false. And to the extent that Ben's analysis involves a change in our conception of chance, his justification for being suspicious of *sui generis* chance becomes more important. For the moment however, grant Ben his suspicions. He sees the role played by chance and judges that a non-supervening magnitude could not play that role. Perhaps this arises from a general inability to see how two worlds identical in all matters of particular fact could be at all different, or perhaps like, Lewis, he wonders how a magnitude which doesn't supervene could play the role that chance plays. Whatever his motivation, if we grant that it is justified there is no reason that he shouldn't say, "Let what we meant all along be damned. I wish to mean something that makes sense of the role that chance plays and my general philosophical views."

It is, of course, possible that Ben's reasons for rejecting *sui generis* chance are inadequate. And it is possible that Ben's analysis isn't really suited to play the role that chance plays. Here arises the philosophical interest of Ben's views. Has he adequately justified the need for his task, and has he succeeded at it? He must respond to charges that he has failed in either of these ways. But as his task was

<sup>7</sup> The example is from Hart (1961, p. 123).

to find a philosophically reputable entity to do what chance does—and not to reveal to us the nuances of what we meant all along—charges that he has failed to capture aspects of our pre-analytical notion which have nothing to do with the role of chance needn't concern him.

Consider a community much like ours was prior to the set-theoretic reduction of numbers, with one possible exception: these people determinately *don't* mean numbers to be sets. (Perhaps *we* took numbers to be determinately not sets prior to the reduction, hence perhaps this community is us.<sup>8</sup>) Is the possibility of unifying all of mathematics via a set-theoretic reduction of any less interest to these people than to us? Is it in any way less rational for them to reap the benefits of the reduction than it is for us?

And so it is with chance. There are at least two ways that one might object to a proposed analysis of chance: claim that it doesn't capture what we meant by chance, or, more interestingly, claim that the analysts cannot adequately play the role of chance. The argument from the Principal Principle is of the second sort. It tells us that any *JC* view of chance comes into conflict with the Principal Principle, which seems to capture a crucial aspect of the role of chance. Since any view according to which chance is reducible is a *JC* view, the argument poses a real threat to the possibility of an analysis of chance.

#### IV

Fortunately for those who seek an analysis of chance, the argument does contain an illicit move. The flaw rests on the notion of admissibility. The restriction to *admissible* background information in the Principal Principle is essential to its plausibility. If you believe that the chance of heads is 0.5, but also believe that you have reliable information from the future that the coin will in fact land heads, it would be unreasonable to give only 0.5 credence to heads. Direct information about outcomes can sever or complicate the link between credence and chance.<sup>9</sup> That is why such information is inadmissible.

Lewis recognized that a proposition isn't admissible (or inadmissible) *tout court*, but rather that admissibility is a time dependent notion. A proposition is admissible (or inadmissible) at a certain time. However, Lewis used to think<sup>10</sup> that admissibility depends only on time, that any time *t* will fully determine which propositions are admissible and which are not. This is a mistake, and moreover, the argument from the Principal Principle depends upon it.

<sup>8</sup> It is interesting that the introduction one gets to set theory in elementary school is often preceded by a warning not to confuse the null set with zero. When I was so warned, I wondered how anyone could make such a mistake.

<sup>9</sup> See Hall (1994) for a detailed discussion of how inadmissible information affects the link between credence and chance.

<sup>10</sup> In Lewis (1980). He has since changed his mind. See Lewis (1994).

A proposition is inadmissible if it provides direct information about what the outcome of some chance event is. This explanation of admissibility adverts to a particular chance event; that is, propositions are (in)admissible with respect to the outcome of some chance process. Moreover this feature of admissibility is not a peculiarity of my characterization; rather, it follows from the role that the notion of admissibility is to play. We are to keep out a certain sort of information about how the *relevant chance process* turned out when applying the Principal Principle. But then admissibility is not only a time dependent notion. The Principal Principle connects belief about the chances of a given proposition with degree of credence in that proposition, and inadmissible propositions are those that yield a certain kind of information *about the given proposition*. Propositions are admissible or inadmissible with respect to other propositions.

Consider once again the coin toss. Direct evidence about the outcome of the toss is inadmissible. But if it is believed that there is temporally backward causation, and that an outcome of heads determinately causes some event *E* to happen in the past, then the information that *E* has occurred may be inadmissible. In contrast consider some event which will occur after the coin toss and suppose that it is not believed that there is any correlation between this event and the outcome of the toss. Then beliefs about this are admissible, even though it occurs in the future. Lewis says that in general, propositions about events prior to *t* are admissible at *t*, but suggests that this would be false were we to believe that there is backward causation. It is only our belief that there is no backward causation that makes the temporal categorization of admissibility tenable; and I suggest that this is only because admissibility is always relative to a proposition, and a subject's disbelief in backward causation severely limits the way in which one can obtain inadmissible evidence about propositions that concern the future.<sup>11</sup>

So admissibility is not a property of single propositions at a time, but rather a relationship that holds between propositions. A proposition is inadmissible with respect to another proposition if it provides direct enough evidence about it. This is where Lewis's argument goes wrong. The special case of the Principal Principle states that  $C(A/T_w H_{tw}) = P_{tw}(A)$ . Lewis used to believe that this holds for any *A*, since he believed that  $H_{tw}$  is admissible with respect to any such *A*, and  $T_w$  is admissible with respect to any proposition. But this is not so. If one has a *JC* view of chance, then  $T_w$  itself provides direct evidence about the future, since on such a view accepting  $T_w$  forces one to rule out futures which would undermine belief in  $T_w$ . Hence,  $T_w$  cannot be admissible with respect to such futures. But Lewis's "proper argument" depends upon  $T_w$  being admissible with respect to those very futures. Let *F* again be an undermining future. We then have: (i)  $C(F/T_w H_{tw}) = 0$ , but (ii)  $P_{tw}(F) = r > 0$ . The special case of the Principal Principle in conjunction with (ii) does not yield that  $C(F/T_w H_{tw}) = r$ , since on *JC* views of chance  $T_w$  is inadmissible with respect to any undermining future *F*. The derivation of the contra-

<sup>11</sup> For a detailed discussion of backward causation and chance, see Hall (1994).

diction is blocked by the recognition that admissibility is a relation.<sup>12</sup> However, this recognition suggests other problems for *JC* views of chance.

## V

The argument from the Principal Principle is blocked because on *JC* views of chance that admit undermining futures, the theory of chance is inadmissible with respect to them. So far, we have only established that the chances are inadmissible with respect to propositions about segments of future history long enough to rationally undermine belief in the theory of chance. However, we might have intuitively thought that the Principal Principle should always apply where credence about the future is concerned. We know already that this is false for an agent who believes that the future can be foretold. Still, setting aside belief in soothsayers, backward causation *et al.*, it seems plausible that all credence about the future should reflect beliefs about the chances, that this is the normative force of chance. If this is so then any *JC* construal of chance will count as revolutionary, placing more weight on the justification for such a view.

We have the following datum: whenever we actually make use of the concept of chance, credence should reflect chance where the future is concerned. When we are concerned with whether the outcome of a measurement of an electron's spin will be up or down, the subjective probability of the outcome should equal its chance. There are at least two relations that our actual practice could have to the view that credence should always reflect chance where the future is concerned. First, our actual practice may result from the fact that we take this view to express the normative force of chance; our practice is as it is because we are guided by a general principle—because it is constitutive of our concept of chance that chance has this normative force. Second, the view could be an extrapolation of a normative ideal from our practices. We look at our seemingly unrestricted use of the Principal Principle where the future is concerned and theorize that the best explanation for this is that such use of the principle is completely unrestricted. Rather than resulting from an ideal that we already hold, our practices naturally suggest an ideal to us when we reflect upon them.

These two cases are extremes and, as almost always, the middle ground is not unoccupied. Although it may be strictly speaking false to say that a certain prin-

<sup>12</sup> The relational nature of admissibility undermines Lewis's claim that the Principal Principle captures all that we know about objective chance. Lewis argues for this claim by showing that the things we take ourselves to know about chance—e.g., that it obeys the axioms of probability theory, that present chances are obtained from past chances by conditionalising on prior history, that the past has no chance of being false, that it is rational to postulate chances which mirror relative frequencies—can be derived from the Principal Principle. But each derivation depends upon the chances being admissible with respect to long stretches of future history. Since this is not so on *JC* views of chance, if the Principal Principle captures all that we know about chance, then we will have to know that *JC* views are false. But we certainly do not know this. Many of us don't even believe it.

ciple has guided our practices, postulating that it is a normative ideal may be a natural and immediate result of reflecting upon our practices. The more a certain ideal immediately presents itself to us, the more likely we are to say that it is constitutive of the relevant concept. Compare: when asked whether you believe a proposition that you have never considered, the less thought it takes to determine that it is credible, the more likely you are to say that you've believed it all along.

That credence should always reflect chance where the future is concerned naturally suggests itself. Reflection reveals that this is too simplistic; we need only imagine an agent who believes in soothsayers to see that the Principal Principle need have no special hold on the future. Thus, our first inclinations need to be revised. But any such revision loses some claim to being constitutive of our notion of chance, in virtue of being a revision.

A natural move is to make information about the future inadmissible. As we have seen, any *JC* version of chance subjects the Principle to greater restrictions; but prior to any discovered *JC* worries we were already in the business of coming up with an ideal that accounts for our practices, and not of fleshing out one which we knew to be responsible for them. Thus, the additional restrictions that *JC* views of chance need to place on admissibility aren't so bad

It would be bad, however, if even our ordinary uses of the Principle Principal needed to be qualified in some way. For then something constitutive of our use of chance is incompatible with *JC* views. We would then have a strong argument against such views. And shouldn't this be the case? If beliefs about the chances provide direct information about the future, then shouldn't they be inadmissible with respect to the future? The answer is that beliefs about chance provide direct information about the future only to the extent that the future might provide direct evidence against the theory of chance. Even on *JC* views of chance, the small portions of history that we are ordinarily concerned with need have no such potential. To be acceptable, *JC* views need only make sure that small segments of history do not provide evidence against the theory of chance.

Unfortunately, many analyses of chance do not have this feature. Consider simple frequentism—the view that the chances just are the relative frequencies. If history is finite, then any difference in chancy outcomes might change the relative frequencies, and hence be inconsistent with the true theory of chance, thus any non-actual but probabilistically possible future might be an undermining future. So the chances are never admissible and hence the Principal Principle is never applicable. Since the Principal Principle is constitutive of the function of chance, it seems that analyses of chance according to which the chances are sensitive to minor differences in future history are untenable.

However, there might be a way out. Our actual assignment of credence is generally vague. The Principal Principle seems plausible because it seems that it is often rationally required that credence reflect beliefs about chance. Perhaps the requirement is only that credence must often be very close to what one believes the chances to be—often so close that the vagueness of our opinion masks the difference. On this view, the Principal Principle is an approximation, and an

adequate view of chance must only be consistent with the approximate truth of the Principal Principle with respect to short stretches of history. Two points remain. First, it would be nice to know exactly what the Principal Principle is an approximation to. Second, to the extent that we are inclined to view the Principal Principle as underwriting our practice, a view that can only countenance it as an approximation is a revolutionary view—perhaps revolutionary enough that its motivation calls for careful consideration. See the companion papers by Hall and Lewis for illuminating discussions of both these points.<sup>13</sup>

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